Journal of Development Economics 171 (2024) 103311

Contents lists available at ScienceDirect

Journal of Development Economics

journal homepage: www.elsevier.com/locate/devec

Regular article

Information frictions, belief updating and internal migration: Evidence from Ghana and Uganda[☆]

Sarah Frohnweiler[a,*], Bernd Beber[b], Cara Ebert[b]

[a] RWI - Leibniz Institute for Economic Research, University of Göttingen, Germany
[b] RWI - Leibniz Institute for Economic Research, Germany

ARTICLE INFO

Dataset link: https://fdz.rwi-essen.de/en/doi-detail/id-107807fbeinfotreat2024v1

JEL classification:
J31
J68
O15

Keywords:
Income differentials
Migration decision
Belief updating

ABSTRACT

Information frictions about benefits of migration can lead to inefficient migration choices. We study the effects of randomly assigned information treatments concerning regional income differentials in Ghana and Uganda to explore participants' belief updating and changes in internal migration intentions, destination preferences, and actual migration. Treated participants prefer higher income destinations, while effects on intent plausibly follow subjects' initial under- or overestimation of potential gains, with asymmetric updating propensities. Effects persist for 18 months, and discussions with others about migrating increase, but actual migration does not. Knowledge about income affects intentions and destination choices, but barriers to actual relocation are complex.

One in eight people around the globe are internal migrants (UNDP, 2009). This is four times the number of international migrants, and this figure is even higher in many developing countries. At the same time, large gaps in income, consumption, and the value of non-monetary factors exist within and across countries (e.g., Acemoglu and Dell, 2010; Young, 2013; Gollin et al., 2014) and closing these gaps through further migration is expected to improve overall economic outcomes (e.g., Bryan and Morten, 2019; Tombe and Zhu, 2019; Lagakos et al., 2023).[1] What can explain this unused migration potential? One explanation could be that the costs of migration – such as leaving behind family and friends, adverse living conditions at destination, or uncertainty about migration outcomes – are outweighing income gains in earnings. Another hypothesis, often posed in the literature, is that individuals lack knowledge about income differences, but would migrate if only the (perceived) economic returns were higher.

We study how information frictions about regional incomes and income expectations affect internal migration intentions, destination preferences, and migration behavior in urban Ghana and Uganda.[2] We measure biases in beliefs about regional incomes and investigate how providing information on regional incomes can affect internal migration decisions. Because our study participants originate from urban areas, we argue that amenities at destinations are relatively more comparable to those at origins in our study contexts than would

☆ We would like to thank Krisztina Kis-Katos, Jasper Tjaden, Ferdinand Rauch, and the seminar and conference participants at MoPED, NOVAFRICA, GDE, CSAE, EALE, and VfS among others that have commented on earlier versions of this study for their helpful feedback. We further thank our enumerator teams in Ghana and Uganda for their hard work and especially our consultant Peter Anti Partey for his excellent field support. Data collection was carried out via the projects RéUsSITE in Ghana, funded by the German Federal Ministry for Economic Cooperation and Development (BMZ), and Employment and Skills for Development in Africa (E4D) in Uganda, funded by BMZ, the European Union, the Norwegian Agency for Development Cooperation (Norad), the Korean International Cooperation Agency (KOICA), the Department for International Development (DFID) of the United Kingdom and the companies Sasol, Quoniam Asset Management, and Shell. IRB approval was obtained from the Mildmay Uganda Research Ethics Committee (REC REF 0209-2019), the University of Ghana Ethics Committee (ECH 068/20-21), and the German Institute for Global and Area Studies Ethics Committee (GIGA 03/2020). The project was registered in the AEA RCT Registry before the intervention started (AEARCTR-0006733). The authors declare that they have no relevant or material financial interests related to the research described in this paper.

* Corresponding author.
E-mail addresses: sarah.frohnweiler@rwi-essen.de (S. Frohnweiler), bernd.beber@rwi-essen.de (B. Beber), cara.ebert@rwi-essen.de (C. Ebert).

[1] The equalizing effect of internal migration is likely limited by individuals sorting into specific regions. Nevertheless, studies have shown that regional differences persist even after controlling for sorting. See Lagakos (2020) for a comprehensive overview.

[2] In Uganda, not all participants live in urban areas. 14% live in a rural area and 18% live in a peri-urban area.

https://doi.org/10.1016/j.jdeveco.2024.103311
Received 25 January 2024; Received in revised form 1 May 2024; Accepted 8 May 2024
Available online 14 May 2024

typically be the case for rural–urban migration (Fafchamps and Shilpi, 2013; Gollin et al., 2021; Lagakos et al., 2023). This enables us to home in on the role of income expectations in the migration calculus.[3]

First, we introduce a basic theoretical framework for belief updating about incomes at destination in response to regional income information and following changes in migration intentions. Second, we test the predictions empirically using two experiments with 6249 participants in Ghana and Uganda. In both countries, internal migration is common. Twenty-five and twenty percent of household heads in Ghana and Uganda, respectively, live in another region or district than their place of birth (IPUMS, 2002, 2010). Incomes and wages differ substantially across regions within countries. In Ghana, the average income of the wealthiest region, Greater Accra, is more than six times the income of the poorest Upper East region. In Uganda, average wages are 2.5 times higher in the wealthiest region, Kampala, than in the poorest regions, Western and Eastern Uganda. In the experiment, we randomly provide information on regional incomes for the different regions of the respective country, including the region of residence. The income information was drawn from each country's most recent and publicly available official statistics, i.e., the Ghana Living Standards Survey of 2017 and the Uganda National Household Survey 2016/17. We then measure the information's impact on individual migration intentions and destination preferences immediately after the intervention and on migration behavior 18 months later.

Our results show that study participants have biased perceptions about regional incomes at baseline. Participants overestimate income for all regions. In Uganda, overestimation is even more pronounced than in Ghana, with baseline beliefs more than doubling the actual value for some regions. Providing participants with information about regional incomes appears to reduce informational inefficiencies: Participants from both countries who received the information are significantly more likely to correct their destination preferences towards destinations reported to have higher incomes. In Ghana, the probability of selecting the highest-income region as the first destination increases by 3.3 percentage points (6.3% relative to the control mean) and in Uganda by 12.5 percentage points (46.5% relative to the control mean).

A more nuanced picture presents itself for migration intentions. In the full samples, treatment leads to a substantial decline in migration intentions among Ugandans, and no immediate effect in Ghana. However, estimating these effects separately for participants who initially overestimated or underestimated regional income differentials reveals effects that plausibly follow from these initial beliefs as well as notable asymmetries in the updating process. For Ugandans who overestimated potential gains due to regional income differences (i.e., for whom the provided information should have a discouraging effect), treatment reduces their intentions to migrate, driving the effect in the full sample. In Ghana those who initially underestimated potential gains due to differences in incomes (i.e., for whom information should have an encouraging effect) appear immediately responsive to treatment and increase their migration intentions. For destination preferences, too, updating propensities vary with prior beliefs, and the strong overall effects are the result of individuals who initially underestimated potential income gains changing their destination preferences and selecting higher income regions upon treatment.

A follow-up in Ghana shows that treatment effects persist even eighteen months later. Treated subjects continue to express a preference for higher income destination regions compared to the control group, and they are more likely to have discussed plans to internally migrate with family and friends. However, the intervention does not impact decisions to actually migrate. Even in the presence of sizable regional income differentials and an information intervention that persistently affected individuals' destination preferences, the treatment did not cause individuals to actually relocate. While expected income gains (or losses) evidently play an important role when individuals are considering whether they would like to move in principle and to where they might like to move, the decision to actually do so is more complex and arguably shaped more by barriers to relocation such as fears of worse living arrangements or family attachments than by informational deficiencies.

The study contributes to the existing literature in three ways. First, our paper speaks to the literature on belief updating. While Bayes' rule is broadly appreciated as a benchmark for updating behavior under uncertainty within social sciences, extant theory and evidence indicate that individuals sometimes process information asymmetrically by allocating more weight to good than bad news. Several studies have tested this hypothesis across different contexts, with highly heterogeneous results. While some studies suggest stronger responsiveness to good news (e.g., Sharot et al., 2012; Wiswall and Zafar, 2015; Möbius et al., 2022; Masatlioglu et al., 2023), others find stronger responsiveness to bad news (e.g., Coutts, 2019), and some do not find any evidence for asymmetrical updating (e.g., Barron, 2021). Our design allows to differentiate between individuals who should have perceived the provided information as migration encouraging and individuals who should have perceived it as discouraging. The results show particularly clearly that in our context of internal migration, encouraging information is able to persistently update individuals' destination preferences whereas discouraging information is not.

Second, we study information frictions as one relevant barrier to optimal internal migration. Since internal migration is much more prevalent and less expensive than international migration, information frictions might be more relevant for explaining unexploited returns to internal migration than they are for international migration. However, the literature on information frictions tends to focus on international and especially irregular migration. These studies often find that (potential) migrants have incomplete or biased knowledge of the risks of dying *en route*, the probability of obtaining legal residence status in Europe, wages at destination points, or the quality of placement agencies among other aspects (e.g., Beam et al., 2016; Bah and Batista, 2020; Shrestha, 2020; Bazzi et al., 2022; Tjaden and Gninafon, 2022), whereas Beber and Scacco (2022) show that potential migrants are better informed about international destinations than many information campaigns assume. Bryan et al. (2014), Baseler (2023), and Gao et al. (2023) are exceptions as they too study information frictions in the context of internal migration. Gao et al. (2023) show that information frictions about air pollution were reduced through a national pollution monitoring and disclosure program in China and that urban to urban migration responded to the removal of the information frictions. Bryan et al. (2014) and Baseler (2023) randomly provided rural households with information about earnings and employment opportunities at urban destinations. Whereas Baseler (2023) documents an increase in internal migration in Kenya, Bryan et al. (2014) find no impact of the provided information in Northern Bangladesh and argue that households seem well informed about the benefits of internal migration from the outset. Our analysis instead looks at internal migration intentions of mostly young male and skilled individuals living in urban areas. This is a population that is typically perceived to be very mobile and likely to take advantage of potential economic gains, whereas in rural contexts migration is often studied in relation to seasonal hunger or economic needs as a push factor of migration.

Third, we provide experimental evidence on the importance of economic conditions at destination as a variable in the migration calculus. While income differences have been identified as a key explanation for migration, they are far from the only element in a

[3] Of course there are differences in amenities across urban areas too, but they are much less stark than when migrating from rural areas to cities in Uganda and Ghana. We provide evidence in this regard in Appendix VIII, using an amenity index constructed based on the most recent Afrobarometer and DHS data as well as available air quality information. Living conditions of migrants and non-migrants within cities may differ and will remain a relevant decision factor.

complex decision (e.g., de Haas, 2010) and their specific effects are challenging to identify. A nascent experimental literature uses discrete choice experiments to study trade-offs between different decision factors in moving to a foreign country or place. Laboratory studies in Slovakia, Lisbon, and Nairobi show that while wages and living costs are important decision factors, they together make up only about a third to a half of decision weights (Baláž et al., 2016) and that risk of unemployment, liquidity constraints, and legal status are also important factors (Batista and McKenzie, 2023; Detlefsen et al., 2022). Bryan et al. (2014) and Baseler (2023) study the role of potential income gains for internal rural to urban migration decisions in Bangladesh and Kenya and find mixed evidence for the role of the earnings potential at destination, as discussed above. In a survey experiment in Bangladesh, Lagakos et al. (2023) find that internal migrants care most about the probability of unemployment and living conditions, less about wages, and conclude that people face a large non-monetary disutility from migrating from rural to urban areas. Our experiment contributes to that literature by testing income as a decision factor for internal migration decisions in terms of *whether* and *where* to migrate in a population that originates in urban areas and therefore a population for whom amenity trade-offs are less relevant and labor market conditions more similar to their origin compared to rural to urban migration.[4] We show that income beliefs shape individuals' preferences about whether and where to go, but do not affect actual migration. We conclude that binding disutilities from migrating, such as lower quality accommodation or uncertainties revolving around life at destination, are not limited to rural–urban contrasts but likely exist among potential migrants in urban areas.

In migration policy, information campaigns are a common and broadly implemented tool. Between 2014 and 2019, over 100 migration information campaigns were commissioned by EU Member States and the European Commission addressing origin and transit countries (Hahn-Schaur, 2021). Yet rigorous evidence on their impact is scarce, and researchers have criticized that the implementation of migration information campaigns has outpaced any rigorous assessment of their effectiveness (e.g., Alpes and Nyberg Sørensen, 2015; Schans and Optekamp, 2016; Tjaden et al., 2018). While our study regards internal migration in a specific urban sample with similar culture, language, and amenity traits of potential destinations, our results are likely informative for the mechanisms that underlie migration-relevant information updating and the design of information campaigns elsewhere.

