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

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Abstract

Urbanization generates large economic gains, but it presents electoral risks for urban incumbents. We study the political feasibility of integrating rural migrants using a field experiment conducted before municipal elections in a rapidly growing city. We randomly vary access to an integration program for rural migrants, and vary its political salience by delivering it through local political leaders or administratively. The program increases rural-urban migration, but economic integration and electoral gains arise only with leader involvement, which increases political support for the incumbent without generating resident backlash. These findings show that urban integration can deliver both economic and political returns.

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1 Introduction

Urbanization generates large economic benefits, from higher productivity (Glaeser & Gottlieb, 2009; Bryan & Morten, 2019; Duranton & Puga, 2023) to improved market integration and labor market outcomes (Egger et al., 2024; Imbert & Ulyssea, 2026). Yet, governments often fail to facilitate the movement and integration of rural populations into cities, even in settings where the economic returns are highest. A key explanation lies in the political incentives surrounding urbanization (Bryan et al., 2020). Urban growth can impose costs on incumbent residents, creating democratic pressure to restrict new in-migration — even as potential migrants who would benefit lack a voice in the process (Feler & Henderson, 2011). Autocratic regimes may also oppose urbanization, worrying it will endanger them by facilitating collective action (Wallace, 2013). As a result, reallocating labor from agriculture to cities may face political barriers despite being economically efficient. Evidence on how such incentives shape urban policy and electoral outcomes remains limited in the literature.

This paper studies the political feasibility of urban integration policies. For urban incumbents, integrating rural migrants poses a potential trade-off between expanding the electorate and risking political backlash from residents. We ask how facilitating integration alters political behavior in the city. To do so, we use a field experiment embedded in a city-level integration program implemented in the run-up to municipal elections. Exploiting within-city randomized variation in access to and administration of the program through city government intermediaries, we demonstrate that urbanization is not merely an economic process, but can also serve as a political strategy.

We study this question in Mozambique, a setting that provides unusually sharp insight into the political economy of urban integration. Mozambique combines large potential gains from rural–urban reallocation with institutional features that make the political incentives of internal migration salient. National power remains anchored in rural constituencies, while a small number of opposition-governed municipalities face rapid urban growth and competitive local elections. As a result, rural-to-urban migration is often viewed as a political threat at the national level but as a potential electoral opportunity for urban incumbents. This divergence in political incentives is common across many cities in Sub-Saharan Africa and creates a setting in which integration policies may be economically efficient and advantageous to

opposition parties, but politically contested by national ruling parties.[1] Mozambique thus offers a unique context to examine how urban governments can implement integration policies without electoral backlash.

We designed and implemented the field experiment in collaboration with the municipality of Quelimane, a mid-sized and rapidly growing regional capital governed by an opposition party. In the run-up to the 2023 municipal elections, an integration policy for recent rural migrants was launched. The program consisted of repeated face-to-face coaching visits over the course of one year, offering job-matching services, mobile money training, and information about the city, its public services, and local voting.

The experiment randomly varied access to the program, as well as who administered it. The city is divided into roughly 500 basic administrative units we call "blocks," each with an assigned leader. These leaders act as intermediaries between residents and politicians by leveraging local knowledge and social ties (Balán et al., 2022). We randomly assigned blocks into three groups. In the first treatment group, block leaders were involved in administering the program. In principle, there are two main mechanisms through which leaders' involvement might affect the operation of the program: reducing informational frictions irrespective of beneficiaries' politics, or mediating access to economic opportunities in return for political support. Both potential channels are particularly relevant in urban developing-country settings, where rapid population growth makes it difficult for low-capacity governments to reach residents. In the second treatment group, the program was administered without leaders' involvement. Finally, in the control group, the intervention was not offered at all.

To capture the economic and political effects of treatments, we combine experimental variation with comprehensive outcome measurement among leaders, recent migrants, existing city residents, and migrants' origin-area contacts. We interpret the intent-to-treat effects as policy-relevant estimates, recognizing the inevitable information spillovers present in dense urban environments. Multiple rounds of survey data captured program awareness, interactions among actors, political preferences, and economic outcomes, including migration responses. Surveys were supplemented with behavioral measures to capture politically salient

[1] Urbanization in Sub-Saharan Africa remains lower than expected relative to other regions (United Nations, 2024), despite large urban–rural wage gaps (Young, 2013; Gollin et al., 2014; Hamory et al., 2021), substantial human-capital gains from migration (Alesina et al., 2021; Van Maarseveen, 2025), and rising climate pressures that push households toward urban relocation (Henderson et al., 2017; Burzyński et al., 2022).

actions, including partisan mobilization and participation in the elections.

We show that the program reinforces the process of urbanization and influences the economic integration of migrants. Although the retention of recent migrants is already high where the program was not offered, both treatment variants induce substantial additional rural–urban migration from the migrants' origin areas. These migration responses arise even without leader involvement, indicating that information and visibility alone are sufficient to stimulate further mobility through social networks. By contrast, economic integration within the city depends on the involvement of leaders. While both treatments increase migrants' exposure to job opportunities and mobile-money use, only the involvement of leaders translates into higher employment. Leaders thus play a central role in converting information into realized economic outcomes.

The program generates political effects, especially when leaders participate in program delivery. Leaders report knowing more migrants; migrants report being more likely to know the leader. Migrants and residents contact leaders more frequently, and approach them for job assistance. These changes do not arise without leader involvement, despite the identical program content. In addition, leader involvement increases mobilization in support of the incumbent and verified voter turnout among migrants, without reducing participation among residents. These findings suggest that involving local leaders makes the delivery of the program more politically feasible for the opposition-led city government. Importantly, we provide evidence supporting a coordination mechanism for these effects, in which leaders translate program access into economic integration and political mobilization by providing information and opportunities, not by distributing targeted favors. These effects extend beyond the city: migrants' origin contacts exhibit shifts in reported vote choice against the national ruling party, consistent with information spillovers.

Our results show how urban integration policies can be used as a political instrument by local governments. We contribute to the literature on local governance, which shows that political competition and decentralization can improve governance and service delivery (Dal Bó et al., 2021; Akhtari et al., 2022; Callen et al., 2023; Dahis & Szerman, 2025), highlighting the central role of local leaders in building state capacity and shaping political accountability (Acemoglu et al., 2014; Banerjee et al., 2019; Armand et al., 2020; Balán et al., 2022; Henn,

2023; Manara & Regan, 2025). We show that leaders not only enhance program effectiveness but also generate electoral returns. This result matters for our understanding of the political foundations of urbanization because urban incumbents can expand their political base by facilitating integration in ways that are economically productive and politically viable, rather than brokering votes among migrants through clientelism (Anderson et al., 2015; Bobonis et al., 2022) or vote buying (Larreguy et al., 2016; Shenoy & Zimmermann, 2026; Duarte et al., 2025).

