WORKING PAPER
#670 | 2025

WP

On the Political Economy of Urbanization: Experimental Evidence from Mozambique

ALEX ARMAND, FREDERICA MENDONÇA, WAYNE AARON SANDHOLTZ, PEDRO C. VICENTE

NOVA SCHOOL OF BUSINESS & ECONOMICS

On the Political Economy of Urbanization: Experimental Evidence from Mozambique*

Alex Armand† Frederica Mendonça‡

Wayne Aaron Sandholtz§ Pedro C. Vicente¶

PRELIMINARY
March 7, 2025

Abstract

Urbanization is a force for structural change. However, it has been slow in Sub-Saharan Africa, possibly due to conflicting political interests of national incumbents. We study the political impacts of a randomized program integrating rural migrants in a Mozambican city with the participation of local leaders. We find that the program increases the mobilization of local leaders, who conduct more electoral campaigning. We observe migrants to be more politically active and more supportive of the city incumbent (national opposition). Migrants' contacts at the origin align with the national opposition and migrate to the city. We conclude that urbanization is political.

Keywords: political economy, urbanization, rural migrants, migrant integration, political behavior, Mozambique, Africa.
JEL Codes: D72, O18, J61, O12, 055.

*We thank seminar participants at NOVAFRICA, LISER, and Nova SBE for useful comments. We are grateful to Mayor Manuel de Araújo and his team for the fruitful collaboration with the Municipality of Quelimane. Patrícia Caetano, Rita Neves, Constantin Nixdorff, and Benjamim Português provided excellent field coordination. We are also thankful to a large team of enumerators. We acknowledge main funding grants from Fundação para a Ciência e a Tecnologia (FCT), the International Growth Centre (IGC), and the Structural Transformation for Economic Growth (STEG). This work was also funded by Fundação para a Ciência e a Tecnologia (UIDB/00124/2020, UIDP/00124/2020, UID/00124, Nova School of Business and Economics and Social Sciences DataLab - PINFRA/22209/2016), POR Lisboa and POR Norte (Social Sciences DataLab, PINFRA/22209/2016). Universidade Nova de Lisboa gave ethics clearance to this project. We pre-registered the project at the AEA Registry (AEARCTR-0013066). All errors are our own.

†NovaSBE, CEPR, NOVAFRICA, and Institute for Fiscal Studies. Email: alex.armand@novasbe.pt.
‡NovaSBE and NOVAFRICA. Email: frederica.mendonca@novasbe.pt.
§NovaSBE, CESifo, IZA, and NOVAFRICA. Email: wayne.sandholtz@novasbe.pt.
¶NovaSBE, NOVAFRICA, BREAD, and CEPR. Email: pedro.vicente@novasbe.pt.

1 Introduction

Urbanization and structural change in the economy are important features of economic development (Kuznets, 1971). While the world has urbanized at an unprecedented rate in the last decades, this process has lagged behind in Sub-Saharan Africa (United Nations, 2018). This is despite significant positive wage gaps between urban and rural areas in that region (Young, 2013; Gollin et al., 2014; Hamory et al., 2021), relevant long-term human capital benefits of migrating to urban areas (Alesina et al., 2021; Cockx, 2021; Nakamura et al., 2022; Van Maarseveen, 2022; Becker et al., 2023), and the pressures of climate change (J. Henderson et al., 2017; Burzyński et al., 2022).[1] There are possibly important frictions impeding these flows (Lagakos, 2020), which opens the stage for thinking about the right policies to overcome them (Glaeser & Xiong, 2017; Bryan et al., 2020).[2]

At the same time, there is a clear sense that urbanization is related to politics, as internal migrants can change the structure of national political constituencies. Even from the perspective of international migration, the relation to politics is clear in both destination (Mayda et al., 2022; Alesina & Tabellini, 2024) and origin (Batista & Vicente, 2011; Docquier et al., 2016) locations of migrants. In Sub-Saharan Africa, it is likely that urbanization improves democracy by facilitating collective action and accountability (Glaeser & Steinberg, 2017). However, perhaps for that reason, there are important frictions to urbanization imposed by politics through policy in that region. One example is land rights, which are often limited and controlled politically (see Byamugisha (2013) for a review of related policy), constituting a fundamental impediment for peasants to leave rural areas.[3] It is then important to align political interests with effective policies that promote development through urbanization.

In this paper, we study the political effects of a policy enacted by a city government in Mozambique aiming to integrate rural migrants. Mozambique has one of the largest agriculture employment proportions in the world and is an appropriate representative of the region in that respect. It also has a ruling party (FRELIMO) that has dominated the politics of the country since independence with a tight control over rural areas and migration, namely through appointed local leaders who have authority over land allocation. Urbanization is underway in the country but has been slowed down by ruling party policy in face of fears of losing political control over the country. In fact, it is in a few cities that the opposition has

[1] Ravallion et al. (2007) remind us that urban poverty should not be disregarded when thinking about urbanization.

[2] The degree to which rural-to-urban migration can produce structural change depends largely on urban economies' ability to absorb workers pushed out of agriculture (Gollin et al., 2015; Colmer, 2021).

[3] J. V. Henderson & Turner (2020) discuss which theoretical factors could explain the relatively low levels of urbanization in developing countries.

gained ground and controls a few municipalities.[4] The policy we study was sponsored by the municipality of Quelimane, one of those opposition-held cities.

We designed and implemented a field experiment in which we randomized access to an integration program for recent rural migrants in the city of Quelimane. This program entailed the face-to-face coaching of migrants in several rounds of visits to their houses over the course of one year. The main component of the program was a job matching service to connect migrants with local employers in the city. For that purpose several censuses of jobs were implemented in the city. Migrants were given information about jobs while taking into account their preferences. Migrants were also given information about how to use mobile money services, as a way to facilitate transfers to their origin households. Finally, they were also given information about the city, its public services, and voting. Importantly, in its main variation, the program was implemented with the active participation of local leaders at the lowest administrative level of the city.

The experimental design included comprehensive measurement of outcomes through behavioral measurements of political outcomes, as well as several rounds of surveys of local leaders, migrants, residents, and migrants' contacts at the origin. Specifically, we observe whether migrants hold political objects of different kinds during a municipal election three months after the program ended and over a year after it started. We measure voter participation in the same election by checking participants' fingers for the indelible ink used at polling stations shortly after the election. And we also observe the political mobilization of leaders through designed activities related to electoral campaigning efforts. These behavioral measurements allow us to minimize biases of standard survey questions about politics (Aker et al., 2017; Grácio & Vicente, 2021; Ahmed et al., 2024). Our survey measures allow us to measure awareness of the program, interactions with leaders, and political preferences. They also allow us to have a comprehensive picture of the economic impacts of the program, namely on migrants' labor market outcomes, both perceptions about migrant integration and use of mobile money, as well as migration.

After verifying that migrants are aware of the program, we observe that, when local leaders helped implementing the program, leaders' awareness of the program increases significantly. This seems to channel program awareness with residents and migrants' origin contacts as well. We also find that the social interaction of local leaders with migrants (as well as residents) in their jurisdictions increases substantially, namely relating to job allocations. Importantly, we observe higher levels of migrants' political engagement and electoral participation, as measured by the holding of political objects and inked fingers. This set of

[4]It was also in the cities that the country witnessed major violent demonstrations in the aftermath of the recent 2024 elections - see for instance https://www.economist.com/middle-east-and-africa/2024/12/11/protests-have-shut-down-mozambique.

results is consistent with the possibility that the program increased clientelism by local leaders during the electoral campaign we follow. We also observe leaders becoming more mobilized in campaigning for the local incumbent, as well as being able to increase public support for that candidate. We find that voting for the local incumbent's party increased when considering reports by migrants' origin contacts. The economic impacts of the program are also relevant, namely to understand the referred political impacts. We find migrants to be exposed to more job opportunities, but not more likely to be working around one year after the beginning of the intervention. However, when the program is implemented by local leaders, migrants do work more hours. Consistently with the contents of the intervention, the program improves leaders' views of migrants and the use of mobile money services by migrants, namely to send transfers to their origin contacts. Finally and importantly, we find that the program induced migration to the city by migrants' contacts at the origin more than one year after the program finished. We infer from this set of results that helping urbanization through migrant integration in a Mozambican city is a powerful political tool for the local incumbent, which is likely to further the process of development-oriented urbanization.

Our paper relates to the branches of the literature on the politics and the policies of urbanization. The literature on the political economy of urbanization is scarce. Davis & Henderson (2003) show a first correlation between urbanization, democracy, and fiscal decentralization. And, indeed, the recent literature shows that decentralization boosts local development through better public services/policies in developing countries (Gulzar & Pasquale, 2017; Dahis & Szerman, 2024). Majumdar et al. (2004) establish a theoretical relationship between urbanization and the political interests of those in power. Both Hodler & Raschky (2014), and Burgess et al. (2014) find that ethnic favoritism, involving the politically biased geographical allocation of resources, is rampant in countries with weak institutions, namely in Africa.

Closer to our paper, a few recent papers have related politics with policy. Akhtari et al. (2022) show that political turnover in mayoral elections in Brazil affects positively public service provision by local governments. Callen et al. (2023) study public sector absenteeism in Pakistan and observe that a reform was more effective where political competition was greater. In the direction of studying the impact of policy on politics, while Manacorda et al. (2011) find political gains from enacting an anti-poverty program, others have found harmful political effects (Blattman et al., 2018; Zimmermann, 2021; Sandholtz, 2023). Other related contributions analyze clientelism in developing countries, an effective political strategy (Wantchekon, 2003) targeting the most vulnerable (Bobonis et al., 2022), which we take to be an important part of electoral politics in Mozambique.

Turning to policies directed at managing urbanization, Wallace (2014) describes in detail the recent

approach of Chinese authorities, including repression and positive incentives from rural areas. Michaels et al. (2021) is an exception, like us, in looking at the impacts of city policy on urbanization in Africa: they find that modest infrastructure investments in Tanzanian cities facilitate long-run neighborhood development. Related, but in the opposite direction, Feler & Henderson (2011) show that withholding public services to the informal housing sector is used in Brazil to deter urbanization.[5]

Our paper is closely connected to the literature studying policies that address frictions to urbanization. Bryan et al. (2014) randomize a small incentive to households in rural Bangladesh to temporarily out-migrate during the lean season. The incentive increases migration, consumption at the origin, and re-emigration after the incentive is removed. Consistently, Bryan & Morten (2019) estimate substantial aggregate productivity gains from reducing barriers to internal labor migration in Indonesia, accounting for movement costs. Also related to the costs of moving, Morten & Oliveira (2018) find clear welfare gains from urbanization movements relatable to road improvements in Brazil. Other important frictions to urbanization relate to information. While McKenzie et al. (2013) observe that migrants can have biased beliefs before migration about their future earnings, Baseler (2023) shows that providing information about urban earnings increases migration to Nairobi, Kenya, due to hidden earnings by current migrants.[6] Heavier programs directed at rural households and providing assets or cash transfers also yield significant impacts on rural to urban migration and structural change: Ardington et al. (2009) analyze a cash transfer program in South Africa; Banerjee et al. (2021) assess the long-term effects of an asset program targeting the ultra-poor in India; and Balboni et al. (2022) look at a similar program in rural Bangladesh.

The city integration intervention we study in this paper is directly related to three strands of the literature. First, the literature on labor market policy interventions in developing countries.[7] This is reviewed by McKenzie (2017), who finds that many evaluations of these policies find no significant impacts on either employment or earnings. One reason could be that urban labor markets appear to work reasonably well. Consistently, Kelley et al. (2024) find that digital job matching platforms do not improve employment outcomes among vocational training graduates in India: they respond to platform access by increasing their reservation wages, and by working significantly less. Other recent papers have found similar difficulties of job matching (Abebe et al., 2017; Caria et al., 2024). However, other studies have found positive employment impacts of job matching interventions: Beam (2016) follow the impacts of a job fair in the rural

[5] In a recent literature from developed countries, city residents often create barriers to new arrivals ("NIMBYism" — see Duranton & Puga, 2023; Tricaud, 2025).

[6] In related work, Batista & Narciso (2018) demonstrate that increasing contact between migrants and their families has positive impacts on remittances sent home.

[7] Imbert et al. (2021) find that urbanization leads to labor-oriented technological change and the adoption of labor intensive product varieties.

Philippines for domestic and overseas work, Abebe et al. (2021) evaluate the impact of helping young job seekers signal their skills to employers in Addis Ababa, Ethiopia.[8] The second line of work behind our intervention is the one on financial inclusion. Suri & Jack (2016) find that the M-PESA in Kenya led to changes in the occupational choice of women from agriculture to business. Batista & Vicente (2024) run a field experiment introducing mobile money in rural Mozambique and conclude that it incentivized rural-to-urban migration. The third and final stream of work relates to persuading local actors to favor the integration of migrants. While Cattaneo & Grieco (2021) show that a narrative about the positive impact of immigrants on the hosting economy affects natives' behavior towards migrants, Baseler et al. (2023) find that redistributing social benefits towards natives turns them more sympathetic about the integration of refugees in Uganda.[9]

This paper is organized as follows. We first describe the context of our study in Mozambique. Then, we describe our experimental design, including treatments, sampling, randomization, measurement, estimation strategy, and hypotheses. Subsequently, we show results and conclude.

2 Context

Mozambique is one of the poorest countries in the world, with the 5th lowest GDP per capita in the world (at USD 1566). This is related to the fact that close to 70% of the population is employed in agriculture, with very low levels of productivity. With 39% of the Mozambican population living in urban areas in 2023, urbanization has been happening in the country, as this figure has clearly increased in the last 20 years: it was 30% in 2004. However, the proportion of urban population is still clearly below the average of Sub-Saharan Africa (43%) and of the world (57%).[10]

At the same time, Mozambique has been governed by a strong party at the central level (FRELIMO) since independence in 1975. Until the first elections in 1994, the approach was explicitly socialist with tight control over the territory from the central government through appointed local leaders. After that, despite externally-induced economic reforms, the ruling party has not dramatically changed the development and political approach over the territory, maintaining the traditional discourse in favor of rural

[8] In related work, Dillon et al. (2024) follow the assignment of small and medium enterprises in Tanzania to be listed in a telephone directory and find that they expand their communication networks, increase sales, and make greater use of mobile money, with positive spillovers to firms in the same village.

[9] Two related papers, Shenoy & Zimmermann (2022) and Bergeron et al. (2023), highlight how the knowledge of local leaders can be valuable in both public administration and political mobilization, respectively. Our paper shows how these two aspects of local leaders' roles are connected.

[10] All figures are from the World Development Indicators 2024, latest available years.

development, which emphasizes supporting the small peasant, with no clear benefits seen in urbanization. One important example in terms of consistent public policy is the continuing conservative approach over land property, which is to this day (since independence), held by the state in the whole country. The political interests of the ruling party are difficult to separate from these positions: while in rural areas the ruling party easily controls the population through incentives mediated by appointed local leaders (e.g., who have a degree of discretion in allocating land), that is less the case in urban areas. In fact, the ruling party only lets municipal elections happen in cities – it is only in a few of those that the opposition has made some ground and won elections.

Quelimane is one of those cities, as it has been held by the opposition since 2011, when the current mayor, Manuel de Araújo, was elected for the first mandate. He now represents the main opposition party, RENAMO. Like many African cities, Quelimane has grown in recent decades, largely driven by the natural arrival of rural migrants seeking better economic opportunities. Being the capital and largest city of the province of Zambézia, Quelimane has received many rural migrants from that province but also from the rest of the country. The city's population more than doubled since 2010 to reach over 500,000 today, making it the 7th largest city in Mozambique.[11] The city is divided geographically into three administrative layers, depending on the municipal council headed by the mayor: five "administrative posts," which are subdivided into 54 "neighborhoods," which are subdivided into 540 blocks. Each block is headed by a block leader, who is appointed by the hierarchical structure stemming from the mayor.[12]

Block leaders are therefore the lowest level of city government hierarchy. They do not receive formal wages but tend to be respected figures in their blocks. Their role consists largely in helping to settle conflicts between block residents, which requires knowing the residents and being aware of when people move in or out. They also serve as a bridge to the neighborhood leaders and the rest of the municipal government hierarchy, being responsible for passing information up the chain about the needs of the block (e.g., resources for coping with floods, which are common in Quelimane), as well as down the chain, enabling the local implementation of public projects (e.g., construction works). The block leader is not a formal partisan member, and less than two-thirds of block leaders report being registered in a political party at the beginning of our project (though over 90% of those belonged to RENAMO). Insofar as they owe their position to the incumbent government, their incentives generally align with its electoral fortunes.

[11] World Population Review: https://web.archive.org/web/20240123115845/https://worldpopulationreview.com/world-cities/quelimane-population.

[12] However, there is often some element of popular will in their selection: block residents can propose a candidate for the job, and neighborhood chiefs often approve them.

It is important to note that our project was implemented in the final half of the previous mandate of the current mayor of Quelimane and that we measure most outcomes during the October 2023 municipal elections in the city.[13] These elections were won by the incumbent mayor/RENAMO after a heated post-electoral period which ended with a supreme court decision supporting RENAMO's allegations of electoral fraud against FRELIMO.

3 Experimental design

3.1 The program

The program we study in this paper provided an integration package to support recent rural migrants in Quelimane, Mozambique. It was sponsored by the corresponding municipality and known as "Quelimane trabalha com todos" (Quelimane works with everybody). The program was tailored to recent rural migrants whom we define as having set residence in Quelimane up to 12 months prior to the beginning of the project implementation, and as intending to stay in the city at least one year. It aimed to reduce the economic and psychological barriers migrants face, easing their integration into their new environment (McKenzie, 2024). It featured individual coaching sessions through five house visits to migrants, entailing approximately one hour of face-to-face contact per visit. The first round of visits was in August 2022 and the last in July 2023. Contents included general information about the city, job matching between the migrant and opportunities in the city, and an introduction to mobile money. When migrants were not at home, appointments were made to visit at another time. Importantly, in its main treatment variation, the program delivery was mediated by the block leader. We now turn to detailing these contents.[14]

3.1.1 Job matching, mobile money, and the city

The main component of the face-to-face visits was job matching: most of the rural migrants in Quelimane are economic migrants who come to the city in search for better economic opportunities. Program participants were allocated contacts (name and phone number) of potential job offers to rural migrants. To collect the information relating to these job offers, we conducted two censuses of job offers by visiting every house and establishment in the city[15] as well as three rounds of job updating by phone with the

[13] We also have some data from the 2024 national elections, which we report in the Appendix.

[14] A full coverage is found in Section A of the Appendix. In Tables A1-A2 in the Appendix we report that each migrant in the program had on average three visits. The fifth visit had the largest reach, as it included 72% of the targeted migrants.

[15] Details are given in Figures A4-A5 in the Appendix.

previously collected contacts. We managed to collect 1582 job offers during this project.[16] Program implementers allocated these jobs to specific migrants based on the elicitation of the migrants' job preferences. Each migrant was entitled to up to ten job offer possibilities and given the corresponding contacts. In the last two visits, the implementer linked each potential employer and migrant by contacting the employer during the house visit and setting an interview date. As a final step of each visit, implementers always sent a text message to each migrant with the potential employers' contacts. The main sectors of the job opportunities that were shared in this program included construction and housekeeping (see full details in Table A3 in Appendix). In the fourth visit, program participants were shown a video of the testimony of a migrant who successfully integrated in the city through finding a job with the help of the program.[17]

Another important component of the information package shared through the program was an introduction to mobile money. As part of the face-to-face contact, program implementers shared a presentation on Mozambique's leading mobile money service (M-PESA).[18] It included information on how to open an account, cash-in and cash-out electronic money, as well as on how to make transfers. In the third round of the visits, participants were given a small endowment to cash-in and transfer to a contact at the origin district. This module served the purpose of incentivizing the opening of accounts for those not holding one, and trialing transfers to the migrants' contacts at the origin using mobile money, which was likely to induce information sharing (namely about the program) between treated migrants and their connections/household at the origin. The inclusion of this module was guided by the idea that the financial inclusion of migrants is an important dimension of their integration.

Finally, institutional information about the city was added to the package. The first two visits to migrant participants in the program included a general presentation of the city developed by the municipality which encompassed information about the political context of the city, administrative divisions, documentation needed for residence in the city, electoral registration and voting process (namely in face of the 2023 municipal elections), as well as access to local schooling, healthcare, other infrastructures, and culture. Details are provided in the Appendix. By the third visit, the presentation was incorporated into a survey platform, which allowed to turn it into an interactive experience centered on asking migrants questions regarding the information presented.

[16] In related work, Dillon et al. (2024) examine the effects on small and medium enterprises in Tanzania of being listed in a telephone directory. They find that the firms expand their communication networks, increase sales, and make greater use of mobile money, with positive spillovers to firms in the same village.

[17] Details, including the script of the video (Figure A6), are included in Appendix.

[18] This is included in Figure A9 in the Appendix.

3.1.2 The participation of block leaders

The main version of the program submission contained the explicit support and active participation of the block leaders corresponding to the blocks where migrant participants resided. In each round of visits the field team initiated face-to-face conversations with the visited migrants by showing a video on tablets with a short message from the corresponding block leader, who expressed clear support for the program and incentivized migrants to follow the instructions and advice of the program implementers. At the end of each visit, implementers reminded migrants about the leader's name and contact information to enable reaching him/her in case of need. The field team also sent a text message with the leader's name and contacts at the end of the conversation.

Block leaders were encouraged to be present in all rounds of face-to-face contact with the migrants. However their presence was only systematic in the fifth visit when a large majority (77%) participated in the house visits belonging to their corresponding blocks alongside the field team. We note that in the fourth round all leaders were asked to emphasize the relevance of participating in elections when speaking in the video that was shown in the face-to-face visits to migrants. The content and framing of such message was left at their discretion, with most leaders delivering a political message related to the approaching municipal elections of 2023.[19]

3.2 Sampling and randomization

Our baseline sample of recent migrants (as defined above) set the stage for sampling in this project. It is representative of the full population of households containing at least one recent migrant, clustering by city blocks. Our enumerators sampled within each block by starting at a randomly chosen point and following a deterministic algorithm to dictate the order in which they approached households to ask if they included any recent migrants. In all affirmative cases, they conducted a baseline survey interview. In each block, enumerators continued this sampling process until all houses had been visited, or until (at least) eight migrant households had been found. We display the distribution of number of migrants found per block of the city in Figure B1 in Appendix. No migrants were found in a few blocks, which made them not eligible for treatment. Our study sample is then composed of 493 city blocks.

We note that the sample of migrants in the measurement of our study was recruited in two waves: the initial one already referred, from October to December 2021, and a second wave recruited in September

[19] More details on the participation of the block leaders in the program, including scripts of videos, are provided in Section A.4 in Appendix.

2022.[20] We interviewed 2320 migrants in the first wave of recruitment and another 1312 migrants in the second wave (3632 in total).[21]

At the same time of the recruitment of the first wave of migrants, we also sampled residents in each block, defined as those residing in Quelimane for more than two years. The sampling process was equivalent to the one mentioned for migrants but with the above-referred criterion. We targeted two residents per block and ended up with a total of 995 residents in our sample. Immediately following the sampling of migrants (first wave) and residents in January to February of 2022, we interviewed the block leaders corresponding to the 493 blocks in the experiment (we managed to reach 441 at that point in time). Figure B2 in the Appendix depicts the geographical distribution of the different samples.

During the first project surveys, migrants were also asked to report the name and contact of their closest person in their origin districts, with whom they still keep contact.[22] We collected 2519 contacts. From these contacts 95% are family members. Migrants' children and siblings are the ones most represented. Hence, there is a high probability that these contacts at the origin districts of the migrants offer an appropriate representation of migrants' households at the origin.

We randomly allocated city blocks in our experiment to three comparison groups: one receiving the full treatment, including the participation of the block leader (leader treatment); one receiving the same integration package but with no participation of the block leader (basic treatment); and a control group receiving no intervention. Randomization was stratified within strata of up to three blocks. These strata were created by sorting blocks within neighborhoods by the number of migrants in our baseline survey.[23] The 493 blocks in the study were then split into the leader treatment (165 blocks), the basic treatment (163 blocks), and the control group (165 blocks). A map representing the randomization of blocks into treatment conditions is presented in Appendix Figure B3.

3.3 Measurement

Our measurement in this field experiment comes from a set of surveys and behavioral activities we organized. We display the full timeline of the project in Appendix Figure C1. We collected survey data from

[20] In this wave, we used the same criteria to define (recent) migrants as before. We looked for three migrants in each of the 493 blocks of the study. By the time the program began in August 2023, migrants from the first wave had been in the city for between about 1 to 2 years; migrants in the second wave began the second round of the intervention having arrived in the city at most 12 months prior. The migrants in the second wave were recruited after treatment had already begun; the first round of the intervention they received was the second, so the treated participants in this wave were only targeted with four rounds of treatments in total. Our results control for this sample difference.

[21] This design allows some additional variation in treatment effects employing date of arrival in the city.

[22] The survey question that we employed for this purpose was: "Now think about the adult person to whom you feel closest, who still lives in your district of origin, and with whom you can talk by phone. Who is that person?"

[23] Each stratum consists of up to three blocks because some neighborhoods' number of blocks is not divisible by three.

block leaders, migrants, and residents at the baseline (as described above), at a midline close to the end of the intervention (before the last round), and at the endline (after the end of the intervention, in August - leaders - and November - migrants and residents - of 2023). Two phone surveys of migrants' contacts at the origin were conducted at the time of the midline and endline (as defined above).[24] We also conducted two small post-endline phone surveys, one for migrants' contacts at the origin, approximately one year after the treatment finished, and another for leaders, after the October 2024 national elections. All surveys measured the demographic and socioeconomic traits of the corresponding individuals and households. In addition, they measured civic and political attitudes.

We also designed and implemented a set of behavioral measures related to political behaviors. The first behavioral measurement was directed at migrants and residents and aimed at capturing their political mobilization. While surveying migrants and residents at the midline and the endline (right after the 2023 municipal election), enumerators looked for displayed political objects in their homes or vests, like stickers, posters, t-shirts, caps, etc, and recorded their observations.