The remainder of the study is structured as follows. In Section 1 we outline the underlying conceptual framework of the intervention and its expected effects. In Section 2 we explain the design and implementation of the information experiment as well as the data. In Section 3 we describe the estimation strategy. In Section 4 we discuss the immediate impact of the information treatment on migration preferences and potential mechanisms. In Section 5 we present the long-term impacts of the information treatment on migration preferences and behavior. We offer concluding remarks and policy recommendations in Section 6.

1. Conceptual framework

Migration decisions depend on a multitude of observable and unobservable characteristics of individuals and households including wealth, employment opportunities, information, abilities, risk preferences, ambitions, and family ties. Yet income differences have been singled out as one of the key explanatory factors of migration both theoretically and empirically (e.g., de Haas, 2010). This study addresses the importance of expected income at destination within this complex decision calculus of migration. Classical economic theory predicts that a rational individual intends to migrate to another region ($Y = 1$) if the expected net present value from migrating V^m is positive (e.g., Burda et al., 1998). V^m is positive if the expected income differential $\hat{D}_i$ between the destination region z with the highest expected income $\hat{I}_{i,z}$ (i.e., $\max_z \hat{I}_{i,z}$) and the expected income at origin $\hat{I}_{i,o}$ exceeds the associated costs C_i. Here C_i includes all other migration-relevant factors apart from income. The decision rule for *whether* to migrate can be formally written as

$$Y_i = \begin{cases} 1 & \text{if } V_i^m = \hat{D}_i - C_i > 0 \\ 0 & \text{otherwise} \end{cases} \tag{1}$$

with $\hat{D}_i = (\max_z \hat{I}_{i,z}) - \hat{I}_{i,o}$ and $z \neq o$.

The subsequent decision on *where* to migrate can be formalized by a multi-market Roy (1951) model of mobility and earnings. As done by Borjas (1992) and Dahl (2002) the model can be adapted such that individuals do not choose among occupations but among different migration destinations z. Further, Lee (1983) showed that in a multi-choice selection model the error terms can be summarized by the maximum order statistic. Drawing on this insight, one can expect only the first-best choice to matter in optimal decision-making (or the next-best among any remaining options). In our setting in which individuals have to indicate their top two destination preferences (Z^1 and Z^2) among all regions excluding the region of origin o, this implies that individuals should select the region with the highest expected income as first preference and the region with the second highest expected income as second preference:

$$\begin{aligned} Z_i^1 &= \arg\max_z \hat{I}_{i,z}, \text{ and} \\ Z_i^2 &= \arg\max_{z \neq Z_i^1} \hat{I}_{i,z}. \end{aligned} \tag{2}$$

In our experiment, we elicit an exogenous updating of income expectations among study participants by providing a random subsample of subjects with information about true average regional incomes. We hypothesize that treated individuals update their region-specific income expectations based on the information they receive. If their prior expectations deviated from the true maximum regional income differential, this implies a change in the expected potential income gain (or loss). Formally,

$$\Delta\hat{D}_i = \left[(\max_z I_z) - I_o\right] - \left[(\max_z \hat{I}_{i,z}) - \hat{I}_{i,o}\right]. \tag{3}$$

The first term on the right-hand side gives the true maximum income differential (D_i), and the second term is the initially expected maximum income differential ($\hat{D}_i$). If treated individuals update their beliefs based on the information provided, the expected income differential will increase among individuals whose prior expectation was lower than the true maximum and decrease if an individual's initial expectation was higher. In turn, this affects migration intent, following Eq. (1). We expect migration intentions to intensify among treated individuals whose expected maximum income differential rises, and to lessen among those whose expected maximum income differential declines. We further anticipate that the provided information will change destination preferences if it gives individuals a reason to update the top of their regional income ranking. Destination choices will not change if initial expectations about the ranking reflect the actual income ranking, irrespective of income level expectations.

The literature on belief updating remains divided on whether individuals process information asymmetrically and if yes whether more weight is allocated to positive or to negative information. We test this by distinguishing individuals who initially underestimate income differentials, and therefore receive migration encouraging information, and individuals who initially overestimate income differentials, and therefore receive migration discouraging information.[5]

[4] We provide suggestive evidence in Appendix VIII that this is in fact the case for our contexts of Ghana and Uganda, but note that urban–urban migration elsewhere, for example in parts of Asia, can be amenity-driven (Khanna et al., 2021).

[5] We measure belief updating indirectly by eliciting Y_i, Z_i^1, and Z_i^2 post-treatment, which are valid measures of treatment-induced changes in $\hat{I}_{i,\{z,o\}}$

2. Experimental design and data

2.1. Sample selection

Participants of the experiment formed part of two impact evaluations assessing the effectiveness of distinct employment and income-promoting programs in Ghana and Uganda. Both of these programs were implemented by the German agency for international cooperation (Deutsche Gesellschaft für Internationale Zusammenarbeit (GIZ) GmbH). In Ghana, GIZ carried out a vocational skills training program for craftspersons in the construction sector called *Professionalization of Artisans* (ProfArts). Artisans working in the construction sector, aged 18 years and older, and having at least completed an apprenticeship or obtained the formal qualification of Proficiency I could register for the program by completing a comprehensive interview. Registration was possible in the capital cities of Greater Accra, Ashanti, Western, and Northern regions between November and December 2020 (Batch I) and August and September 2021 (Batch II). These interviews are used as the baseline survey for a randomized controlled trial on the effectiveness of the training program and included the present information experiment.

In Uganda, GIZ implemented a different skills promoting program called *Skills for Construction* (S4C) consisting of a certified training in soft, life and technical skills targeted to the construction sector and a subsequent internship placement. The S4C program targeted Ugandan youths aged 18 to 24 years with basic numeracy and literacy skills and, ideally, prior experience in the construction sector and previous training at a technical vocational and educational training (TVET) institute. The age criterion was not enforced and participants ranged between 18 to 40 years. The S4C program was also combined with an impact evaluation and study participants consisted of individuals who had registered their interest in participating in the S4C program. In November and December 2020, one to two years after the program was implemented, study participants were followed up for an endline survey, which included the information experiment.

Our sample for the information experiment consists of applicants for these technical training programs in the construction sector, i.e., predominantly young men with above average educational attainment.[6] While this means that our findings and policy implications may not be generalizable to a broader population, it also means that we focus on a set of individuals that are likely to be responsive when incentives to migrate for work in our study contexts change. As McKenzie and Yang (2022) suggest in a recent review of migration-related experiments, such targeting may be preferable both to improve statistical power and to optimally inform effective policy-making.

Our main sample consists of 5410 observations for Ghana and 828 observations for Uganda.[7] For some observations full information on the outcome variables is missing: 254 participants never answered one or more of the outcome-relevant questions and in 76 cases we lack a pre-treatment response. In total, only 0.53% of the Ghanaian respondents and 0.60% of the Ugandan respondents failed to provide post-treatment information, resulting in a post-treatment non-response rate of 0.54%. The response rate neither depends on treatment assignment nor on pre-treatment outcomes (Tables are available in Appendix II). In Ghana, we further conducted a follow-up survey approximately 18 months after the information provision with 4908 treated and control participants (90.7% of baseline).[8] Again, attrition is independent from treatment assignment and pre-treatment outcomes.

2.2. Intervention

The information experiment took place between November and December 2020 in Uganda and August and September 2021 in Ghana. Prior to treatment, participants were asked about their intentions to migrate internally, their top two destination regions within their country, and their income expectations for the different regions of their country.[9] Then, a random half of the sample received the information treatment from the enumerator who was conducting the interview. Randomization was performed in situ using the software SurveyCTO. Afterwards, and within the same interview, each individual was asked again about intentions to migrate internally and the top two destination regions, irrespective of the assigned treatment status. Eighteen months later we surveyed Ghanaian participants again and asked about their migration intentions, destination preferences, migration preparations, as well as current and past regions of residence. Appendix Figure A.I.1 shows the chronological sequence of the intervention and data collections.

The information treatment was designed to be easy to understand and based on official statistics. We harmonized the intervention as much as possible between the two countries, but the information content and intervention delivery differed slightly due to differences in available official statistics and survey methods. At the regional level, recent representative survey data were available for average cross-sector nominal incomes in Ghana and sex-specific median cross-sector nominal wages in Uganda.[10] These regional income differentials are in line with average regional incomes of our study participants at baseline and with available income information for the construction sector, which is the focus of the employment programs within which this study is embedded. While for both, sample incomes and construction sector incomes, levels and differences tend to be smaller than the average cross-sector incomes, the region with the highest income is the same.

In Uganda, participants were recent graduates with little prior construction sector experience: At the time of our intervention, almost 70% of Ugandan respondents were employed in sectors outside construction. In Ghana the situation is different: All participants were working in the construction sector when they registered for the training program and at the 18-month follow-up only 4% had changed sector. Because we wanted the treatment to consist of comparable information that

within this conceptual framework. We decided against directly measuring $\hat{I}_{i,\{z,o\}}$ post-treatment, because we would not have been able to tell to what extent treated individuals, having just been shown $I_{\{z,o\}}$, provide an actual posterior belief or simply engage in informational recall, whether for reasons of social desirability or because the numbers they were shown are still top-of-mind. Worse, this measurement issue would only affect the treatment group, which means we would not have been able to compellingly attribute differences in this measure between treatment and control to the intervention.

[6] Table A.II.1 compares characteristics of our samples with national sample characteristics.

[7] Originally, 5491 observations were sampled in Ghana and 1158 in Uganda. We dropped 70 observations in Ghana and 34 in Uganda due to missing sociodemographic background characteristics and 11 in Ghana that miss all outcome variables. An additional 296 interviews in Uganda were dropped due to procedural deviations during the first twelve days of data collection as enumerators provided the treatment to all respondents irrespective of the assigned treatment status.

[8] Due to the timing of the experiment, a follow-up was possible in Ghana only. In Ghana, the treatment was incorporated in the baseline survey of an impact evaluation, whereas in Uganda, it was part of an endline survey. The collection of long-term migration intentions and behavior in Ghana was part of the pre-analysis plan.

[9] In Ghana, respondents were only asked about their income expectations for six out of the ten regions. Expectations were always asked for Greater Accra, Ashanti, Western, and Northern region and then for one additional region randomly picked out of the remaining five. For their preferred destination preferences, respondents could select among all existing regions.

[10] We expect our intervention to affect migration preferences through the emphasized regional differences in incomes. Figure A.I.3 shows that the distribution of median income differences is similar to the distribution of mean income differences in Uganda. Based on this and our theoretical framework, we expect belief updating to work similarly when mean or median income information is provided.

is relevant in both countries, and ex-ante we could not rule out the possibility that participants might change to another sector, we provided cross-sector treatment information. Cross-sector incomes can also inform migration decisions of participants' household members. We revisit the implications of using cross-sector incomes in Sections 4.3 and 4.5

Income differences that are consistent with the treatment information remain when controlling for regional consumer price indices, such that potential income gains from migration are unlikely offset by higher living costs. Nominal income rankings also equal the ranking of real incomes.[11,12] Our data further suggests that the earnings potential of migrants and locals is similar in our relatively well-educated study sample. Baseline incomes of individuals who do not reside in their region of birth are slightly higher than incomes of those who did not move. Finally, the use of official figures, as published in reports by each country's statistical bureau, ensures that the information we provide is similar to the kind of information that would be used in related policy interventions or enlightenment campaigns. In Section 4.5 we discuss the relevance of the provided information in more detail.

In Ghana, interviews were conducted face-to-face and treated individuals were shown a map outlining the ten regions of Ghana and depicting the average monthly income in each region based on the main report of the GLSS7.[13] To make the information easily comprehensible, monthly income was presented as a number and illustrated with stacks of coins, with one coin for each 100 GHS. After the implementation of the experiment, we learned that the average per capita income for the Ashanti region is misreported in the GLSS7 main report.[14] With the correct value, Ashanti region ranks behind the regions of Greater Accra, Brong Ahafo, and Central rather than first. The GLSS7 data processing team at the Ghana Statistical Service acknowledged this error in September 2022, and we then immediately debriefed Ghanaian study participants with a set of text messages correcting the income information. For the descriptive statistics we use the corrected Ashanti figure but for the treatment effect analysis, we keep to the information reported in the GLSS7 report and provided in the experiment. We discuss the potential implications of doing so throughout the paper. In Section 5 we show how we can use the downstream rectification of this income figure to validate our results.