We further contribute to the literature on internal migration and urbanization by shifting attention from migration decisions at the individual level to the institutional conditions that shape migrant integration and mobility. Existing evidence on the drivers of internal migration has focused almost exclusively on economic mechanisms in both low- and middle-income countries (Lagakos, 2020; Henderson & Turner, 2020) and high-income countries (Jia et al., 2023).[2] While existing work on policies that facilitate the integration of rural-to-urban migrants has focused primarily on property rights and tenure security (Field, 2007; Galiani & Schargrodsky, 2010; Marx et al., 2013), we provide one of the first studies to examine a city-level policy explicitly designed to integrate recent rural migrants. Beyond the positive economic effects of such a policy, we show that promoting integration in the city is crucial for internal migration, as it can induce additional migration responses through network spillovers. Moreover, we show how political frictions can limit rural-to-urban migration, as internal migration affects political behavior not only in destination cities but also in migrants' origin locations, contributing to a growing literature on how internal migration reshapes political equilibria across space (Glaeser & Steinberg, 2017; Kramon et al., 2022; Bazzi et al., 2020, 2023).[3]

The remainder of the paper is organized as follows. Section 2 describes the context of our study in Mozambique. Section 3 describes the experimental design and the data instruments. Section 6 discusses the results, and Section 7 provides concluding remarks.

[2]Rural–urban mobility depends on migration costs (Bryan et al., 2014; Bryan & Morten, 2019; Lagakos et al., 2023), infrastructure investments (Morten & Oliveira, 2024), asset constraints (Ardington et al., 2009; Banerjee et al., 2021), and information frictions (Bazzi et al., 2021; Baseler, 2023; McKenzie, 2024).

[3]A related literature studies the political effects of international migration (e.g., Docquier et al., 2016; Dustmann et al., 2019; Karadja & Prawitz, 2019), which differ from those of internal migration because they operate through cross-border regimes rather than sub-national incentives within a common institutional framework.

2 Background

Urbanization in low-income countries presents a paradox: the economic returns to reallocating labor from agriculture to cities are large, but urbanization often proceeds slowly. Mozambique exemplifies this pattern. It is among the poorest countries in the world, ranking fifth from the bottom in GDP per capita (USD 1,566; World Bank, 2024). Nearly 70% of the population is employed in agriculture, where productivity levels are very low, implying large potential returns to reallocating labor toward cities. Yet, urbanization has progressed slowly. The share of the population living in urban areas increased from 29% in 2000 to 37% in 2020, remaining well below both the Sub-Saharan African average of 47% and the global average of 56% (UN-Habitat, 2024).

This pattern reflects not only economic constraints but also political incentives arising from Mozambique's combination of strong central political control and highly decentralized local governance (Alexander, 1997). Since independence in 1975, Mozambique has been governed at the national level by a dominant ruling party, FRELIMO (Frente de Libertação de Moçambique). Political authority has historically been anchored in rural areas, where control has been exercised through appointed local leaders and strong central oversight. Although multiparty elections were introduced in 1994 and market-oriented reforms followed, the ruling party has largely maintained a development strategy centered on rural constituencies, with limited emphasis on urban expansion. At the same time, decentralization reforms have granted municipalities autonomy over service delivery, local administration, and political mobilization, particularly in opposition-governed cities. A salient example is land policy: while land remains state-owned, access is mediated by local authorities, creating a political lever over rural populations (e.g., World Bank, 1997). In this institutional environment, rural-to-urban migration can weaken national-level control rooted in rural constituencies while simultaneously expanding the electorate of urban incumbents. As a result, internal migration is often perceived as a threat at the national level but a potential opportunity at the municipal level.

Political incentives differ in urban areas. Municipal elections are held only in cities, and it is primarily there that opposition parties have gained electoral ground and govern a limited number of municipalities. For city governments facing electoral competition, migrant inflows

can therefore represent a political opportunity by expanding the urban electorate. This divergence between national and local political incentives links urbanization directly to political competition.[4]

This institutional configuration is not unique to Mozambique. As documented by Resnick (2014), decentralization across Sub-Saharan Africa has transferred responsibility for urban governance to municipal authorities, while political competition has led opposition parties to govern a growing number of cities under conditions of vertically divided authority. Similar tensions between the economic returns of urbanization and the political incentives of incumbents arise in many low- and middle-income countries where political authority is centralized and urban governance is electorally contested. In these settings, national governments often have limited incentives to support urban policy initiatives that could strengthen local incumbents, rendering urban service delivery and urban expansion highly politicized.

Quelimane is one such city. It has been governed by the opposition since 2011 and is led by the current mayor, Manuel de Araújo, of RENAMO (Resistência Nacional Moçambicana), FRELIMO's main and long-standing electoral challenger. Like many mid-sized African cities, Quelimane has experienced rapid growth driven largely by rural in-migration. As the capital of Zambézia province, it attracts migrants from surrounding rural areas as well as from other parts of the country. Its population more than doubled after 2010, surpassing 500,000 today and making it the seventh largest city in Mozambique (United Nations, 2024).

Quelimane is governed by a municipal council consisting of one elected president and chief executive ("mayor"), as well as 8 councilors. Administratively, the city is divided into five administrative posts, which are subdivided into 54 neighborhoods. At the lowest tier of municipal governance, neighborhoods are divided into small administrative units (540 blocks), each headed by a block leader who mediates interactions between residents and the municipality, including conflict resolution, information transmission, and the local implementation of public projects.[5] In this role, block leaders function as grassroots political leaders, connecting households to public resources and acting as intermediaries between citizens and

[4]Recent national elections have also been followed by violent unrest concentrated in urban areas (e.g., The Economist, 2024), underscoring the political salience of cities in Mozambique's political landscape.