The second behavioral measurement was on voter turnout after the October 2023 local elections in Quelimane through the systematic checking of inked fingers of block leaders, migrants, and residents. In Mozambique, like in many other countries, voters' index fingers are colored with purple ink at the polling station after voting. We understood this feature of electoral procedures as a good opportunity to measure political participation in our study participants. To do so, we hired a large team of enumerators who canvassed the whole city in the two days following the election day, checking whether participants' fingers were inked.

The third measurement was a Structured Community Activity (SCA) (Casey et al., 2012) which targeted block leaders' campaign mobilization as measured by the ability to get together bicycle taxi drivers to campaign for the incumbent mayor (just before the 2023 municipal elections). In this activity, block leaders were instructed to collect contacts of bicycle taxi drivers in their blocks (lists were collected per leader/block) and to get them together for a block meeting at a specific date set and observed by enumerators. Bicycle taxi drivers are the main means of transportation in Quelimane, and highly associated with the incumbent mayor in Quelimane, who initiated and has used bicycle rallies in all his political campaigns. With this activity, we expect to measure leaders' campaign efforts and political influence in favor of the local incumbent.

The fourth behavioral measurement was an SCA based on the distribution of stickers by block leaders

[24] Tests of differential attrition for the midline and endline survey waves of migrants, residents, and origin contacts, as well as the two phone follow-ups, are provided in Table B1. We do not find any significant differences between treatment conditions.

praising the mayor for the integration of migrants through the program. Each leader received 40 brown stickers and was instructed to distribute them among households in their blocks. The protocol encouraged hanging the stickers on the houses' front doors. This allows us to identify stickers visible on migrants' houses as a measure of political mobilization and block leader influence, as well as of corresponding responsiveness by citizens. We also had a version of this sticker measurement directly distributed to migrants and residents (not through block leaders). The corresponding stickers had a different color (pink) but were otherwise identical. During the endline survey with the migrants and the residents, enumerators observed whether the stickers (of both types) were hanging on the doors of respondents. The two versions of the stickers allow us to isolate the role of the leader when distributing stickers. We show images of these stickers in Figure D2 in Appendix.[25] In Appendix E, we provide a detailed description of all outcome variables we employ in this paper.

4 Estimation strategy and hypotheses

We estimate treatment effects of the leader and basic interventions employing standard econometric analysis of experiments. The following specification is estimated using ordinary least squares (OLS):

$$Y_{ibs} = \alpha + \beta_L TL_b + \beta_B TB_b + \lambda_s + \omega \mathbf{Z}_b + \gamma \mathbf{X}_i + \varepsilon_{ibs} \quad (1)$$

where TL_b and TB_b are indicator variables for living in a block in the leader treatment or the basic treatment (respectively), λ_s are strata fixed effects, $\mathbf{Z}_b$ is a vector of block-level controls,[26] and $\mathbf{X}_i$ is a set of individual characteristics[27]. ε_{ib} is an individual-specific error term.

When baseline data are available, we implement an ANCOVA specification by including the dependent variable at baseline ($Y_{ibs,0}$) as a control variable:

$$Y_{ibs} = \alpha + \beta_L TL_b + \beta_B TB_b + Y_{ibs,0} + \lambda_s + \omega \mathbf{Z}_b + \gamma \mathbf{X}_i + \varepsilon_{ibs}. \quad (2)$$

For outcomes measured in both post-baseline surveys ($t = 1, 2$), i.e., midline and endline, we can also estimate effects employing the multiple measures in time using the following specification (McKenzie,

[25] See Appendix D for further details on measurement.
[26] This includes a proxy for the block population.
[27] These are: age, gender, and the baseline survey wave (in the case of migrants).

2012):

$$Y_{ibs,t} = \alpha + \beta_L TL_b + \beta_B TB_b + \sum_{t=1}^{2} \delta_t + \lambda_s + \omega \boldsymbol{Z}_b + \gamma \boldsymbol{X}_i + \varepsilon_{ibs,t} \quad (3)$$

where δ_t boil down to one time dummy distinguishing post baseline periods 1 and 2.

Standard errors are clustered at the city block level in all regressions of individual migrants, residents, or migrants' contacts at the origin. In our main results, in order to address potential biases from multiple hypotheses testing, we report p-values of the procedure described in Romano & Wolf (2016), which we employ to account for multiple hypothesis at the row/treatment level, within each table of outcome variables. This procedure improves on the ability to detect false hypotheses by capturing the joint-dependence structure of individual test statistics on treatment impacts.

In the analysis of this experiment we follow closely the pre-analysis plan we published as part of this project (Armand et al., 2024). Our main hypotheses are the following.

First, we expect that both treatments increase awareness of the program. Interaction with local leaders could also increase. Migrants could participate more in campaigning and elections. Local leaders could take the opportunity of the program to mobilize migrants politically in favor of the local incumbent, namely through clientelism. As a result, migrants could become more supportive of the local incumbent. It is possible that these effects travel to migrants' contacts at the origin as well.

In terms of economic effects of the treatments, we expect, in face of the additional job opportunities, positive impacts on employment and work hours. It is possible that views about migrants improve, and that migrants increase their use of mobile money, namely for transfers to migrants' households at the origin. Importantly, we hypothesize potential impacts on migration, in terms of retaining migrants in the city, and promoting the migration of their relatives from the origin.

Linking to the specifications above, and assuming the referred outcome variables to be measured positively, we can summarize our first hypothesis as:

Hypothesis 1: $\beta_L, \beta_B > 0$.

Our design includes a treatment variation that erases the involvement of the leader in the program implementation with migrants. Our expectation is that all referred treatment effects are lower for this basic treatment than for the leader treatment. Block leaders are locally influential figures and are expected

to increase the effectiveness of the program. We expect that block leaders are particularly able to influence political outcomes, given the political dimension of their role and its clientelistic nature. Our second hypothesis is then:

Hypothesis 2: $\beta_L > \beta_B$.

5 Results

5.1 Descriptive statistics

We begin by describing our sample at the baseline (Table B2 in the Appendix). Control blocks have 145 households on average.[28] They also have 4.8 sampled migrants, a 43%-probability of illegal construction, approximately 25 taxi drivers, and similar distances to closest school and water fountain, between 1.5 and 1.6 Kms.

Block leaders in the control group are on average 50 years old, and are typically male (67%). Seventy-two percent are married or cohabiting and 66% are Catholic. Education levels are relatively low, with 22% illiterate and 42% having completed primary school. Ninety-five percent of the block leaders own the dwelling where they live. We also observe that leaders have been in office for 3.6 years. Seventy-seven percent of them report liking migrants but only 2% think the government is helping the poor (at the baseline). These results are shown in Appendix, Table B3.

As shown in the Appendix, Table B4, the sample of migrants collected during the baseline is much younger, with an average of 24 years of age for the control group. Sixty-six percent are male. In view of the mean age, it is not surprising that only 37% are married or cohabiting and that their average number of children is just over one. Fifty-nine percent of the migrants in the control group are Catholic. In terms of schooling, 34% are illiterate and 32% have completed primary school. Twenty-two percent had no occupation at the baseline. Only 24% of the migrant sample at the baseline had contacted the local leader in the previous year. Fifty percent moved to Quelimane to work. They report their main struggle to be finding a job (33%).

Now turning to the sample of residents (Table B5 in the Appendix), we observe that the control group

[28] This household proxy was constructed based on the visits conducted throughout the city during the first census of job offers. Field administrators were instructed to attempt every house in every block and register it as a survey entry, regardless of the outcome.

had a mean age of 34 years and 40% are male. Consistently with their older mean age, 46% percent of them are married or cohabiting and they have an average of slightly more than 2 children. Sixty-three percent are Catholic. Despite the lower levels of illiteracy - 18% - only 36% percent of residents completed primary schooling. Forty percent of the resident sample had contacted the local leader in the previous year.

Finally, turning to the table for the migrants' contacts at the origin district (Table B6 in the Appendix), we see that this sample is slightly older than the migrants' one, with a mean mean age of 33 years. Fifty-six percent are male. Forty-seven percent are either married or cohabiting and they have on average 2 children. Surprisingly, only 14% are illiterate but only thirty-five percent completed primary education. Eighteen percent have no occupation and only 6% of contacts at the origin are students.

These tables also show balance between treatment and control groups. From the 180 tests shown including the null that the characteristics of the treatments are (individually or together) the same as the control, as well as the null that the two treatments are jointly equal to zero in explaining the characteristics of the sample units, we do not find more significant tests at standard levels than what is expected (10%). This reassures us that the randomization was effective at building comparable groups.

5.2 Treatment adherence

We now turn to the analysis of treatment effects, starting with the outcome variables related to treatment adherence. We begin by showing results on program awareness in Figure 1. These include survey questions asked to leaders, migrants, residents, and migrants' contacts at the origin districts on whether they have heard about the program "Quelimane trabalha com todos," and whether the survey respondent, the family of the respondent, rural migrants, and block people in general were involved in the program. Our regressions employ the stacked specification in equation 3 including midline and endline survey measures.[29]

We find that the leader treatment was particularly effective with leaders: they are more likely to have heard about the program, by 13 percentage points; they are also more likely to report being involved, by 18 percentage points, as they are to report that rural migrants and block people in general were involved, by 13 and 12 percentage points, respectively. All referred effects are significant at the 1 percent level. They are also statistically different from those of the basic treatment, which is never significantly different from the control. We note that a substantial proportion of the leaders in the control group has heard about the

[29] We also include in Appendix Tables H1 to F4, the corresponding tables with fuller details.

program (72%).

We observe positive and significant effects of both treatments for migrants: hearing about the program increases by 7 and 8 percentage points for the leader and basic treatments, respectively. These effects are statistically significant at the 1 percent level and undistinguishable. Similar patterns are found for the migrant respondent's involvement, and his/her reports of rural migrants' and block people involvement. The leader treatment also leads to an increase in the probability of the migrant reporting the involvement of his/her family in the program, differently from the basic treatment, although this difference is only marginally significant. Seventy percent of migrants in the control group have heard about the program, which is suggestive of contamination of treatment effects to control areas.

Treatment effects on residents and migrants' contacts at the origin are less clear. However, we find that the leader treatment increases the probability that residents report rural migrants to be involved in the program, by 4 percentage points (significant at the 10 percent level), and the probability that migrants' origin contacts report hearing about the program. We find several significant differences between the leader and the basic treatments in the direction of stronger impacts of the leader treatment.

In Table 1 we show treatment effects for outcome variables measuring social interactions with the block leader. The first two outcomes are measured from the leader side: in column (1) we analyze whether the leader reports knowing any migrants in his/her block; in column (2) we consider the percentage of migrants in our sample that the leader recognizes individually. The remaining outcomes considered are for migrants and for residents: whether they know their block leader, whether they contacted him/her since the previous survey wave, and whether they resorted to the leader for a job, i.e., whether they contacted or paid the leader for a job. We employ specifications 1 and 3 depending on data availability.

We find systematic effects of the leader treatment on social interactions with the leader. This intervention increases the probability of the leader knowing any migrants in his/her block by 9 percentage points and the proportion of migrants recognized by the leader by 6 percentage points. Migrants are 7 and 4 percentage points more likely to know and to have contacted the leader. They resort to leaders for jobs more frequently as well – this probability increases by 1 percentage point. Interestingly, we find effects on interaction of leaders with residents as well: they are more likely to know and to have contacted the leader by 8 and 6 percentage points, respectively; and to resort to the leader for jobs by 2 percentage points. The coefficients of the basic treatment are never significant, except for migrants resorting to leaders for jobs. Most effects of the leader treatment are statistically different from the effects of the basic treatment.

We conclude that the leader treatment was particularly effective in creating awareness about the pro-

gram, namely with its direct participants, i.e., leaders and migrants. This was despite significant awareness about the program in the control blocks. We also report that information about the leader treatment seems to have reached residents and migrants' contacts at the origin, more strongly than the basic treatment and the control group. Finally, the leader treatment systematically increased the interactions of block leaders with migrants and residents, namely regarding the allocation of jobs, consistently with the contents of the interventions.

5.3 Political effects

We now turn to the political effects of the program. We begin by describing impacts on political participation. This is shown in Table 2. In columns (1) and (2) we analyze impacts on whether enumerators observed migrants and residents (respectively) holding/owning any political objects when they were interviewed in their homes. In columns (3)-(5) we report treatment effects on our measure of electoral turnout in the 2023 municipal elections, which is based on the observation of inked fingers in the two days after the election day. We employ the stacked specification in 3 including midline and endline measures in columns (1)-(2). In columns (3)-(5) we employ the simple specification in 1.

We find that migrants are more likely to hold political objects when faced with both treatment conditions. The magnitudes of these effects are 2 and 3 percentage points for the leader and the basic treatments, both statistically significant at the 1 percent level, not distinguishable from each other. These effects pass the Romano-Wolf multiple hypotheses testing. We do not observe significant effects for residents. We note that the additional political objects held by migrants are from both the local incumbent RENAMO and the national ruling party FRELIMO. This is an indication that migrants are mobilized in general for the election: they are likely to be more often accepting political objects from both parties. This is shown in Appendix Table F5.

Electoral turnout as measured by inked fingers increases with the leader treatment for migrants. The magnitude of this effect is 3 percentage points, statistically significant at the 10 percent level (the Romano-Wolf procedure yields marginal statistical significance at the same level). This is over a high 70% rate of electoral participation in the control group. We also find a large point estimate for leaders which is statistically insignificant in the standard individual test. This is likely related to the lower statistical power we have with leaders and their 90% rate of electoral participation in the control group. Interestingly, we find a negative point estimate for residents, which is not significant either. This effect is however statistically different from the effect of the basic treatment (at the 5 percent level). The effects of the basic

treatment, although positive, are never significant in these regressions.

In Table **??**, we show treatment effects on political support for the local incumbent mayor and his party (RENAMO). In columns (1)-(2), we show results employing outcome variables from the cyclists SCA in which block leaders were asked to mobilize bicycle taxi drivers for campaigning in favor of the incumbent mayor. We employ variables on whether leaders report any cyclists mobilized (column 1) and the number of cyclists observed by enumerators to be mobilized (column 2). In column (3) we show results for the stickers SCA, brown version, in which leaders were asked to distribute stickers associating the mayor to the program we study ("Quelimane trabalha com todos"). Column (4) shows results for the stickers SCA version in which migrants and residents in our sample were directly targeted (not through leaders) – the pink version of the stickers. The hanging of these stickers visibly on house doors implies some degree of political support for the mayor. The brown version also carries an important clientelistic interpretation as leaders are asking for a public display of support for the mayor after they were involved in the provision of benefits to migrant citizens through the integration program. In columns (5)-(7) we show results on self-reported voting for RENAMO in the 2023 municipal elections for migrants, residents, and migrants' contacts at their origin districts, respectively. We employ the simple specification in 1 in all regressions of this table.

We find positive effects of both treatments on leader mobilization of cyclists for campaigning in favor the incumbent mayor. This is the case when considering the number of cyclists observed by enumerators: the magnitude of this effect is 0.7 more cyclists for both treatments (both are significant at the 10 percent level and pass multiple hypotheses testing). The effect on the extensive margin of reporting any cyclists is only significant for the leader treatment: this probability increases by 11 percentage points (significant at the 5 percent level). The differences between the two treatments are not significant for these outcomes.

Brown stickers, i.e., those distributed by the block leaders in their blocks, are more often found hanging on doors of migrants and residents in blocks subject to the leader treatment. The magnitude of this effect is 4 percentage points, significant at the 5 percent level (and passing the Romano-Wolf procedure at the 10 percent level). This effect is significantly different from the one of the basic treatment, which is close to zero. We infer that the leader treatment was effective at increasing support for the mayor through likely clientelism. The effects of the treatments on the hanging of pink stickers, i.e., those distributed directly to migrants and residents, are insignificant. This pattern of results emphasizes the importance of leader influence on political behavior.

We also find some evidence that treatments led to increases in voting for RENAMO in the municipal

elections of 2023. We find a positive and significant effect of the basic treatment for migrants, with a 2 percentage-point magnitude, significant at the 10 percent level in both standard and Romano-Wolf testing. This effect is not statistically distinguishable from the effect of the leader treatment. Importantly, the leader treatment increased reported voting for RENAMO by migrants' contacts at the origin. This is a large 13 percentage-point effect, which is statistically significant at the 10 percent level (but not surviving multiple hypotheses testing). It is also marginally different from the basic treatment. We observe a negative point estimate on voting for RENAMO when looking at the residents. This is however statistically insignificant at standard levels. Looking at the broader picture of voting for all parties (Table F6 in Appendix), we confirm that the leader treatment led migrants' contacts at the origin to state a clear intention to vote against FRELIMO, i.e., for the national opposition. The magnitude of this effect is 11 percentage points, significant at the 5 percent level, and statistically distinguishable from the basic treatment (at the 10 percent level).

In the Appendix Table F7, we show some additional results for leaders' political positions. We find that although there are no treatment effects in the municipal elections of 2023 – as 99% of leaders in the control group report voting for RENAMO –, there are some important treatment effects in 2024 measurements. We find statistically significant differences between the leader and the basic treatments showing that the leader treatment increased the probability that leaders belong to a party, RENAMO in particular. We also find that both treatments led to higher self-reported support for RENAMO in the presidential elections of October 2024. The other main parties display negative effects (although statistically insignificant). We also note an important heterogeneity pattern in Table I5 in the Appendix, which divides treatment effects between leaders (and their corresponding blocks) declaring support for RENAMO and those not doing it at the baseline. We find that effects of the leader treatment on migrants' program awareness, interaction with block leader, and political participation are driven by leaders supporting RENAMO.

We conclude that the leader treatment was particularly effective at mobilizing leaders and migrants during the electoral campaign of 2023. We also have evidence that it led to higher electoral participation by migrants and higher public display of support for the mayor. Our pattern of results is consistent with the clientelistic use of the program for political purposes. We also find that the leader treatment had political impacts with the migrants' contacts at the origin districts who have reported to vote more often for the national opposition, showing that the city integration program could have regional impacts in political terms.

5.4 Economic effects

In this section, we analyze the economic effects of the migrant integration program we study in our experiment. These can be seen as some relevant mediators for political impacts we uncovered. We begin with addressing outcomes related to migrants' labor market outcomes since job matching was the central part of the program. In Table 4 we show treatment effects for migrants on having heard about job offers in the previous 12 months, on having heard about these offers but through the program "Quelimane trabalha com todos," on whether migrants are working, on the total number of jobs that they had since the beginning of the project, on the number of hours they working per day, and on the wage they are receiving per week. We employ the stacked specification in 3 including midline and endline survey measures. We include the baseline values of the dependent variable as controls when available.

We first verify that there are clear treatment effects of both program variants on having heard about jobs. The magnitudes of these effects are 7 and 8 percentage points for the leader and basic treatments, respectively. Both are significant at the 1 percent level (and pass Romano-Wolf multiple hypotheses testing). When it comes to having heard about job opportunities through the migrant integration program, these effects are 23 and 22 percentage points, also statistically significant at the 1 percent level (and passing the Romano-Wolf procedure). Note however that 79% of the migrants in the control group report having heard about these jobs through the program, which implies considerable contamination to the control group.

Turning to employment and wages, we find that the basic treatment decreases the probability of having a job, by 3 percentage points (significant at the 5 percent level but not passing Romano-Wolf). This is not particularly surprising since Kelley et al. (2024) find a similar result from the introduction of a digital job matching platform in India (through increased reservation wages). Consistently, the basic treatment decreases the number of jobs and the number of hours worked by migrants. It also increases their wages. However, these effects are not statistically significant at standard levels. Importantly, the leader treatment produces a different pattern of results. First, it increases the number of hours worked by migrants – the magnitude of this effect is 0.4 hours per day, which is significant at the 1 percent level and passes multiple hypotheses testing at the 5 percent level. Second, it increases significantly the probability of working and the number of jobs of migrants, relative to the basic treatment. For these employment outcome variables, differences to the basic treatment are statistically significant at the 1 percent level. Like the basic treatment, the leader treatment does not change wages. This pattern of results shows that the leader treatment was effective at getting migrants to work, relative to the basic version of the intervention.

We now devote attention to a few secondary but likely direct effects of the treatments, namely on perceptions of migrant integration in the city, the main topic of the program, and on the use of mobile money, which was introduced to migrants in a module of the program. In Table F8 in the Appendix, we show results on perceptions about migrants' integration in the city. These are based on survey questions asked to block leaders, migrants, and residents, on whether migrants are treated unfairly by community members, and on whether the presence of migrants is positive for the community.

We report that leaders become more concerned with migrants being unfairly treated and more positive about the presence of migrants in the community. This is particularly the case for the leader treatment, with magnitudes 9 and 12 percentage points for "migrants unfairly treated' and "migrants are positive' (both significant at the 10 percent level). The basic treatment also increases the probability that leaders find migrants positive for the community (by 11 percentage points, significant at the 10 percent level). It is never statistically distinguishable from the leader treatment. Perhaps surprisingly, we do not find significant treatment effects on these outcome variables for migrants, although point estimates are always positive. Finally, we find a positive impact of the leader treatment on the probability that residents find the presence of migrants positive for the community. The magnitude is 6 percentage points, which is statistically significant at the 5 percent level. All other effects for residents are also positively signed, although insignificant.

As part of the program, migrants were introduced to information about mobile money and were given small endowments to trial transfers to closest links in their origin districts, outside Quelimane. In Table F9 we show treatment effects on the number of mobile money services used by migrants (column 1) as well as migrants' transfers to the origin and from the origin in the 30 days before the corresponding survey interview, as reported by migrants (columns 2 and 4) and migrants' origin contacts (columns 3 and 5).

We find that both treatments triggered higher mobile money adoption by migrants in terms of number of services used and of transfers from the migrant to the origin household. The number of mobile money services used increased by 0.2 and 0.3 for the leader and basic treatments respectively, with statistical significance varying between 5 and 1 percent. The probability of transferring money from the Quelimane to the origin district increased by 9 percentage points (as reported by the origin contact) for the leader treatment, and by 5-8 percentage points (as reported by the migrant and the origin contact) for the basic treatment. These effects are all significant at the 5 percent level. We do not find significant effects of the treatments on the probability that the migrant/Quelimane receives transfers from the origin (despite systematically positive point estimates). There are no statistically significant differences between treatments

on any of the outcome variables considered.

An important outcome of the migrants' integration program is migration. We expect that if successful, the program would be able to retain existing migrants in Quelimane, and would attract new migrants through the existing ones, namely from their households in the origin district. Table 5 shows treatment effects on migration. In column (1) we look at whether the migrants in our sample are still in Quelimane at the end of the program. In the following columns we analyze the behavior of the surveyed migrants' contacts at the origin in terms of: intention to move to Quelimane in the following year (column 2), whether they are in Quelimane at the end of the program (column 3), and whether they are in Quelimane approximately one year after the program finished (column 4). We employ specification 3 when considering the data at the midline and endline together (columns 2) and specification 1 when considering the endline and the phone follow-up in 2024 (columns 1 an 3-4).

We begin by observing that the basic treatment increases the probability that migrants are in the city at the endline. The magnitude of this effect is 1 percentage point (from a relatively high base of 95% in the control group), which is significant at the 10 percent level and passes Romano-Wolf multiple hypotheses testing. The effect of the leader treatment is positive but insignificant (and cannot be distinguished from that of the basic treatment). With regards to migrants' contacts at the origin, we find clear treatment effects of both intervention variants. First, the relatives at the origin are more likely to report an intention to move to Quelimane in the final part of the program implementation: the magnitudes of these effects are 4 and 3 percentage points for the leader and basic treatments, respectively (significant at the 1 and 10 percent levels and passing the Romano-Wolf procedure). There are no significant treatment effects on the actual moving to Quelimane of these individuals at the end of the program (endline). Still, at this point in time, the leader treatment has differential positive effect relative to the basic treatment which is significant at the 5 percent level. Around a year after the intervention finished, we find that these contacts at the origin are significantly more likely to have moved to Quelimane: the effect sizes are 5 and 4 percentage points for the leader and the basic treatments (both statistically significant at the 1 percent level and passing Romano-Wolf; not distinguishable from each other). This is evidence that the program was able to promote additional urbanization over and above the integration of its targeted recent migrants in the city.

We conclude that the leader treatment led to more active participation in the labor market, when compared to the basic treatment. It also led to better views about migrants in the city, mainly from leaders but also from residents. Both treatments caused more transfers to the migrants' origin contacts. Importantly,

we observe that they also promoted their migration to the city around one year after the program finished. These impacts are consistent with and possibly mediate the political impacts of the leader treatment. Note that RENAMO, the local sponsor of the program but (main) national opposition party, seems to gain from the urbanization process strengthened by the program, which includes additional migration to the city and additional electoral support at the origin.

5.5 Outcome aggregation

In order to address the risks posed by the analysis of multiple outcomes, we now devote attention to aggregating the outcomes we analyzed in detail in the previous sections. This is a straightforward alternative to the Romano-Wolf method we employed when presenting our main results. We bundle outcomes in indices that are built using the procedure detailed in Kling et al. (2007). We then calculate within-sample z-scores for each individual outcome, employing the mean and the standard deviation of the control group. Subsequently, we obtain the unweighted average z-score for each set of outcomes. The sets of outcomes are aggregated at the level of the figure/table taking into account the same level of observations.[30] Figure 2 shows these treatment effects employing specification 1 and confidence intervals at the 5 percent level of statistical significance. We confirm in these results the main takeaways we already selected in the previous section.

5.6 Other results and robustness

In this section, we report on additional econometric results related to treatment intensity for migrant outcomes, heterogeneous effects, and robustness regarding the selection of control variables in our main results employing the Post-double Selection Lasso (PDSL) procedure. These are shown in Appendix Table H1 (intensity), Appendix Tables I2 to I6 (heterogeneity), and Appendix Tables J2 to J3 (PDSL). Note that when analyzing the additional results on intensity and heterogeneity we focus on our aggregated outcomes as shown in Figure 2.