In Uganda, treated individuals received sex-specific information on the median monthly wages for Uganda's four different regions plus the capital city Kampala. Together with the income information, enumerators provided an explanation of the concept of a median. The income information was provided in absolute terms and relative to the individuals' region of residence, i.e., how many times more or less the income is compared to the region they live in. The survey was conducted over the phone and thus no map could be shown. Instead enumerators explained the information verbally. The income information was gathered from the 2016–17 Ugandan National Household Survey (UNHS).[15] The infographic for Ghana (Figure A.I.2) and an example script for Uganda can be found in Appendix I.

Enumerators were instructed to never directly link the provided information to participants' migration preferences and to always present income details in a neutral fashion, without insinuating "right" or "wrong" responses. Implementing the experiment in both contexts lends external validity to our findings. However, due to the differences in the intervention and the study population, we refrain from drawing conclusions from comparisons of treatment effects across the two countries.

2.3. Outcome variables

Our analysis includes four main outcomes: internal migration intentions, destination preferences, migration preparations, and migration behavior.[16] The first two outcomes were measured immediately after the information provision in Ghana and Uganda and 18 months after in Ghana. The latter two outcomes were measured only in Ghana 18 months after the information treatment.

Internal migration intentions is measured by individuals' self-reported interest in moving either temporarily or permanently to live in another region within the country of residence, which ranges from zero ("Not at all") to one ("A lot") on a 4-point Likert scale. For destination preferences, we consider three main variables. One indicator variable each for whether the first, second, or both preferred destinations were selected according to the first-highest, second-highest, or first and second-highest possible income differential between the home region and all potential destinations, respectively. We also pre-registered three alternative ways to measure destination preferences, by using (i) the actual logarithmized income in USD of the preferred destinations, (ii) the income rank of the preferred destinations, and (iii) an indicator variable for whether the preferred destination has a higher income than the region of residence.

Migration preparations were collected by asking "What kind of planning or preparation have you done to move to another region within Ghana to live?". Enumerators prompted all potential answer options and selected all realized preparations.[17] Out of the selected options we construct an index varying between zero and one. We identify internal migration behavior of respondents if they live in another region within Ghana at the time of the follow-up survey than during the implementation of the experiment. We further asked respondents "how many members of [their] household moved to another region within Ghana since our last interview" and construct an indicator variable equal to one if at least one member migrated. In Section 4.4 we assess the potential influence of experimenter demand effects for self-reported outcomes.

2.4. Summary statistics

In Table 1 we compare individuals of treatment and control groups of the estimation sample in Ghana and Uganda. Columns (3) and (7) show only small differences in socio-economic characteristics and pre-treatment outcomes, and only few of these differences are significant, suggesting that the randomization was successful. Treatment and

---

[11] We use consumer price indices as reported in the GLSS7 microdata and by the Ugandan Bureau of Statistics.

[12] In Uganda, the mobility section was followed by a debriefing in which enumerators explained that not only wages but also costs of living differ across regions and encouraged respondents to obtain additional information before migration decisions will be made.

[13] In 2018, six new regions were added to what had been ten Ghanaian regions. The regions of Brong-Ahafo, Northern, Volta, and Western were split up in two or three sub-regions. Since the 2016–17 GLSS contained income information only for the original ten regions, the infographic only depicted those ten regions. However, individuals could choose among all 16 regions when asked about their destination preferences.

[14] Average annual income for the Ashanti region as reported in the GLSS7 is 56,664 GHS, whereas the correct figure is 11,635 GHS.

[15] For the sake of brevity, we subsequently refer to income information when we mean average monthly income for Ghana and median monthly wages, i.e., labor income, for Uganda.

[16] de Haas et al. (2019) define migratory mobility as changes of residence across administrative borders, including internal and international borders and temporary or permanent residence changes. It excludes, for example, commuting, tourism and business travel. We follow this definition but limit our study to potential internal migrants who (intend to) cross regional borders within Ghana and Uganda, either temporarily or permanently. Other definitions of migration exist: for example, Bilsborrow (2016) requires migration to be a change of the usual or permanent residence and therefore excludes seasonal mobility from his definition of migration.

[17] The prompted answer options covered (i) have discussed plans with family members, (ii) have discussed plans with friends, (iii) have started to save money, (iv) have taken out a loan, (v) started looking for a job, and (vi) any other preparations with a text field to specify.

Table 1
Balance checks across treatment and control groups.

	Ghana				Uganda				Uganda-Ghana	
	Control (1)	Treatment (2)	Diff. (3)	P-value (4)	Control (5)	Treatment (6)	Diff. (7)	P-value (8)	Diff. (9)	P-value (10)
Socio-demographic characteristics										
Age	33.63	33.63	−0.00	0.99	25.87	25.48	0.38	0.14	−7.96	0.00
	(0.18)	(0.17)	(0.24)		(0.20)	(0.18)	(0.26)		(0.32)	
Male	1.00	1.00	-0.00	0.38	0.87	0.87	0.00	0.99	−0.13	0.00
	(0.00)	(0.00)	(0.00)		(0.02)	(0.02)	(0.02)		(0.01)	
Married, binary	0.55	0.52	0.04	0.01	0.21	0.18	0.02	0.41	−0.34	0.00
	(0.01)	(0.01)	(0.01)		(0.02)	(0.02)	(0.03)		(0.02)	
Unemployed	0.02	0.02	−0.00	0.39	0.29	0.26	0.03	0.40	0.26	0.00
	(0.00)	(0.00)	(0.00)		(0.02)	(0.02)	(0.03)		(0.01)	
Employed, employee	0.43	0.41	0.02	0.09	0.51	0.56	−0.05	0.14	0.12	0.00
	(0.01)	(0.01)	(0.01)		(0.02)	(0.02)	(0.03)		(0.02)	
Employed, selfemployed	0.55	0.57	−0.02	0.14	0.15	0.15	0.01	0.77	−0.41	0.00
	(0.01)	(0.01)	(0.01)		(0.02)	(0.02)	(0.02)		(0.02)	
Written contract	0.51	0.53	−0.02	0.64	0.25	0.25	0.01	0.83	−0.27	0.00
	(0.03)	(0.03)	(0.05)		(0.03)	(0.03)	(0.04)		(0.03)	
Oral agreement	0.48	0.45	0.03	0.52	0.47	0.43	0.04	0.42	−0.02	0.48
	(0.03)	(0.03)	(0.05)		(0.03)	(0.03)	(0.05)		(0.03)	
No agreement	0.00	0.01	−0.01	0.36	0.28	0.33	−0.05	0.28	0.30	0.00
	(0.00)	(0.01)	(0.01)		(0.03)	(0.03)	(0.04)		(0.02)	
No formal education	0.04	0.04	−0.01	0.35	–	–	–	–	–	–
	(0.00)	(0.00)	(0.01)							
Primary	0.09	0.08	0.01	0.17	0.03	0.02	0.01	0.37	−0.06	0.00
	(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)		(0.01)	
Junior secondary	0.48	0.47	0.01	0.55	0.14	0.19	−0.06	0.03	−0.31	0.00
	(0.01)	(0.01)	(0.01)		(0.02)	(0.02)	(0.03)		(0.02)	
Senior secondary	0.33	0.35	−0.02	0.11	0.28	0.26	0.02	0.59	−0.07	0.00
	(0.01)	(0.01)	(0.01)		(0.02)	(0.02)	(0.03)		(0.02)	
TVET	0.03	0.03	0.00	0.47	0.46	0.44	0.01	0.72	0.42	0.00
	(0.00)	(0.00)	(0.00)		(0.02)	(0.02)	(0.03)		(0.01)	
Tertiary	0.03	0.03	0.00	0.42	0.10	0.08	0.02	0.42	0.06	0.00
	(0.00)	(0.00)	(0.00)		(0.01)	(0.01)	(0.02)		(0.01)	
Household asset index (mean)	0.47	0.48	−0.01	0.14	0.43	0.41	0.01	0.48	−0.05	0.00
	(0.00)	(0.00)	(0.01)		(0.01)	(0.01)	(0.02)		(0.01)	
Income total, monthly (USD)	461.20	420.16	41.03	0.57	101.69	77.59	24.11	0.12	−350.41	0.00
	(73.03)	(19.86)	(72.88)		(15.08)	(5.28)	(15.69)		(94.74)	
ProfArts, tech treatment	0.69	0.66	0.02	0.09	–	–	–	–	–	–
	(0.01)	(0.01)	(0.01)							
ProfArts, non-tech treatment	0.34	0.34	−0.00	0.89	–	–	–	–	–	–
	(0.01)	(0.01)	(0.02)							
S4C, treatment	–	–	–	–	0.58	0.57	0.00	0.89	–	–
					(0.02)	(0.02)	(0.03)			
Accra (GH)	0.31	0.32	−0.01	0.50	–	–	–	–	–	–
	(0.01)	(0.01)	(0.01)							
Kumasi (GH)	0.26	0.26	0.00	0.91	–	–	–	–	–	–
	(0.01)	(0.01)	(0.01)							
Takoradi (GH)	0.30	0.30	−0.01	0.69	–	–	–	–	–	–
	(0.01)	(0.01)	(0.01)							
Tamale (GH)	0.13	0.12	0.01	0.18	–	–	–	–	–	–
	(0.01)	(0.01)	(0.01)							
Kampala (UG)	–	–	–	–	0.36	0.37	−0.01	0.75	–	–
					(0.02)	(0.02)	(0.03)			
Central (UG)	–	–	–	–	0.24	0.18	0.06	0.02	–	–
					(0.02)	(0.02)	(0.03)			
Eastern (UG)	–	–	–	–	0.06	0.06	−0.00	0.82	–	–
					(0.01)	(0.01)	(0.02)			
Northern (UG)	–	–	–	–	0.26	0.33	−0.07	0.04	–	–
					(0.02)	(0.02)	(0.03)			
Western (UG)	–	–	–	–	0.07	0.06	0.01	0.43	–	–
					(0.01)	(0.01)	(0.02)			
Joint F-stat.	–	–	–	0.895	–	–	–	0.534	–	–

(*continued on next page*)

control observation are also balanced across regions and assignment status to the ProfArts and S4C training program in Ghana and Uganda, respectively. F-tests for the joint significance of baseline characteristics suggest that treatment and control groups are similar prior to the treatment.

The last two columns of Table 1 contrast the total samples of Uganda and Ghana and highlight the differences between study participants of the two countries in terms of age, gender, employment status, and education, among others. Subsequent analyses and interpretations of the results are done for the two countries separately.

In both countries, participants are very interested in internal migration already prior to treatment (Appendix Figure A.II.1). In all regions, more than 75% of participants indicate that they want to migrate to another region within their country either "a lot" or "a fair amount". This matches with nationally representative surveys like Gallup World Poll or Afrobarometer showing that the desire to migrate is highest in

Table 1 (*continued*).

Pre-treatment outcomes										
Migration intention	0.77	0.76	0.01	0.45	0.82	0.83	−0.01	0.52	0.06	0.00
	(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.02)		(0.01)	
1st choice mirrors income ranking	0.44	0.45	−0.00	0.80	0.20	0.22	−0.02	0.43	−0.24	0.00
	(0.01)	(0.01)	(0.01)		(0.02)	(0.02)	(0.03)		(0.02)	
2nd choice mirrors income ranking	0.26	0.25	0.01	0.42	0.23	0.25	−0.01	0.66	−0.02	0.32
	(0.01)	(0.01)	(0.01)		(0.02)	(0.02)	(0.03)		(0.02)	
1st and 2nd choice mirror ranking	0.18	0.17	0.01	0.47	0.03	0.06	−0.03	0.08	−0.13	0.00
	(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.02)		(0.01)	
Ln(monthly income, USD)	6.08	6.05	0.02	0.17	3.64	3.67	−0.04	0.11	−2.41	0.00
	(0.01)	(0.01)	(0.02)		(0.01)	(0.02)	(0.02)		(0.02)	
Income ranking	8.06	8.02	0.04	0.38	7.66	7.77	−0.11	0.11	−0.32	0.00
	(0.04)	(0.03)	(0.05)		(0.05)	(0.05)	(0.07)		(0.06)	
Higher income	0.56	0.56	0.00	0.90	0.38	0.39	−0.01	0.81	−0.18	0.00
	(0.01)	(0.01)	(0.01)		(0.02)	(0.02)	(0.03)		(0.02)	
Joint F-stat.	–	–	–	0.454	–	–	–	0.377	–	
N	2586	2824	5410		409	416	825		6235	

Note: Table shows averages measured at baseline using all observations with full information on control variables. Observations with partially missing information on outcome variables were kept. Diff. refers to the differences in means between control and treatment group, with standard errors in parentheses below, and p-values for a test of the difference being equal to zero on the right. Joint F-stat. refers to F-statistics of a joint orthogonality test of the respective treatment arm indicator on the variables as diplayed in the table.