[5]A block contains, on average, 145 households. This proxy is based on the city-wide census of job offers (Section 3), during which enumerators attempted to visit every house in the city. Considering that Mozambique's urban households average approximately 4.2 persons per household (UN-Habitat, 2023), each leader is responsible, on average, for roughly 600 individuals.

elected officials. Although block leaders do not receive formal wages and are not formally partisan actors, their appointment through the municipal hierarchy closely links their incentives to those of the incumbent local government. Many leaders have strong political ambitions and engagement, as nearly half aspire to political positions in municipal government, two-thirds participate in political rallies and campaigns, and close to one-third report assisting citizens with voter registration or vote choice (Appendix B). As a result, policies affecting urban residents are shaped not only by national priorities or mayoral strategies, but also by the incentives of grassroots political intermediaries. This institutional structure makes block leaders a natural channel through which policies affecting migrant integration can acquire political salience.

The study was implemented during the final half of the mandate of Mayor de Araújo, in 2022–23, with key outcomes measured in close temporal proximity to the October 2023 municipal elections. De Araújo had already governed the city for more than a decade, having first been elected mayor in a 2011 by-election and subsequently re-elected in the municipal elections of 2013 and 2018. During the 2018 elections, RENAMO obtained 59% of the votes, compared to 36% for FRELIMO, with a turnout of 65% (Comissão Nacional de Eleições, 2013, 2018). This history of repeated electoral contests underscores the degree of political competition in Quelimane and the salience of municipal elections in shaping local political incentives. The timing of the intervention, therefore, places it in a period of heightened political competition, when local incumbents and their intermediaries face strong incentives to mobilize support and signal responsiveness to voters. The 2023 municipal elections were ultimately won by the incumbent mayor following a contested post-electoral period that concluded with a Supreme Court ruling in favor of RENAMO's allegations of electoral irregularities against FRELIMO.

3 The experiment

3.1 The urban integration program

The intervention is a migrant integration program for recent rural migrants. The identification of this target group and of program participants is described in Section 3.3. The program was

sponsored by the municipality of Quelimane and implemented under the name *Quelimane trabalha com todos* ("Quelimane works with everybody").

The objective of the program was to ease the economic and psychological barriers that rural migrants face upon arrival, facilitating their integration into urban life. The program consisted of individualized coaching delivered through up to five in-home visits conducted by the implementing team between August 2022 and July 2023. On average, each visit lasted approximately one hour. Appendix A provides operational details of the program, including scripts and graphical examples of the materials used.[6] Section 6 provides details about the actual participation in the program.

The content of the coaching focused on three main components combining economic, financial, and institutional support. The design of the program was informed by both baseline evidence of constraints among migrants and recent evidence from the literature studying the economic and financial inclusion of vulnerable populations in urban areas. In our setting, half of recent migrants report moving primarily for work, with family and education as secondary motives. For one third of them, the main challenge they encounter in the city is finding work, followed by financial constraints, while social and housing difficulties are less binding. Concerning supporting evidence from the literature, see, for instance, McKenzie (2017), for a review on labor-market policies in low- and middle-income countries, and Suri & Jack (2016) and Batista & Vicente (2025) for the role of mobile money in facilitating occupational change and migration.

The first and core component being job matching, reflecting that most rural migrants move to the city in search of employment. Program staff provided migrants with tailored information on local job opportunities, including the names and phone numbers of potential employers, which were compiled through two city-wide censuses of job opportunities covering households and establishments, followed by three rounds of phone-based updates with previously identified employers. In total, 1,582 job offers were identified during the project, with their geographical distribution presented in Appendix Figure A2. Job offers were matched to migrants based on elicited skills and preferences, with each participant eligible to receive

[6]When possible, visits were scheduled in advance using the phone numbers provided by migrants at the time of listing. When this was not feasible, the implementing team visited participants' dwellings while covering the area. If migrants were unavailable at the time of a visit, at least one follow-up visit was actively arranged. Nevertheless, it was not possible to reach every participant in each round, and a share of listed migrants was never enrolled in the program (see Section 3.3).

up to ten job leads. Job opportunities matched with program participants (Appendix Table A1) were concentrated in the construction and housekeeping sectors and were predominantly informal and low-skilled. After each visit, migrants received a text message summarizing the employer contacts shared, and during the fourth visit, they were also shown a short testimonial video featuring a former participant who successfully integrated into the city through employment found with program support. In the last two visits, implementers further facilitated contact by calling potential employers during the visit and arranging interview dates.

The second component of the program focused on financial inclusion through mobile money. This module aimed to promote account adoption among unbanked migrants and facilitate the initial use of transfers, which could also foster information sharing between migrants and their origin-area networks. During the visits, implementers introduced migrants to Mozambique's leading mobile money service (M-PESA), providing practical information on account opening, cash-in and cash-out procedures, and transfers. To incentivize the use of and practice with mobile banking, during the third visit, participants received an endowment of 20 Meticais ($\approx$ 0.30 US$ or 23% of a daily wage in our target population) that they were encouraged to deposit and transfer to a contact in their district of origin.

The third component provided institutional information about city life. In the first two visits, migrants received a presentation developed by the municipality that covered administrative divisions, documentation required for residence, access to public services such as schooling and healthcare, and the local political context, including voter registration and the electoral process ahead of the 2023 municipal elections. From the third visit onward, this content was incorporated into a survey platform, allowing for an interactive format centered on reinforcing understanding through questions.

3.2 Program participants

Identifying and contacting recent rural migrants in a growing city in a low-income country is intrinsically challenging. The city lacks an up-to-date population census, administrative records do not systematically track recent arrivals, and the migrant population is highly mobile, with frequent changes in residence and household composition.

We proceeded in two steps. First, we defined the target population of the program as

individuals who had settled in the city in the last 12 months and who reported an intention to remain for at least one additional year. Second, using this rule, we created a listing of rural migrants in the city. Within each block, the enumerators started from a randomly chosen point and followed a deterministic algorithm that dictated the order in which households were approached to assess whether they included any recent migrants. This process continued until all houses had been visited or until at least eight migrant households had been identified. Appendix Figure B1 displays the distribution of the number of migrants found per block.

The listing of migrants was conducted between October and December 2021. Following the same procedure, in September 2022, a second listing was performed to identify three additional migrant households in each block.[7] In total, we identified 2,320 migrants in the first wave and an additional 1,312 migrants in the second wave, totaling 3,632 migrants. The listing is a representative sample of recent rural migrants in the city and served as the basis for our baseline survey (see Section 4). The vast majority of migrants moved to the city from the rural areas in the same province, while only a small percentage migrated from other provinces within Mozambique (Appendix D.5).