We find that treatment intensity is generally correlated with the outcomes in the same way as treatment effects. We observe some interesting heterogeneity patterns, namely with regards block leader characteristics. The leader treatment works better with earlier migrant arrivals in terms of political participation and employment. The leader treatment is also generally more impactful on migrants and their origin contacts when leaders are female. The effects of that treatment are generally more effective for older and more

[30] The exception is the labor market outcomes of Table 4 provided their clearly different nature.

experienced leaders, as well as those supporting RENAMO at the baseline. Some of the treatment effects are driven by blocks with a number of migrants above the median. When considering the results of PDSL, we do not find relevant departures from our main results. If anything, we find slightly more significant results.

6 Concluding remarks

In this paper, we report on a randomized controlled trial we designed and conducted in the city of Quelimane, Mozambique, to understand the political impacts of an integration program involving the face-to-face coaching of rural migrants as they arrive in the city. Importantly, the program was sponsored by the city government and had the active participation of local leaders; it was centered on job matching with the migrants. This is an innovative policy intervention in a rural country where urbanization opposes the political interests of the ruling party. We find that the version of the program involving local leaders in implementation increased awareness about the program and leaders' contact with migrants and residents. Importantly, we directly observe leaders becoming more mobilized during a municipal electoral campaign, more than one year after the program started. Our evidence is consistent with the use of the program as a tool for clientelism. At the same time, migrants participated more often in the electoral process, namely in terms of voter turnout. Support for the local incumbent increased, including from migrants' households at the origin. The program led to higher labor market participation, better views about migrants, more mobile money transfers to rural areas, and higher rates of migration to the city from migrants' households at the origin one year after the program finished.

We believe the implications of these results for development policy are vast. Urbanization and structural change have been an important part of the typical development path. In countries that still have large population majorities in rural areas, often in poverty pockets around subsistence agriculture, and often in Sub-Saharan Africa, urbanization is needed. Doing it well requires appropriate policies at the central and local levels. In many countries in Sub-Saharan Africa, policy at the central level has opposed urbanization (e.g., land rights have been limited). We have shown in this paper that an integration policy sponsored by a city program can be politically interesting from the perspective of local leaders and the opposition party at the national level. We have also confirmed that it is likely uninteresting from the short-term political perspective of the national ruling party. In another perspective, despite the fact that immigrants are often seen as a political problem in many settings around the world, we can infer from our results that it is

politically viable for cities in countries like Mozambique to support the integration of rural migrants. City government policy can then be explored as an important channel to target optimal rates of urbanization while influencing the quality of the integration of rural migrants in the cities. We conclude that development and structural change have a chance when politics and policy are aligned, feasibly at the local level, with regards to promoting urbanization.

References

Abebe, G., Caria, S., Fafchamps, M., Falco, P., Franklin, S., Quinn, S., & Shilpi, F. (2017). *Job Fairs: Matching Firms and Workers in a Field Experiment in Ethiopia*. World Bank, Washington, DC. Retrieved 2025-02-20, from https://hdl.handle.net/10986/27289 doi: 10.1596/1813-9450-8092

Abebe, G., Caria, S., Falco, P., Fafchamps, M., Franklin, S., & Quinn, S. (2021). Anonymity or Distance? Job Search and Labour Market Exclusion in a Growing African City. *Review of Economic Studies*, *88*(3), 1279–1310.

Ahmed, F., Hodler, R., & Islam, A. (2024). Partisan effects of information campaigns in competitive authoritarian elections: Evidence from bangladesh. *The Economic Journal*, *134*(660), 1303–1330.

Aker, J. C., Collier, P., & Vicente, P. C. (2017). Is Information Power? Using Mobile Phones and Free Newspapers during an Election in Mozambique. *The Review of Economics and Statistics*, *99*(2), 185–200. doi: 10.1162/REST_a_00611

Akhtari, M., Moreira, D., & Trucco, L. (2022). Political turnover, bureaucratic turnover, and the quality of public services. *American Economic Review*, *112*(2), 442–493.

Alesina, A., Hohmann, S., Michalopoulos, S., & Papaioannou, E. (2021). Intergenerational Mobility in Africa. *Econometrica*, *89*(1), 1–35. doi: 10.3982/ecta17018

Alesina, A., & Tabellini, M. (2024). The Political Effects of Immigration: Culture or Economics? *Journal of Economic Literature*, *62*(1), 5–46.

Ardington, C., Case, A., & Hosegood, V. (2009). Labor Supply Responses to Large Social Transfers: Longitudinal Evidence from South Africa. *American Economic Journal: Applied Economics*, *1*(1), 22-48.

Armand, A., Mendonça, F., Sandholtz, W. A., & Vicente, P. C. (2024). Integrating rural migrants in the city. *AEA RCT Registry*. Retrieved from https://doi.org/10.1257/rct.13066-1.1

Balboni, C., Bandiera, O., Burgess, R., Ghatak, M., & Heil, A. (2022). Why Do People Stay Poor? *The Quarterly Journal of Economics*, *137*(2), 785–844.

Banerjee, A. V., Duflo, E., & Sharma, G. (2021). Long-Term Effects of the Targeting the Ultra Poor Program. *American Economic Review: Insights*, *3*(4), 471–486. doi: 10.3386/W28074

Baseler, T. (2023). Hidden income and the perceived returns to migration. *American Economic Journal: Applied Economics*, *15*(4), 321–352.

Baseler, T., Ginn, T., Hakiza, R., Ogude, H., Woldemikael, O., & Ginn, T. (2023). Can Redistribution Change Policy Views ? Aid and Attitudes toward Refugees in Uganda. *CGD Working Paper 645*(May 2023).

Batista, C., & Narciso, G. (2018). Migrant remittances and information flows: Evidence from a field experiment. *World Bank Economic Review*, *32*(1), 203–219. doi: 10.1093/wber/lhw058

Batista, C., & Vicente, P. (2011). Do Migrants Improve Governance at Home? Evidence from a Voting Experiment. *World Bank Economic Review*, *25*(1), 77-104.

Batista, C., & Vicente, P. (2024). Is Mobile Money Changing Rural Africa? Evidence from a Field Experiment. *The Review of Economics and Statistics*, *forthcoming*.

Beam, E. (2016). Do Job Fairs Matter? Experimental Evidence on the Impact of Job-fair Attendance. *Journal of Development Economics*, *120*, 32-40.

Becker, S. O., Grosfeld, I., Grosjean, P., Voigtländer, N., & Zhuravskaya, E. (2023). Forced Migration and Human Capital: Evidence from Post-WWII Population Transfers.

Belloni, A., Chernozhukov, V., & Hansen, C. (2013). Inference on treatment effects after selection among high-dimensional controls. *Review of Economic Studies*, *81*(2), 608–650. doi: 10.1093/restud/rdt044

Bergeron, A., Ngindu, E. K., Tourek, G., & Weigel, J. L. (2023). Does collecting taxes erode the accountability of informal leaders? evidence from the drc.

Blattman, C., Emeriau, M., & Fiala, N. (2018). Evidence From Uganda. *The Review of Economics and Statistics*, *100*(December), 891–905. doi: 10.1162/rest

Bobonis, G. J., Gertler, P. J., Gonzalez-Navarro, M., & Nichter, S. (2022). Vulnerability and Clientelism. *American Economic Review*, *112*(11), 3627–3659. doi: 10.1257/aer.20190565

Bryan, G., Chowdhury, S., & Mobarak, A. M. (2014). Underinvestment in a Profitable Technology: The Case of Seasonal Migration in Bangladesh. *Econometrica*, *82*(5), 1671–1748. doi: 10.3982/ecta10489

Bryan, G., Glaeser, E., & Tsivanidis, N. (2020). Cities in the developing world. *Annual Review of Economics*, *12*, 273–297. doi: 10.1146/annurev-economics-080218-030303

Bryan, G., & Morten, M. (2019). The Aggregate Productivity Effects of Internal Migration: Evidence from Indonesia. *Journal of Political Economy*, *127*(5), 000–000. doi: 10.1086/701810

Burgess, R., Jedwab, R., Miguel, E., & Morjaria, A. (2014). The Value of Democracy: Evidence from Road Building in Kenya. *American Economic Review*, *105*(6), 1817-1851.

Burzyński, M., Deuster, C., Docquier, F., & de Melo, J. (2022, June). Climate Change, Inequality, and Human Migration. *Journal of the European Economic Association*, *20*(3), 1145–1197. Retrieved 2025-02-20, from https://doi.org/10.1093/jeea/jvab054 doi: 10.1093/jeea/jvab054

Byamugisha, F. (2013). Securing Africa's Land for Shared Prosperity: A Program to Scale Up Reforms and Investments. *Africa Development Forum, World Bank Group*.

Callen, M., Gulzar, S., Hasanain, A., Khan, M. Y., & Rezaee, A. (2023). The political economy of public sector absence. *Journal of Public Economics*, *218*, 104787. Retrieved from https://doi.org/10.1016/j.jpubeco.2022.104787 (Publisher: The Author(s) ISBN: 9550091031) doi: 10.1016/j.jpubeco.2022.104787

Caria, A. S., Gordon, G., Kasy, M., Quinn, S., Shami, S. O., & Teytelboym, A. (2024, April). An Adaptive Targeted Field Experiment: Job Search Assistance for Refugees in Jordan. *Journal of the European Economic Association*, *22*(2), 781–836. Retrieved 2024-12-08, from https://academic.oup.com/jeea/article/22/2/781/7424979 doi: 10.1093/jeea/jvad067

Casey, K., Glennerster, R., & Miguel, E. (2012). Reshaping institutions: Evidence on aid impacts using a preanalysis plan. *The Quarterly Journal of Economics*, *127*(4), 1755–1812.

Cattaneo, C., & Grieco, D. (2021). Turning opposition into support to immigration: The role of narratives. *Journal of Economic Behavior Organization*, *190*, 785–801. Retrieved from https://doi.org/10.1016/j.jebo.2021.08.015 doi: 10.1016/j.jebo.2021.08.015

Cockx, L. (2021). Moving towards a better future ? Migration and children ' s health and education. *Economic Development and Cultural Change*.

Colmer, J. (2021, October). Temperature, Labor Reallocation, and Industrial Production: Evidence from India. *American Economic Journal: Applied Economics*, *13*(4), 101–124. Retrieved 2025-02-20, from https://www.aeaweb.org/articles?id=10.1257/app.20190249 doi: 10.1257/app.20190249

Dahis, R., & Szerman, C. (2024). *Decentralizing Development: Evidence from Government Splits* [SSRN Scholarly Paper]. Rochester, NY. Retrieved 2024-09-09, from https://papers.ssrn.com/abstract=4717751 doi: 10.2139/ssrn.4717751

Davis, J., & Henderson, J. (2003). Evidence on the Political Economy of the Urbanization Process. *Journal of Urban Economics*, *53*(1), 98-125. doi: doi.org/10.1016/S0094-1190(02)00504-1

Dillon, B., Aker, J., & Blumenstock, J. (2024). Yellow pages: Information, connections, and firm performance. *Economic Journal*, *forthcoming*.

Docquier, F., Lodigiani, E., Rapoport, H., & Schiff, M. (2016). Emigration and Democracy. *Journal of Development Economics*, *120*, 209-223.

Duranton, G., & Puga, D. (2023). Urban Growth and Its Aggregate Implications. *Econometrica*, *91*(6), 2219–2259. Retrieved 2025-02-20, from https://www.econometricsociety.org/doi/10.3982/ECTA17936 doi: 10.3982/ECTA17936

Feler, L., & Henderson, J. V. (2011). Exclusionary policies in urban development: Under-servicing migrant households in Brazilian cities. *Journal of Urban Economics*, *69*(3), 253–272. Retrieved from http://dx.doi.org/10.1016/j.jue.2010.09.006 doi: 10.1016/j.jue.2010.09.006

Glaeser, E. L., & Steinberg, B. M. (2017). Transforming cities: does urbanization promote democratic change? *Regional Studies*, *51*(1), 58–68. Retrieved from http://dx.doi.org/10.1080/00343404.2016.1262020 doi: 10.1080/00343404.2016.1262020

Glaeser, E. L., & Xiong, W. (2017, July). Urban productivity in the developing world. *Oxford Review of Economic Policy*, *33*(3), 373–404. Retrieved 2025-02-20, from https://doi.org/10.1093/oxrep/grx034 doi: 10.1093/oxrep/grx034

Gollin, D., Jedwab, R., & Vollrath, D. (2015). Urbanization with and without industrialization. *Journal of Economic Growth*, *21*(1), 35–70. doi: 10.1007/s10887-015-9121-4

Gollin, D., Lagakos, D., & Waugh, M. E. (2014). The Agricultural Productivity Gap. *Quarterly Journal of Economics*, *129*(2), 939–993. doi: 10.1093/qje/qjt056.Advance

Grácio, M., & Vicente, P. C. (2021). Information, Get-out-the-vote Messages, and Peer Influence: Causal Effects on Political Behavior in Mozambique. *Journal of Development Economics*, *151*, 102665.

Gulzar, S., & Pasquale, B. J. (2017, February). Politicians, Bureaucrats, and Development: Evidence from India. *American Political Science Review*, *111*(1), 162–183. Retrieved 2017-06-07, from https://www.cambridge.org/core/product/identifier/S0003055416000502/type/journal_article (Publisher: Oxford University Press ISBN: 0003055416000) doi: 10.1017/S0003055416000502

Hamory, J., Kleemans, M., Li, N. Y., & Miguel, E. (2021). Reevaluating Agricultural Productivity Gaps with Longitudinal Microdata. *Journal of the European Economic Association*, *19*(3), 1522–1555. doi: 10.1093/jeea/jvaa043

Henderson, J., Storeygard, A., & Deichmann, U. (2017). Has Climate Change Driven Urbanization in Africa? *Journal of Development Economics*, *124*(C), 60-82.

Henderson, J. V., & Turner, M. A. (2020). Urbanization in the Developing World: Too Early or Too Slow? *Journal of Economic Perspectives*, *34*(3), 150–173.

Hodler, R., & Raschky, P. (2014). Regional Favoritism. *The Quarterly Journal of Economics*, *129*(2), 995-1033.

Imbert, C., Seror, M., Zhang, Y., & Zylberberg, Y. (2021). Migrants and Firms: Evidence from China. *SSRN Electronic Journal*. doi: 10.2139/ssrn.3338794

Kelley, E. M., Ksoll, C., & Magruder, J. (2024). How do digital platforms affect employment and job search? evidence from india. *Journal of Development Economics*, *forthcoming*.

Kling, J. R., Liebman, J. B., & Katz, L. F. (2007). Experimental analysis of neighborhood effects. *Econometrica*, *75*(1), 83–119.

Kuznets, S. (1971). Modern Economic Growth: Findings and Reflections. *Nobel Prize Lecture*.

Lagakos, D. (2020). Urban-Rural Gaps in the Developing World: Does Internal Migration Offer Opportunities? *Journal of Economic Perspectives*, *34*(3), 174–192.

Majumdar, S., Mani, A., & Mukand, S. W. (2004). Politics, information and the urban bias. *Journal of Development Economics*, *75*(1), 137–165. doi: 10.1016/j.jdeveco.2003.08.003

Manacorda, M., Miguel, E., & Vigorito, A. (2011, July). Government transfers and political support. *American Economic Journal: Applied Economics*, *3*(3), 1–28. Retrieved 2016-10-05, from http://pubs.aeaweb.org/doi/10.1257/app.3.3.1 (arXiv: gr-qc/9809069v1 ISBN: 9780874216561) doi: 10.1257/app.3.3.1

Mayda, A. M., Peri, G., & Steingress, W. (2022). The Political Impact of Immigration: Evidence from the United States. *American Economic Journal: Applied Economics*, *14*(1), 358–389. doi: 10.1257/app.20190081

McKenzie, D. (2012). Beyond baseline and follow-up: The case for more T in experiments. *Journal of Development Economics*, *99*(2), 210–221. Retrieved from http://dx.doi.org/10.1016/j.jdeveco.2012.01.002 doi: 10.1016/j.jdeveco.2012.01.002

McKenzie, D. (2017). How Effective Are Active Labor Market Policies in Developing Countries? A Critical Review of Recent Evidence. *The World Bank Research Observer*, *32*(2), 127–154.

McKenzie, D. (2024). Fears and Tears: Should More People Be Moving within and from Developing Countries, and What Stops this Movement? *The World Bank Research Observer*, *39*(1), 75–96. Retrieved 2024-10-25, from https://doi.org/10.1093/wbro/lkac009 doi: 10.1093/wbro/lkac009

McKenzie, D., Gibson, J., & Stillman, S. (2013). A land of milk and honey with streets paved with gold: Do emigrants have over-optimistic expectations about incomes abroad? *Journal of Development Economics*, *102*, 116–127. Retrieved from http://dx.doi.org/10.1016/j.jdeveco.2012.01.001 doi: 10.1016/j.jdeveco.2012.01.001

Michaels, G., Nigmatulina, D., Rauch, F., Regan, T., Baruah, N., & Dahlstrand, A. (2021). Planning ahead for better neighborhoods: Long-run evidence from tanzania. *Journal of Political Economy*, *129*(7), 2112–2156. doi: 10.1086/714119

Morten, M., & Oliveira, J. (2018). The Effects of Roads on Trade and Migration: Evidence from a Planned Capital City. *NBER Working Paper*, 1–52. Retrieved from https://web.stanford.edu/$\sim$memorten/ewExternalFiles/Morten_Oliveira_Brasilia.pdf

Nakamura, E., Sigurdsson, J., & Steinsson, J. (2022). The gift of moving: Intergenerational consequences of a mobility shock. *The Review of Economic Studies*, *89*(3), 1557–1592.

Ravallion, M., Chen, S., & Sangraula, P. (2007). New evidence on the urbanization of global poverty. *Population and Development Review*, *33*(4), 667–701. doi: 10.1111/j.1728-4457.2007.00193.x

Romano, J. P., & Wolf, M. (2016). Efficient computation of adjusted p-values for resampling-based stepdown multiple testing. *Statistics & Probability Letters*,, *113*, 38-40.

Sandholtz, W. A. (2023). The Politics of Public Service Reform: Experimental Evidence from Liberia. *CESifo Working Paper Series*(August). doi: 10.2139/ssrn.4564445

Shenoy, A., & Zimmermann, L. V. (2022). Political organizations and political scope.

Suri, T., & Jack, W. (2016). The Long-run Poverty and Gender Impacts of Mobile Money. *Science*, *354*(6317), 1288-1292.

Tricaud, C. (2025, January). Better Alone? Evidence on the Costs of Intermunicipal Cooperation. *American Economic Journal: Applied Economics*, *17*(1), 160–207. Retrieved 2025-02-20, from https://www.aeaweb.org/articles?id=10.1257/app.20230344 doi: 10.1257/app.20230344

United Nations. (2018). World urbanization prospects: The 2018 revision [Online Edition].

Van Maarseveen, R. (2022). The Effect of Urban Migration on Educational Attainment: Evidence from Africa. *SSRN Electronic Journal*. doi: 10.2139/ssrn.3836097

Wallace, J. (2014). *Cities and stability: Urbanization, redistribution, and regime survival in china*. Oxford University Press.

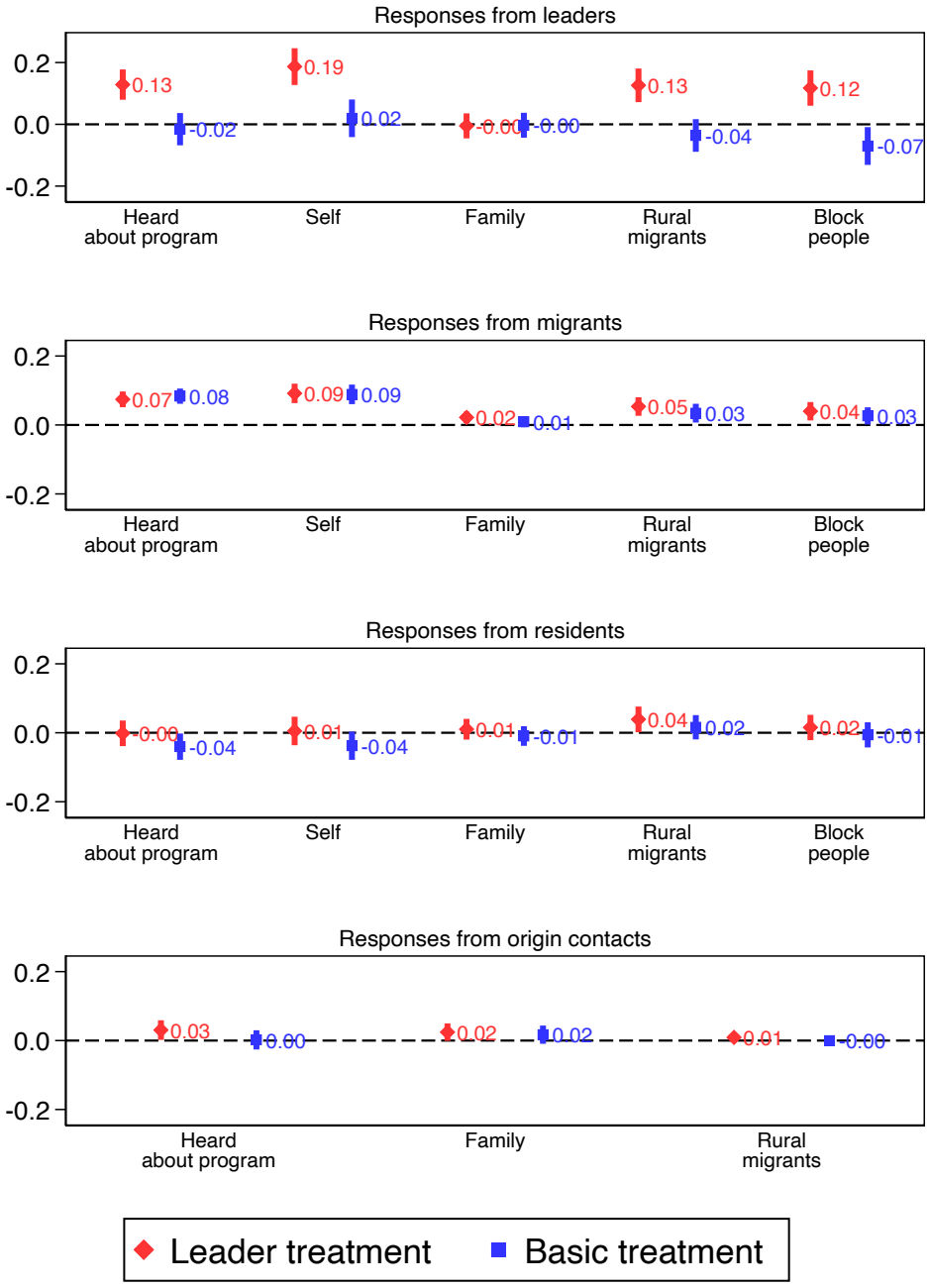
Wantchekon, L. (2003). Clientelism and Voting Behavior: Evidence from a Field Experiment in Benin. *World Politics*, *55*, 399-422.

Young, A. (2013). Inequality, the Urban-rural Gap, and Migration. *Quarterly Journal of Economics*, 1727–1785. doi: 10.1093/qje/qjt025.Advance

Zimmermann, L. (2021). The Dynamic Electoral Returns of a Large Antipoverty Program. *The Review of Economics and Statistics*, *103*(December), 803–817.

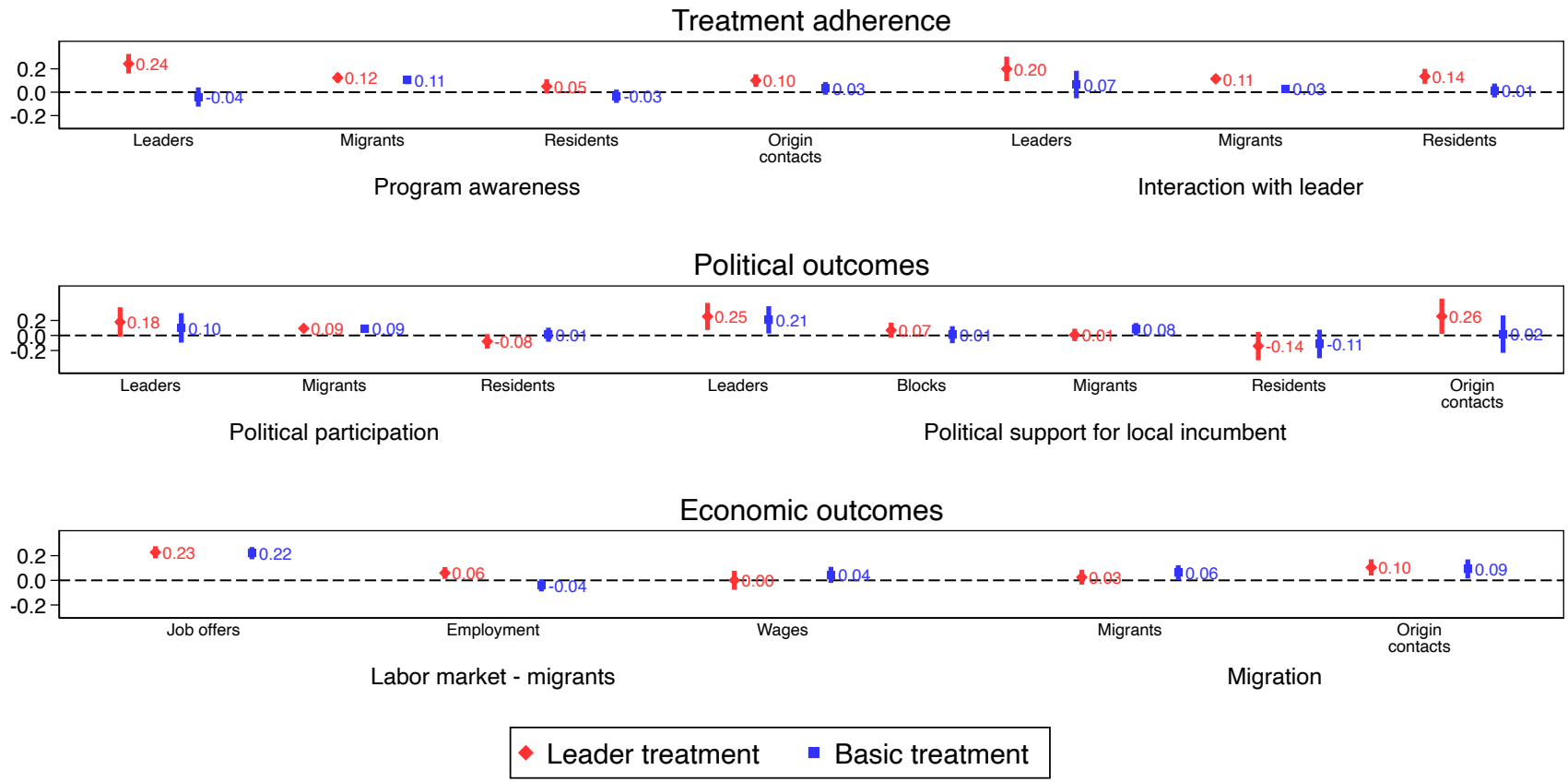
7 Figures

Figure 1: Who is involved in the program?