African countries, both in absolute and relative numbers (e.g., Laczko et al., 2017; Appiah-Nyamekye Sanny et al., 2020). A large share of respondents was born in another region than the one in which they currently live, although proportions vary substantially across regions (Appendix Figure A.II.2).

3. Estimation strategy

We estimate the average treatment effect of the information intervention on different outcomes using variations of the following model:

$$y_{i,1} = \beta_0 + \beta_1 Info_i + \beta_2 y_{i,0} + X'_{i,0}\gamma + \delta_o + \varepsilon_i, \tag{4}$$

where $y_{i,1}$ is the outcome for individual i at post-treatment time $t = 1$ and $Info_i$ is an indicator for whether individual i received the information treatment. We control for the pre-treatment outcome $y_{i,0}$ when available and a vector of covariates $X'_{i,0}$ including age, sex, marital status, employment status, education, and wealth. We additionally control for region of origin fixed effects, δ_o. The average treatment effect of the information treatment is given by β_1. We use robust standard errors to correct for heteroskedasticity.

To account for the categorical nature of most outcomes for migration intentions and destination preferences, we also run ordered logit regressions to assess effects on migration intentions and multinomial logit and probit regressions for the impact on destination preferences in addition to the main linear probability model estimations.

As described in our conceptual framework, we estimate Eq. (4) separately for individuals who initially underestimated the true maximum income differential between their region of origin and the destination region with the highest income and for individuals who initially overestimated the true maximum income differential to examine individuals' belief updating behavior and their subsequent adaptation of migration intentions.[18] For individuals who underestimated the true maximum income differential, we anticipate the information treatment to cause an increase in the expected differential ($\hat{D}_i \uparrow$) and therefore expect the income information to have an encouraging effect on migration, reflected in an increase in migration intentions. Reversely, for individuals who overestimated the true maximum income differential, we anticipate the information treatment to cause a decrease in the expected differential ($\hat{D}_i \downarrow$) and therefore expect the income information to have a discouraging effect on migration, reflected in a decrease in migration intentions.

In addition to intention-to-treat effect estimations using treatment assignment as an explanatory variable, we use an instrumental variables approach to estimate complier average causal effects (CACE) to address variation in treatment intensity. Low display durations of the treatment for some interviews suggest that enumerators did not always implement the treatment correctly or at least with varying intensity. The CACE analysis uses treatment assignment as an instrument for treatment delivery measured as the time spent by enumerators explaining the regional income information to the respondent. We set the thresholds for completed treatment delivery at 45 seconds in Ghana and 60 seconds in Uganda.[19]

The pre-analysis plan (AEARCTR-0006733) specified the information intervention, all outcome variables – except migration preparations measured at follow-up – and the empirical specification as presented above. Sub-sample analyses by country and by whether participants over- or underestimated pre-treatment income differentials were registered as heterogeneity analyses. Estimations of heterogeneous effects by region, correctness of income expectations, intentions to migrate, education, and wealth presented in Section 4.5 were also pre-registered.[20] CACE estimations were not pre-specified and were added in response to the observed variation in treatment intensity. Similarly, the checks for experimenter demand effects, the sample restrictions as part of the robustness checks, as well as the heterogeneity analysis by cognitive skills were not part of the pre-analysis plan.

4. Belief updating and migration preferences

4.1. Descriptive analysis

Respondents on average overestimate regional incomes in both countries pre-treatment. The bars in Fig. 1 show respondents' expectations for the different regions in Ghana (top) and Uganda (bottom). The black dots indicate the inflation adjusted true mean income for

[18] In Ghana, respondents were only asked about their income expectations for five out of the ten regions, while they could select among all existing regions for their preferred destination preferences. Moreover, some respondents did not indicate their expectations for each of the requested five regions in both countries. These respondents could not be allocated to one of the sub-samples since we lack their initial $\hat{D}_i$. Consequently, the number of observations of the two sub-samples does not sum up to the total sample.

[19] The time differs by country as reading the information is expected to take longer than viewing and explaining the info-graphic.

[20] Additionally, we pre-specified heterogeneity analyses by risk preference, employment status, age, marital status, migration preparations, and beneficiary status in the respective employment program and the results for these analyses are included in Appendix X. A heterogeneity analysis by gender was also pre-specified but not conducted because only 0.35% of the Ghanaian and 13.16% of the Ugandan sample were female participants.

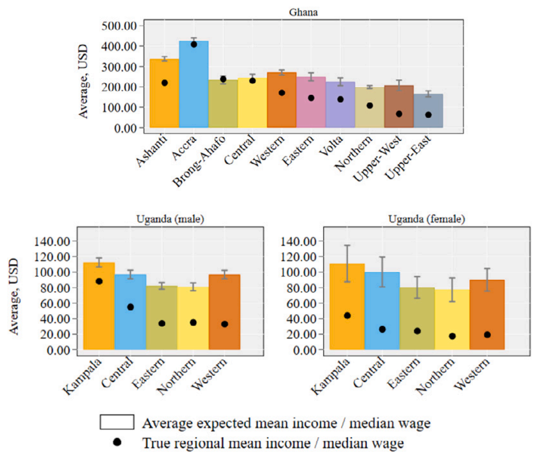

Fig. 1. Income and wage expectations prior to treatment. The bars indicate the average expected monthly mean income in Ghana and median wage in Uganda for the respective region in USD. The black dots represent the true inflation-adjusted mean income in Ghana and median wage in Uganda in USD from secondary data. Sex-specific income statistics were only available in Uganda.

each region.[21] The extent of overestimation is stronger in Uganda than in Ghana, with expectations more than doubling the true value for some regions and especially pronounced for the female, though very small, subsample.

The extent to which beliefs are biased varies depending on whether respondents are asked about their home or potential destination region (Appendix Table A.II.2). In both countries, participants' overestimation of regional incomes is significantly lower for their current region of residence. In Ghana, the discrepancy between participants' expected regional monthly per capita income and the true value is on average 14 USD lower for participants' home region than for their potential destination regions. Given an average overestimation of potential destination regions by 80.7 USD this equals a reduction by 17.4%. In Uganda, the relative difference is very similar: 7.6 USD or 15.5% compared to the average level of overestimation of potential destinations.

Participants also have biased perceptions about income *differentials* across regions and thereby about the potential monetary returns to internal migration. Ghanaian participants of the Western region undervalue the potential gains of moving to several regions, whereas participants of the Northern region tend to overvalue the potential gains of moving in general (Appendix Figure A.II.3). In Uganda, participants' misperceptions about the regional differences are also more pronounced (Appendix Figure A.II.4). The potential income gains of moving to Kampala is always underestimated, whereas income gains of moving to the Western region are overestimated.

Whether individuals over- or underestimate regional income differentials varies by participant characteristics. While married individuals and employees are more likely to underestimate maximum income differentials, self-employed are more likely to overestimate them. Those who underestimate the differentials tend to have a slightly higher asset index than those who overestimate (Appendix Table A.II.3). We include pre-treatment control variables in all regressions, and we further address these differences with weighted estimations discussed in Section 4.3.

To assess the relationship of income perceptions and migration decisions pre-treatment, we plot individuals' maximum expected income differentials $\hat{D}$ against their pre-treatment internal migration intentions (Appendix Figure A.II.5).[22] The small correlation coefficient and flat line of the Gaussian kernel smoother in both countries suggest no association of perceived income differentials with individuals' intentions to migrate. For the relation between income perceptions and destination preferences we look at the ranking of regional income expectations (Appendix Figure A.II.6). Prior to treatment, 67.6% and 47.6% of respondents in Ghana and Uganda, respectively, selected the region as their first destination preference for which they expected the highest income, suggesting a relatively strong relationship between income perceptions and individuals' preferences where to migrate to.

Table 2
Effect on migration intentions, OLS and IV.

	Ghana			Uganda		
	Total	$\hat{D}\uparrow$	$\hat{D}\downarrow$	Total	$\hat{D}\uparrow$	$\hat{D}\downarrow$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: OLS estimates						
Treated (assigned)	0.002	0.011*	−0.008	−0.047***	−0.038	−0.056**
	(0.005)	(0.006)	(0.010)	(0.015)	(0.024)	(0.022)
Panel B: IV estimates						
Treated (delivered)	0.006	0.026*	−0.019	−0.062***	−0.051	−0.071**
	(0.012)	(0.015)	(0.023)	(0.020)	(0.032)	(0.028)
1st stage F-stat.	1842	1205	472	1283	476	659
Observations	5389	3163	1195	824	376	403
Control mean	0.782	0.793	0.799	0.797	0.797	0.799
Covariates	✓	✓	✓	✓	✓	✓

Note: Table shows estimation results from OLS (Panel A) and IV (Panel B) regressions of internal migration intentions on treatment assignment, pre-treatment outcome, and covariates. Covariates include age, gender, marriage, employment situation, education, household asset index, and region of residence dummies. Regressions are run on the total sample, the subsample of individuals who underestimated the true maximum income differential ($\hat{D}\uparrow$), and the subsample who overestimated the differential ($\hat{D}\downarrow$). IV estimations use treatment assignment as an instrument for treatment intensity (display duration of at least 45 seconds in Ghana and 60 seconds in Uganda). The outcome variable varies between 0 (Not at all) and 1 (A lot). Robust standard errors are displayed in parentheses (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$).

4.2. Effects on migration intentions

Table 2 presents the results for migration intentions measured immediately after the information was received. Panel A shows intent-to-treat OLS estimation results and panel B the CACE results based on the IV estimation. Columns (1) to (3) refer to the Ghanaian sample and columns (4) to (6) to the Ugandan sample. Columns (1) and (4) include all individuals of the respective country sample, columns (2) and (5) only include individuals who initially underestimated regional income differentials, and columns (3) and (6) only include individuals who initially overestimated regional income differentials.[23]

The results show no impact of information provision on migration intentions in the total Ghanaian sample. Among Ghanaian participants who initially underestimated regional differences, however, we see a marginally significant increase in internal migration intentions by 1.1 percentage points, i.e., an increase of 1.4% relative to the control mean.[24] For Uganda, we observe a significant negative treatment effect in the total sample, driven by the subsample of individuals who initially overestimated regional differences. In this subsample, the provided

[21] We used the GDP deflator of the years 2018, 2019, and 2020 for Ghana and Uganda, respectively. "True" refers to the figures reported in the UNHS main report and the corrected figures of GLSS7.

[22] For the scatterplot, observations were grouped into bins by means of the quantiles of the expected income differentials. Each dot represents one bin and for each bin the mean expected income differential and mean internal migration intention were calculated. Outlier observations, defined by expected income differentials smaller than −300 (GH: N = 91, UG: N = 2) and greater than 1000 in Ghana (N = 73) or 400 in Uganda (N = 4) were dropped.

[23] The sums of the subsamples in columns (2) and (3) and columns (5) and (6) are smaller than the samples in columns (1) and (4), respectively, because of missingness in income expectations.

[24] Intentions are measured on a Likert scale, as described in Section 2.3, and percentage point changes can be interpreted as latent changes on this scale ranging from zero to maximal intent.

information lowered interest in internal migration by 5.6 percentage points, a moderate reduction of 7.0% relative to the control mean. The IV estimates in panel B are the same with respect to the sign of the coefficients and their significance, but show larger effect sizes.

The findings are in line with the theoretical predictions that migration encouraging information (increased maximum expected income differential) should increase migration intentions, while migration discouraging information (declined maximum expected income differential) should decrease intentions, although we observe the encouraging effect only in Ghana and a significant discouraging effect only in Uganda. We attribute these effect differences in migration intentions between the two samples to differences in the distributions of prior beliefs.[25] In Uganda, the share of the sample for which the information is migration-discouraging is larger than the share of the sample for which it is migration-encouraging, and the average magnitude of any negative shifts in $\Delta\hat{D}_i$ due to new information about potential destinations is larger than the average magnitude of any positive shifts in $\Delta\hat{D}_i$, holding beliefs about participants' home region constant, as shown in Figure A.III.2.[26] In other words, there are relatively more participants that learn that migration gains are lower than they thought, and on average those downward corrections are much more substantial than the upward corrections seen by those that underestimated their potential gains. As a result, the Uganda sample is particularly well-suited to identify the effect of migration-discouraging information. In contrast, the Ghana sample is particularly well-suited to identify the effect of migration-encouraging information, given that the share of the sample for which the information is migration-encouraging is larger, and the average magnitude of positive shifts is larger than the average magnitude of negative shifts.[27] In Section 4.3, we discuss explanations for the overall larger magnitude of the coefficients in Uganda.