3.3 The experimental variation

The listing exercise described in Section 3.2 establishes a representative sample of rural migrants to whom the program could have been offered. Using the universe of city blocks—the smallest administrative division of the city—in which at least one recent rural migrant was identified (493 blocks), we designed a randomized experiment to study both access to the program and its political salience.[8] Blocks were randomly allocated into three comparison groups that determined how the program was offered to listed migrants.

In the first group of blocks, the *leader treatment* group, the program was offered with the explicit support and active participation of the block leaders corresponding to the blocks where migrant participants resided. The engagement of the leader was stimulated through

[7]The number of migrant households listed per block is capped at 11. At program start (August 2023), first-wave migrants had been in the city for one to two years, while second-wave migrants had arrived within the previous 12 months. Second-wave participants joined after implementation began and received only four visits. All analyses control for these two sub-samples.

[8]From the total number of blocks (540), we excluded 47 blocks where no migrants were found during the listing process (see Section 3.2).

two distinct channels. First, during each round of visits, the field team initiated face-to-face interactions by showing a short video on a tablet featuring a message recorded by the relevant block leader. In these videos, leaders expressed clear support for the program and encouraged migrants to follow the instructions and advice provided by the program implementers. In the fourth round, leaders were additionally asked to emphasize the importance of participating in elections in the video messages shown to migrants. The content and framing of these messages were left to the leaders' discretion, and most delivered political messages related to the upcoming 2023 municipal elections. At the end of each visit, implementers reminded migrants of the leader's name and contact information so they could reach out if needed. The field team also sent a follow-up text message with the leader's name and contact details after the visit. Second, block leaders were encouraged to be physically present during the face-to-face visits with migrants. In practice, their presence was systematic only during the fifth visit—the closest to the elections—when a large majority (77%) participated in house visits within their respective blocks alongside the field team. Throughout the visits, implementers explicitly framed the program as being carried out under the authority of the corresponding block leader, reinforcing the attribution of responsibility and political salience. Section 4 provides descriptive characteristics of block leaders. Appendix A.3 provides further details about the participation of block leaders in the program.

In the second group of blocks, the *basic treatment* group, the program was offered without the support and participation of the block leader. In these blocks, the implementing team provided the same coaching content and visit schedule, but no leader-related engagement was included. As a result, for this group, the program was delivered in a low–political-salience benchmark against which the leader treatment can be compared.

In the third group of blocks, the *control* group, the program was not offered. While randomization at the block level and the restriction of program activities to treated blocks were designed to limit contamination, some degree of information diffusion across blocks is expected in practice, given social interactions among migrants and spatial proximity within the city. Rather than viewing such diffusion as a design limitation, we interpret the resulting estimates as policy-relevant treatment effects that incorporate realistic patterns of informal information sharing. We discuss the extent of spillovers and their implications for interpreta-

tion in more detail in Section 5.

Randomization was stratified within strata of up to three blocks. These strata were created by sorting blocks within neighborhoods by the number of migrants listed (see Section 3.2). 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 B1.

Using administrative data from the implementation of the program, Appendix A.4 shows descriptive statistics about the take-up of the program and the intensity of the intervention. Among the listed migrants, the take-up of the program was 77% in both the leader and basic treatment groups. Migrants who took up the program in the leader and basic treatment groups participated, on average, in 3.31 and 3.43 visits out of the 5 planned visits, respectively. Importantly, none of the migrants originally listed in the control group blocks took up the program or received any visits. In both the leader and basic treatment groups, the share of migrants who participated in a visit ranges from 28% of the corresponding treatment group in the first round of visits to 72% in the last round and was never below 50% in all other rounds. Appendix A.4 shows that the take-up is higher among migrants without an occupation and students, and lower among migrants with primary education, while demographic characteristics and religious affiliation are not predictive of participation. Leader characteristics are unrelated to take-up, indicating that leader involvement does not affect selection into participation.

4 Data

Understanding both the direct and indirect effects of the program requires collecting information from multiple actors involved in or affected by the integration of rural migrants into urban life. While rural migrants are the primary targets of the intervention, the program operates through local political authorities, interacts with incumbent city residents, and may generate informational and political spillovers toward migrants' origin communities. To capture these channels, we collected data not only from migrants but also from block leaders, other city residents, and migrants' contacts in their districts of origin, combining a set of surveys and behavioral activities that we designed and implemented for this purpose. All

surveys collected information on demographic and socioeconomic characteristics, as well as civic and political attitudes. Behavioral activities were used to measure political behavior directly, thus reducing reliance on self-reported outcomes. Appendix A provides implementation details, and Appendix C presents a comprehensive description of all outcome variables used in the analysis.

The sample of migrants is described in Section 3.2, while its geographical distribution within the city is provided in Appendix B. The sample of block leaders includes all leaders of the 493 blocks participating in the experiment.[9] To measure outcomes among incumbent city residents, we sampled up to two households per block that had resided in the city for more than two years, yielding a total of 995 residents. These households were selected during the first migrant listing wave and followed the same random selection procedure. For the sample of migrants' contacts in their districts of origin, at the time of the first survey, migrants were asked to report the name and contact information of the adult person in their district of origin to whom they felt closest and with whom they were still in phone contact. During the second survey, we asked for an additional contact, the next closest person, using the same phrasing, thus expanding the sample of contacts. In total, we collected 2,519 such contacts, 95% of whom are family members. Among contacts, there is a high prevalence of family ties, suggesting that the sample of origin contacts proxies for migrants' origin households. Due to the requirement of phone contact, not all migrants reported a contact. Appendix D.5 provides additional information on the share of migrants that reported a contact, discusses the determinants of reporting origin contact, and provides robustness checks for outcomes related to origin contacts.

Survey data were collected at three main points in time (see Appendix Figure A1 for the exact timeline): baseline (prior to the start of the program), midline (toward the end of the intervention, before the final visit), and endline (after program completion and before or after the October 2023 municipal elections, depending on the actor surveyed). Migrants and city residents were interviewed in person at baseline (in conjunction with the listing exercise), midline, and endline (in November 2023, right after the elections). Block leaders were interviewed in person at baseline (immediately after the listing phase), midline, and

[9] Approximately 10% of the blocks changed leaders between survey waves. All activities involved leaders in office. In all estimates, we use the information for the leader in office at the time of the survey.

endline (in August 2023, right before the municipal elections).[10] Migrants' origin contacts were interviewed by phone at midline and endline (in December 2023, after the elections). In addition, to capture rural-to-urban migration in the medium run, we conducted a follow-up phone survey among migrants' origin contacts approximately one year after the end of the intervention. Appendix B reports tests for differential attrition across survey waves and actor types; we find no significant differences across treatment arms.