Note: Estimates based on OLS regressions using equation 3 (stacked regressions). The top panel presents results for the block leaders; the second panel presents results for the sampled migrants; the third panel presents results for the sampled residents; the bottom panel presents results for the sampled district relatives. Dependent variables: (1) *Heard about program*: variable equal to 1 if the respondent has heard about the program "Quelimane trabalha com todos', and 0 otherwise; (2) *Self*: variable equal to 1 if the respondent reports to have been involved in the program, and 0 otherwise; (3) *Family*: variable equal to 1 if the respondent reports that his/her family was involved in the program, and 0 otherwise; (4) *Block people*: variable equal to 1 if the respondent reports that people living in the same block were involved in the program, and 0 otherwise; (5) *Rural migrants*: variable equal to 1 if the respondent reports that rural migrants were involved in the program, and 0 otherwise. Additional details about the dependent variables are presented in the Appendix in Table E1. All specifications include block and individual controls, as well as strata fixed effects. Section 4 presents the full list of controls. Standard errors, reported in parentheses, are clustered at the block level. *** p<0.01, ** p<0.05, * p<0.1.

Figure 2: Outcome aggregation



Note. Estimates based on OLS regressions using equation 3. The confidence intervals are built using statistical significance at the 10 percent level. Standard errors are clustered at the block level in regressions with observations at a lower level. All specifications include block and individual controls, as well as strata fixed effects. Section 4 presents the full list of controls. Outcomes are grouped by table in indices built using the Kling et al. (2007) procedure. Outcomes are first normalized in standardized units (using the mean and standard deviation of the control group), and then averaged within each category.

8 Tables

Table 1: Interaction with block leader

	Leader knows block migrants		Knows leader		Contacted leader		Resorted to leader for job	
	General	% sampled	Migrant	Resident	Migrant	Resident	Migrant	Resident
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(TL) Leader treatment	0.088**	0.058***	0.066**	0.080*	0.041***	0.055***	0.012***	0.017**
	(0.037)	(0.017)	(0.026)	(0.045)	(0.010)	(0.021)	(0.003)	(0.007)
(TB) Basic treatment	0.041	0.003	-0.037	-0.037	0.008	0.005	0.009***	0.004
	(0.039)	(0.019)	(0.028)	(0.044)	(0.010)	(0.022)	(0.003)	(0.006)
Observations	827	859	2849	745	6105	1575	6079	1567
Adjusted R^2	0.072	0.127	0.240	0.197	0.056	0.084	0.009	0.036
Mean (control group)	0.672	0.159	0.543	0.580	0.097	0.165	0.011	0.007
T1 = T2 (p-value)	0.199	0.005	0.000	0.008	0.002	0.020	0.499	0.084
Outcome data	Pooled	Pooled	Midline	Midline	Pooled	Pooled	Pooled	Pooled
Romano-Wolf p-value TL	0.010	0.001	0.016	0.525	0.007	0.039	0.091	0.039
Romano-Wolf p-value TB	0.429	0.875	0.195	0.388	0.314	0.852	0.017	0.728

Note. Estimates based on OLS regressions. Columns (1)-(2) as well as (5)-(8) use equation 3, employing midline and endline surveys (stacked regressions). Columns (3) and (4) use equation 1 and include only data from the first post-baseline survey wave. We did not collect the lagged values for any of the dependent variables. Dependent variables by column: (1) *Leader knows block migrants – General*: variable equal to 1 if the respondent knows any rural migrants living in the same block, and 0 otherwise; (2) *Leader knows block migrants – % sampled*: variable ranging from 0 to 1 indicating the percentage of rural migrants that the respondent selects from the list of migrants sampled in the same block; (3)-(4) *Knows leader*: variable equal to 1 if the respondent correctly identifies the name of the current leader in the same block, and 0 otherwise; (5)-(6) *Contacted leader*: variable equal to 1 if the respondent reports to have approached the block leader at least once since the previous survey waves, and 0 otherwise; (7)-(8) *Resorted to leader for a job:* Indicator variable equal to 1 if the respondent reports having contacted or paid the leader for a job in the previous year, and 0 otherwise. Additional details about the dependent variables are presented in the Appendix in Table E2. All specifications include block and individual controls, as well as strata fixed effects. Section 4 presents the full list of controls. Standard errors, reported in parentheses, are clustered at the block level. *** p<0.01, ** p<0.05, * p<0.1. We report p-values of the procedure described in Romano & Wolf (2016) which allows for multiple hypotheses testing.

Table 2: Political participation

	Party objects observed		Electoral turnout: inked finger		
	Migrant (1)	Resident (2)	Leader (3)	Migrant (4)	Resident (5)
(TL) Leader treatment	0.017***	0.005	0.051	0.030*	-0.053
	(0.006)	(0.015)	(0.035)	(0.017)	(0.035)
(TB) Basic treatment	0.026***	-0.004	0.031	0.019	0.020
	(0.006)	(0.014)	(0.034)	(0.017)	(0.034)
Observations	6103	1572	399	3322	807
Adjusted R^2	0.045	0.034	-0.039	0.079	0.033
Mean (control group)	0.046	0.083	0.896	0.697	0.825
T1 = T2 (p-value)	0.147	0.553	0.557	0.533	0.032
Outcome data	Pooled	Pooled	Election	Election	Election
Romano-Wolf p-value TL	0.012	0.680	0.089	0.103	0.132
Romano-Wolf p-value TB	0.001	0.818	0.328	0.169	0.410

Note. Estimates based on OLS regressions. Columns (1)-(2) use equation 3, employing midline and endline survey waves (stacked regressions). Columns (3)-(5) use equation 1 and present results for outcomes collected in the two days following the 2023 local elections in Mozambique. We did not collect the lagged values for any of the dependent variables. Columns (1) and (4) present results for migrants; columns (2) and (5) present results for residents; column (3) presents results for block leaders. Dependent variable by column: (1)-(2) *Party objects observed*: variable equal to 1 if the enumerator identified any objects with a political content held by the respondent, and 0 otherwise; (3)-(5) *Electoral turnout: inked finger*: variable equal to 1 if the respondent's finger was marked with purple ink during the enumerators' visit in the two days after the 2023 local elections in Mozambique, and 0 otherwise. Additional details about the dependent variables are presented in the Appendix in Table E3. All specifications include block and individual controls, as well as strata fixed effects. Section 4 presents the full list of controls. Standard errors, reported in parentheses, are clustered at the block level. *** p<0.01, ** p<0.05, * p<0.1. We report p-values of the procedure described in Romano & Wolf (2016) which allows for multiple hypotheses testing.

Table 3: Political support for the local incumbent

	Leader mobilization		Stickers		Self-reported voting RENAMO		
	Reports cyclists (1)	Observed # mobilized (2)	From leader (brown) (3)	From field team (pink) (4)	Migrant (5)	Resident (6)	Origin contacts (7)
(TL) Leader treatment	0.112**	0.709*	0.038**	0.000	0.002	-0.047	0.128*
	(0.053)	(0.395)	(0.017)	(0.046)	(0.014)	(0.040)	(0.071)
(TB) Basic treatment	0.077	0.662*	-0.007	0.023	0.024*	-0.039	0.009
	(0.053)	(0.393)	(0.019)	(0.056)	(0.014)	(0.040)	(0.075)
Observations	429	429	429	429	2084	530	418
Adjusted R^2	0.130	0.053	0.716	0.492	0.018	0.139	0.132
Mean (control group)	0.629	1.490	0.436	0.745	0.911	0.851	0.539
T1 = T2 (p-value)	0.509	0.917	0.020	0.645	0.126	0.842	0.094
Romano-Wolf p-value TL	0.055	0.086	0.053	0.998	0.888	0.166	0.183
Romano-Wolf p-value TB	0.285	0.200	0.874	0.874	0.098	0.184	0.203

Note. Estimates based on OLS regressions using equation 1, including only data from the endline survey wave. We did not collect the lagged values for any of the dependent variables. Columns (1)-(2) show results for the block leader; columns(3)-(4) show block level results; column (5) shows results for migrants; column (6) shows results for residents; column (7) shows results for the origin contacts. Dependent variables by columns: (1) *Reports cyclists*: variable equal to 1 if the list left with leaders for cyclist mobilization contains any names at the time of collection, and 0 otherwise; (2) *Observed # mobilized*: variable counting the number of cyclists mobilized by leaders that attended the second visit by enumerators; (3) *Stickers from leader (brown):* variable ranging between 0 and 1, illustrating the percentage of leader brown stickers found hanging on households' doors during the enumerators' visit to the blocks, out of the 40 stickers given to block leaders for distribution; (4) *Stickers from field team (pink)*: variable ranging between 0 and 1, illustrating the percentage of field team pink stickers found hanging at households' doors during the enumerators' visit to the blocks, out of the total distributed directly in each block; (5)-(7) *Self-reported voting RENAMO:* variable equal to 1 if the respondent reports having voted RENAMO in the 2023 Mozambican municipal elections, and 0 otherwise. Additional details about the dependent variables are presented in the Appendix in Table E4. All specifications include block and individual controls, as well as strata fixed effects. Section 4 presents the full list of controls. Standard errors, reported in parentheses, are clustered at the block level. *** p<0.01, ** p<0.05, * p<0.1. We report p-values of the procedure described in Romano & Wolf (2016) which allows for multiple hypotheses testing.

Table 4: Labor market outcomes - migrants

	Heard of job	Heard of job through program	Working	# jobs	# hours working	Wage
	(1)	(2)	(3)	(4)	(5)	(6)
(TL) Leader treatment	0.070***	0.226***	0.020	0.028	0.419***	0.000
	(0.014)	(0.030)	(0.016)	(0.018)	(0.156)	(0.036)
(TB) Basic treatment	0.082***	0.220***	-0.032**	-0.019	-0.147	0.032
	(0.014)	(0.030)	(0.016)	(0.018)	(0.167)	(0.031)
Observations	6105	1714	6100	6100	6105	4704
Adjusted R^2						
Mean (control group)	0.233	0.476	0.625	0.702	4.518	-0.002
T1 = T2 (p-value)	0.398	0.790	0.001	0.008	0.000	0.366
Romano-Wolf p-value TL	0.001	0.001	0.364	0.273	0.029	0.986
Romano-Wolf p-value TB	0.001	0.001	0.260	0.601	0.601	0.601

Note. Estimates based on OLS regressions using equation 3. All columns combine the midline and endline surveys (stacked regressions). Columns (3) and (6) include the lagged value of the dependent variable. We did not collect the lagged values of the remaining dependent variables. All outcomes presented are concerning migrants. Dependent variables by column: (1) *Heard of job*: variable equal to 1 if the respondent has heard of a job offer in the 12 months before the interview date, and 0 otherwise; (2) *Heard of job through program*: variable equal to 1 if the respondent heard of a job offer through the migrants' integration program, and 0 otherwise; (3) *Working*: variable equal to 1 if the respondent was employed at the time of the interview, and 0 otherwise; (4) # *jobs*: number of jobs that the respondent reports having had since the beginning of the project; (5) # *hours working*: variable reporting the number of hours that the respondent reports having worked on the day before the interview date; (6) *Wage* : variable with the standardized value of the reported wage per week. Additional details about the dependent variables are presented in the Appendix in Table E5. All specifications include block and individual controls, as well as strata fixed effects. Section 4 presents the full list of controls. Standard errors, reported in parentheses, are clustered at the block level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. We report p-values of the procedure described in Romano & Wolf (2016) which allows for multiple hypotheses testing.

Table 5: Migration

	Migrants	Origin contacts		
	Stayed in Quelimane	Likely to move	In Quelimane	In Quelimane
	(1)	(2)	(3)	(4)
(TL) Leader treatment	0.005	0.039***	0.006	0.046***
	(0.008)	(0.014)	(0.014)	(0.014)
(TB) Basic treatment	0.013*	0.025*	-0.020	0.036***
	(0.008)	(0.013)	(0.013)	(0.013)
Observations	3579	3724	1488	2313
Adjusted R^2	0.040	0.023	-0.015	0.003
Mean (control group)	0.954	0.114	0.056	0.089
T1 = T2 (p-value)	0.294	0.297	0.043	0.484
Outcome data	Endline	Pooled	Endline	Follow-up I
Romano-Wolf p-value TL	0.490	0.008	0.689	0.003
Romano-Wolf p-value TB	0.082	0.066	0.131	0.027

Note. Estimates based on OLS regressions. Columns (1) and (3)-(4) use equation 1. Column (2) uses equation 3, combining the midline and endline survey waves (stacked regressions). Columns (1) and (3) use data collected in the second post-baseline survey wave. Column (4) presents results for data collected in the first follow-up phone survey, around one year after the program finished. We did not collect the lagged values of any of these dependent variables. Column (1) presents results for migrants; columns (2)-(4) present results for origin contacts. Dependent variables by column: (1) *Stayed in Quelimane*: variable equal to 1 if the respondent reports still living in Quelimane at the time of the interview, and 0 otherwise; (2) *Likely to move*: variable equal to 1 if the respondent reports being likely or very like to move to Quelimane within the following 12 months, and 0 otherwise (3)-(4) *In Quelimane*: variable equal to 1 if the respondent reports living in Quelimane at the time of the interview, and 0 otherwise. Additional details about the dependent variables are presented in the Appendix in Table E6. All specifications include block and individual controls, as well as strata fixed effects. Section 4 presents the full list of controls. Standard errors, reported in parentheses, are clustered at the block level. *** p<0.01, ** p<0.05, * p<0.1. We report p-values of the procedure described in Romano & Wolf (2016) which allows for multiple hypotheses testing.

ONLINE APPENDIX

Supplementary material to

On the Political Economy of Urbanization: Experimental Evidence from Mozambique

Alex Armand, Frederica Mendonça, Wayne Aaron Sandholtz and Pedro C. Vicente

A Treatment contents **2**
A.1 Administrative information . . . 3
A.2 Job matching . . . 7
A.3 Mobile Money . . . 13
A.4 Block leaders . . . 15

B Sampling and randomization **20**

C Timeline **30**

D Measurement **31**
D.1 Behavioral measurements . . . 31

E Outcome variables and detailed results **32**
E.1 Outcome variables . . . 32

F Additional analysis **40**

G Results for aggregated outcomes **49**

H Treatment intensity **52**

I Heterogeneous effects **53**

J Robustness to selection of control variables **59**

A Treatment contents

The program 'Quelimane trabalha com todos' (Quelimane works with everybody) targeted recent rural migrants who became residents of Quelimane city. The program consisted of individual coaching sessions through five house visits to migrants, entailing a one hour of face-to-face conversation per visit, divided into three types of contents, i.e., administrative information, job matching, and mobile money, which we describe in further detail below. See Appendix Figure C1 for the specific timing of the five rounds of home visits. In the main variation of the program, its delivery was mediated by the corresponding block leader (also detailed below). Tables A1-A2 show the number of rounds per treatment and the shares treated per round.

Table A1: Number of rounds by treatment

	Mean	S.E.	Min	Max	N
	(1)	(2)	(3)	(4)	(5)
TL	2.55	1.74	0	5	1567
TB	2.63	1.76	0	5	1494
Control	0.00	0.00	0	0	1567

Table A2: Number of rounds by treatment

	Round 1	Round 2	Round 3	Round 4	Round 5
	(1)	(2)	(3)	(4)	(5)
TL	0.20	0.54	0.55	0.55	0.72
	(0.40)	(0.50)	(0.50)	(0.50)	(0.45)
TB	0.20	0.55	0.58	0.57	0.72
	(0.40)	(0.50)	(0.49)	(0.49)	(0.45)
Control	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	4628	4628	4628	4628	4628

A.1 Administrative information

Throughout the five home visits to migrants, the conversation was initiated with a module providing practical administrative information about the city. This was intended as a general introduction to living in Quelimane. In the first two visits, the module consisted of a presentation developed by the municipality. It included information on the administrative divisions and political context of the city, the locations where a citizen card and residence documentation could be obtained, the timing and locations for electoral registration as well as details of the voting process, details of schooling and healthcare in the city, as well as of other public infrastructures, and a description of some of the city's cultural traditions. By the third visit, the same content was provided but the presentation was incorporated into the survey platform and turned into an interactive experience in the form of a quiz: respondents were first asked to guess based on the two prior visits and then were given the true answer. Figures A1-A3 summarize the information presented.

Figure A1: Presentation on administrative information

Figure A2: Presentation on administrative information (continued)

Figure A3: Presentation on administrative information (continued)

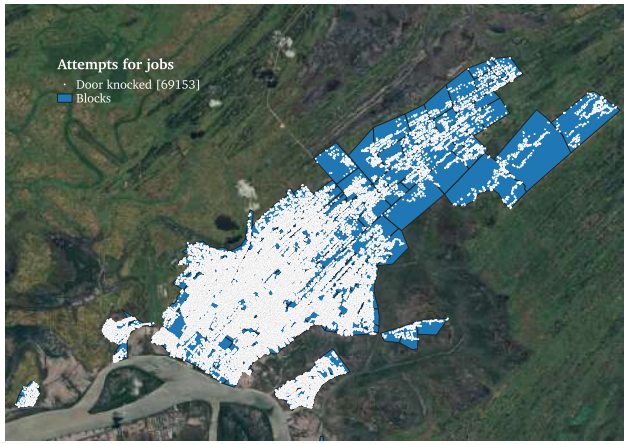
A.2 Job matching

The main part of the conversation in the five face-to-face home visits to migrants was about job matching. Program participants were allocated contacts (name and phone number) of potential job offers. To collect the information relating to these job offers, we conducted two censuses of job offers suitable to rural migrants having recently arrived in the city, in which we visited every house and establishment in the city. Figures A4 and A5 map all the attempted houses/establishments, the ones where we got answers, and the ones where we actually found job offers for rounds 1 and 3, respectively. Additionally, we conducted three rounds of job updating by phone with the previously collected contacts, just before each round of treatment as in the timeline of Appendix Figure C1.

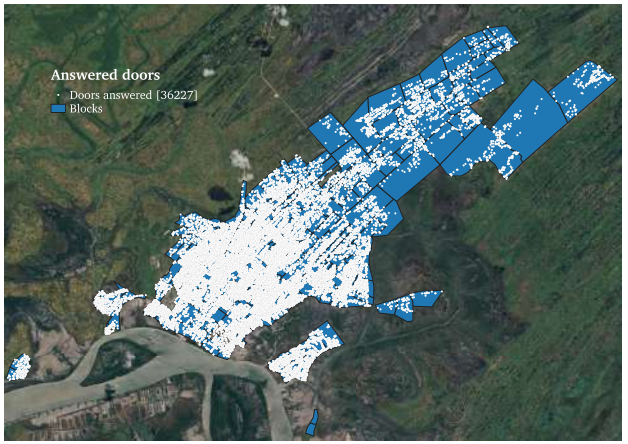
We managed to identify 1582 job offers during this project, mostly for construction and housekeeping. Table A3 provides descriptive statistics on the job offers collected for each round. Program implementers allocated these jobs to specific migrants based on the elicitation of the migrants' job preferences. During the first round, program implementers were allocating three contacts for each person. In the second round, this number increased to seven contacts per migrant. By the third round, and from then on, the program allocated 10 job contacts to each participant. In the last two visits, the implementer was also linking each potential employer and migrant by contacting the employer during the house visit and setting an interview date. As a final step of all visits, implementers always sent a text message to each migrant with the potential employers' contacts.

To further enhance treatment adherence, during the fourth round we included video of a recently arrived migrant describing a successful experience with the program. Two similar videos with a female and a male migrants were filmed. The survey then followed a deterministic algorithm to randomly determine on the spot which of the two versions of the video to exhibit. Figure A6 depicts the script followed for the videos, and Figures A7 and A8 illustrate the two videos.

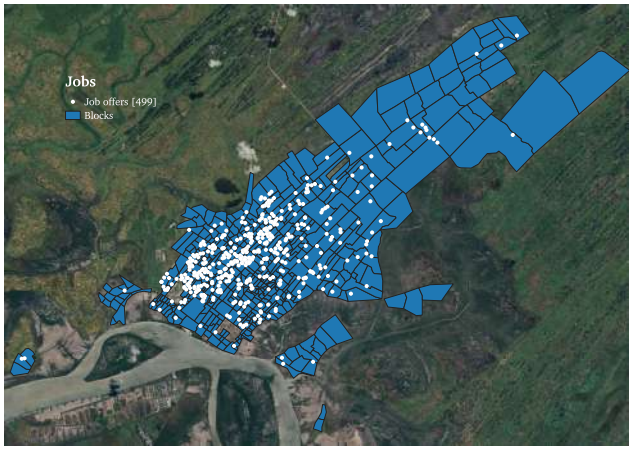
Figure A4: CENSUS - Round 1



(a) Attempts

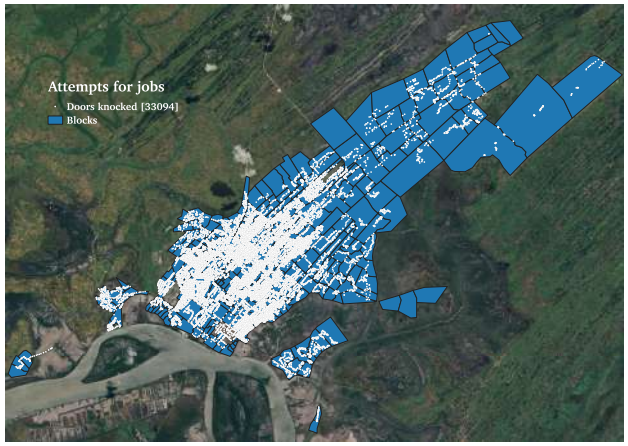


(b) Doors answered

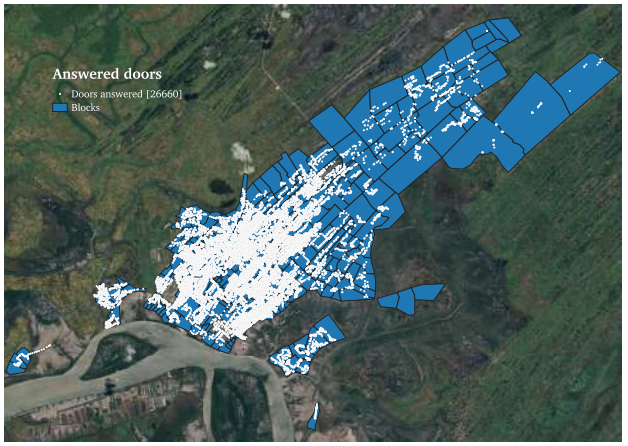


(c) Job offers

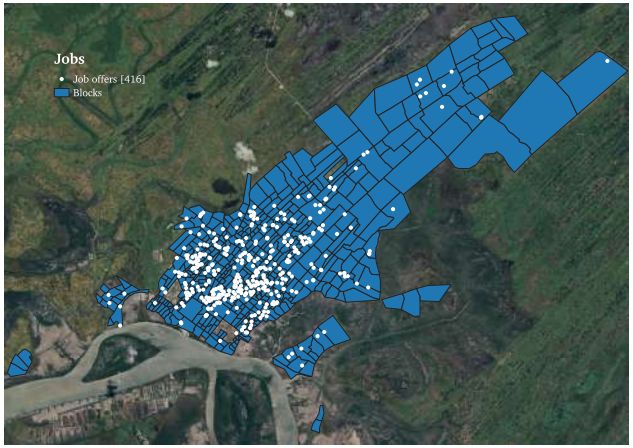
Figure A5: CENSUS - Round 3



(a) Attempts



(b) Doors answered



(c) Job offers

Table A3: Jobs offers

	Round 1		Round 2		Round 3		Round 4		Round 5		Total	
	Mean / (SD)	N	Mean / (SD)	N	Mean / (SD)	N	Mean / (SD)	N	Mean / (SD)	N	Mean / (SD)	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Construction	0.14	70	0.01	6	0.15	62	0.13	39	0.85	996	0.35	1173
	(0.35)		(0.08)		(0.36)		(0.33)		(0.36)		(0.48)	
Domestic worker	0.58	290	0.04	39	0.54	224	0.44	138	0.53	110	0.34	801
	(0.49)		(0.20)		(0.50)		(0.50)		(0.50)		(0.47)	
Farming & cattle / fishing	0.16	80	0.01	8	0.16	65	0.15	48	0.10	21	0.09	222
	(0.37)		(0.09)		(0.36)		(0.36)		(0.30)		(0.29)	
Merchant	0.04	19	0.01	10	0.04	18	0.05	16	0.06	12	0.03	75
	(0.19)		(0.10)		(0.20)		(0.22)		(0.23)		(0.17)	
Guard	0.07	33	.	0	0.04	15	0.03	8	0.08	16	0.05	72
	(0.25)		(.)		(0.19)		(0.16)		(0.27)		(0.22)	
Handyman	0.02	9	0.00	2	0.02	7	0.01	4	0.03	6	0.01	28
	(0.13)		(0.05)		(0.13)		(0.11)		(0.17)		(0.11)	
Barber/Hairdresser	0.01	7	0.00	0	0.02	9	0.02	6	0.02	4	0.01	26
	(0.12)		(0.00)		(0.15)		(0.14)		(0.14)		(0.10)	
Assistant to mechanic	0.01	6	.	0	0.02	10	0.01	3	0.01	3	0.02	22
	(0.11)		(.)		(0.15)		(0.10)		(0.12)		(0.12)	
Driver	0.01	4	0.00	1	0.02	7	0.00	1	0.03	6	0.01	19
	(0.09)		(0.03)		(0.13)		(0.06)		(0.17)		(0.09)	
Office work	0.01	5	0.00	0	0.01	6	0.01	4	0.00	1	0.01	16
	(0.10)		(0.00)		(0.12)		(0.11)		(0.07)		(0.08)	
Sewer	0.00	2	.	0	0.01	4	0.01	3	0.00	1	0.01	10
	(0.06)		(.)		(0.10)		(0.10)		(0.07)		(0.08)	
Adman	0.02	11	.	0	0.00	1	1.00	1	.	0	0.01	13
	(0.45)		(.)		(0.05)		(.)		(.)		(0.34)	
Shoemaker	0.00	1	.	0	0.00	2	.	0	0.00	1	0.00	4
	(0.04)		(.)		(0.07)		(.)		(0.07)		(0.06)	
Services	.	0	.	0	.	0	1.00	2	0.00	1	0.01	3
	(.)		(.)		(.)		(0.00)		(0.07)		(0.12)	
Docker	0.00	2	.	0	.	0	.	0	.	0	0.00	2
	(0.06)		(.)		(.)		(.)		(.)		(0.06)	
Mobile money agent	0.00	1	.	0	.	0	.	0	.	0	0.00	1
	(0.04)		(.)		(.)		(.)		(.)		(0.04)	
Attempts		69150		952		33094		1462		1171		
Doors answered		36227		952		26660		1403		983		
Job offers		499		149		416		310		208		

Figure A6: Script for migrants' videos - round 4

Good morning/afternoon,

My name is {name}, and I was born in {district}. I moved to Quelimane in {year} for {work/visit family/illness,...}.