Table 3 presents ordered logit regression estimates for the different categories of migration intent for each country and confirms the OLS estimation results. Among Ghanaians who underestimated the true maximum income differential, the intent to migrate internally significantly increased after being shown and explained the infographic. On average, their probability of indicating that they want to migrate "A lot" increased by 2.0 percentage points compared to the control group, while their likelihood of selecting any of the lower categories significantly reduced. For Ugandans who overestimated income differentials, we observe a significantly reduced probability of selecting "A lot" by 10.1 percentage points, while the probability of selecting "A fair amount" or "A bit" significantly increased after receiving the income information compared to the control group. We do not observe treatment impacts among Ghanaians who overestimated and Ugandans who underestimated income differentials.

Due to large income differentials across regions, we also estimate the information treatment effects for each region of residence for the respective total country sample (Appendix Table A.III.1).[28] In Uganda, effects are very homogeneous with negative coefficients in all regions, although they are only significant in Kampala and the Central region. In Ghana, we observe small positive and insignificant coefficients for all regions except for Ashanti as this is – according to the shown infographic – the region with the highest average income. The subsample analysis by region suggests that, despite stark income differences across regions in both countries, treatment effects do not significantly vary across regions.

The erroneous income figure for the Ashanti region in Ghana did not seem to have compromised the internal validity of the experiment. The large majority of participants who remembered the infographic in a follow-up survey 18 months after the experiment, indicated that they trusted the provided information. Although levels of trust and perceived relevance differ across region of residence, respondents living in the Ashanti region are not among the most skeptical. To the contrary, respondents in Ashanti are among those with the highest reported levels of trust and perceived relevance. The substantially higher income value for the region of Ashanti did not cause participants to mistrust the information.

4.3. Effects on destination preferences

Table 4 presents the OLS results for destination preferences. The different panels refer to whether the first (A), second (B), or the average across both destination preferences (C) was used as outcome. The outcomes indicate whether the preferred destination matches with the ranking of the maximum possible income differential between the home region and all potential destination regions. For the updating of destination preferences it too matters whether the received information is encouraging or discouraging. For example, one can imagine that the receipt of migration encouraging information results in participants thinking harder about the right destination choice. Whereas migration discouraging information might render the destination choice less relevant. Columns (1) and (4) of Table 4 present results for the respective total country-specific sample, columns (2) and (5) refer to the subsamples of individuals who underestimated and columns (3) and (6) to the subsamples of individuals who overestimated the actual regional income differentials.

The information treatment significantly increased the probability to select the region with the highest income as the first destination preference among individuals whose maximum expected income differential is assumed to have increased through the treatment (Panel A). In Ghana, the probability increased by 4.7 percentage points (10.5% relative to the control mean) and in Uganda by 13.6 percentage points (84.5% relative to the control mean). Individuals who overestimated the maximum expected income differential do not update their preferences for the first destination. Regarding the probability of selecting the region with the second-highest income differential as the second destination preference, we only observe a weakly significant increase for Ugandans whose maximum expected income differential is assumed to have declined (Panel B). The significance disappears for Ghana when looking at both destinations jointly in Panel C. For Ugandans, we continue to observe a significantly increased probability of selecting both preferences in line with the true income differentials and again only among those who initially underestimated. Results are almost identical when we use IV estimations instead (Appendix Table A.IV.1).

We attribute the larger magnitude of the treatment effects on destination preferences in Uganda – as well as the more pronounced negative effect on migration intentions among Ugandan participants who overestimated the maximum potential income differential – to four sample differences that influence how the treatment affects participants' beliefs and how beliefs affect participants' migration calculus. First, the control means in Table 1 show that participants in Uganda have on average less accurate baseline beliefs about regional incomes, thereby leaving more room for change.

[25] An additional explanation for the absence of a positive effect on intentions in the Ugandan sample may be that the information in Uganda generally showed income levels substantially below participants' prior beliefs for all regions, which may have led to a level effect that dampened migration intentions across the board.

[26] This exploratory empirical insight also suggests two plausible future extensions of our parsimonious conceptual framework: The magnitude of any change in expectations may matter, and participants may update beliefs about potential destinations more readily than beliefs about their home region.

[27] The updating of destination preferences in response to encouraging and discouraging information operates differently than the updating of migration intentions: a signal that discourages migration does not induce effort to select the optimal destination, regardless of signal strength, as we will see in Section 4.3.

[28] Analyses at the regional level do not separate the sample into individuals with increased $\hat{D}$ and reduced $\hat{D}$ because the direction of the change in $\hat{D}$ strongly correlates with the region of residence.

Table 3
Effect on categories of migration intentions, ordered logit.

	Ghana			Uganda		
	Total (1)	$\hat{D}\uparrow$ (2)	$\hat{D}\downarrow$ (3)	Total (4)	$\hat{D}\uparrow$ (5)	$\hat{D}\downarrow$ (6)
Treated (assigned)						
Not at all	−0.001	−0.004**	0.004	0.006**	0.004	0.005
	(0.001)	(0.002)	(0.004)	(0.003)	(0.004)	(0.003)
A bit	−0.002	−0.007**	0.003	0.026**	0.019	0.035**
	(0.002)	(0.003)	(0.003)	(0.010)	(0.016)	(0.016)
A fair amount	−0.003	−0.010**	0.009	0.044***	0.028	0.061**
	(0.003)	(0.004)	(0.009)	(0.017)	(0.023)	(0.025)
A lot	0.006	0.020**	−0.015	−0.076***	−0.051	−0.101**
	(0.007)	(0.009)	(0.015)	(0.029)	(0.043)	(0.042)
Observations	5389	3163	1195	824	376	403
Covariates	✓	✓	✓	✓	✓	✓

Note: Table shows estimation results from ordered logit regressions of internal migration intentions on treatment assignment, pre-treatment outcome, and covariates. Rows display the coefficients on the different outcome categories of the ordered logit estimation. Covariates include age, gender, marriage, employment situation, education, household asset index, and region of residence dummies. Regressions are run on the total sample, the subsample of individuals who underestimated the true maximum income differential ($\hat{D}\uparrow$), and the subsample who overestimated the differential ($\hat{D}\downarrow$). Robust standard errors are displayed in parentheses (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$).

Second, respondents could compare the provided information to their perceptions about regional incomes in the construction sector, and discount the provided details to some extent — particularly in Ghana, where a much larger share of the sample is employed in the construction sector, and the correlation between overall and construction-sector incomes is more muted than in the Uganda sample. In Section 4.5, we discuss that while regional income rankings in the construction sector are consistent with those across sectors, differences across regions are smaller in the construction sector than across all sectors in Ghana, whereas construction sector incomes are very comparable to cross-sector incomes in Uganda. This may reduce the effect size in Ghana, but not in Uganda.

Third, participants in Uganda are on average younger and have less work experience than those in Ghana (see Table 1), and beliefs that are likely grounded in lived and accumulated experience may be more difficult to dislodge. Fourth, the fact that Ghanaian participants are on average older, more likely to be married, and less likely to be unemployed could also mean that they are more settled in their preferences. Their expressed preferences may be less responsive to new information from the outset, whether or not they update their beliefs about regional incomes.

Multinomial logit estimations on the probability of selecting each region confirm the results of Table 4. Treated individuals' probability of selecting the regions with the highest indicated incomes (Ashanti in Ghana, Kampala in Uganda) as their first destination preference significantly increased compared to the control group and the effect is driven by individuals who underestimated the maximum income differentials (Appendix Figure A.IV.1).[29]

Alternative ways to measure destination preferences are the actual logarithmized income in USD of the preferred destinations, the income rank of the preferred destinations, and an indicator variable for whether the preferred destination has a higher income than the region of residence. The results for all three alternative outcome measures confirm the previous findings (Appendix Table A.IV.2). In Ghana, treated individuals select their first destination preferences such that they have on average a 3.1% higher income than the preferences selected by individuals of the control group. For Ghanaians who underestimated the income difference, the income difference is 4.6%, whereas the effect is small and insignificant for those who overestimated regional income differentials. In the total Ugandan sample, the average potential income gain is 2.5% larger than for the control group. Assessing the incomes of the first and second destination preference separately shows that, as for the main outcome, the impacts on the three alternative measures are driven by adaptations of the first destination preference (Appendix Table A.IV.3 and Table A.IV.4). Results are similar when we use IV estimation (Appendix Table A.IV.5).

We further assess whether the impact on destination preferences depends on individuals' prior knowledge about the destinations' income ranking. We split the sample into individuals whose pre-treatment regional income expectations ranked destinations correctly and individuals who did not. Results show that, as expected, only individuals with incorrect pre-treatment ranking update their destination preferences towards higher-income destinations (Appendix Table A.IV.6). Compared to Ghana, a smaller share of Ugandans selected their destination preferences in line with their expected income ranking leaving room for the information treatment to correct the destination preferences though to a lesser extent (see Appendix Figure A.II.6). The results of this alternative sample split demonstrate that our information successfully corrected destination preferences towards higher-income destinations among individuals who had incorrect information prior to treatment.

Using different subsamples we checked whether the results on intentions and destination preferences are driven by specific subgroups. We also implement weighted estimations that establish balance for a range of covariates across the subsamples of over- and underestimating respondents. Appendix IX provides a short discussion and summarizes the results. Overall, our results remain extremely robust to each of the alternative subsamples and the inclusion of weights.

In Appendix X we also report the additionally pre-registered heterogeneity analyses by age, marital status, employment, income, wealth,

[29] In order to facilitate convergence of these models, we use a categorical variable for education instead of indicators for each educational level, add noise to binary control variables, and combine the Ghanaian destination regions of Upper East and Upper West. Given that the different destination choices might not be completely independent of each other, we additionally run multinomial probit regressions, with results that are almost identical.

Table 4
Effect on destinations reflecting the maximum income differentials.

	Ghana			Uganda		
	Total	$\hat{D}\uparrow$	$\hat{D}\downarrow$	Total	$\hat{D}\uparrow$	$\hat{D}\downarrow$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 1st ***destination preference***						
Treated (assigned)	0.033***	0.047***	0.009	0.074***	0.136***	0.020
	(0.010)	(0.014)	(0.015)	(0.026)	(0.041)	(0.034)
Observations	5195	3108	1125	821	375	403
Control mean	0.524	0.449	0.759	0.160	0.161	0.165
Panel B: 2nd ***destination preference***						
Treated (assigned)	0.005	0.014	−0.011	0.034	−0.001	0.072*
	(0.010)	(0.012)	(0.021)	(0.026)	(0.039)	(0.039)
Observations	5105	3079	1105	803	365	398
Control mean	0.329	0.249	0.492	0.216	0.204	0.246
Panel C: 1st ***and*** 2nd ***destination preference***						
Treated (assigned)	0.006	0.018	−0.019	0.036**	0.055**	0.014
	(0.009)	(0.012)	(0.021)	(0.014)	(0.023)	(0.019)
Observations	5098	3078	1102	801	364	397
Control mean	0.263	0.201	0.400	0.025	0.027	0.026
Covariates	✓	✓	✓	✓	✓	✓

Note: Table shows estimation results from OLS regressions of the probability of selecting the destination preferences such that it mirrors the highest possible income differential on treatment assignment, pre-treatment outcome, and covariates. Panel A only considers the first preference, Panel B only the second preference, and Panel C both preferences jointly. Covariates include age, gender, marriage, employment situation, education, household asset index, and region of residence dummies. Regressions are run on the total sample, the subsample of individuals who underestimated the true maximum income differential ($\hat{D}\uparrow$), and the subsample who overestimated the differential ($\hat{D}\downarrow$). Robust standard errors are displayed in parentheses (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$).

risk preferences, prior migration experience, and migration preparations at baseline.[30] In neither country do we observe pronounced effect heterogeneity with respect to these variables. In Uganda, migration intentions are reduced to a lesser extent among individuals with higher risk preferences, and in Ghana, prior migration experience increases the treatment effect on destination preferences.