In addition to surveys, we implemented behavioral measures of political behavior in close temporal proximity to the 2023 municipal elections. These measures are based on observed behavior and on Structured Community Activities (SCAs; see, e.g., Casey et al., 2012), capturing political mobilization capacity and effort among leaders, actual mobilization among migrants and residents, leaders' propensity for particularistic allocation, and participation in the 2023 municipal elections. Behavioral measures substantially reduce the biases associated with standard survey questions on political behavior (see, e.g., Aker et al., 2017; Grácio & Vicente, 2021). We describe each measure in detail in Section 6, when presenting the corresponding results.

Descriptive statistics at baseline for all actors are reported in Appendix B. Blocks contain, on average, 4.8 sampled migrants. Migrants are young (mean age 24) and predominantly male, with low family attachment and low socioeconomic status: only 37% are married or cohabiting, they have just over one child on average, 34% are illiterate, and 31% report having no occupation. Half of them moved to Quelimane in search of work, and their most frequently cited difficulty is finding a job, while engagement with local authorities remains limited. Migrants' origin contacts exhibit a similar profile but are older, more likely to be married or cohabiting, have more children, and have low educational attainment.

Block leaders are substantially older than migrants (mean age 50), predominantly male, and socially more established, as 72% are married or cohabiting, 95% own their dwelling, and they have held office for an average of 3.6 years, but education levels are low. Incumbent city residents lie between migrants and leaders in age and socioeconomic status. Their average age is 34 years, 40% are male, and 46% are married or cohabiting. Illiteracy is lower

[10] Due to operational limits, the baseline survey for migrants listed in the second wave of listings includes fewer sections compared to the one completed by migrants listed in the first wave (see Appendix C). We conducted the leader survey before the elections to capture campaign mobilization at a moment of maximum political salience, before electoral outcomes could affect leaders' behavior or reporting.

than among migrants, though only 36% have completed primary schooling. Compared to migrants, residents are more engaged with local authorities, with 40% reporting contact with their block leader in the previous year.

5 Estimation strategy

The analysis closely follows the pre-analysis plan (Armand et al., 2024). Appendix B assesses the baseline balance across treatment arms and the control group for all actors in the study. Across 180 balance tests, including comparisons of each treatment arm to the control separately and joint tests of equality across all treatment assignments, we find no more statistically significant differences than would be expected by chance at conventional significance levels (approximately 10%). This pattern indicates that the treatment and control groups are observationally equivalent at baseline, providing reassurance that block-level randomization successfully generated comparable groups.

Let $Y_{ibs,t}$ be the outcome variable of interest for individual i (migrant, leader, resident, or migrant's contact) living in block b, which is part of the randomization stratum s, and is measured at time t, where $t = 0$ indicates the baseline, $t = 1$ the midline, and $t = 2$ the endline. Using post-intervention observations ($t > 0$), we estimate treatment effects by employing the following OLS specification:

$$Y_{ibs,t} = \alpha + \beta_L TL_b + \beta_B TB_b + \delta_t + \lambda_s + \omega \mathbf{Z}_b + \gamma \mathbf{X}_{ibs,0} + \varepsilon_{ibs,t} \quad (1)$$

where TL_b and TB_b are indicator variables for living in a block in the leader or the basic treatment, respectively, λ_s and δ_t are strata and survey-wave fixed effects, $\boldsymbol{Z}_{bs}$ and $\boldsymbol{X}_{ibs}$ are a set of block and individual control variables.[11] Appendix D.9 shows the robustness of our results to alternative selections of controls using the Post–Double Selection Lasso (PDSL)

[11] Block-level controls include a proxy for block population. Individual-level controls include age, gender, primary schooling, religion indicators (Muslim and other religions, with Catholic as the omitted category), and, when available, the baseline value of the dependent variable ($Y_{ibs,0}$), thus estimating an ANCOVA specification.For migrants, we additionally control for baseline occupational status and listing wave (see Section 3.2). Missing values in control variables are imputed using the block-level median at the time of the survey, with indicators for imputed values included as controls. Because neighborhoods contain unequal numbers of blocks, the randomization procedure (see Section 3.3) generated some singleton strata. To retain these observations, we reassign singleton strata to the immediately preceding stratum in the randomization ordering; if a neighborhood contains only one block, reassignment follows the same ordering in the preceding neighborhood.

procedure of Belloni et al. (2013). Finally, the error term, ε_{ibs}, is assumed to be clustered at the block level.

For outcomes measured in both post-baseline surveys, we exploit the repeated observations over time by estimating treatment effects using a multiple-period specification. This approach increases statistical power by pooling information across survey rounds and improves precision by accounting for within-unit correlation over time (McKenzie, 2012). In each table's footer, we indicate which round of surveying is used in the estimation, reporting it as *pooled* when the data are stacked.

In terms of the interpretation of β_L and β_B, a salient feature to consider is the potential presence of information spillovers across treatment units, i.e., blocks. In dense urban environments, social networks are highly interconnected, and information about valuable or scarce opportunities can diffuse rapidly through word-of-mouth and social reciprocity (see, e.g., Banerjee et al., 2019; Beaman & Dillon, 2021). This possibility implies that treatment exposure is unlikely to be geographically sealed. We therefore interpret our estimates as policy-relevant intention-to-treat effects, capturing the consequences of alternative program implementation regimes rather than as individual-level treatment effects while holding others' exposure fixed (see, e.g., Manski, 2013). Given the setting and the experimental design, some degree of information diffusion is expected and is best viewed as an integral feature of the policy environment. Evidence rules out spatial contamination as an alternative explanation for the estimated treatment effects of equation (1) and supports the interpretation that information diffusion operates through social networks rather than through local exposure to treatments. Appendix D.10 provides an empirical test for spatial contamination by exploiting available data on the exact geo-location of each migrant's dwelling and estimating versions of equation (1) controlling for the shares of migrants in the geographical proximity of a respondent that are allocated to different treatment groups. To the extent that spillovers attenuate differences between treated and control blocks, our estimates should be understood as conservative lower bounds on the direct effects of treatment assignment. We further discuss this issue in Section 6. Finally, Appendix D.7 complements intention-to-treat estimates presented in the main text with treatment-on-the-treated estimates using the share of visits attended by a migrant as a measure of treatment intensity.