In September 2021, I joined the NOVAFRICA integration program in collaboration with the Quelimane Municipality. Since then, I have received three home visits from the program team.

During these visits, I was given contacts of people potentially looking for an employer. However, the first number I called didn't go through, and the second person said they didn't have time to talk. This made me start doubting the program.

By the third visit, I had received a new list with 10 contacts, so I decided to keep trying. I started calling the numbers one by one. The third person I reached finally picked up, but the job was no longer available—same with the fourth. Finally, with the fifth and seventh contacts, I was able to schedule short interviews. Both were for positions as a {occupation}. In each interview, I introduced myself, explained my qualifications and conditions, and one of them gave me the opportunity to start on a trial basis. It went well.
Today, thanks to the program, I am still employed as a {occupation}. I also have additional contacts to explore in the future if needed, but for now, I am happy where I am.

The key takeaway is that the "Quelimane works with everyone!" program truly works—you just need to be persistent. There are jobs and opportunities in the city for everyone. Keep trying!

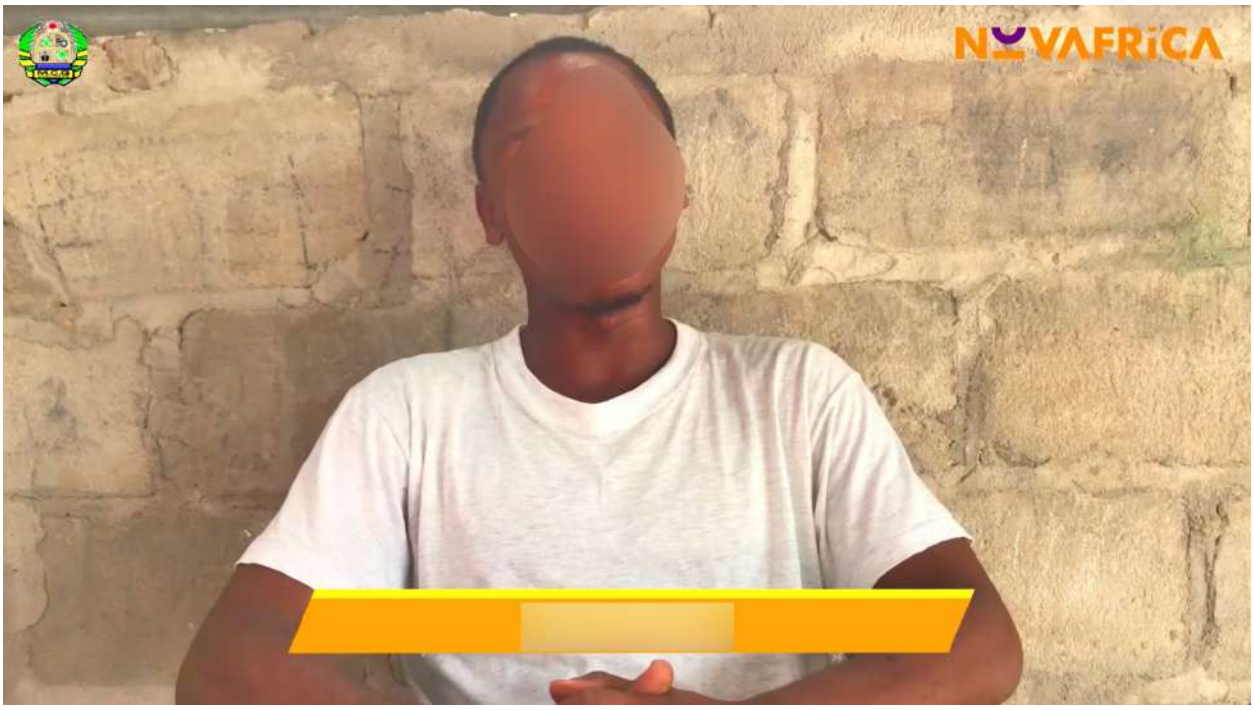


Figure A7: Migrant video (male)



Figure A8: Migrant video (female)

A.3 Mobile Money

The third component of each of the five house visits introduced migrants to mobile money. Program implementers shared a presentation on Mozambique's leading mobile money service (M-PESA). It included information on how to open an account, cash-in and cash-out electronic money, as well as on how to make transfers. The slides employed in the visits are shown in Figure A9. In the third round of the visits, participants were given 20 Meticais (around 0.3 USD) to cash-in and transfer to a rural family member. It served the purpose of incentivizing the opening of accounts for those not holding one, and trialing transfers to the migrants' origin household using mobile money.



Figure A9: Presentation on mobile money

A.4 Block leaders

One of the two versions of the program contained the explicit support and active participation of the block leaders (within their corresponding block). In each round of visits in the leader treatment, at different moments of the conversation, the field team mentioned the support of the block leader – the contacts of the block leader were shared in that context. This is depicted in the script of Figure A10. Close to the beginning of the conversations with migrants program implementers showed a video on tablets with a short message from the corresponding block leader, who expressed clear support for the program and incentivized migrants to follow the instructions and advice of the program implementers. The corresponding script is shown in Figure A11. We note that in the fourth round all leaders were asked to emphasize the relevance of participating in elections when speaking in the video that was shown in the face-to-face visits to migrants. The corresponding script is shown in Figure A12. The content and framing of such message was left at their discretion, with most leaders delivering a political message related to the approaching municipal elections of 2023. We display a frame from one of these videos in Figure A13. We show in Figure A14 an example of the leaders' contact cards that were distributed at the end of the visits.

Figure A10: Protocol of the leader treatment - all rounds

[Enumerator, please read the following information to the respondent]

In this block, the person in charge is {block leader name}. This program is supported by the block leader, who will be present at our visits whenever possible. You can contact him/her through the following numbers: {contact 1}{contact 2}, {contact 3}.

*[Enumerator, please share now the leader video with the respondent. Ensure that the video exhibited belongs to leader {*block leader name*} in block {block code}.]*

Module on administrative information

I remind you that this program is sponsored by your block leader {block leader name}. We count on his/her support and you can turn to him whenever you have any struggles in the block. You can reach him at: {contact 1}{contact 2}, {contact 3}.

Module on job matching

Once more, I remind you that this program has the support of the block leader *{*block leader name*}*. All these offers have been approved and made available by him.

Module on mobile money

This presentation is also supported by the block leader *{*block leader name*}*. He acknowledges and promotes the benefits of adhering to Mobile Money.

Thank you very much for the time spent with us. We plan to revisit you next month to talk to you a bit. See you soon

[Enumerator, please hand in the contact card. I remind you that this is a leader treatment block, so you must please hand in the card with the corresponding block leader details.]

Figure A11: Script for leaders' videos - round 1

I am {name} and I have been the leader in block {block code} for {years} years.

I am here today to express my interest and support the integration of rural migrants into the city of Quelimane. I am deeply interested in understanding what we can do to improve your current living conditions and the support you provide to your family outside Quelimane.

Over the past year and with my support, there were conducted interviews to people recently arrived in Quelimane. You are one of these people. Over the next six (6) months, we plan to carry out monthly visits to you to understand your main challenges, do a little follow-up on job opportunities in Quelimane and help you better understand how you can support your family and friends more easily.

I personally invite you to join the program `Quelimane works with all`. We hope to be of help to you during this transition period into this city."

Figure A12: Script for leaders' videos - round 4

[Enumerator, please read the following instructions to the respondent.]

Hi, my name is {enumerator name}, and I'm a member of the NOVAFRICA team. As you know, we have been working with the Municipality for some time to implement a program aimed at integrating recent rural migrants into the community.

We believe that by fostering a more dignified life and ensuring better integration for migrants, we are contributing to the overall development of the city. Your support has been invaluable in promoting this initiative, and today, we're here once again to ask for your cooperation.

We came to you last year to ask you to shoot a video promoting and supporting our program to integrate rural migrants in Quelimane. Today we return to shoot one more video with similar content. The idea is, once again, to promote our program and build momentum for this final round. The program aims to have a positive impact on the block's life and the Municipality in general, and since you as the leader are someone important to the community, we are certain you are key in getting the migrants to join our program. At the same time, the local elections will take place in September, this year, so you can take this opportunity to remind migrants of the importance of voting in elections. It is also a good opportunity to show migrants how the Municipality has policies that help the community and the well-being of its citizens.

In summary, we ask you to mention three topics in this video:

- Your perception of how this program matters for integrating rural migrants into the community;

- How the program is advocated by the Municipality;

- How important it is for everyone to participate in the 2023 local elections.

We ask you to make a short recording of about 3/4 minutes. Are you ready to record?



Figure A13: Leader video



Figure A14: Card with leader's contact

B Sampling and randomization

The program studied in this paper was tailored to rural migrants who had recently arrived in Quelimane. With this in mind, we looked for a representative sample of the population of households containing at least one recent migrant. We display the map of Quelimane in Figure B1 with the distribution of migrants at the baseline, following the first wave of recruitment of migrants, in each block of the city. Figure B2 shows the fuller sample of migrants, residents, and block leaders at the baseline.

Randomization was performed within strata of up to three blocks. The following variables were used to compute the stratification metric:

- Neighborhood: Administrative division defined by the municipality. The city encompasses 54 neighborhoods, each one divided in blocks.
- Number of migrants per block: only migrants sampled in the first wave of baseline recruitment of migrants were accounted for stratification.

Some blocks changed leaders between survey waves. In such blocks, we interviewed both the former and the new leader at midline but show results only for the latter. At endline and follow-up II, only the leaders in office at that point were interviewed. The same migrants and residents sampled at the baseline were re-interviewed at midline and at endline, with no allowed substitutions. We found 78% and 76% of the migrants' and residents' sample at midline, respectively; and 90% and 83% of the migrants' and residents' samples at endline (values for the control group). Attrition in the sample of origin contacts was 38% in the midline, 6% in the endline, and 10% in the follow-up I. We test whether there were significant differences in attrition across treatment arms and find, reassuringly, that this is not the case. These results are shown in Table B1.

Figure B1: Number of migrants sampled at baseline in each block

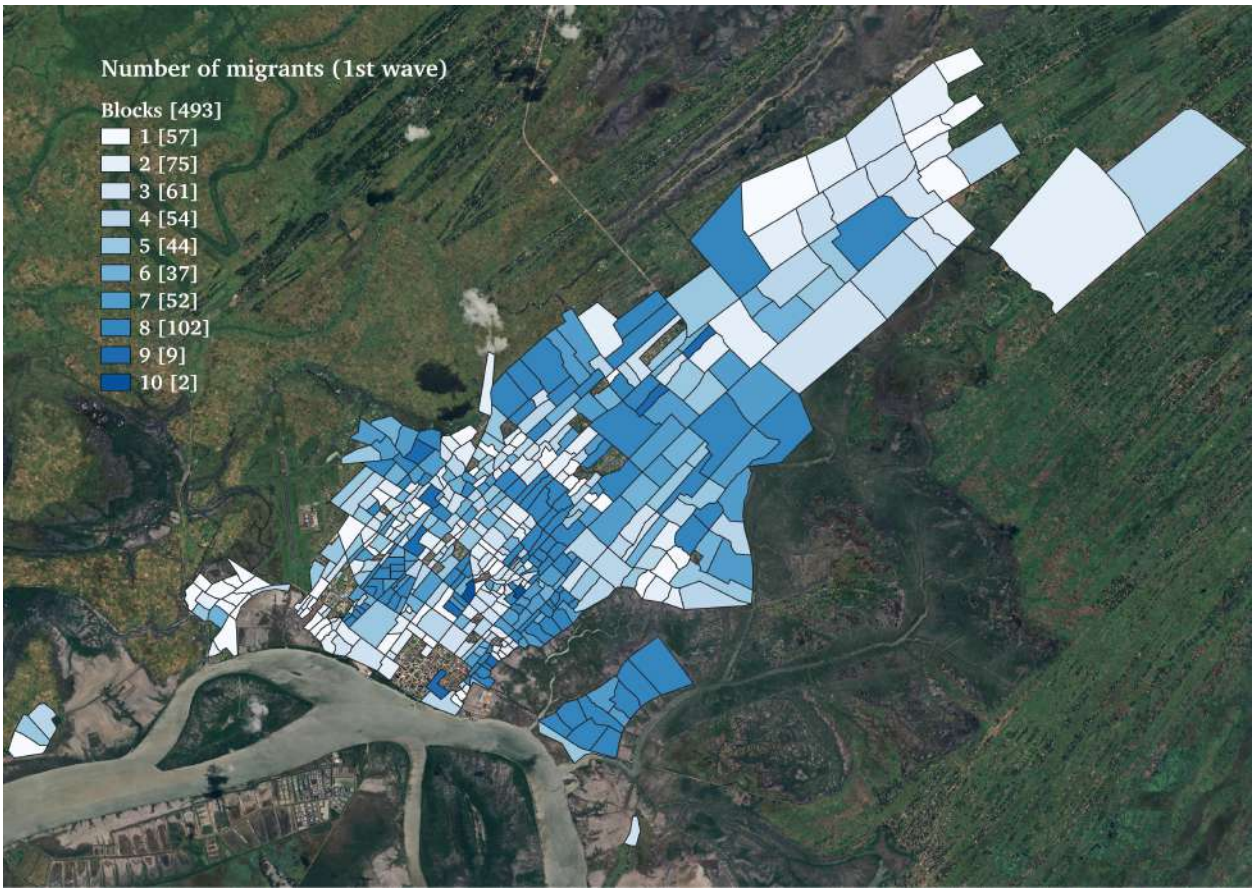
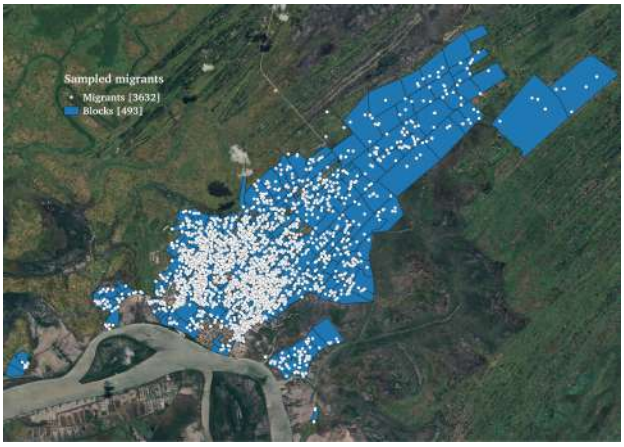
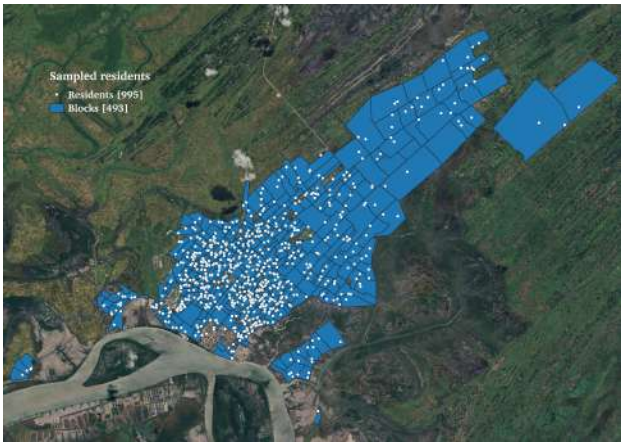


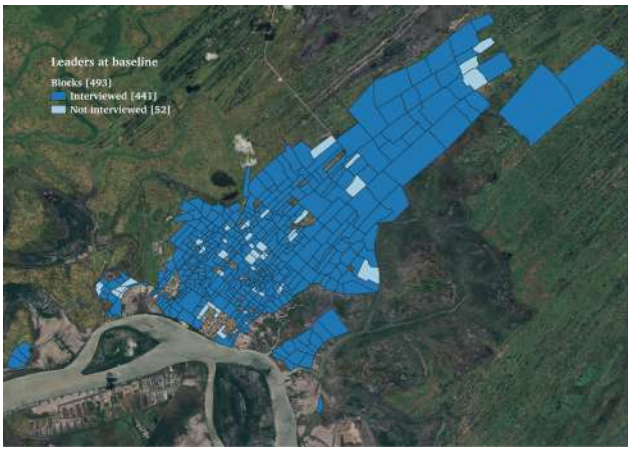
Figure B2: Sampling



(a) Sampled migrants



(b) Sampled residents



(c) Leaders found

Figure B3: Sample distribution by treatment group across the city

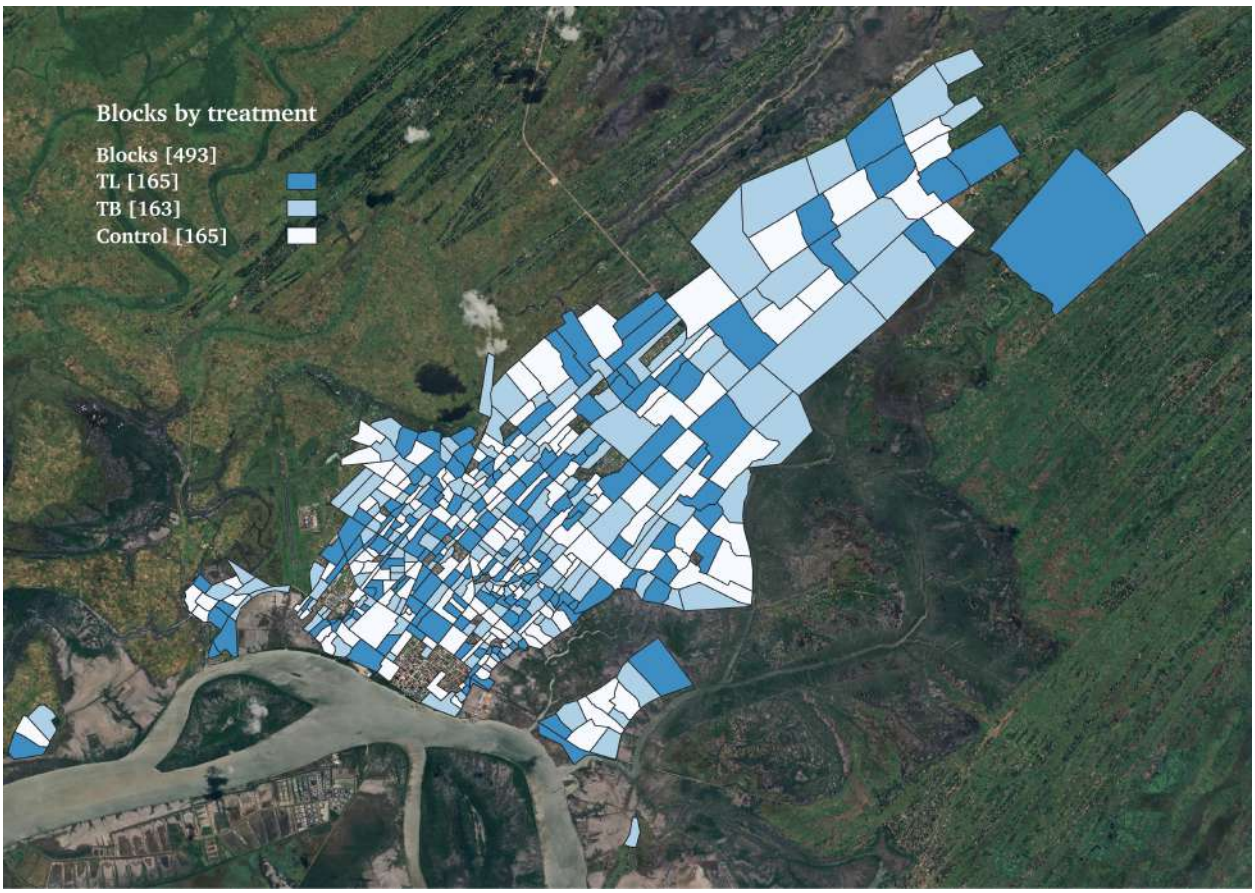


Table B1: Attrition by survey sample

	Midline				Endline				Follow-up I	Follow-up II
	Leaders	Migrants	Residents	Origin contacts	Leaders	Migrants	Residents	Origin contacts	Origin contacts	Leaders
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
(TL) Leader treatment	-0.039	-0.009	0.028	-0.002	-0.026	0.011	0.013	0.013	-0.003	-0.031
	(0.040)	(0.023)	(0.036)	(0.029)	(0.025)	(0.014)	(0.030)	(0.014)	(0.018)	(0.040)
(TB) Basic treatment	-0.017	-0.009	0.028	0.030	-0.038	0.005	0.009	0.001	0.007	-0.019
	(0.041)	(0.022)	(0.034)	(0.027)	(0.024)	(0.013)	(0.032)	(0.014)	(0.017)	(0.042)
Observations	493	3632	995	2518	493	3632	995	2519	2519	493
R^2	0.247	0.137	0.111	0.035	0.424	0.066	0.084	0.114	0.110	0.279
Mean (control group)	0.212	0.220	0.234	0.378	0.097	0.096	0.172	0.064	0.098	0.224
TL = TB (p-value)	0.607	0.974	0.998	0.254	0.618	0.679	0.892	0.386	0.598	0.787

Note. Estimates based on OLS equations using equation 1. Columns (1) and (5) present estimates for leaders; columns (2) and (6) present estimates for migrants; columns (3) and (7) present estimates for residents; columns (4) and (8) present estimates for origin contacts. Dependent variables by column: (1)-(4) Attrition from baseline to midline: dummy varible equal to 1 if the respondent was not interviewed at midline; (5)-(8) Attrition from baseline to endline: dummy variable equal to 1 if the respondent was not found at endline; (7) Attrition from baseline to follow-up I: dummy variable equal to 1 if the respondent was not found at the follow-up I; (8) Attrition from baseline to follow-up II: dummy variable equal to 1 if the respondent was not found at the follow-up II. All specifications include block and individual controls, as well as strata fixed effects. Section 4 presents the full list of controls. Standard errors, reported in parentheses, are clustered at the block level. *** p<0.01, ** p<0.05, * p<0.1.

Table B2: Balance - blocks

	Mean control (1)	Any treat (2)	TL (3)	TB (4)	p-value (5)	N (6)
Household proxy	143.81	-14.35*	-8.82	-19.77**	0.13	493
	[118.42]	(8.26)	(8.93)	(9.83)		
# sampled migrants	4.78	-0.08	-0.05	-0.11	0.51	493
	[2.63]	(0.08)	(0.10)	(0.10)		
Ilegal construction	0.42	0.03	0.02	0.04	0.74	482
	[0.50]	(0.04)	(0.05)	(0.05)		
# of taxi drivers	25.34	-8.70	-8.05	-9.38	0.24	418
	[96.44]	(6.31)	(7.71)	(5.69)		
Distance to school	1.61	-0.04	-0.03	-0.04	0.90	483
	[0.84]	(0.08)	(0.09)	(0.08)		
Distance to water fountain	1.55	0.00	-0.12	0.12	0.15	465
	[1.16]	(0.11)	(0.12)	(0.12)		

Note. Column (1) reports the mean and standard deviation for the whole sample. Column (2) reports the difference between both treatment groups pooled together and the control group using and OLS regression of the corresponding characteristic on the treatment indicator. Columns (3) and (4) report the differences between the leader/basic treatment and the control group, respectively. Column (5) presents a joint test of significance of the coefficients for each treatment dummy (TL, TB). Column (6) reports the number of observations at baseline.