4.4. Demand effects

Experimenter demand effects refer to changes in behavior or survey responses of experimental subjects based on their beliefs about what is expected of them rather than an intrinsic change in their behavior or response. Treatment effect estimates using self-reported outcomes are in particular prone to demand effect biases. We intended to limit concerns about experimenter demand effects by emphasizing the anonymity of responses. In addition, we asked a random subsample of Ghanaian participants to select the answers on the survey tablets on their own in a way that enumerators could not observe the selected responses. For another subsample in Ghana, enumerators emphasized that responses will not affect respondents' chances of being selected for the ProfArts program.[31] The treatment effects for participants who self-selected their response or received the disclaimer did not differ from those who did not (Appendix Table A.V.1). Coefficients of the interactions of the information treatment with indicators for these demand effect measures are small, positive and insignificant. We take this as evidence that experimenter demand effects do not drive our results.

4.5. Moderating factors of belief updating

The theory of change of our experiment is based on two interlinked updating processes. In a first step, we expect the information campaign to cause an update of region-specific income expectations among treated participants. In a second step, we expect the induced change in income expectations to cause a change in participants' migration preferences. The actual change in participants' income expectations for the different regions post-treatment was not directly measured to avoid participants feeling like they were tested. Instead, participants in Uganda were asked about their income aspirations in five years in a later interview section. Individuals who received the information treatment indicated monthly earnings that are significantly lower by 78.3 USD than those of control participants (Appendix Table A.VI.1). This effect is driven by the subsample of individuals who underestimated the maximum income differential. Given that, overall, participants in Uganda overestimated regional income *levels* (see Fig. 1), a reduction in income aspirations is exactly what we would expect to see, both for individuals who over- and underestimate income *differentials*, and shows that individuals do use the provided information to update their income expectations.

The updating procedures that mediate the effect of the information treatment on migration preferences are subject to a multitude of moderating factors including (1) participants' prior income expectations, (2) participants' prior migration preferences, (3) participants' understanding of, trust in, and perceived relevance of the provided information, and (4) participants' weighting of income as one relevant factor within the migration calculus.

The first moderating factor has partly been addressed already by the differentiation between participants who underestimated and overestimated the true maximum income differential. Results have shown that, as predicted by our conceptual framework, overoptimistic Ugandan respondents reduce intentions to migrate, while pessimistic respondents in Ghana shifted their internal migration intentions towards higher

[30] The inclusion of region fixed effects controls for any regional differences in these baseline characteristics.

[31] In Uganda, the experiment took place after program completion, so there is no reason to think that concerns about program enrollment could have lead to demand effects.

Table 5
Moderating factors.

	Ghana								Uganda					
	Internal migration intentions				1st destination maximizes $\hat{D}$				Internal migration intentions			1st destination maximizes $\hat{D}$		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Treated (assigned)	0.001	0.012	0.000	0.009	0.018	0.038***	0.022*	0.026**	−0.035	−0.024	−0.059**	0.032	0.053	0.078**
	(0.005)	(0.009)	(0.006)	(0.006)	(0.011)	(0.015)	(0.013)	(0.013)	(0.025)	(0.025)	(0.023)	(0.038)	(0.040)	(0.038)
Treatment X spearman(−1 to −0.5)	0.015				0.032				−0.085			0.021		
	(0.024)				(0.050)				(0.055)			(0.121)		
Treatment X spearman(−0.5 − 0)	−0.023				0.092**				0.002			0.078		
	(0.023)				(0.043)				(0.043)			(0.065)		
Treatment X spearman(0 − 0.5)	0.017				0.071**				−0.029			0.109*		
	(0.014)				(0.033)				(0.037)			(0.062)		
Combined P-value, spearman(−1 to −0.5)	0.504				0.302				0.018			0.643		
Combined P-value, spearman(−0.5 − 0)	0.333				0.009				0.346			0.038		
Combined P-value, spearman(0–0.5)	0.171				0.004				0.021			0.004		
Treatment X high intentions		−0.018*				−0.009				−0.041			0.034	
		(0.010)				(0.020)				(0.031)			(0.053)	
Combined p-value, intentions		0.215				0.042				0.001			0.010	
Treatment X higher education			0.005				0.027				0.022			−0.008
			(0.010)				(0.021)				(0.031)			(0.051)
Combined p-value, education			0.489				0.002				0.067			0.044
Treatment X higher cognitive skills				−0.023**				0.023						
				(0.010)				(0.021)						
Combined p-value, cognitive skills				0.078				0.004						
Observations	5389	5389	5389	5389	5195	5195	5195	5195	824	824	824	821	821	821
Control mean	0.782	0.782	0.782	0.782	0.524	0.524	0.524	0.524	0.797	0.797	0.797	0.160	0.160	0.160
Covariates	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Note: Table shows estimation results from OLS regressions of the two main outcomes on treatment assignment, pre-treatment outcome, selected interaction terms, and covariates. Covariates include age, gender, marriage, employment situation, education, household asset index, and region of residence dummies. Coefficients in each column belong to a separate regression. Regressions are run only on the total sample without differentiating between over- and underestimation. Robust standard errors are displayed in parentheses (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$).

categories of intent. Further, destination preferences are only corrected towards higher-income destinations among initially pessimistic participants. This finding adds to the belief updating literature about asymmetric updating and suggests that also within the migration calculus, individuals might be more responsive to good news, i.e., migration encouraging information, than bad news, i.e., migration discouraging information.[32]

Whether participants change their income expectations likely also depends on how accurate their income expectations were already at the outset. We summarize an individuals' accuracy of expectations over all regions by calculating an individual level Spearman rank order correlation of the expected and the true income ranking, i.e., for each participant we have 5 observations, one for each region. The Spearman coefficient varies between −1 and +1, where +1 indicates a perfect association of ranks, 0 no association, and −1 a perfect negative association. The results in columns (5) and (12) of Table 5 show that only participants with slighter deviations (Spearman correlation of −0.5 to 0.5) update their destination preferences, whereas participants whose income expectations were very far from the actual ranking (−1 to −0.5) do not update. One explanation for the more effective belief updating among individuals with more accurate expectations could be that the information seemed more credible to them. The effects on migration intentions presented in column (1) show no heterogeneity by participants' accuracy of prior income expectations.

We asses the second moderating factor, initial migration preferences, by estimating effect heterogeneities between participants with higher and lower pre-treatment intentions to migrate internally. For participants with higher migration intentions the information might be more relevant and, thus, updating of migration expectations more salient. We define high migration intentions as wanting to migrate "a lot". Columns (2), (6), (10), and (13) of Table 5 present the treatment effects by prior migration intentions. In Ghana, the effect on migration intentions is significantly lower for high-intention-participants (column (2)), suggesting that lower level intentions may be less firm and easier to move. In Uganda, the direction of the effects differ for intentions and destination preferences and the coefficients are insignificant.

The third group of moderating factors regards the understanding of, trust in, and perceived relevance of the provided income information. To a large extent these factors are addressed by the consistency of

[32] An alternative or additional explanation for these different treatment effects could be a certain type of Roy sorting, in which idiosyncratic location-specific wage draws leave some participants personally optimistic about potential migration gains and unresponsive to a non-individualized treatment.

We think this is unlikely for three reasons. First, our experimental design tried to minimize this problem by eliciting participants' expectations of average/median monthly incomes and emphasizing that we are not asking about their personal income potential in different regions. In this sense, the categorization of a participant as "optimistic" or "pessimistic" should be independent of idiosyncratic wage draws, although of course some people may still have answered based on their personal income expectations.

Second, if idiosyncratic wage draws can lead to a participant being personally optimistic and hence unresponsive to general income information, it is not clear why the same could not also be the case when idiosyncratic wage draws lead to a participant being pessimistic about his or her personal income potential elsewhere. Yet here we do see responsiveness to treatment. Asymmetric responsiveness may well emerge in a variant of a Roy model, but asymmetric responsiveness need not obviously imply Roy sorting.

Third, we provide suggestive empirical evidence based in variance decomposition, a general approach commonly taken in analyses of Roy sorting (Lentz et al., 2023; Taber and Vejlin, 2020). In our specific case, if Roy sorting as described here plays a dominant role, we would expect to see substantial within-individual variation in ex ante beliefs about expected incomes in different regions (because each participant evaluates an idiosyncratic personal wage draw for each location). Conversely, if overall variation in beliefs is relatively more attributable to between-individual variation (because participants' beliefs move in tandem with each location's general wage conditions, and differences reflect participants' comparative advantages that are applicable everywhere as opposed to idiosyncratic location-specific wage draws), this suggests that sorting may be a more minor concern. We find the latter to be the case in our data, with variation between individuals 1.9 times as high as variation within individuals in Ghana, and 1.7 times in Uganda.

our results. If individuals would not understand, trust, or perceive the information as relevant at all, we would expect to observe no impact on migration intentions, destination preferences, or income aspirations. The follow-up surveys in Ghana reveal that about one third of the treated individuals remembered the infographic. The share jumps to 45% if looking only at respondents for whom the display duration of the infographic was longer than 45 seconds. Among those who remembered, 72.3% indicated that they trusted the provided information and 68.0% perceived the information as relevant (Appendix Figure A.VI.1). To further examine to what extent the understanding of the information matters, we look at effect heterogeneities by participants' cognitive skills (Ghana only, column (4) and (8) of Table 5) and educational level (columns (3), (7), (11), and (14) of Table 5). There are no significant differential treatment effects by educational status on migration intentions or destination preferences in either country. Effect heterogeneities by cognitive skills are insignificant for destination preferences. For intentions to migrate, the information treatment impact for Ghanaians with higher cognitive skills is significantly lower than for Ghanaians with lower cognitive skills. These results suggest that individuals with higher cognitive skills or educational status do not update more consistently than individuals with lower cognitive skills or educational status. Insufficient understanding of the provided information does not seem to mute treatment effects.

Low perceived relevance of the provided information might render the treatment ineffective, irrespective of the importance of income at destination as a decision factor in the migration calculus. Mean incomes and median wages may not be specific enough to the individual. Participants might consider the income figures as either out of their reach or far below their income expectations, or they believe that the sector-, position-, and task-independent wages are just not informative about their personal income potential at destination. To address this concern, we compare the cross-sectoral incomes from the treatment with incomes of participants in our study by region (Appendix Figure A.VI.2) and construction sector-specific incomes which is a prominent sector especially among Ghanaian participants (Appendix Figure A.VI.3). In Ghana, the variation of incomes across regions is lower when looking only at the construction sector but regional income differentials persist. On average, the monthly construction sector-specific per capita income is 41.1 USD lower than the cross-sectoral income. The deviation reduces to 16.3 USD when we exclude Accra and Brong-Ahafo, which have construction sector incomes that are substantially higher than the other regions. In Uganda, deviations are minimal with wages being 5.6 USD lower in the construction sector than the cross-sectoral average and the regional ranking changes only slightly. As discussed in Section 4.3, this smaller magnitude of regional income differentials in the Ghanaian construction sector, together with the much larger proportion of the Ghanaian sample working in the construction sector, may be one explanation for the smaller treatment effect size in Ghana than in Uganda. The highest income region of both countries does not differ when either cross-sectoral or construction sector incomes are used, and remains identical when considering only urban areas. A similar picture emerges when looking at the income variation across regions in our study sample. Overall, it seems that the provided cross-sectoral incomes are relevant also for construction workers. Using cross-sectoral information has the advantage that these incomes are also relevant for individuals who do not work in the construction sector, such as some participants in our sample or participants' households members, to whom the information might be passed on. We further compare the incomes of migrants and locals in our sample at baseline to understand the earnings potential of the two groups in the same location. Incomes of individuals who do not reside in their region of birth are slightly higher than incomes of those who did not move when controlling for background characteristics and location (Appendix Figure A.VI.4). This suggests that migrants in our relatively well-educated sample can earn similar or even higher incomes than locals.

The last point we turn to is the weighting of income at destination as a factor in the migration calculus. For example, the probability to be employed might be a more relevant decision factor than income conditional on employment. This is precisely a core question this study helps to answer: To what extent and in what ways is income a decision factor in the migration calculus? We can think of the weighting of decision factors as an effect moderator situated at the link between income expectations and migration decisions. If income is a factor with a low rather than a high weight in a person's migration calculus, then, all else equal, this person's income expectations will be updated to the same extent, but the change in migration preferences will be less pronounced. We asked Ghanaian participants in the follow-up survey about the aspects they consider before they decide to migrate in a multiple-response question. 58.8% indicated that income is one of the aspects they would consider. While this does not speak to the relative importance of income compared to other decision factors, it suggests that income is one relevant factor. However, this also means that more than two in five respondents did not mention income at all. Indeed, our treatment effects estimates indicate that income considerations do not dominate all migration decision-making and that their role in the migration calculus may differ for different dimensions of migration decisions. In particular, results suggest that income is a low weight factor for the decision *whether* to migrate, but a high weight factor for *where* to migrate. This is a key insight of this study that has not previously been articulated in the relevant literature.