In Section 6, we evaluate treatment effects across multiple related outcome variables, which raises concerns about inflated Type I error due to multiple hypothesis testing. To address this issue, we supplement conventional significance indicators with adjusted p-values based on the procedure of Romano & Wolf (2016). We apply this correction at the actor-by-treatment level within each table of outcomes, accounting for the joint dependence of test statistics across related measures. The procedure tests the joint null hypothesis that a given treatment arm has no effect on any of the outcomes presented in a table for a specific actor and provides adjusted p-values for individual outcomes that control the family-wise error rate. Relative to standard multiple-testing adjustments, this approach improves statistical power while maintaining rigorous control of false rejections.

6 Results

Section 3.3 highlights that the implementation of the experiment has generated differential exposure to the program. Administrative records capture the intensity of program delivery but do not measure its perceived salience or diffusion beyond directly visited households. Because political and economic effects may operate through awareness, visibility, and social interactions rather than direct participation alone, we complement administrative data with self-reported measures of program awareness, perceived targeting, and involvement among different actors. Figure 1 reports treatment effects on these outcomes, estimated using equation 1 and pooling midline and endline observations. Appendix A.4 reports the corresponding regression results.

The leader treatment was particularly effective in inducing engagement among block leaders (panel A). Leaders in treated blocks are significantly more likely to be aware of the program and report that it involves rural migrants, both effects amounting to 13 percentage points. They are also 17-percentage-points more likely to report being involved in the program. In contrast, the effects of the basic treatment are never statistically distinguishable from those of the control group. Among migrants (panel B), both treatments significantly increase program awareness by 7–8 percentage points and knowledge of the targeted group by 4–5 percentage points. A comparable pattern emerges for migrants' involvement in the program, with an effect of 7 percentage points for both treatments. For all outcomes, the ef-

fects are statistically indistinguishable across treatments. Panels C and D show more limited effects among city residents and migrants' origin contacts, but patterns consistently point to a stronger reach under the leader treatment. In particular, the leader treatment increases residents' knowledge about the targeted group by 4 percentage points and raises the probability that migrants' origin contacts are aware of the program by 3 percentage points.

These results show that treatment assignment translated into meaningful differences in program reach and that leader involvement substantially amplified program salience beyond that of direct beneficiaries. At the same time, Appendix A.4 shows that awareness among leaders, migrants, and residents in control blocks is nonetheless high (72%, 70%, and 59%, respectively), while it is lower among the origin contacts of control migrants (22%).

At the same time, it is important to note that much of the diffusion of information occurs around election-time. If we estimate effects considering only pre-election observations, we observe much larger effects and smaller sample means in the control group. Growing awareness over time in control blocks highlights the role of information diffusion in dense urban settings, especially during politically-salient periods. As discussed in Section 5, this finding reinforces the interpretation of the results as policy-relevant effects under realistic implementation conditions.

In Sections 6.2–6.3, we consider the effects on two broad sets of outcomes: economic and political. On the economic dimension, we expect improved labor-market outcomes driven by enhanced access to job opportunities, including higher employment rates and increased work hours. We also anticipate increased use of mobile money, particularly for transfers to households in migrants' districts of origin, and potential improvements in attitudes toward migrants among city residents. Finally, we hypothesize effects on migration dynamics, including higher retention of migrants in the city and increased migration of relatives from origin areas.

On the political dimension, we expect both treatment variants to increase interaction with local leaders. Migrants may become more politically engaged, participate more in campaigning and elections, and increase contact with block leaders. In the leader treatment, block leaders may also leverage the program to mobilize migrants politically in favor of the local incumbent, potentially through clientelistic channels. As a result, migrants may express

greater support for the incumbent. Given migrants' ongoing ties to their origin communities, these political effects may also diffuse to migrants' contacts at the origin.

We discuss pre-specified expectations in Armand et al. (2024). In summary, assuming that outcomes are coded so that higher values reflect positive effects, we expect the program to generate improvements relative to the control group (i.e., $\beta_L > 0$ and $\beta_B > 0$). Because our experimental design allows us to isolate the role of local leaders by comparing the leader treatment to a basic version, thanks to block leaders having a direct political role, we expect their participation to amplify treatment effects (i.e., $\beta_L > \beta_B$).

6.1 Urbanization and economic integration

This section analyzes the economic effects of the program, focusing first on migration responses among migrants and their origin contacts as a direct measure of urbanization. An integration policy that reduces internal migration frictions should not only retain migrants already in the city but also induce additional rural–urban migration through social networks linking migrants to their origin households. We then examine the economic channels through which the program operates, which plausibly mediate both urbanization and political effects.

Table 1 reports the treatment effects on these outcomes, estimated using equation (1). Columns (1)–(3) examine migrant retention among existing migrants (i.e., whether migrants remained in the city at the end of the program) and migration to the city among origin contacts (i.e., whether their origin contacts intended to migrate to the city during the program and whether they migrated to the city one year after the end of the program).[12] Columns (4)–(7) instead analyze the economic integration of migrants. Column (4) reports effects on financial inclusion by focusing on mobile money adoption, while columns (5)–(7) focus on the labor-market exposure of migrants, captured by the probability of having been exposed to job opportunities, the probability of working, and the number of hours worked. To avoid the experimenter demand effect in reporting of employment, we base these indicators on a time use module that asked respondents about their activities during each of the 24 hours in the day prior to the interview. Appendix D.3 provides descriptive statistics on time use and

[12]Information on whether a migrant is in the city at the end of the program is based on field reports from the endline report. If a migrant is not found, information on whether the migrant left the city was obtained from phone calls or by asking neighbors. Information about the migration of origin contacts is based on surveys.

shows further evidence using self-reported employment.

We begin by analyzing migration responses. Control-group outcomes indicate a high attachment to the city among sampled migrants, as 95.5% remain in the city at endline, while migration from origin areas is relatively rare, with 11.4% of contacts intending to migrate and 8.9% having migrated to the city within the year after the end of the program. The program does not affect the probability that sampled migrants remain in the city at endline, reflecting the high baseline retention rates. By contrast, we find clear effects on migration among origin contacts. Both treatments significantly increase their intentions to migrate by 3.9 percentage points under the leader treatment and 2.7 percentage points under the basic treatment. One year after the end of the program, the likelihood that origin contacts are in Quelimane is also significantly higher, with an effect of 4.9 percentage points under the leader treatment and 3.7 percentage points under the basic treatment, robust to multiple hypothesis testing. These findings indicate that the program induced additional rural–urban migration through migrants' social networks. The magnitudes of these effects are large, corresponding to 41.6% and 55.1% relative to the control group mean.