Table B3: Balance - leaders

	Mean control (1)	Any treat (2)	TL (3)	TB (4)	p-value (5)	N (6)
Age	49.91	-1.00	-0.54	-1.46	0.61	441
	[12.21]	(1.22)	(1.39)	(1.47)		
Male	0.67	-0.02	-0.02	-0.01	0.92	441
	[0.47]	(0.05)	(0.06)	(0.06)		
Married/cohabiting	0.72	-0.01	0.02	-0.03	0.60	441
	[0.45]	(0.05)	(0.06)	(0.06)		
Catholic	0.66	-0.04	-0.03	-0.04	0.79	441
	[0.48]	(0.05)	(0.06)	(0.06)		
Literate	0.78	-0.03	-0.00	-0.06	0.48	441
	[0.42]	(0.05)	(0.05)	(0.05)		
Primary schooling	0.42	-0.02	-0.00	-0.05	0.74	441
	[0.49]	(0.06)	(0.06)	(0.06)		
Own dwelling	0.95	0.01	-0.00	0.02	0.80	441
	[0.23]	(0.03)	(0.03)	(0.03)		
Years in position (leader)	3.59	0.34	0.13	0.55	0.56	441
	[3.99]	(0.43)	(0.49)	(0.52)		
Likes migrants	0.77	0.05	0.02	0.08	0.33	431
	[0.42]	(0.04)	(0.05)	(0.05)		
Gov. is helping the poor	0.02	0.01	0.00	0.02	0.59	434
	[0.14]	(0.02)	(0.02)	(0.02)		

Note. Column (1) reports the mean and standard deviation for the whole sample. Column (2) reports the difference between both treatment groups pooled together and the control group using and OLS regression of the corresponding characteristic on the treatment indicator. Columns (3) and (4) report the differences between the leader/basic treatment and the control group, respectively. Column (5) presents a joint test of significance of the coefficients for each treatment dummy (TL, TB). Column (6) reports the number of observations at baseline.

Table B4: Balance - migrants

	Mean control (1)	Any treat (2)	TL (3)	TB (4)	p-value (5)	N (6)
Age	24.30	-0.29	-0.25	-0.34	0.57	3582
	[8.40]	(0.29)	(0.33)	(0.32)		
Male	0.66	-0.03*	-0.03*	-0.03	0.17	3632
	[0.48]	(0.02)	(0.02)	(0.02)		
Married/cohabiting	0.37	0.01	0.00	0.01	0.77	3627
	[0.48]	(0.02)	(0.02)	(0.02)		
Number of children	1.16	-0.01	0.00	-0.03	0.83	3507
	[1.68]	(0.05)	(0.06)	(0.06)		
Catholic	0.59	0.02	0.02	0.01	0.63	3515
	[0.49]	(0.02)	(0.02)	(0.02)		
Literate	0.66	0.01	0.02	0.00	0.49	3609
	[0.47]	(0.02)	(0.02)	(0.02)		
Primary schooling	0.32	0.03*	0.02	0.03*	0.20	3629
	[0.47]	(0.02)	(0.02)	(0.02)		
Primary occupation: none	0.22	-0.00	0.02	-0.03	0.07	2312
	[0.42]	(0.02)	(0.02)	(0.02)		
Returned to district since migration	0.25	-0.01	-0.00	-0.01	0.74	3626
	[0.43]	(0.02)	(0.02)	(0.02)		
Moved to work	0.50	-0.01	-0.02	-0.00	0.75	3632
	[0.50]	(0.02)	(0.02)	(0.02)		
Main struggle w/ moving: finding a job	0.33	0.03	0.03	0.03	0.33	3632
	[0.47]	(0.02)	(0.02)	(0.02)		
Contacted local leader (last 12 months)	0.24	0.02	0.03	0.01	0.79	2105
	[0.63]	(0.04)	(0.04)	(0.04)		

Note. Column (1) reports the mean and standard deviation for the whole sample. Column (2) reports the difference between both treatment groups pooled together and the control group using and OLS regression of the corresponding characteristic on the treatment indicator. Columns (3) and (4) report the differences between the leader/basic treatment and the control group, respectively. Column (5) presents a joint test of significance of the coefficients for each treatment dummy (TL, TB). Column (6) reports the number of observations at baseline.

Table B5: Balance - residents

	Mean control (1)	Any treat (2)	TL (3)	TB (4)	p-value (5)	N (6)
Age	34.25	-0.75	-0.76	-0.74	0.70	991
	[14.68]	(0.88)	(1.03)	(1.00)		
Male	0.40	0.03	0.04	0.01	0.47	995
	[0.49]	(0.03)	(0.04)	(0.04)		
Married/cohabiting	0.46	0.05	0.04	0.06*	0.22	995
	[0.50]	(0.03)	(0.03)	(0.03)		
Number of children	2.33	-0.14	-0.16	-0.11	0.59	994
	[2.25]	(0.14)	(0.16)	(0.16)		
Catholic	0.63	0.02	0.03	0.00	0.67	991
	[0.48]	(0.03)	(0.04)	(0.04)		
Literate	0.82	0.03	0.05*	0.01	0.21	978
	[0.39]	(0.02)	(0.03)	(0.03)		
Primary schooling	0.36	-0.03	-0.04	-0.01	0.56	995
	[0.48]	(0.03)	(0.03)	(0.03)		
Contacted local leader (last 12 months)	0.40	0.13**	0.10	0.15**	0.07	965
	[0.79]	(0.06)	(0.07)	(0.07)		

Note. Column (1) reports the mean and standard deviation for the whole sample. Column (2) reports the difference between both treatment groups pooled together and the control group using and OLS regression of the corresponding characteristic on the treatment indicator. Columns (3) and (4) report the differences between the leader/basic treatment and the control group, respectively. Column (5) presents a joint test of significance of the coefficients for each treatment dummy (TL, TB). Column (6) reports the number of observations at baseline.

Table B6: Balance - origin contacts

	Mean control (1)	Any treat (2)	TL (3)	TB (4)	p-value (5)	N (6)
Age	33.07	-0.68**	-0.33	-0.98**	0.04	2519
	[8.22]	(0.35)	(0.42)	(0.39)		
Male	0.56	0.02	0.01	0.04	0.21	2321
	[0.50]	(0.02)	(0.03)	(0.02)		
Married/cohabiting	0.46	-0.00	0.01	-0.02	0.50	2519
	[0.50]	(0.02)	(0.02)	(0.02)		
Number of children	2.24	0.08	0.30*	-0.11	0.03	1535
	[2.57]	(0.15)	(0.17)	(0.17)		
Catholic	0.62	0.01	-0.00	0.01	0.86	2286
	[0.49]	(0.02)	(0.03)	(0.02)		
Literate	0.89	0.00	0.00	-0.00	0.99	1539
	[0.31]	(0.02)	(0.02)	(0.02)		
Primary schooling	0.35	0.02	0.01	0.03	0.46	2383
	[0.48]	(0.02)	(0.02)	(0.02)		
Primary occupation: none	0.17	0.02	0.02	0.01	0.60	2383
	[0.37]	(0.02)	(0.02)	(0.02)		
Primary occupation: student	0.07	0.01	0.02	0.01	0.38	2383
	[0.25]	(0.01)	(0.01)	(0.01)		

Note. Column (1) reports the mean and standard deviation for the whole sample. Column (2) reports the difference between both treatment groups pooled together and the control group using and OLS regression of the corresponding characteristic on the treatment indicator. Columns (3) and (4) report the differences between the leader/basic treatment and the control group, respectively. Column (5) presents a joint test of significance of the coefficients for each treatment dummy (TL, TB). Column (6) reports the number of observations at baseline.

C Timeline

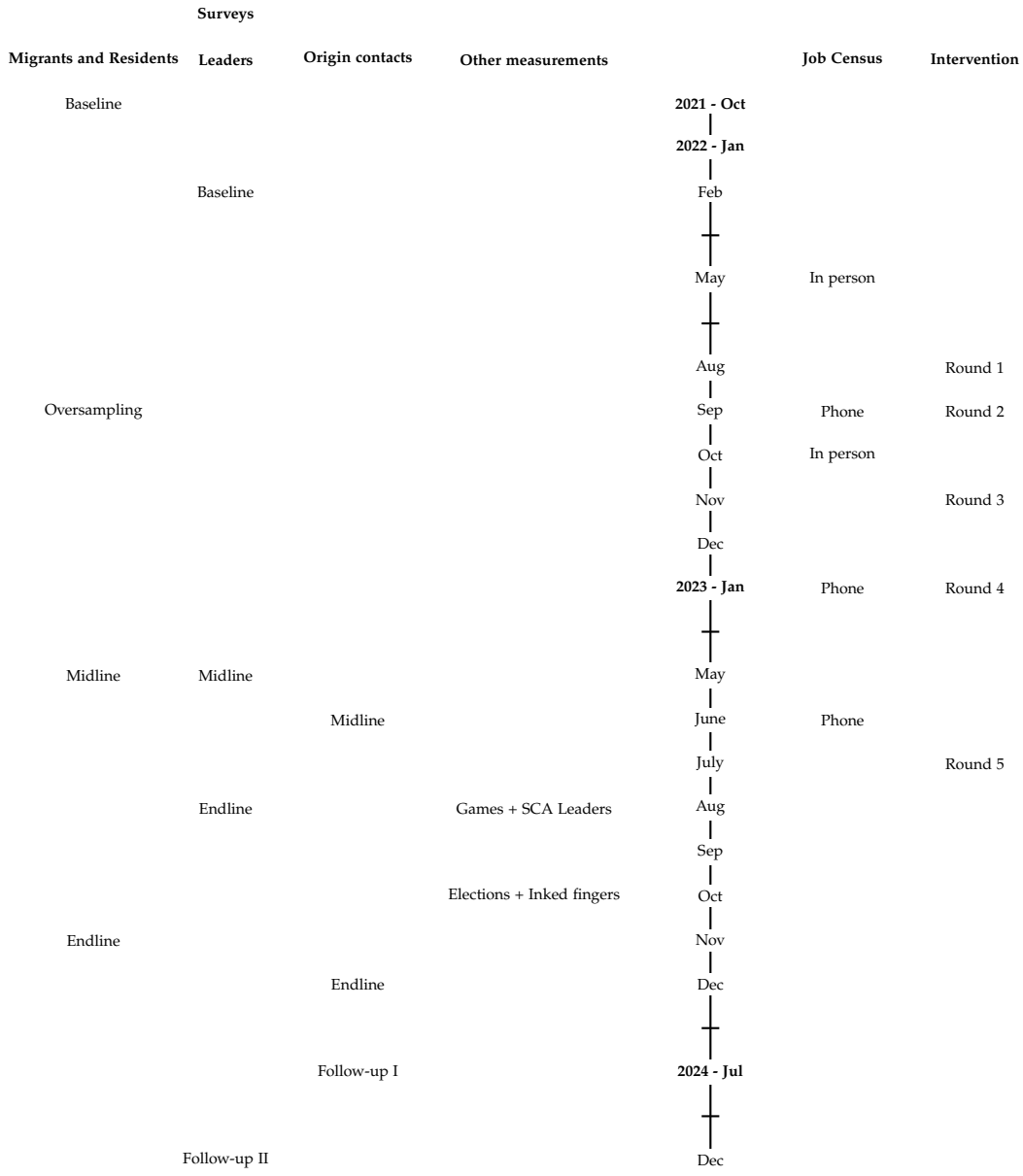


Figure C1: Timeline

D Measurement

D.1 Behavioral measurements

We provide below a photo of mobilized cyclists by a block leader (Figure D1) as well as the depiction of the stickers employed as part of the corresponding behavioral measurement (Figure D2).

Figure D1: Mobilized cyclists for political rally



Figure D2: Stickers





E Outcome variables and detailed results

E.1 Outcome variables

Table E1: Set of outcomes for program awareness

Topic	Variable and Description
Program knowledge	**Heard about program:** Indicator variable equal to 1 if the respondent reports having heard of the program "Quelimane trabalha com todos", and zero otherwise. The survey question is asked literally as represented in this table, without describing any details of what the program entailed. The variable is self-reported. The same variable was asked during the first and second post-baseline survey waves. The figure presents the stacked version, which employs the two outcomes simultaneously. The same survey question was not asked during the baseline survey wave.
Who is involved in the program?	**Self:** Indicator variable equal to 1 if the respondent reports having been involved in the program, and zero otherwise. This question was displayed in the survey conditional on having responded positively to being familiar with the program *"Quelimane trabalha com todos"*. The variable was manually given a value of zero if the respondent reports not having heard of the program before. The respondent was directly asked whether they were involved in the program. The variable is self-reported. The same variable was asked during the first and second post-baseline survey waves. The figure presents the stacked version, which employs the two outcomes simultaneously. The same survey question was not asked during the baseline survey wave. **Family:** Indicator variable equal to 1 if the respondent reports that their family was involved in the program, and zero otherwise. This question was displayed in the survey conditional on having responded positively to being familiar with the program *"Quelimane trabalha com todos"*. The variable was manually given a value of zero if the respondent reports not having heard of the program before. The respondent was directly asked about whether their families were involved in the program. The variable is self-reported. The same variable was asked during the first and second post-baseline survey waves. The figure presents the stacked version, which employs the two outcomes simultaneously. The same survey question was not asked during the baseline survey wave. **Block people:** Indicator variable equal to 1 if the respondent says that the people living in their block were involved in the program, and zero otherwise. This question was displayed in the survey conditional on having responded positively to being familiar with the program *"Quelimane trabalha com todos"*. The variable was manually given a value of zero if the respondent had not heard of the program before. The respondent was directly asked about whether the people in their block were involved. The variable is self-reported. The same variable was asked during the first and second post-baseline survey waves. The figure presents the stacked version, which employs the two outcomes simultaneously. The same survey question was not asked during the baseline survey wave. **Rural migrants:** Indicator variable equal to 1 if the respondent says that rural migrants were involved in the program, and zero otherwise. This question was displayed in the survey conditional on having responded positively to being familiar with the program *"Quelimane trabalha com todos"*. The variable was manually given a value of zero if the respondent reports not having heard of the program before. The respondent was directly asked about whether the rural migrants were involved. The variable is self-reported. The same variable was asked during the first and second post-baseline survey waves. The figure presents the stacked version, which employs the two outcomes simultaneously. The same survey question was not asked during the baseline survey wave.

Table E2: Set of outcomes for interaction with block leader

Topic	Variable and Description
Leader knows migrants	**General:** Indicator variable equal to 1 if the leader reports personally knowing migrants living in the same block as the respondent, and zero otherwise. This question did not specify any individuals. The variable is self-reported. The same variable was asked during the first and second post-baseline survey waves. The table presents the stacked version, which employs the two outcomes simultaneously. The same survey question was not asked during the baseline survey wave. **% sampled** For this question, leaders was initially presented with a list of migrants living in their assigned blocks. The list displayed the names of all the people in that block who were sampled for this project (migrants and residents). The respondent was asked to select the names of the people with whom they were acquainted. The variable ranges from 0 to 1, and indicates the percentage of rural migrants that the leader selects from the list, out of the total number of migrants sampled in the block. The variable is self-reported. The same variable was asked during the first and second post-baseline survey waves. The table presents the stacked version, which employs the two outcomes simultaneously. The same survey question was not asked during the baseline survey wave.
Contact with leader	**Knows leader:** Indicator variable equal to 1 if the respondent correctly names the current leader in their living block, and zero otherwise. The respondents are asked to report the leader's name, which was considered correct if it matched the one in the field records. The variable is self-reported. Due to a field mistake, the variable was not correctly collected during the second post-baseline survey wave and so the table presents results for the first post-baseline survey wave only. The same variable was not collected at baseline. **Contacted leader:** Indicator variable equal to 1 if the respondent contacted reports having contacted the block leader in between survey waves, and zero otherwise. The variable represents the extensive margin as it does not dive into the purpose of the contact. The variable is self-reported. The same variable was asked during the first and second post-baseline survey waves. The table presents the stacked version, which employs the two outcomes simultaneously. The same survey question was not asked during the baseline survey wave. **Resorted to leader for job:** Indicator variable equal to 1 if the respondent reports having contacted or paid the leader in the previous year to get a job, and zero otherwise. Variable was constructed by combining a survey questions on whether the respondent contacted the leader for a job in the previous year and another on whether the respondent paid the leader for a job in the previous year. The variable is equal to one if either is equal to one. The variable is self-reported. The same variable was asked during the first and second post-baseline survey waves. The table presents the stacked version, which employs the two outcomes simultaneously. The same survey question was not asked during the baseline survey wave.

Table E3: Set of outcomes for political participation

Topic	Variable and Description
Political participation	**Party objects:** Indicator variable equal to 1 if the field administrator identified any object with political content in the respondent's living place - which include hats, t-shirts, posters, pins, or others - and zero otherwise. The variable is observational: the field administrator was instructed not to ask the question but to observe the surroundings and report if any items were found. The same variable was asked during the first and second post-baseline survey waves. The figure presents the stacked version, which employs the two outcomes simultaneously. The same survey question was not asked during the baseline survey wave. **In a party (2024):** Indicator variable equal to 1 if the respondent reports being a member of a political party, and zero otherwise. The variable is self-reported. The question was asked during the follow-up II phone survey conducted in December 2024, after the national 2024 elections in Mozambique. **In RENAMO (2024):** Indicator variable equal to 1 if the respondent reports being a member of the political party RENAMO, and zero otherwise. The variable was manually assigned to zero when the respondent reports not being part of a political party in general. The variable is self-reported. The question was asked during the follow-up II survey conducted in December 2024, after the national 2024 elections in Mozambique. **Municipal elections 2023:** Indicator variable equal to 1 if the respondent reports intent to vote in the political party RENAMO for the 2023 local elections in Mozambique, and zero otherwise. The variable is missing if the respondent previously reported not having voted at all. The variable is self-reported. The variable was collected during the second post-baseline survey wave.
Turnout	**Inked finger:** Indicator variable equal to 1 if the respondent's finger was colored with purple ink at the time of the field team's visit, and zero otherwise. Mozambique has a long-standing tradition of marking fingers with ink after voting as a sign of voting participation. The ink mark should stay up to two or three days after. The field team visited the entire project sample in the two days following the 2023 local elections in Mozambique to check for the ink mark on the fingers. This measurement was collected in October 2023.
Voting:	**Voted RENAMO/FRELIMO/OTHERS (2023):** Indicator variable equal to 1 if the respondent reports having voted for the political party RENAMO/FRELIMO/OTHERS during the 2023 local elections in Mozambique, and zero otherwise. The variable is missing if the respondent has previously reported not having voted at all. The variable is self-reported. The question was asked during the second post-baseline survey wave. **Voted RENAMO/FRELIMO/PODEMOS (2024):** Indicator variable equal to 1 if the respondent reports having voted for the political party RENAMO/FRELIMO/PODEMOS during the 2024 national elections in Mozambique, and zero otherwise. The variable is missing if the respondent has previously reported not having voted at all. The variable is self-reported. The question was asked during the follow-up II survey conducted in December 2024, after the national 2024 elections in Mozambique.
Holding partisan political objects:	**RENAMO/FRELIMO/MDM:** Indicator variable equal to 1 if the field administrator identified any object with a political affiliation to RENAMO/FRELIMO/MDM at the respondent's living place - which include hats, t-shirts, posters, pins, or others -, and zero otherwise. The variable is observational: the field administrator was instructed not to ask the question but to observe the surroundings and report if any items were found. The same variable was asked during the first and second post-baseline survey waves. The figure presents the stacked version, which employs the two outcomes simultaneously. The same survey question was not asked during the baseline survey wave.

Table E4: Set of outcomes for political support for local incumbent

Topic	Variable and Description
Leader mobilization	**Reports cyclists:** This variable derives from the behavioral activity described in section D. Indicator variable that takes the value of 1 if the list delivered to the leaders contained at least one name of a cyclist to participate in the political bicycle rallies, and zero otherwise. Quelimane is largely dependent on bicycle taxi drivers as its main transportation method. These drivers have often been used by the incumbent Mayor for political campaigning through bicycle rallies. During the second post-baseline survey wave, block leaders were given a blank list to fill with names of bicycle taxi drivers living in their blocks that could be mobilized for these rallies. Field administrators conducted a second visit to the blocks to collect these lists. This visit was announced two days in advance, and block leaders were also instructed to request the people on the list to attend. These lists were collected up to two weeks after the initial visit. For names which did not attend the visit, field administrators confirmed their veracity with a phone call. The variable reflects whether the list contained at least one *"true"* name (extensive margin). This activity was not conducted during the baseline survey wave. **Observed # mobilized** Numerical variable counting the number of bicycle taxi drivers that attended the field team's confirmation visit. Each of these people represents a roster entry in a confirmation survey. The registration was not conditional on prior record on the list, and it reprents the sum of all the entered rosters. This activity was not conducted during the baseline survey wave.
Stickers	**From leader:** Variable ranging from 0 to 1 as a percentage of brown (leader) stickers found hanging at the front doors of the blocks' inhabitants. Each leader received 40 brown stickers, as presented in figure D2, and was instructed to distribute them to the population living in their block. Two weeks after, the field team was instructed to circle the sampled blocks and count the number of brown stickers identified hanging in front doors. This variable is constructed as the percent number out the 40 distributed identified in front doors. This activity was not conducted during the baseline survey wave. **From field team:** Variable ranging from 0 to 1 as a percentage of pink (field team) stickers found hanging at the front doors of the blocks' inhabitants. Field administrators visited every migrant and resident in the sample to distribute a pink sticker, as presented in figure D2. Two weeks after, the field team was instructed to circle the sampled blocks and count the number of pink stickers identified hanging in front doors. This variable is constructed as the percentage of pink stickers found out of the total number of respondents (migrants and residents) sampled in each block. This activity was not conducted during the baseline survey wave.
Support for incumbent	**Self-reported voting RENAMO:** Variable equal to 1 if the respondent reports having voted for RENAMO political party for the 2023 local elections in Mozambique, and zero otherwise. The variable was collected in the second post-baseline survey wave, which took place after the elections in October 2023. It was not possible to collect the same variable for block leaders as the survey for this subsample was implemented before the local elections. The same variable does not exist for the baseline survey.

Table E5: Set of outcomes for labor market outcomes - migrants

Topic	Variable and Description
Job opportunities	**Heard of job (12 months)**: Indicator variable equal to 1 if the respondent heard of a job opportunity in the 12 months previous to the interview, and zero otherwise. The variable is self-reported by the migrant. The same variable was asked during the first and second post-baseline survey waves. The table presents the stacked version, which employs the two outcomes simultaneously. The same survey question was not asked during the baseline survey wave. **Heard of job through program**: Indicator variable equal to 1 if the respondent heard of a job opening through the program *"Quelimane trabalha com todos"*, and zero otherwise. This variable was displayed in the survey conditional on having heard of a job opening in the 12 months previous to the interview date. The variable is missing if respondents had not heard of a job offer in the previous question. With this said, it represents all respondents who heard of a job opening through the program in the 12 months before the interview date. The variable is self-reported by the migrant. The same variable was asked during the first and second post-baseline survey waves. The table presents the stacked version, which employs the two outcomes simultaneously. The same survey question was not asked during the baseline survey wave.
Employment	**Working:** Indicator variable equal to 1 if the respondent is currently employed and earning monetary compensation, and zero otherwise. Variable constructed from a categorical variable with multiple employment options, converted to 1 if the respondent selects any option other than student, retired or unemployed, and zero otherwise. The variable is self-reported by the migrant. The same variable was asked during the first and second post-baseline survey waves. The table presents the stacked version, which employs the two outcomes simultaneously. The same survey question was asked during the baseline survey wave and is included as a control variable in the displayed regression. **Tot # jobs:** Variable summing the total number of jobs that the respondent reports having had since the beginning of the project. Respondents were asked whether they were currently employed and then the whole employment history was constructed since the project's start. The variable represents the sum of all jobs that the respondent reports having had since October 2021. **# hours working:** The variable is constructed out of a subset of 24 other variables, in which the respondent is asked about the activity conducted in each hour of the day before the interview date (options include *sleeping* or *eating*, for example). This variable is constructed by summing the number of hours the respondent reported being at work - urban or rural. The variable is self-reported. The same variable was asked during the first and second post-baseline survey waves. The table presents the stacked version, which employs the two outcomes simultaneously. The same survey question was asked during the baseline survey wave and is included as a control variable in the displayed regression.
Wages	**Wage p/ week:** Variable constructed by subtracting the mean to the reported wage per week and dividing by the standard deviation. It should be interpreted in standard deviation units. This variable is displayed conditional on the respondent being employed, and is considered as a missing for unemployed respondents. The variable is self-reported. The same variable was asked during the first and second post-baseline survey waves. The table presents the stacked version, which employs the two outcomes simultaneously. The same survey question was not asked during the baseline survey wave.

Table E6: Set of outcomes for migration

Topic	Variable and Description
Migration	**Moved back to district**: Variable equal to 1 if the respondent has returned to the origin district, and 0 otherwise. Variable constructed based on records from field administrators, and it takes the value of one if the respondent has not answered the survey because they report having returned to their home district. The variable was constructed for the first and second post-baseline survey waves. The table presents the stacked version, which employs the two outcomes simultaneously. For obvious reasons, the same variable does not exist at for the baseline. **In Quelimane:** Variable equal to 1 if the respondent reports being permanently living in Quelimane at the time of the interview, and zero otherwise. Column (3) employs data collected in the second post-baseline survey wave. Column (4) uses data from a follow-up phone survey conducted in July 2024 with district relatives in which respondents were inquired about their current place of living. The same survey question was not asked during the baseline survey wave. **Likely to move:** Indicator variable constructed from a categorical variable with options *"Very likely"*, *"Likely"*,*"Not very likely"* and *"Not likely at all"*, in which respondents were asked about the likelihood of moving to Quelimane within the next year. The variable is equal to 1 if the respondent reports being *"Very likely"* or *"Likely"*, and zero otherwise. The same variable was asked during the first and second post-baseline survey waves. The table presents the stacked version, which employs the two outcomes simultaneously. The same survey question was not asked during the baseline survey wave.