5. Persistence in beliefs and effects on migration behavior

5.1. Effects on migration intentions and destination preferences

The Ghanaian follow-up was divided in two batches, Batch I was re-interviewed between May and June and Batch II between October and November 2022. The debriefing on the correct income information for Ashanti region happened on September 30, 2022. Thus, we can only use follow-up responses of Batch I to assess the long-term effects of the original experiment.

In the follow-up, the marginal positive effect on intentions among Ghanaians who underestimated income differences, as observed immediately after the treatment, dissipates. However, Ghanaians who initially overestimated potential gains now express migration intentions that are on average 15.9% (12.8 percentage points) lower than those of the control group (Panel A of Table 6). This mirrors the effect found in Uganda, but while migration-discouraging information reduced migration intentions among overestimating Ugandan respondents immediately, it seems that for Ghanaian respondents this updating required a period of reflection. Concerning individuals' destination preferences, the significant positive effects persist in the total sample and continue to be especially pronounced in the subsample of individuals who underestimated income differentials (Panel B of Tables 6 and A.VII.1).

We can use responses from Batch II to test the responsiveness to another informational shock. The debriefing informed all treated individuals that the true income for the Ashanti region was substantially lower than initially shown and that instead of ranking first, Ashanti ranks only fourth of ten. Following the same rational as for the original intervention, we thus differentiate between (i) individuals living in Ashanti who should have perceived the information as migration encouraging and (ii) individuals living outside of Ashanti who should have perceived the information as migration discouraging.[33] Mirroring this and our previous results on encouraging and discouraging information, we observe significantly higher migration intentions among individuals living in Ashanti (3.8 percentage points or 5.0% relative to

[33] Instead of controlling for pre-treatment outcome variables we now control for post-treatment outcome variables measured immediately after the original intervention.

Table 6
Long-term effects among Batch I.

	Ghana		
	Total (1)	$\hat{D}\uparrow$ (2)	$\hat{D}\downarrow$ (3)
Panel A: Internal migration intentions			
Treated (assigned)	−0.036**	−0.007	−0.128***
	(0.018)	(0.022)	(0.037)
Observations	1258	817	321
Control mean	0.729	0.705	0.807
Panel B: 1st ***preference maximizes*** $\hat{D}$			
Treated (assigned)	0.074***	0.086***	0.024
	(0.026)	(0.032)	(0.054)
Observations	1245	814	318
Control mean	0.362	0.269	0.562
Covariates	✓	✓	✓

Note: Table shows estimation results from OLS regressions of follow-up outcomes on treatment assignment, pre-treatment outcome, and covariates. Panel A uses internal migration intentions and Panel B uses the probability of selecting the first destination preference such that it mirrors the highest possible income differential. Covariates include age, gender, marriage, employment situation, education, household asset index, and region of residence dummies. Robust standard errors are displayed in parentheses (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$).

the control mean), whereas we do not see any significant difference among those living outside of Ashanti (Panel A of Appendix Table A.VII.2). No effect can be found on destination preferences for those individuals interviewed after the debriefing (Panels B to E).

5.2. Migration preparations and behavior

To assess effects on migration preparations and behavior, we look at responses from Batch I and Batch II jointly. The debriefing happened only a few weeks prior to the follow-up of Batch II and is unlikely to have interfered with migration decisions based on the original intervention 18 months ago. At the same time it is unlikely that migration decisions caused by the debriefing would materialize that quickly as respondents were interviewed only a few weeks after the debriefing. Table 7 displays the treatment effects for the probability of living in any other region within Ghana at the time of the follow-up survey (columns (1) to (3)), the probability that any of the respondent's household members moved to another region within Ghana during the past 12 months (columns (4) to (6)), and the probability of the respondents having done preparations to migrate internally ((7) to (9)). Each outcome variable is assessed using the total sample, the sample of individuals who overestimated, and the sample of individuals who underestimated potential income gains.

Neither ITT nor CACE show an impact of the information experiment on actual migration. The estimation coefficient for respondents is close to zero.[34] We also looked at migration behavior among subsamples that might have a high probability to migrate (e.g., male, young, unmarried) and tested for effect heterogeneity. We do not observe significant effects in these subsamples, although power for these subgroup estimations is low. Our overall analysis is well-powered, with a minimum detectable effect size of 1.8 percentage points (or 0.09 SD) that is small enough to detect substantively meaningful effects on migration.[35]

Given that migration is often a household decision, treatment effects need not necessarily be limited to study participants themselves, but could extend to other household members. Results in columns (3) to (6) show that this was not the case. Information provision did not change the probability that any household member moved internally.[36] We do observe a slightly increased probability of treated subjects having done preparations to migrate internally driven by the subsample of individuals who underestimated potential income gains (column (11)). Disentangling the different types of preparations reveals that the observed effect is driven by an increased probability of having talked to family and friends about migrating internally (Appendix Table A.VII.3).

We conclude that the provision of regional income information did not affect individuals' migration status 18 months later, i.e., at the time of the follow-up survey, nor does it appear to have affected actual migration among household members. Treated individuals also do not perform better in terms of income, working hours, or job benefits than control individuals (Appendix Table A.VII.4 and Table A.VII.5).

Spillover effects cannot bias outcomes measured immediately after the information provision because treated and control individuals did not interact. But for outcomes measured 18 months later, treated individuals could have forwarded the provided information. We expect spillovers to be most likely among individuals who were later assigned to the same ProfArts training group, i.e., individuals in the same region working in the same trade area. We thus look at the treatment saturation across region-trade groups of Batch I as per assigned information and ProfArts treatment status.[37] Saturation varies between 33.3% and 66.7%. We use this random variation to assess whether potential spillover effects are more pronounced in training groups with higher saturation rates. We do not observe a meaningful difference either between individuals of the information control group or the information treatment group depending on the information treatment saturation rate of the region-trade groups for any of the outcome variables (Appendix Table A.VII.6).[38]

5.3. Discussion on migration behavior

Current figures from the Gallup World Poll reveal that Ghanaians have one of the highest intentions to migrate among ECOWAS countries: 44% compared to an ECOWAS-average of 36%. The data further shows that intentions are particularly high among young (56%), male (48%) and those with intermediate educational level (54%) (OECD, 2022). The average age of our sample is 34 years in Ghana and 26 in Uganda, the sample consists of exclusively men in Ghana and 87.0% men in Uganda, and the large majority completed at least junior high school. Individuals with such characteristics are typically considered to be very mobile which is also reflected in the high pre-treatment migration intentions reported by our study participants.

Further, at the time of each data collection, the respective GIZ programs had either not yet started (at time of intervention in Ghana) or already come to an end at least 6 months prior to the data collection

[34] We do not observe differential sample attrition between our first survey in 2020/21 and the follow-up in 2022, so this null result is not attributable to treated subjects having migrated and exited the sample.

[35] This power calculation is based on the sample size of 4908 observations for the Ghanaian follow-up and a migration incidence of 3.6% in the control group.

[36] The effect is precisely estimated and very well-powered. The larger values for migration among household members, compared to our subject-specific migration measure, can be explained by, first, the fact that this aggregates migration across a number of individuals within each household, and second, somewhat different survey measures of migration for respondents themselves and household members. We count a primary respondent as having migrated if he or she lives in another region at the time of the follow-up survey, while household-level migration captures whether any member has moved permanently or temporarily to another region during the past 12 months.

[37] Appendix Figure A.VII.1 displays the saturation rates of each group. For the spillover regressions, we set the information treatment saturation of the ProfArts control group and those identified as ineligible to zero.

[38] It is also not clear why spillovers would diminish our estimated migration effects, but not the estimates for migration intentions and destination preferences reported in Table 6.

Table 7
Effect on migration behavior and preparations.

	Migrated			HH member moved			Migration prep.		
	Total (1)	$\hat{D}\uparrow$ (2)	$\hat{D}\downarrow$ (3)	Total (4)	$\hat{D}\uparrow$ (5)	$\hat{D}\downarrow$ (6)	Total (7)	$\hat{D}\uparrow$ (8)	$\hat{D}\downarrow$ (9)
Panel A: OLS - ITT estimates									
Treated (assigned)	0.003	−0.004	0.009	−0.010	−0.012	0.017	0.012*	0.016*	−0.007
	(0.005)	(0.007)	(0.011)	(0.012)	(0.015)	(0.025)	(0.006)	(0.008)	(0.014)
Panel B: IV estimates									
Treated (delivered)	0.006	−0.010	0.020	−0.024	−0.029	0.036	0.029*	0.038*	−0.014
	(0.013)	(0.017)	(0.023)	(0.029)	(0.036)	(0.055)	(0.016)	(0.020)	(0.031)
1st stage F-stat.	1804	1172	482	1805	1173	482	1802	1170	482
Observations	4893	2854	1112	4890	2851	1112	4890	2852	1111
Control mean	0.036	0.043	0.029	0.224	0.231	0.210	0.247	0.252	0.257
Covariates	✓	✓	✓	✓	✓	✓	✓	✓	✓

Note: Table shows estimation results from OLS (Panel A) and IV estimations (Panel B) for the treatment effect on migration behavior. The outcomes are defined as the probability that the respondent lives in another region at the time of the follow-up survey (columns 1–3), the probability that any of the respondent's household members moved to another region within Ghana during the past 12 months (columns 4–6), and a migration preparation index (columns 7–9). Regressions are run on the total sample, the subsample of individuals who underestimated the true maximum income differential ($\hat{D}\uparrow$), and the subsample who overestimated the differential ($\hat{D}\downarrow$). IV estimations use treatment assignment as instrument for treatment intensity. Models include age, gender, marriage, employment situation, education, and household asset index as controls. Robust standard errors are displayed in parentheses (* $p<0.1$, ** $p<0.05$, *** $p<0.01$).

(at time of follow-up in Ghana and of intervention in Uganda). This minimizes the possibility that the null result on behavior could be due to program specific factors preventing respondents from migrating.

Lastly, in our setting potential income gains of migration are substantial. For 75% of the Ghanaian study sample the infographic showed that there is at least one other region within Ghana for which the GLSS7 reports an average monthly per capita income that is at least 663 GHS higher than the reported average income of respondents' home region. Relative to the mean (median) reported monthly income of the Ghanaian study participants of 2542 GHS (1764 GHS), this would reflect a potential monthly income gain of 26.1% (37.6%).

The present setting and results suggest that – even in the presence of sizable regional income differentials and a successfully implemented intervention that led to a persistent change in individuals' destination preferences – a change in income expectations associated with migrating does not affect actual migration propensities. Although individuals update their destination preferences and are more likely to discuss migration with family and friends, perceptions of potential income gains do not drive migration in our sample and appear to have been dominated by other decision factors or frictions.

6. Conclusion

We conducted an information experiment about regional income differences in Ghana and Uganda to study the role of information frictions and earnings potential for internal migration decisions in a population with high migration potential. Based on our theoretical framework, we expected participants to adapt their migration intentions and destination preferences due to an update in regional income expectations spurred by the provided information.

Our analysis shows that study participants of both countries overestimate income levels and have biased perceptions of regional income differentials. This is consistent with previous studies in which participants have overestimated the earnings potential from migrating (Shrestha, 2020; Bhatiya et al., 2023) and the probability of obtaining legal status at destination (Bah and Batista, 2020) or frequently report not being well informed about migration-related aspects (Dunsch et al., 2019). In a study similar to ours, Baseler (2023) finds that rural households in Kenya underestimate urban incomes. However, he looks at information frictions of households who remain in the rural origin, whereas our sample is mostly urban and we target potential migrants themselves.

The information treatment reduced internal migration intentions among Ugandan participants by 5.9% (4.7 percentage points) relative to the control mean immediately after the treatment, whereas in Ghana we observe a significant and substantively meaningful reduction in migration intentions among those who overestimated potential gains in an 18 months follow-up. Our effects lie within the magnitude of the reductions of intentions found by Bah and Batista (2020) (2.9 percentage points) and Dunsch et al. (2019) (6.9 percentage points).[39]

The experiment significantly revised migration preferences towards regions with higher incomes in both countries. The probability to chose the destination with the highest potential income gain increases by 3.3 and 7.4 percentage points in Ghana and Uganda, respectively. These effect sizes are somewhat smaller than those for rural Kenyan households in Baseler (2023), who finds an increase in choosing Nairobi as their preferred destination of 9 percentage points. The subsample analysis by individuals' prior expectations shows that the effect on destination preferences is concentrated among individuals who previously underestimated existing income differentials. Individuals who underestimated would increase their expected income from updating their destination preferences due to the received information by 4.2% in Ghana and 12.5% in Uganda. No significant change in destination preferences occurs among initially overoptimistic individuals. Updated preferences towards destinations that maximize potential income gains persist even 18 months later among treated individuals who initially underestimated income differences. These results suggest that the impact and efficacy of information campaigns depend on *how* individuals' prior knowledge contrasts with provided information. In our study, individuals update their beliefs asymmetrically, in particular when considering to which region they should relocate. In that case, migration-encouraging information appears to be weighted much more substantially than migration-discouraging information. Information campaign policies should thus carefully consider how provided information differs from the target population's existing priors. Further, such campaigns might be better suited to guiding individuals' migration intentions rather than trying to talk them out of migration.