Concerning financial inclusion, we find that mobile money use is already common in the control group, as migrants use, on average, 2.5 mobile money services. Although these statistics indicate strong baseline financial linkages, both treatments increase migrants' use of mobile money by 0.19 services under the leader treatment and by 0.28 services under the basic treatment, with both effects being statistically significant and robust to multiple hypothesis testing.

Concerning labor-market exposure, we observe that in the control group, exposure to job opportunities is limited (23.4 percent), while 53% of migrants work, corresponding to an average of 4.6 hours per day. Both treatment variants significantly increase migrants' exposure to job opportunities. Migrants in leader-treatment blocks are 6.8 percentage points more likely to report having heard about jobs, while those in basic-treatment blocks experience an increase of 8.1 percentage points. Both effects are statistically significant at the 1 percent level and robust to multiple hypothesis testing. These results indicate that the program successfully disseminated labor-market opportunities.

Although both the leader and the basic treatments increase exposure to job opportunities

among migrants, the two treatments differ sharply in their effects on employment. The leader treatment significantly increases the probability of working by 4.8 percentage points and the number of hours worked by 0.4 hours per day on average. Both estimates are significant at the 1 percent level and robust to multiple hypothesis testing. The magnitude of the effects of the leader treatment on employment, corresponding to increases in both the probability of working and the number of hours worked by 9.1% relative to the control group mean, is economically meaningful, given that the program primarily improves access to opportunities without altering labor demand or wages and that job opportunities available to recent migrants are often temporary. By contrast, the basic treatment has no positive effect on hours worked. The difference between the two treatments is statistically significant, suggesting that leader involvement is critical for translating job opportunities into actual employment rather than merely increasing awareness.

Instrumental-variable estimates presented in Appendix D.7 imply that having attended all of all program's visits increases the probability that migrants report having access to job opportunities by about 9 to 11 percentage points, and having attended all visits with leader engagement increases the likelihood of being employed by about 6 percentage points and raises labor supply by 0.51 hours per day. These effects are consistent with self-reported employment history, as the leader treatment increases the probability of having gained a job from baseline to post-intervention surveys, reduces the probability of having lost a job from baseline to post-intervention surveys, and increases the number of hours worked since baseline (Appendix D.3).

Employment effects are specific to migrants. Appendix D.3 shows that at baseline, migrants spend less time working and more time on leisure than residents, reflecting weaker labor market attachment upon arrival. By midline and endline, migrants' time use shifts toward higher work hours and lower leisure, narrowing the gap with residents. Leaders' time use remains relatively stable over time, with consistently higher hours devoted to administrative tasks, reflecting their intermediary role. Importantly, we do not observe reductions in residents' work time, suggesting that migrants' increased labor supply does not crowd out resident employment.

Appendix D.4 examines whether financial inclusion and economic integration translate

into remittances from the city to origin areas. As part of the program, migrants were incentivized to make a transfer using mobile money (see Section 3). Migrants are asked whether they sent money to, or received money from, anyone in the origin district in the previous month, while origin contacts report whether they sent money to, or received money from, a migrant. Both treatments increase the probability that origin contacts report receiving remittances by 7.8 percentage points under the leader treatment and 7.5 percentage points under the basic treatment, with no statistically significant difference between them. By contrast, effects on migrants' self-reported sending are weaker: the basic treatment increases reported transfers by 5.1 percentage points, while the leader estimate is smaller and not statistically significant. This divergence likely reflects measurement differences and possible strategic delays or concealment of transfers following employment gains (e.g., Jakiela & Ozier, 2016). We find no effects on transfers from origin contacts to migrants.

We further examine whether financial inclusion and economic integration translate into monetary transfers between the city and migrants' origin areas (Appendix D.4. Migrants report whether they sent or received transfers with family or friends living outside the city in the previous 30 days, capturing a general propensity to remit to origin areas, while origin contacts report whether they sent or received transfers with family or friends living outside their own district, including transfers specifically involving the migrant. On average, 24.2% of migrants report having sent a transfer outside Quelimane in the previous month, and 41.7% of origin contacts report having received a transfer from outside their district. Both treatments increase the probability that origin contacts report receiving transfers by 7.8 percentage points under the leader treatment and 7.5 percentage points under the basic treatment, with no statistically significant difference between the two. By contrast, effects on migrants' self-reported sending are weaker, as the basic treatment increases reported transfers by 5.1 percentage points, while the leader estimate is smaller and not statistically significant. As part of the program, migrants were incentivized to make a transfer using mobile money in both the leader and basic treatments (see Section 3). However, as migrants in the leader treatment experience employment gains but do not remit more money to origin communities compared to the control group, it is likely that remittance activity is characterized by possible strategic delays or concealment following employment gains (e.g., Jakiela & Ozier, 2016). We

find no effects on transfers from origin contacts to migrants in the city.

Taken together, these results highlight two main lessons. First, integration policies shape dynamics beyond the individuals directly targeted by the program, inducing additional migration through social networks. These migration responses arise under both treatment variants, suggesting that they are driven primarily by the dissemination of information and improved perceptions of urban opportunities rather than by leader involvement. Second, the relevant margin of leader involvement is not awareness or participation but effectiveness. While many migrants in control blocks have heard about job opportunities, the leader treatment substantially increases employment among migrants, indicating that leader involvement helps convert information into realized employment. When leaders are involved, the program improves the quality of labor-market integration, not only its visibility.

6.2 Interaction between citizens and leaders

Section 6.1 shows that the program improved migrants' integration, with effects that differ depending on whether the policy was delivered with leader involvement. This section examines how leader involvement reshapes interactions between leaders, migrants, and residents. These interactions are central to the program because block leaders are locally influential actors who can facilitate access to jobs, mediate relations with municipal institutions, and mobilize political support. If leader involvement meaningfully shapes behavior rather than merely perceptions, we should observe changes in whom leaders know, who contacts them, and whether they are approached for job-related assistance.

Table 2 examines how the treatments affected social interactions with block leaders. The outcomes capture both leader-side knowledge of migrants and leader-centered interactions reported by migrants and residents. Columns (1)–(2) measure whether leaders report knowing any migrants in their block and the share of sampled migrants they recognize individually. Columns (3)–(8) measure whether migrants and residents know their block leader, have contacted the leader, and have resorted to the leader for job-related assistance. Treatment effects are estimated using equation 1 applied to a single wave of surveying or to pooled observations, depending on data availability (indicated in the table footer).