Table E7: Set of outcomes for integration & mobile money

Topic	Variable and Description
Perceptions towards migrants' integration	**Migs. treated unfairly.** Categorical variable with options *Never*, *Sometimes*, *Many times* and *Always* converted into a dummy variable equal to 1 if the respondent believes that rural migrants are unfairly treated always or many times, and zero otherwise. The variable is self-reported. For leaders, this question was only asked during the first post-baseline survey wave. The question was also not asked during the baseline survey. For migrants and residents, the same variable was asked during the first and second post-baseline survey waves. The same question was also asked during the baseline survey wave and is included in the regression. **Migs. are positive.** Categorical variable with options *Very negative*, *Negative*, *Neither negative nor positive*, *Positive*, *Very positive* converted into a dummy variable equal to 1 if the respondent believes that rural migrants are positive or very positive, and zero otherwise. For migrants, the variable is equal to 1 if the respondent agrees that people view migrants in the city as positive or very positive, and zero otherwise. The variable is self-reported. For leaders, this question was only asked during the first post-baseline survey wave. The question was also not asked during the baseline survey. For migrants and residents, the same variable was asked during the first and second post-baseline survey waves. The same question was also asked during the baseline survey wave and is included in the regression.
Mobile money services	**# services used**: Variable constructed by summing the total number of available mobile money services that the respondent reports using. The respondent is asked to report all the features of mobile money that they use, and field administrators report it in a multiple option question. The variable is then constructed by summing all the selected options. The variable was collected in the first post-baseline survey wave. The same variable was not collected at baseline.
Mobile money transfers	**to district reported by migrant:** Variable equal to 1 if the respondent reports having sent mobile money transfers to someone close living in their origin district in the 30 days previous to the interview, and zero otherwise. This variable was collected in the first post-baseline survey wave. The same survey question was not asked during the baseline survey wave. **to district reported by district relative:** Variable equal to 1 if the migrant's relative still living in the district reports having received a mobile money transfer from the migrant in the 30 days previous to the interview, and zero otherwise. This variable was collected in the first post-baseline survey wave. The same survey question was not asked during the baseline survey wave. **to migrant reported by migrant:** Variable equal to 1 if the respondent reports having sent mobile money transfers to someone close living in Quelimane in the 30 days previous to the interview, and zero otherwise. This variable was collected in the first post-baseline survey wave. The same survey question was not asked during the baseline survey wave. **to migrant reported by resident:** Variable equal to 1 if the migrant's relative still living in the district reports having sent a mobile money transfer to the migrant in the 30 days previous to the interview, and zero otherwise. This variable was collected in the first post-baseline survey wave. The same survey question was not asked during the baseline survey wave.

F Additional analysis

Table F1: The program - leaders

	Heard about program	Who is involved in the program?			
		Self	Family	Rural migrants	Block people
	(1)	(2)	(3)	(4)	(5)
(TL) Leader treatment	0.130***	0.184***	-0.005	0.125***	0.118***
	(0.030)	(0.036)	(0.024)	(0.033)	(0.035)
(TB) Basic treatment	-0.008	0.024	-0.002	-0.033	-0.064*
	(0.032)	(0.037)	(0.024)	(0.032)	(0.037)
Observations	859	859	859	859	859
Adjusted R^2	0.082	0.094	0.031	0.083	0.041
Mean (control group)	0.716	0.450	0.099	0.323	0.397
T1 = T2 (p-value)	0.000	0.000	0.910	0.000	0.000

Note: Estimates based on OLS regressions using equation 3 (stacked regressions). The table presents results for block leaders. Dependent variables by column: (1) *Heard about program*: variable equal to 1 if the respondent has heard about the program "Quelimane trabalha com todos', and 0 otherwise; (2) *Self*: variable equal to 1 if the respondent reports to have been involved in the program, and 0 otherwise; (3) *Family*: variable equal to 1 if the respondent reports that his/her family was involved in the program, and 0 otherwise; (4) *Block people*: variable equal to 1 if the respondent reports that people living in the same block were involved in the program, and 0 otherwise; (5) *Rural migrants*: variable equal to 1 if the respondent reports that rural migrants were involved in the program, and 0 otherwise. Additional details about the dependent variables are presented in the online Appendix in Table E1. All specifications include block and individual controls, as well as strata fixed effects. Section 4 presents the full list of controls. Standard errors, reported in parentheses, are clustered at the block level. *** p<0.01, ** p<0.05, * p<0.1.

Table F2: The program - migrants

	Heard about program	Who is involved in the program?			
		Self	Family	Rural migrants	Block people
	(1)	(2)	(3)	(4)	(5)
(TL) Leader treatment	0.074***	0.092***	0.022**	0.053***	0.040**
	(0.014)	(0.017)	(0.010)	(0.016)	(0.016)
(TB) Basic treatment	0.083***	0.089***	0.010	0.034**	0.026*
	(0.013)	(0.017)	(0.010)	(0.017)	(0.015)
Observations	6104	6102	6104	6096	6101
Adjusted R^2	0.182	0.213	0.137	0.114	0.155
Mean (control group)	0.704	0.584	0.093	0.283	0.295
T1 = T2 (p-value)	0.479	0.856	0.207	0.237	0.404

Note: Estimates based on OLS regressions using equation 3 (stacked regressions). The table presents results for migrants. Dependent variables by column: (1) *Heard about program*: variable equal to 1 if the respondent has heard about the program "Quelimane trabalha com todos', and 0 otherwise; (2) *Self*: variable equal to 1 if the respondent reports to have been involved in the program, and 0 otherwise; (3) *Family*: variable equal to 1 if the respondent reports that his/her family was involved in the program, and 0 otherwise; (4) *Block people*: variable equal to 1 if the respondent reports that people living in the same block were involved in the program, and 0 otherwise; (5) *Rural migrants*: variable equal to 1 if the respondent reports that rural migrants were involved in the program, and 0 otherwise. Additional details about the dependent variables are presented in the online Appendix in Table E1. All specifications include block and individual controls, as well as strata fixed effects. Section 4 presents the full list of controls. Standard errors, reported in parentheses, are clustered at the block level. *** p<0.01, ** p<0.05, * p<0.1.

Table F3: The program - residents

	Heard about program	Who is involved in the program?			
		Self	Family	Rural migrants	Block people
	(1)	(2)	(3)	(4)	(5)
(TL) Leader treatment	-0.004	0.005	0.010	0.040*	0.016
	(0.022)	(0.025)	(0.018)	(0.023)	(0.022)
(TB) Basic treatment	-0.041*	-0.037	-0.009	0.016	-0.006
	(0.023)	(0.025)	(0.017)	(0.021)	(0.022)
Observations	1575	1575	1574	1575	1573
Adjusted R^2	0.369	0.276	0.104	0.176	0.171
Mean (control group)	0.595	0.416	0.105	0.196	0.248
T1 = T2 (p-value)	0.095	0.085	0.288	0.283	0.350

Note: Estimates based on OLS regressions using equation 3 (stacked regressions). The table presents results for residents. Dependent variables by column: (1) *Heard about program*: variable equal to 1 if the respondent has heard about the program "Quelimane trabalha com todos', and 0 otherwise; (2) *Self*: variable equal to 1 if the respondent reports to have been involved in the program, and 0 otherwise; (3) *Family*: variable equal to 1 if the respondent reports that his/her family was involved in the program, and 0 otherwise; (4) *Block people*: variable equal to 1 if the respondent reports that people living in the same block were involved in the program, and 0 otherwise; (5) *Rural migrants*: variable equal to 1 if the respondent reports that rural migrants were involved in the program, and 0 otherwise. Additional details about the dependent variables are presented in the online Appendix in Table E1. All specifications include block and individual controls, as well as strata fixed effects. Section 4 presents the full list of controls. Standard errors, reported in parentheses, are clustered at the block level. *** p<0.01, ** p<0.05, * p<0.1.

Table F4: The program - origin contacts

	Heard about program	Who is involved in the program?	
		Family	Rural migrants
	(1)	(2)	(3)
(TL) Leader treatment	0.031*	0.024	0.009
	(0.017)	(0.015)	(0.006)
(TB) Basic treatment	0.003	0.017	-0.000
	(0.017)	(0.016)	(0.006)
Observations	3804	3795	3794
Adjusted R^2	0.100	0.088	0.030
Mean (control group)	0.219	0.184	0.025
T1 = T2 (p-value)	0.116	0.669	0.097

Note: Estimates based on OLS regressions using equation 3 (stacked regressions). The table presents results for district relatives. Dependent variables by column: (1) *Heard about program*: variable equal to 1 if the respondent has heard about the program "Quelimane trabalha com todos', and 0 otherwise; (2) *Self*: variable equal to 1 if the respondent reports to have been involved in the program, and 0 otherwise; (3) *Family*: variable equal to 1 if the respondent reports that his/her family was involved in the program, and 0 otherwise; (4) *Block people*: variable equal to 1 if the respondent reports that people living in the same block were involved in the program, and 0 otherwise; (5) *Rural migrants*: variable equal to 1 if the respondent reports that rural migrants were involved in the program, and 0 otherwise. Additional details about the dependent variables are presented in the online Appendix in Table E1. All specifications include block and individual controls, as well as strata fixed effects. Section 4 presents the full list of controls. Standard errors, reported in parentheses, are clustered at the block level. *** p<0.01, ** p<0.05, * p<0.1.

Table F5: Holding partisan political objects - 2023

	Migrants			Residents		
	RENAMO	FRELIMO	MDM	RENAMO	FRELIMO	MDM
	(1)	(2)	(3)	(4)	(5)	(6)
(TL) Leader treatment	0.009*	0.008**	-0.000	0.013	-0.014	-0.005*
	(0.005)	(0.004)	(0.001)	(0.011)	(0.011)	(0.003)
(TB) Basic treatment	0.017***	0.007*	-0.001	-0.003	-0.004	-0.001
	(0.005)	(0.004)	(0.001)	(0.009)	(0.011)	(0.003)
Observations	6103	6103	6103	1572	1572	1572
Adjusted R^2	0.046	0.033	0.004	0.075	0.008	0.015
Mean (control group)	0.029	0.018	0.003	0.044	0.039	0.006
T1 = T2 (p-value)	0.104	0.703	0.588	0.134	0.392	0.125

Note. Estimates based on OLS regressions. Columns (1)-(2) use equation 3, employing the the midline and endline (stacked regressions). We did not collect the lagged values for any of the dependent variables. Columns (1)-(3) present results for migrants; columns (4)-(6) present results for residents. Dependent variables by column: (1) and (4) *RENAMO*: observational variable equal to 1 if the enumerator identified any object belonging to the respondent and corresponding to the political party RENAMO during the survey interview, and 0 otherwise; (2) and (5) *FRELIMO*: observational variable equal to 1 if the enumerator identified any object belonging to the respondent and corresponding to the political party FRELIMO during the survey interview, and 0 otherwise; (3) and (6) *MDM*: observational variable equal to 1 if the enumerator identified any object belonging to the respondent and corresponding to the political party MDM during the survey interview, and 0 otherwise. Additional details about the dependent variables are presented in the online Appendix in Table E3. All specifications include block and individual controls, as well as strata fixed effects. Section 4 presents the full list of controls. Standard errors, reported in parentheses, are clustered at the block level. *** p<0.01, ** p<0.05, * p<0.1.

Table F6: Self-reported voting

	Vote RENAMO			Vote FRELIMO			Vote OTHER		
	Migrant	Resident	Origin contact	Migrant	Resident	Origin contact	Migrant	Resident	Origin contact
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(TL) Leader treatment	0.002	-0.047	0.128*	-0.008	0.050	-0.105**	0.006	-0.003	0.020
	(0.014)	(0.040)	(0.071)	(0.014)	(0.037)	(0.043)	(0.005)	(0.018)	(0.024)
(TB) Basic treatment	0.024*	-0.039	0.009	-0.018	0.042	-0.025	-0.006	-0.003	0.012
	(0.014)	(0.040)	(0.075)	(0.013)	(0.036)	(0.048)	(0.006)	(0.018)	(0.032)
Observations	2084	530	418	2084	530	418	2084	530	418
Adjusted R^2	0.018	0.139	0.132	0.020	0.160	0.111	0.011	-0.015	-0.015
Mean (control group)	0.911	0.851	0.539	0.074	0.123	0.149	0.015	0.026	0.026
T1 = T2 (p-value)	0.126	0.842	0.094	0.433	0.830	0.080	0.062	0.989	0.780

Note. Estimates based on OLS regressions using equation 1. Columns (1)-(2) and (4)-(6) show results from a phone survey conducted after the 2024 national elections in Mozambique. Column (3) shows results for the endline survey. We do not include the lagged values of any of the dependent variables. Dependent variables by columns: (1) *In a party (2024)*: variable equal to 1 if the respondent reports to be affiliated to a political party, and 0 otherwise; (2) *In RENAMO (2024)*: variable equal to 1 if the respondent reports to be affiliated to RENAMO, and 0 otherwise; (3) *Municipal elections 2023 – RENAMO*: variable equal to 1 if the respondent reports to have voted for RENAMO in the 2023 municipal elections, and 0 otherwise; (4) *Presidential elections 2024 – RENAMO*: variable equal to 1 if the respondent reports to have voted for RENAMO in the 2024 national elections, and 0 otherwise; (5) *Presidential elections 2024 – FRELIMO*: variable equal to 1 if the respondent reports to have voted for FRELIMO in the 2024 national elections, and 0 otherwise; (6) *Presidential elections 2024 – PODEMOS*: variable equal to 1 if the respondent reports to have voted for PODEMOS in the 2024 national elections, and 0 otherwise. Additional details about the dependent variables are presented in the online Appendix in Table E3. All specifications include block and individual controls, as well as strata fixed effects. Section 4 presents the full list of controls. Standard errors, reported in parentheses, are clustered at the block level. *** p<0.01, ** p<0.05, * p<0.1.

Table F7: Leaders' political behavior

	In a party (2024)	In RENAMO (2024)	Municipal elections 2023	Presidential elections 2024		
			RENAMO	RENAMO	FRELIMO	PODEMOS
	(1)	(2)	(3)	(4)	(5)	(6)
(TL) Leader treatment	0.030	0.051	0.005	0.143*	-0.029	-0.103
	(0.048)	(0.050)	(0.007)	(0.073)	(0.022)	(0.075)
(TB) Basic treatment	-0.077	-0.062	0.001	0.156**	-0.038*	-0.112
	(0.051)	(0.053)	(0.011)	(0.070)	(0.021)	(0.070)
Observations	343	341	369	309	309	309
Adjusted R^2	0.083	0.067	0.033	0.088	-0.006	0.065
Mean (control group)	0.856	0.838	0.992	0.392	0.039	0.559
T1 = T2 (p-value)	0.055	0.047	0.601	0.855	0.475	0.904
Outcome data	Follow-up II	Follow-up II	Endline	Follow-up II	Follow-up II	Follow-up II

Note. Estimates based on OLS regressions using equation 1. Columns (1)-(2) and (4)-(6) show results from a phone survey conducted after the 2024 national elections in Mozambique. Column (3) shows results for the endline survey. We do not include the lagged values of any of the dependent variables. Dependent variables by columns: (1) *In a party (2024)*: variable equal to 1 if the respondent reports to be affiliated to a political party, and 0 otherwise; (2) *In RENAMO (2024)*: variable equal to 1 if the respondent reports to be affiliated to RENAMO, and 0 otherwise; (3) *Municipal elections 2023 – RENAMO*: variable equal to 1 if the respondent reports to have voted for RENAMO in the 2023 municipal elections, and 0 otherwise; (4) *Presidential elections 2024 – RENAMO*: variable equal to 1 if the respondent reports to have voted for RENAMO in the 2024 national elections, and 0 otherwise; (5) *Presidential elections 2024 – FRELIMO*: variable equal to 1 if the respondent reports to have voted for FRELIMO in the 2024 national elections, and 0 otherwise; (6) *Presidential elections 2024 – PODEMOS*: variable equal to 1 if the respondent reports to have voted for PODEMOS in the 2024 national elections, and 0 otherwise. Additional details about the dependent variables are presented in the online Appendix in Table E3. All specifications include block and individual controls, as well as strata fixed effects. Section 4 presents the full list of controls. Standard errors, reported in parentheses, are clustered at the block level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table F8: Views about migrants' integration

	Migrants unfairly treated			Migrants are positive		
	Leader (1)	Migrant (2)	Resident (3)	Leader (4)	Migrant (5)	Resident (6)
(TL) Leader treatment	0.093*	0.012	0.012	0.120*	0.003	0.063**
	(0.049)	(0.008)	(0.012)	(0.065)	(0.019)	(0.029)
(TB) Basic treatment	0.017	0.003	0.014	0.112*	0.001	0.048
	(0.050)	(0.008)	(0.012)	(0.066)	(0.019)	(0.030)
Observations	339	3568	1492	347	5948	1537
Adjusted R^2	0.019	0.043	0.034	0.120	0.090	0.117
Mean (control group)	0.110	0.052	0.033	0.463	0.453	0.445
T1 = T2 (p-value)	0.156	0.336	0.880	0.899	0.931	0.576
Outcome data	Midline	Pooled	Pooled	Midline	Pooled	Pooled

Note. Estimates based on OLS regressions. Columns (1) and (4) use equation 1, and include data from the midline survey. Columns (2)-(3) and (5)-(6) use equation 3, employing the midline and endline surveys (stacked regressions). Columns (2)-(3) and (5)-(6) include the lagged value of the dependent variable as a control. We did not collect the lagged values for columns (1) and (4). Columns (1) and (4) show results for block leaders; columns (2) and (5) show results for migrants; columns (3) and (6) show results for residents. Dependent variables by column: (1)-(3) *Migrants treated unfairly*: variable equal to 1 if the respondent considers that migrants are frequently or very frequently treated unfairly by community members, and 0 otherwise; (4)-(6) *Migrants are positive*: variable equal to 1 if the respondent agrees with the statement that migrants are positive for the community, and 0 otherwise. Additional details about the dependent variables are presented in the Appendix in Table E7. All specifications include block and individual controls, as well as strata fixed effects. Section 4 presents the full list of controls. Standard errors, reported in parentheses, are clustered at the block level. *** p<0.01, ** p<0.05, * p<0.1.

Table F9: Mobile money

	# services used	Transfers to origin district reported by		Transfers to migrant reported by	
	Migrants	Migrant	Origin contact	Migrant	Origin contact
	(1)	(2)	(3)	(4)	(5)
(TL) Leader treatment	0.195**	0.018	0.085**	0.020	0.023
	(0.095)	(0.020)	(0.040)	(0.015)	(0.043)
(TB) Basic treatment	0.279***	0.050**	0.082**	0.021	0.056
	(0.097)	(0.021)	(0.039)	(0.016)	(0.041)
Observations	2849	2849	982	2849	978
Adjusted R^2	0.155	0.060	0.016	0.036	0.049
Mean (control group)	2.552	0.244	0.415	0.115	0.485
T1 = T2 (p-value)	0.391	0.124	0.929	0.938	0.447

Note. Estimates based on OLS regressions using equation 1. All outcomes were collected in the midline survey. We did not collect lagged values of any of the presented variables. Columns (1)-(2) and (4) display results for migrants; columns (3) and (5) display results for origin contacts. Dependent variables by column: (1) # *services used*: variable summing the total number of mobile money services that the respondent reports using from the list of all available services; (2) *Transfers to origin reported by migrant*: variable equal to 1 if the respondent reports having sent mobile money transfers to a relative living in their origin district in the 30 days before the interview date, and 0 otherwise; (3) *Transfers to origin reported by origin contact*: variable equal to 1 if the respondent reports having received mobile money transfers from a relative living in Quelimane in the 30 days before the interview date, and 0 otherwise; (4) *Transfers to migrant reported by migrant*: variable equal to 1 if the respondent reports having received mobile money transfers from a relative living in their origin district in the 30 days before the interview date, and 0 otherwise; (5) *Transfers to migrant reported by origin contact*: variable equal to 1 if the respondent reports having sent mobile money transfers to a relative living in Quelimane in the 30 days before the interview date, and 0 otherwise. Additional details about the dependent variables are in the Appendix in Table **??**. All specifications include block and individual controls, as well as strata fixed effects. Section 4 presents the full list of controls. Standard errors, reported in parentheses, are clustered at the block level. *** p<0.01, ** p<0.05, * p<0.1.

G Results for aggregated outcomes

Table G1: Aggregation of outcome variables - treatment adherence outcomes

	Program awareness				Interaction with leader		
	Leaders	Migrants	Residents	Dist. relatives	Leaders	Migrants	Residents
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(TL) Leader treatment	0.243***	0.124***	0.050	0.099***	0.199***	0.114***	0.135***
	(0.051)	(0.024)	(0.037)	(0.033)	(0.064)	(0.021)	(0.039)
(TB) Basic treatment	-0.041	0.105***	-0.035	0.033	0.065	0.029	0.014
	(0.050)	(0.022)	(0.035)	(0.033)	(0.071)	(0.020)	(0.036)
Observations	849	6091	1570	3808	821	6079	1564
Adjusted R^2	0.141	0.176	0.146	0.054	0.082	0.059	0.070
Mean (control group)	-0.001	-0.002	-0.002	0.003	0.013	-0.001	-0.002
T1 = T2 (p-value)	0.000	0.401	0.019	0.050	0.050	0.000	0.002

Note: Estimates based on OLS regressions using equation 3. Standard errors are clustered at the block level in regressions with observations at a lower level than the block. All specifications include block and individual controls, as well as strata fixed effects. Section 4 presents the full list of controls. Outcomes are grouped by table in indices built using the Kling et al. (2007) procedure. Outcomes are first normalized in standardized units (using the mean and standard deviation of the control group), and then averaged within each category. *** p<0.01, ** p<0.05, * p<0.1.

Table G2: Aggregation of outcome variables - political outcomes

	Political participation			Political support for the local incumbent				
	Leaders	Migrants	Residents	Leaders	Blocks	Migrants	Residents	Origin contacts
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(TL) Leader treatment	0.179	0.094***	-0.078	0.253**	0.070	0.008	-0.141	0.255*
	(0.119)	(0.028)	(0.058)	(0.109)	(0.061)	(0.050)	(0.115)	(0.142)
(TB) Basic treatment	0.101	0.092***	0.011	0.208*	0.010	0.083*	-0.113	0.018
	(0.119)	(0.029)	(0.057)	(0.110)	(0.068)	(0.050)	(0.115)	(0.151)
Observations	399	3027	762	429	429	2084	527	418
Adjusted R^2	-0.047	0.081	0.100	0.101	0.629	0.018	0.133	0.132
Mean (control group)	-0.015	0.002	-0.003	0.012	0.022	-0.000	0.002	0.006
T1 = T2 (p-value)	0.506	0.944	0.140	0.697	0.366	0.126	0.810	0.094

Note: Estimates based on OLS regressions using equation 3. Standard errors are clustered at the block level in regressions with observations at a lower level than the block. All specifications include block and individual controls, as well as strata fixed effects. Section 4 presents the full list of controls. Outcomes are grouped by table in indices built using the Kling et al. (2007) procedure. Outcomes are first normalized in standardized units (using the mean and standard deviation of the control group), and then averaged within each category. *** p<0.01, ** p<0.05, * p<0.1.

Table G3: Aggregation of outcome variables - economic outcomes

	Labor market outcomes - migrants			Migration	
	Job offers	Employment	Wages	Migrants	Origin contacts
	(1)	(2)	(3)	(4)	(5)
(TL) Leader treatment	0.227***	0.060**	0.000	0.025	0.070*
	(0.030)	(0.029)	(0.047)	(0.037)	(0.037)
(TB) Basic treatment	0.221***	-0.040	0.044	0.063*	0.065*
	(0.030)	(0.030)	(0.040)	(0.036)	(0.035)
Observations	1714	6100	4704	3579	1461
Adjusted R^2	0.236	0.158	0.031	0.040	0.035
Mean (control group)	0.879	-0.000	-0.000	0.000	-0.055
T1 = T2 (p-value)	0.789	0.000	0.347	0.294	0.893

Note: Estimates based on OLS regressions using equation 3. Standard errors are clustered at the block level in regressions with observations at a lower level than the block. All specifications include block and individual controls, as well as strata fixed effects. Section 4 presents the full list of controls. Outcomes are grouped by table in indices built using the Kling et al. (2007) procedure. Outcomes are first normalized in standardized units (using the mean and standard deviation of the control group), and then averaged within each category. *** p<0.01, ** p<0.05, * p<0.1.

H Treatment intensity

Table H1: Treatment intensity - migrant indices

	Program awareness	Interaction w/ leader	Political participation	Political support	Job offers	Employment	Wages	Migration
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rounds TL	0.039***	0.029***	0.015**	-0.002	0.056***	0.003	-0.002	0.030***
	(0.006)	(0.005)	(0.007)	(0.013)	(0.007)	(0.008)	(0.012)	(0.009)
Rounds TB	0.036***	0.006	0.014*	0.015	0.057***	-0.019**	0.009	0.035***
	(0.005)	(0.005)	(0.008)	(0.013)	(0.007)	(0.008)	(0.009)	(0.009)
Observations	6105	6093	3035	2090	1719	6114	4715	3589
Adjusted R^2	0.180	0.058	0.079	0.017	0.241	0.158	0.031	0.041
Mean (control group)	-0.005	-0.002	0.012	-0.003	0.878	0.002	-0.003	-0.016
TLI = TBI (p-value)	0.625	0.000	0.958	0.199	0.925	0.007	0.370	0.624

Note. Estimates based on OLS regressions using equation 3. Rounds TL is a categorical variable ranging from 0 to 5, counting the number of leader treatment rounds in which the respondent participated, Rounds TB is a categorical variable ranging from 0 to 5, counting the number of basic treatment rounds in which the respondent participated. Standard errors are clustered at the block level. All specifications include block and individual controls, and strata fixed effects. Section 4 presents the full list of controls. Outcomes are grouped in indices that are built using the procedure followed by Kling et al. (2007): outcomes are first normalized to study mean effect sizes of the indices relative to the standard deviation of the control group and then averaged within each category. *** p<0.01, ** p<0.05, * p<0.1.