[39] Because we are looking at internal migration instead of irregular and international migration, our control means are much larger. Therefore, the effects relative to the control mean are smaller than in studies looking at irregular and international migration.

Despite these meaningful impacts on beliefs, we do not observe effects on actual migration 18 months after the information was provided. Baseler (2023), by contrast, finds large effects on migration to Kenya's capital Nairobi by rural household members in response to their information treatment. Together with Bah et al. (2023), our findings suggest that while information provision can have long-lasting effects on individuals' knowledge and attitudes, this is not enough to affect net migration, not even in our setting of a relatively mobile population of mostly young and well educated men. Although regional income differentials seem to play a salient role for preferences about where to migrate, this does not result in a change in migration behavior. Lagakos et al. (2023) argue that there is a non-monetary disutility associated with moving from rural to urban areas. Our study in a setting with relatively more comparable amenities at origin and potential destinations suggests that these non-monetary costs are not explained solely by rural–urban differences. Instead, these non-monetary costs should be understood as a disutility of being a migrant per se, regardless of whether an individual's place of origin is in the countryside or another city.

CRediT authorship contribution statement

Sarah Frohnweiler: Writing – original draft, Visualization, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Bernd Beber:** Writing – review & editing, Methodology, Investigation, Funding acquisition, Conceptualization, Supervision. **Cara Ebert:** Writing – review & editing, Methodology, Investigation, Funding acquisition, Conceptualization, Supervision.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Replication data and code are available at https://fdz.rwi-essen.de/en/doi-detail/id-107807fbeinfotreat2024v1.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jdeveco.2024.103311.

References

Acemoglu, Daron, Dell, Melissa, 2010. Productivity differences between and within countries. Am. Econ. J.: Macroecon. 2 (1), 169–188.

Alpes, Maybritt Jill, Nyberg Sørensen, Ninna, 2015. Migration risk campaigns are based on wrong assumptions. DIIS Policy Brief.

Appiah-Nyamekye Sanny, Josephine, Rocca, Camilla, Schultes, Ines, 2020. 'Updata-ing' the Narrative about African Migration. MIF Joint Research Paper, URL https://www.afrobarometer.org/publication/updata-ing-the-narrative-about-african-migration/.

Bah, Tijan L., Batista, Catia, 2020. Understanding Willingness to Migrate Illegally? Evidence from a Lab-in-the-Field Experiment. NOVAFRICA Working Paper, 1803, URL https://novafrica.org/wp-content/uploads/2019/05/TijanBah_Understanding-willingness-to-migrate-illegally_Bah_Batista.pdf.

Bah, Tijan L., Batista, Catia, Gubert, Flore, McKenzie, David, 2023. Can information and alternatives to irregular migration reduce "backway" migration from The Gambia? J. Dev. Econ. 165, 103153.

Baláž, Vladimir, Williams, Allan M., Fifeková, Elena, 2016. Migration decision making as complex choice: Eliciting decision weights under conditions of imperfect and complex information through experimental methods. Popul. Space Place 22 (1), 36–53.

Barron, Kai, 2021. Belief updating: does the 'good-news, bad-news' asymmetry extend to purely financial domains? Exp. Econ. 24 (1), 31–58.

Baseler, Travis, 2023. Hidden income and the perceived returns to migration. Am. Econ. J.: Appl. Econ. 15 (4), 321–352.

Batista, Catia, McKenzie, David, 2023. Testing classic theories of migration in the lab. J. Int. Econ. 145 (103826).

Bazzi, Samuel, Cameron, Lisa, Schaner, Simone, Witoelar, Firman, 2022. Information, Intermediaries, and International Migration. Upjohn Inst. Work. Pap. (22-372).

Beam, Emily A., McKenzie, David, Yang, Dean, 2016. Unilateral facilitation does not raise international labor migration from the Philippines. Econom. Dev. Cult. Chang. 64 (2), 323–368.

Beber, Bernd, Scacco, Alexandra, 2022. The myth of the misinformed migrant? Survey insights from Nigeria's irregular migration epicenter. Ruhr Econ. Pap. 957, 36.

Bhatiya, Apurav Yash, Chakravorty, Bhaskar, Imbert, Clement, Rathelot, Roland, 2023. Expected benefits and costs of migration for rural youth: Experimental evidence from India. Unpublished manuscript.

Bilsborrow, Richard E., 2016. Concepts, definitions and data collection approaches. In: White, Michael J. (Ed.), International Handbook of Migration and Population Distribution. Springer Netherlands, Dordrecht, pp. 109–156.

Borjas, J., 1992. Self-selection and internal migration in the United States. J. Urban Econ. 32, 159–185, URL https://www.sciencedirect.com/science/article/abs/pii/0094119092900034.

Bryan, Gharad, Chowdhury, Shyamal, Mobarak, Ahmed Mushfiq, 2014. Underinvestment in a profitable technology: The case of seasonal migration in Bangladesh. Econometrica 82 (5), 1671–1748, URL https://onlinelibrary.wiley.com/doi/abs/10.3982/ECTA10489.

Bryan, Gharad, Morten, Melanie, 2019. The aggregate productivity effects of internal migration: Evidence from Indonesia. J. Polit. Econ. 127 (5), 2229–2268.

Burda, Michael C, Härdle, Wolfgang, Müller, Marlene, Werwatz, Axel, 1998. Semiparametric analysis of german east-west migration intentions: Facts and theory. J. Appl. Econometrics 13 (5), 525–541, URL https://www.jstor.org/stable/223223.

Coutts, Alexander, 2019. Good news and bad news are still news: experimental evidence on belief updating. Exp. Econ. 22 (2), 369–395.

Dahl, Gordon B., 2002. Mobility and the return to education: Testing a roy model with multiple markets. Econometrica 70 (6), 2367–2420, URL https://www.jstor.org/stable/3081990.

de Haas, Hein, 2010. Migration and development: A theoretical perspective. Int. Migr. Rev. 44 (1), 227–264.

de Haas, Hein, Castles, Stephen, Miller, Mark J., 2019. The Age of Migration: International Population Movements in the Modern World. Bloomsbury Publishing.

Detlefsen, Lena, Heidland, Tobias, Schneiderheinze, Claas, 2022. What explains people's migration aspirations? Experimental evidence from Sub-Saharan Africa. SSRN Electron. J..

Dunsch, Felipe Alexander, Tjaden, Jasper, Quiviger, William, 2019. Migrants as Messengers. The Impact of Peer-to-Peer Communication on Potential Migrants in Senegal. In: Impact Evaluation Report, IOM, Geneva, OCLC: 1289168193, URL https://publications.iom.int/books/migrants-messengers-impact-peer-peer-communication-potential-migrants-senegal-impact.

Fafchamps, Marcel, Shilpi, Forhad, 2013. Determinants of the choice of migration destination. Oxf. Bull. Econ. Stat. 75 (3), 0305–9049.

Gao, Xuwen, Song, Ran, Timmins, Christopher, 2023. Information, migration, and the value of clean air. J. Dev. Econ. 163, 103079.

Gollin, Douglas, Kirchberger, Martina, Lagakos, David, 2021. Do urban wage premia reflect lower amenities? Evidence from Africa. J. Urban Econ. 121, 103301.

Gollin, Douglas, Lagakos, David, Waugh, Michael E., 2014. The agricultural productivity gap. Q. J. Econ. 129 (2), 939–994.

Hahn-Schaur, Katharina, 2021. Leveraging Migration Information Campaigns for State and Migrant Security. Lessons Learned and Open Questions. Tech. Rep., ICMPD, URL https://www.icmpd.org/file/download/55837/file/API2021_PolicyBrief_Campaigns.pdf.

IPUMS, 2002. IPUMS International. Vol. 7.3, Minnesota Population Center.

IPUMS, 2010. IPUMS International. Vol. 7.3, Minnesota Population Center.

Khanna, Gaurav, Liang, Wenquan, Mobarak, Ahmed Mushfiq, Song, Ran, 2021. The Productivity Consequences of Pollution-Induced Migration in China. NBER Working Paper Series, 28401, URL https://www.nber.org/system/files/working_papers/w28401/w28401.pdf.

Laczko, Frank, Tjaden, Jasper, Auer, Daniel, 2017. Measuring global migration potential, 2010–2015. URL https://publications.iom.int/system/files/pdf/gmdac_data_briefing_series_issue_9.pdf.

Lagakos, David, 2020. Urban-rural gaps in the developing world: Does internal migration offer opportunities? J. Econ. Perspect. 34 (3), 174–192.

Lagakos, David, Mobarak, Ahmed Mushfiq, Waugh, Michael E., 2023. The welfare effects of encouraging rural-urban migration. Econometrica 91 (3), 803–837.

Lee, Lung-Fei, 1983. Generalized econometric models with selectivity. Econometrica 51 (2), 507–512.

Lentz, Rasmus, Piyapromdee, Suphanit, Robin, Jean-Marc, 2023. The anatomy of sorting—Evidence from Danish data. Econometrica 91 (6), 2409–2455.

Masatlioglu, Yusufcan, Orhun, Yeşim, Raymond, Collin, 2023. Intrinsic information preferences and skewness. Amer. Econ. Rev. 113 (10), 2615–2644.

McKenzie, David, Yang, Dean, 2022. Field and Natural Experiments in Migration. The World Bank, URL https://documents.worldbank.org/en/publication/documents-reports/documentdetail/099443212012229265/idu0f6457fcb092920464c08c02066bb4a4a86ef.

Möbius, Markus M., Niederle, Muriel, Niehaus, Paul, Rosenblat, Tanya S., 2022. Managing self-confidence: Theory and experimental evidence. Manage. Sci. 68 (11), 7793–8514.

OECD, 2022. A review of Ghanaian Emigrants. p. 67, URL https://www.oecd-ilibrary.org/content/publication/5342a9d4-en.

Roy, A.D., 1951. Some thoughts on the distribution of earnings. Oxf. Econ. Pap. 3 (2), 135–146.

Schans, D., Optekamp, C., 2016. Raising Awareness, Changing Behavior? Combatting Irregular Migration Through Information Campaigns. WODC Publications.

Sharot, Tali, Kanai, Ryota, Marston, David, Korn, Christoph W., Rees, Geraint, Dolan, Raymond J., 2012. Selectively altering belief formation in the human brain. Proc. Natl. Acad. Sci. 109 (42), 17058–17062.

Shrestha, Maheshwor, 2020. Get rich or die tryin': Perceived earnings, perceived mortality rates, and migration decisions of potential work migrants from Nepal. World Bank Econ. Rev. 34 (1), 1–27, URL https://academic.oup.com/wber/article-abstract/34/1/1/5588255?redirectedFrom=fulltext.

Taber, Christopher, Vejlin, Rune, 2020. Estimation of a roy/search/compensating differential model of the labor market. Econometrica 88 (3), 1031–1069.

Tjaden, Jasper, Gninafon, Horace, 2022. Raising awareness about the risk of irregular migration: Quasi-experimental evidence from Guinea. Popul. Dev. Rev. 48 (3), 745–766.

Tjaden, Jasper, Morgenstern, Sandra, Laczko, Frank, 2018. Evaluating the Impact of Information Campaigns in the Field of Migration:. Global Migration Data Analysis Centre, IOM.

Tombe, Trevor, Zhu, Xiaodong, 2019. Trade, migration, and productivity: A quantitative analysis of China. Amer. Econ. Rev. 109 (5), 1843–1872.

UNDP, 2009. Human Development Report. Tech. rep., URL https://hdr.undp.org/en/content/human-development-report-2009.

Wiswall, Matthew, Zafar, Basit, 2015. How do college students respond to public information about earnings? J. Hum. Cap. 9 (2), 117–169.

Young, Alwyn, 2013. Inequality, the urban-rural gap, and migration. Q. J. Econ. 128 (4), 1727–1785.