I Heterogeneous effects

Table I1: Heterogeneous effects: earlier vs. later migrant arrivals

	Earlier					Later				
	TL		TB			TL		TB		
Outcome variable	Coeff.	S.E.	Coeff.	S.E.	N	Coeff.	S.E.	Coeff.	S.E.	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Migrants										
Program awareness	0.12***	0.03	0.08***	0.03	2917	0.12***	0.03	0.12***	0.03	3126
Interaction w/ block leader	0.11***	0.03	0.01	0.03	2913	0.13***	0.03	0.05*	0.03	3117
Political participation	0.15***	0.04	0.07*	0.04	1447	0.06	0.04	0.08*	0.04	1548
Political support	0.02	0.08	0.10	0.08	994	0.01	0.07	0.08	0.07	1038
Job offers	0.18***	0.04	0.12***	0.04	773	0.28***	0.04	0.32***	0.04	886
Employment	0.12***	0.04	0.01	0.04	2924	-0.02	0.04	-0.10**	0.04	3128
Wages	0.02	0.08	0.03	0.06	2222	-0.02	0.04	0.06	0.05	2447
Migration	-0.06	0.06	0.00	0.06	1764	0.11**	0.04	0.11**	0.05	1782
Panel B: District relatives										
Program awareness	0.13**	0.05	0.05	0.05	1708	0.08**	0.04	0.06	0.04	2123
Political support	0.16	0.26	0.25	0.28	157	0.15	0.22	-0.08	0.20	193
Migration	-0.02	0.07	0.08	0.07	607	0.13**	0.05	0.07	0.06	784

Note. Estimates based on OLS regressions using equation 1. TL refers to leader treatment, TB refers to basic treatment (see section B of the main text). Standard errors are reported in columns (2), (4), (6) and (8) and clustered at the block level. All specifications include block and individual controls, and strata fixed effects. Section 4 presents the full list of controls. Outcomes are grouped in indices that are built using the procedure followed by Kling et al. (2007): outcomes are first normalized to study mean effect sizes of the indices relative to the standard deviation of the control group and then averaged within each category. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table I2: Heterogeneous effects: leader is male vs. female

	Leader is male					Leader is female				
	TL		TB			TL		TB		
Outcome variable	Coeff.	S.E.	Coeff.	S.E.	N	Coeff.	S.E.	Coeff.	S.E.	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Leaders										
Program awareness	0.29***	0.08	-0.04	0.07	525	0.04	0.10	-0.30***	0.11	264
Interaction w/ block people	0.16*	0.10	0.02	0.10	504	0.37***	0.14	0.03	0.16	254
Political participation	0.14	0.15	0.22	0.16	213	0.14	0.36	-0.14	0.37	59
Political support	0.12	0.10	0.05	0.11	222	0.03	0.13	-0.03	0.16	69
Mobilization	0.16	0.15	0.26	0.16	222	0.40	0.33	-0.15	0.33	69
Panel B: Migrants										
Program awareness	0.11***	0.03	0.09***	0.03	3620	0.16***	0.05	0.10*	0.06	1923
Interaction w/ block leader	0.10***	0.03	0.03	0.03	3612	0.02	0.05	-0.05	0.05	1921
Political participation	0.08*	0.04	0.11***	0.04	1826	-0.06	0.06	0.03	0.08	938
Political support	-0.15**	0.06	0.02	0.07	1230	0.27***	0.09	0.45***	0.10	677
Job offers	0.16***	0.04	0.20***	0.04	1001	0.25***	0.07	0.16**	0.07	549
Employment	0.05	0.05	-0.02	0.05	3625	0.01	0.06	-0.03	0.08	1925
Wages	-0.02	0.05	0.08*	0.04	2771	-0.19	0.12	0.00	0.11	1507
Migration	0.04	0.06	0.10*	0.05	2125	0.14	0.11	-0.00	0.15	1133
Panel C: Residents										
Program awareness	0.01	0.05	-0.08	0.05	901	-0.05	0.06	-0.01	0.07	499
Interaction w/ block leader	0.13**	0.06	0.01	0.05	898	-0.05	0.10	-0.21*	0.12	496
Political participation	0.02	0.08	0.02	0.09	422	-0.32*	0.18	-0.24	0.16	229
Political support	0.06	0.18	-0.28	0.17	278	-0.22	0.29	-0.02	0.39	160
Panel D: District relatives										
Program awareness	0.06	0.05	0.06	0.04	2274	0.23***	0.08	0.01	0.09	1244
Political support	0.41*	0.25	0.20	0.21	221	-0.26	0.29	-0.08	0.35	124
Migration	0.05	0.05	0.04	0.05	832	0.02	0.07	0.09	0.10	453

Note. Estimates based on OLS regressions using equation 1. TL refers to leader treatment, TB refers to basic treatment (see section B of the main text). Standard errors are reported in columns (2), (4), (6) and (8) and clustered at the block level. All specifications include block and individual controls, and strata fixed effects. Section 4 presents the full list of controls. Outcomes are grouped in indices that are built using the procedure followed by Kling et al. (2007): outcomes are first normalized to study mean effect sizes of the indices relative to the standard deviation of the control group and then averaged within each category. *** p<0.01, ** p<0.05, * p<0.1.

Table I3: Heterogeneous effects: leader's age

	Leader is <50 years old					Leader is ≥50 years old				
	TL		TB			TL		TB		
Outcome variable	Coeff.	S.E.	Coeff.	S.E.	N	Coeff.	S.E.	Coeff.	S.E.	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Leaders										
Program awareness	0.26***	0.07	-0.02	0.08	475	0.28**	0.11	-0.04	0.12	314
Interaction w/ block people	0.15	0.10	0.04	0.12	460	0.30**	0.13	-0.12	0.16	300
Political participation	-0.10	0.16	-0.13	0.18	178	0.23	0.25	0.27	0.30	92
Political support	0.04	0.09	-0.02	0.10	186	0.21	0.14	0.12	0.23	100
Mobilization	0.33*	0.19	0.31	0.21	186	0.32**	0.15	0.21	0.21	100
Panel B: Migrants										
Program awareness	0.13***	0.04	0.09**	0.04	3215	0.19***	0.04	0.17***	0.05	2328
Interaction w/ block leader	0.03	0.03	-0.02	0.03	3203	0.07**	0.04	-0.01	0.05	2330
Political participation	0.01	0.04	-0.05	0.04	1604	0.18***	0.06	0.09	0.07	1162
Political support	-0.02	0.10	0.13	0.09	1068	0.10	0.07	-0.01	0.07	841
Job offers	0.33***	0.05	0.32***	0.05	941	0.14***	0.05	0.15**	0.06	616
Employment	0.07	0.04	0.02	0.05	3215	0.14**	0.06	0.08	0.08	2335
Wages	-0.16***	0.06	-0.00	0.05	2477	0.06	0.06	0.25**	0.10	1799
Migration	0.06	0.06	0.09	0.08	1909	-0.03	0.07	0.11	0.07	1350
Panel C: Residents										
Program awareness	0.04	0.05	-0.00	0.06	828	0.06	0.07	0.07	0.09	568
Interaction w/ block leader	0.04	0.05	-0.03	0.05	823	0.24**	0.09	-0.08	0.09	568
Political participation	-0.22**	0.09	-0.09	0.09	387	-0.13	0.11	-0.04	0.14	263
Political support	-0.23	0.22	-0.24	0.20	245	0.10	0.20	-0.32	0.21	186
Panel D: District relatives										
Program awareness	0.17***	0.05	0.09*	0.04	1984	0.09*	0.05	0.02	0.06	1536
Political support	0.41	0.28	-0.34	0.26	190	-0.03	0.24	-0.14	0.30	150
Migration	0.01	0.06	0.08	0.08	712	-0.03	0.04	-0.08	0.05	570

Note. Estimates based on OLS regressions using equation 1. TL refers to leader treatment, TB refers to basic treatment (see section B of the main text). Standard errors are reported in columns (2), (4), (6) and (8) and clustered at the block level. All specifications include block and individual controls, and strata fixed effects. Section 4 presents the full list of controls. Outcomes are grouped in indices that are built using the procedure followed by Kling et al. (2007): outcomes are first normalized to study mean effect sizes of the indices relative to the standard deviation of the control group and then averaged within each category. *** p<0.01, ** p<0.05, * p<0.1.

Table I4: Heterogeneous effects: leader is new vs. experienced

	New leaders (<2y experience)					Leader is experienced (≥2y experience)				
	TL		TB			TL		TB		
Outcome variable	Coeff.	S.E.	Coeff.	S.E.	N	Coeff.	S.E.	Coeff.	S.E.	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Leaders										
Program awareness	0.35**	0.14	-0.13	0.12	268	0.22***	0.07	-0.09	0.07	521
Interaction w/ block people	0.27	0.19	-0.04	0.17	254	0.18*	0.10	0.14	0.11	506
Political participation	-0.34	0.31	0.06	0.23	62	0.23	0.17	0.04	0.17	202
Political support	-0.00	0.16	-0.07	0.13	73	0.11	0.09	0.04	0.09	210
Mobilization	-0.09	0.31	0.25	0.43	73	0.24	0.16	0.18	0.14	210
Panel B: Migrants										
Program awareness	0.03	0.06	0.03	0.05	1907	0.17***	0.03	0.12***	0.03	3699
Interaction w/ block leader	-0.02	0.05	-0.05	0.04	1906	0.12***	0.03	0.06*	0.03	3690
Political participation	0.11*	0.07	0.09	0.06	924	0.06	0.04	0.04	0.04	1872
Political support	0.13	0.08	0.08	0.10	654	-0.02	0.09	0.15*	0.08	1263
Job offers	0.25**	0.10	0.15*	0.08	444	0.17***	0.04	0.19***	0.04	1133
Employment	0.08	0.06	0.04	0.06	1909	0.01	0.05	-0.10**	0.05	3704
Wages	-0.01	0.07	0.19***	0.07	1467	-0.10*	0.05	-0.00	0.06	2857
Migration	-0.12	0.08	-0.09	0.07	1128	0.05	0.05	0.08	0.05	2166
Panel C: Residents										
Program awareness	0.13	0.09	-0.00	0.07	482	0.08	0.05	-0.01	0.05	934
Interaction w/ block leader	0.09	0.09	-0.01	0.09	480	0.15**	0.06	-0.00	0.05	930
Political participation	0.04	0.16	0.19	0.14	205	-0.10	0.09	0.05	0.08	442
Political support	-0.17	0.27	-0.19	0.28	131	-0.07	0.21	-0.05	0.19	302
Panel D: District relatives										
Program awareness	0.10	0.08	0.16**	0.06	1151	0.09*	0.05	0.05	0.05	2414
Political support	0.42	0.64	-0.12	0.45	97	0.31	0.24	0.35	0.23	245
Migration	0.03	0.09	0.12	0.13	420	-0.04	0.05	-0.01	0.05	886

Note. Estimates based on OLS regressions using equation 1. TL refers to leader treatment, TB refers to basic treatment (see section B of the main text). Standard errors are reported in columns (2), (4), (6) and (8) and clustered at the block level. All specifications include block and individual controls, and strata fixed effects. Section 4 presents the full list of controls. Outcomes are grouped in indices that are built using the procedure followed by Kling et al. (2007): outcomes are first normalized to study mean effect sizes of the indices relative to the standard deviation of the control group and then averaged within each category. *** p<0.01, ** p<0.05, * p<0.1.

Table I5: Heterogeneous effects: Leader's support for RENAMO

	Leader is RENAMO declared supporter					Leader is not RENAMO declared supporter				
	TL		TB			TL		TB		
Outcome variable	Coeff.	S.E.	Coeff.	S.E.	N	Coeff.	S.E.	Coeff.	S.E.	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Leaders										
Program awareness	0.37***	0.07	0.01	0.07	564	0.31	0.21	-0.05	0.18	230
Interaction w/ block people	0.21**	0.10	0.07	0.10	552	0.19	0.27	0.25	0.30	210
Political participation	0.16	0.17	0.28*	0.17	219	0.32	0.31	0.21	0.20	46
Political support	0.14	0.10	0.04	0.09	236	-0.29**	0.14	-0.47**	0.17	52
Mobilization	0.41**	0.17	0.38**	0.17	236	-0.16	0.21	0.06	0.28	52
Panel B: Migrants										
Program awareness	0.16***	0.03	0.13***	0.03	3948	-0.13**	0.06	-0.05	0.05	1595
Interaction w/ block leader	0.13***	0.03	0.02	0.03	3945	-0.06	0.05	-0.00	0.04	1588
Political participation	0.13***	0.04	0.09**	0.04	1958	-0.19**	0.07	-0.12	0.09	807
Political support	0.03	0.05	0.05	0.06	1338	0.12	0.21	0.21	0.24	569
Job offers	0.21***	0.04	0.19***	0.04	1110	0.21***	0.07	0.31***	0.06	445
Employment	0.01	0.04	-0.03	0.04	3954	0.21*	0.12	0.12	0.11	1596
Wages	-0.05	0.05	0.06	0.05	3006	-0.00	0.08	0.12	0.11	1272
Migration	0.06	0.05	0.05	0.05	2312	0.09*	0.06	0.03	0.07	947
Panel C: Residents										
Program awareness	0.04	0.06	-0.03	0.04	993	0.17*	0.10	0.09	0.12	407
Interaction w/ block leader	0.13**	0.06	0.03	0.05	991	0.12	0.13	-0.01	0.13	404
Political participation	0.04	0.09	0.05	0.08	463	0.06	0.21	-0.02	0.27	184
Political support	-0.05	0.17	-0.00	0.17	320	-0.35	0.43	-0.28	0.51	128
Panel D: District relatives										
Program awareness	0.11**	0.05	0.09*	0.05	2444	0.22*	0.11	0.02	0.10	1075
Political support	-0.04	0.21	-0.22	0.21	251	1.44***	0.38	1.12***	0.36	95
Migration	-0.04	0.05	-0.06	0.05	891	-0.09	0.11	-0.07	0.11	392

Note. Estimates based on OLS regressions using equation 1. TL refers to leader treatment, TB refers to basic treatment (see section B of the main text). Standard errors are reported in columns (2), (4), (6) and (8) and clustered at the block level. All specifications include block and individual controls, and strata fixed effects. Section 4 presents the full list of controls. Outcomes are grouped in indices that are built using the procedure followed by Kling et al. (2007): outcomes are first normalized to study mean effect sizes of the indices relative to the standard deviation of the control group and then averaged within each category.

Table I6: Heterogeneous effects: Migrant density - median

	# migrants above median					# migrants below median				
	TL		TB			TL		TB		
Outcome variable	Coeff.	S.E.	Coeff.	S.E.	N	Coeff.	S.E.	Coeff.	S.E.	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Leaders										
Program awareness	0.24***	0.08	-0.08	0.08	440	0.25***	0.08	0.01	0.07	412
Interaction w/ block people	0.18**	0.09	0.04	0.10	427	0.13	0.12	0.03	0.12	392
Political participation	0.22	0.14	0.02	0.14	188	0.21	0.21	0.24	0.20	187
Political support	0.01	0.08	0.06	0.07	199	0.12	0.10	0.02	0.12	205
Mobilization	0.18	0.18	0.04	0.19	199	0.31**	0.14	0.31**	0.13	205
Panel B: Migrants										
Program awareness	0.19***	0.03	0.16***	0.03	3955	-0.01	0.04	-0.02	0.04	2136
Interaction w/ block leader	0.12***	0.03	0.04	0.03	3955	0.11***	0.03	0.00	0.03	2124
Political participation	0.12***	0.04	0.08**	0.04	1948	0.10**	0.05	0.12**	0.05	1079
Political support	-0.01	0.07	0.06	0.07	1347	-0.00	0.08	0.14*	0.07	735
Job offers	0.22***	0.03	0.21***	0.04	1132	0.14**	0.06	0.13**	0.06	580
Employment	0.02	0.04	-0.10**	0.04	3965	0.06	0.05	-0.04	0.05	2135
Wages	-0.04	0.07	0.03	0.06	3080	-0.02	0.06	0.00	0.05	1624
Migration	0.07	0.05	0.09*	0.05	2351	-0.04	0.06	0.01	0.06	1228
Panel C: Residents										
Program awareness	0.07	0.05	-0.07	0.06	793	-0.01	0.06	-0.04	0.05	777
Interaction w/ block leader	0.14**	0.06	0.03	0.06	788	0.13**	0.06	-0.03	0.05	775
Political participation	0.03	0.08	0.08	0.08	365	-0.12	0.09	-0.07	0.09	386
Political support	-0.27	0.17	0.04	0.18	248	-0.04	0.15	-0.14	0.16	270
Panel D: District relatives										
Program awareness	0.13***	0.04	0.06	0.04	2436	0.08	0.06	0.04	0.05	1432
Political support	0.15	0.20	-0.00	0.20	269	0.11	0.25	-0.17	0.23	142
Migration	0.08*	0.04	0.07	0.05	892	0.03	0.06	0.04	0.06	544

Note. Estimates based on OLS regressions using equation 1. TL refers to leader treatment, TB refers to basic treatment (see section B of the main text). Standard errors are reported in columns (2),(4),(6) and (8) and clustered at the block level. All specifications include block and individual controls, and strata fixed effects. Section 4 presents the full list of controls. Outcomes are grouped in indices that are built using the procedure followed by Kling et al. (2007): outcomes are first normalized to study mean effect sizes of the indices relative to the standard deviation of the control group and then averaged within each category.

J Robustness to selection of control variables

Table J1: Description of variables included in the PDSL procedure

Variable group	Description
Block characteristics	
	Stratum indicator variables. Indicator variables for whether there is illegal construction in the block. Categorical variable measuring the number of migrants sampled at baseline, the block population (approximate), the number of bicycle taxi drivers, the distance to nearest school, nearest market and nearest water fountain.
Leader characteristics	
Demographics	Indicator variables for sex, whether the respondent is married, illiterate, has less than primary education, has primary education, is catholic and is muslim. Categorical variables for age reported in number of years and household size.
Wealth	Indicator variable for whether the respondent owns their flat.
Political	Indicator variable for whether the leader is a member of RENAMO political party at baseline, voted for RENAMO in 2018 and 2019 elections, likes migrants and is employed. Categorical variable for number of years as the block leader.
Migrant characteristics	
Demographics	Indicator variables for sex, whether the respondent is married, illiterate, has less than primary education, has primary education, is catholic, is muslim, has no occupation at baseline, is a student at baseline, is working at baseline, moved to Quelimane for work. Categorical variables for age reported in number of years, number of children and household size.
Migration	Indicator variables for whether the main struggle at baseline was finding a job, main struggle at baseline was making friends, feeling a strong connection to Quelimane at baseline and feeling discriminated at baseline. Average of 4 indicator variables as an index for trust (trust in market seller, in the Mozambican president, in the provincial government and in the mayor of Quelimane).
Wealth	Indicator variables for whether the respondent rents their flat, owns their flat, has walls made of concrete, has floors made of concrete, has a roof made of zinc, owns a bank account, owns a radio, a television, a mattress, a fan, a motorcycle, a fridge, a phone, a bicycle and a solar panel. Average of 3 variables (walls, floor made of concrete, and roof made of zinc) as an indicator of house quality.

Note. Migrant characteristics are included only in the PDSL procedure for migrant-level outcomes. All continuous outcomes are also included in their squared term and are standardized. To have the same sample size in the Post-ModelSelection and PDSL, for all variables, missing values are replaced by 0 and an indicator variable equal to 1 if the observation was missing is included for all variables.

Table J2: Comparison with PDSL: leader outcomes

	Post-Model Selection				Post-Double Selection LASSO				
	TL		TB		TL		TB		
Outcome variable	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Heard about program	0.12	0.03	-0.04	0.04	0.13	0.03	-0.02	0.03	852
Involved in program	0.16	0.04	-0.01	0.04	0.18	0.03	0.01	0.03	852
Family was involved in program	-0.01	0.03	-0.01	0.02	-0.00	0.02	-0.01	0.02	852
Block people were involved in program	0.09	0.04	-0.09	0.04	0.11	0.03	-0.07	0.03	852
Rural migrants were involved in program	0.11	0.04	-0.03	0.04	0.12	0.03	-0.04	0.03	852
Knows migrants (general)	0.06	0.04	0.04	0.04	0.10	0.03	0.06	0.03	825
Knows sampled migrants	0.06	0.02	0.02	0.02	0.06	0.02	0.01	0.02	852
Inked finger	0.03	0.03	0.04	0.03	0.05	0.03	0.02	0.03	429

Note. Estimates based on OLS regression using equation 1. TL refers to leader treatment, TB refers to basic treatment (see section B of the main text). Standard errors are reported in columns (2),(4),(6) and (8) and clustered at the block level. In columns (1)-(4), the specifications are constant across outcome variables (see section 4). In columns (5)-(8), the specifications are outcome-specific and include individual and block-level controls, selected using the Post-Double Selection LASSO (PDSL) procedure (Belloni et al. (2013)). The full list of control variables included in the procedure are presented in table J1.

Table J3: Comparison with PDSL: migrant outcomes

	Post-Model Selection				Post-Double Selection LASSO				
	TL		TB		TL		TB		
Outcome variable	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Heard about program	0.07	0.02	0.08	0.02	0.07	0.01	0.08	0.01	6104
Involved in program	0.07	0.03	0.09	0.03	0.09	0.02	0.08	0.02	6102
Family was involved in program	0.02	0.02	0.01	0.02	0.02	0.01	0.01	0.01	6104
Block people were involved in program	0.03	0.03	0.02	0.03	0.04	0.02	0.03	0.01	6101
Rural migrants were involved in program	0.04	0.02	0.02	0.02	0.05	0.02	0.03	0.02	6096
Knows block leader	0.03	0.02	-0.03	0.02	0.03	0.02	-0.02	0.02	6105
Contacted block leader	0.04	0.01	0.00	0.01	0.04	0.01	0.01	0.01	6105
Resorted to leader for job	0.01	0.00	0.01	0.00	0.01	0.00	0.01	0.00	6079
Party objects	0.02	0.01	0.02	0.01	0.02	0.01	0.02	0.01	6103
Inked finger	0.04	0.02	0.02	0.02	0.02	0.02	0.02	0.02	3323
Voted RENAMO	-0.00	0.02	0.02	0.02	0.01	0.01	0.02	0.01	2088
Stayed in Quelimane	0.00	0.01	0.02	0.01	0.00	0.01	0.01	0.01	3580

Note. Estimates based on OLS regression using equation 1. TL refers to leader treatment, TB refers to basic treatment (see section B of the main text). Standard errors are reported in columns (2),(4),(6) and (8) and clustered at the block level. In columns (1)-(4), the specifications are constant across outcome variables (see section 4). In columns (5)-(8), the specifications are outcome-specific and include individual and block-level controls, selected using the Post-Double Selection LASSO (PDSL) procedure (Belloni et al. (2013)). The full list of control variables included in the procedure are presented in table J1.

Table J4: Comparison with PDSL: resident outcomes

	Post-Model Selection				Post-Double Selection LASSO				
	TL		TB		TL		TB		
Outcome variable	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Heard about program	-0.01	0.03	-0.05	0.03	-0.01	0.02	-0.05	0.02	1573
Involved in program	-0.01	0.03	-0.03	0.03	0.00	0.02	-0.04	0.02	1573
Family was involved in program	0.01	0.02	-0.00	0.02	0.01	0.02	-0.01	0.02	1572
Block people were involved in program	0.01	0.03	-0.00	0.03	0.01	0.02	-0.00	0.02	1571
Rural migrants were involved in program	0.03	0.03	0.01	0.03	0.03	0.02	0.01	0.02	1573
Knows block leader	0.04	0.03	-0.01	0.03	0.02	0.02	-0.02	0.02	1573
Contacted block leader	0.05	0.03	0.00	0.02	0.05	0.02	-0.00	0.02	1573
Resorted to leader for job	0.02	0.01	0.00	0.01	0.02	0.01	0.01	0.01	1565
Party objects	0.00	0.02	-0.01	0.02	0.00	0.01	-0.01	0.01	1570
Inked finger	-0.05	0.04	0.01	0.03	-0.07	0.03	0.03	0.03	810
Voted RENAMO	-0.05	0.04	-0.02	0.04	-0.06	0.03	-0.03	0.03	551

Note. Estimates based on OLS regression using equation 1. TL refers to leader treatment, TB refers to basic treatment (see section B of the main text). Standard errors are reported in columns (2),(4),(6) and (8) and clustered at the block level. In columns (1)-(4), the specifications are constant across outcome variables (see section 4). In columns (5)-(8), the specifications are outcome-specific and include individual and block-level controls, selected using the Post-Double Selection LASSO (PDSL) procedure (Belloni et al. (2013)). The full list of control variables included in the procedure are presented in table J1.

Table J5: Comparison with PDSL: origin contact outcomes

	Post-Model Selection				Post-Double Selection LASSO				
	TL		TB		TL		TB		
Outcome variable	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Heard about program	0.00	0.02	0.00	0.02	0.03	0.02	0.01	0.02	3868
Family was involved in program	0.00	0.02	0.02	0.02	0.02	0.02	0.02	0.02	3859
Rural migrants were involved in program	0.01	0.01	0.00	0.01	0.01	0.01	0.00	0.01	3858
Voted RENAMO	-0.04	0.04	-0.04	0.04	-0.05	0.02	-0.04	0.02	2096
Likely to move	0.02	0.02	0.02	0.01	0.04	0.01	0.03	0.01	3787
In Quelimane	-0.01	0.01	-0.01	0.01	0.00	0.01	-0.01	0.01	3015
In Quelimane (7 months post)	0.03	0.01	0.04	0.01	0.04	0.01	0.04	0.01	5032

Note. Estimates based on OLS regression using equation 1. TL refers to leader treatment, TB refers to basic treatment (see section B of the main text). Standard errors are reported in columns (2),(4),(6) and (8) and clustered at the block level. In columns (1)-(4), the specifications are constant across outcome variables (see section 4). In columns (5)-(8), the specifications are outcome-specific and include individual and block-level controls, selected using the Post-Double Selection LASSO (PDSL) procedure (Belloni et al. (2013)). The full list of control variables included in the procedure are presented in table J1.

Nova School of Business & Economics
Campus de Carcavelos
Rua da Holanda 1
2775-405 Carcavelos | Portugal
novasbe.pt

Accredited by
AACSB ACCREDITED
EFMD EQUIS
ASSOCIATION AMBA ACCREDITED
Member of
CEMS
gbsn Global Business School Network
PRME
P