Baseline levels of interaction with block leaders are already substantial in control blocks:

a majority of leaders report knowing migrants in their block, and more than half of migrants and residents report knowing their leader. Active engagement is also common, with roughly 10 percent of migrants and 17 percent of residents reporting having recently contacted the leader. By contrast, resorting to block leaders for job-related assistance is rare at baseline, reported by only about one percent of migrants and by less than one percent of residents.

The leader treatment generates systematic and economically meaningful increases in interactions with block leaders. Leaders in treated blocks are more likely to report knowing migrants in their block (9.1 percentage points) and recognize a larger share of the sampled migrants (6.4 percentage points). At the same time, migrants are more likely to know their leader (6.3 percentage points), to have contacted him or her (5.2 percentage points), and to resort to the leader for job-related assistance (1.2 percentage points). Importantly, these effects extend beyond migrants: residents in leader-treatment blocks are also more likely to know and contact the leader and to resort to the leader for jobs, by 7.1, 4.9 and 1.6 percentage points, respectively. The effect of knowing the leader is marginally insignificant at standard confidence levels, but it becomes statistically significant when we select controls using the PDSL procedure (Appendix D.9).

The basic treatment has no systematic effects on leader–citizen interactions, except for a small increase in migrants' likelihood of resorting to leaders for jobs. This pattern is potentially consistent with the program increasing the salience of local leaders as potential intermediaries for job matching, even in the absence of formal leader involvement. Importantly, this effect is limited in scope and does not extend to other interaction margins or to residents, highlighting the distinct role of formal leader participation in generating systematic changes in leader–citizen interactions.

The Romano–Wolf adjusted p-values confirm that the pattern of results is robust to multiple hypothesis testing. The effects of the leader treatment remain statistically significant for most interaction outcomes after adjustment, while the basic treatment generally does not. This reinforces the interpretation that formal leader involvement, rather than program exposure alone, is the primary driver of increased leader–citizen interactions.

These findings indicate that leader involvement did more than increase program salience. It reshaped patterns of interaction between leaders and residents, positioning block leaders

as more central intermediaries. This is consistent with the content and framing of the intervention and points to a plausible mechanism through which leader endorsement can amplify both the economic and political effects of the program. In line with this, we find that the leader treatment also improves self-reported views about migrants among leaders (Appendix D.2).

6.3 Political mobilization and electoral participation

Sections 6.1 and 6.2 demonstrate that leader involvement increases their centrality in the community and mediates economic integration, two key channels through which policies can translate into political responses. The program was implemented in close temporal proximity to municipal elections, making political responses both salient and observable. However, these effects need not be confined to a single group. Leaders may respond by intensifying mobilization efforts, migrants by increasing engagement and participation, residents by adjusting political support in response to perceived redistribution, and migrants' origin contacts by updating political preferences through information and migration networks. We therefore examine whether the program affected broader political engagement and participation among all actors involved, moving from political mobilization to realized electoral behavior. To address the sensitivity of political outcomes and the limitations of self-reported measures, we combine survey data with multiple behavioral measures, allowing us to capture politically salient actions while reducing concerns about reporting bias.

Table 3 reports treatment effects on a set of behavioral measures capturing mobilization in support of the incumbent prior to the 2023 elections. We focus on two measures: the leader's ability and effort in mobilizing migrants in the block, and the expressed support for the incumbent.

Columns (1)–(2) focus instead on block leaders' campaign mobilization capacity ahead of the 2023 elections, measured using a purpose-designed SCA. Bicycle taxi drivers, who constitute the main mode of transportation in the city and are closely associated with the incumbent mayor's campaign infrastructure, were used as the target group, many of whom are recent migrants. Block leaders were instructed to compile a list of drivers in their block and to convene them for a block meeting on a specific date. We distinguish between leaders'

reported mobilization capacity, captured by the size of the list they provided, and realized mobilization, captured by the number of drivers who actually attended the meeting, as observed by enumerators. This distinction allows us to separate leaders' claimed outreach from their ability to convert contacts into political participation.

In the control group, 63% of leaders report at least one person on the list to mobilize, while on average, they manage to mobilize 1.5 cyclists. Leaders in treated blocks are 10.9 percentage points more likely to report mobilizing cyclists for campaign activities, and enumerators observe approximately 0.75 additional cyclists relative to control blocks, corresponding to an increase of 50% relative to the control group mean. These effects are robust to correcting for multiple hypothesis testing. While the basic treatment also increases mobilization, none of the estimates are statistically significant. This pattern suggests that leader involvement in the program enhances its mobilization capacity.

In columns (3)–(4), we verify whether visible political support for the incumbent responds to leader involvement. To measure support, we designed and implemented another SCA based on the distribution prior to the elections of political stickers praising the mayor for the integration of migrants through the program. Each block leader received 40 brown stickers (see Appendix Figure C1) and was instructed to distribute them to households in their block, with encouragement to place them on front doors. Sticker visibility on migrants' homes serves as a measure of leader influence and citizen responsiveness in support of the incumbent. In parallel, we implemented a version in which stickers were distributed directly to migrants and residents rather than through block leaders. These stickers were identical in content but differed in color (pink). During endline interviews, enumerators recorded the presence of both sticker types on respondents' doors, allowing us to isolate the role of leaders in mobilization.

In the control group, brown and pink stickers appear on 44% and 75% of houses, respectively. Brown stickers are 3.8 percentage points more likely to be observed on doors in leader-treatment blocks, corresponding to an increase of 8.7% relative to the control mean, robust to multiple hypothesis testing. By contrast, pink stickers show no treatment effects, and in the basic treatment, we observe no effect on either sticker type. This sharp contrast indicates that political expression increases only when it is channeled through local leaders.

A useful way to interpret these patterns is through a political mobilization lens. In Quelimane, block leaders are mayoral appointees who are unpaid, politically active, and often aspire to future positions within the municipal administration. Therefore, local leaders have incentives to exert observable effort to demonstrate loyalty and mobilization capacity to higher-level authorities (Stokes et al., 2013; Larreguy et al., 2016). The distribution of brown stickers, which are visible, low-cost, and explicitly tied to a municipal program, provides an opportunity for such signaling. When the program is implemented through leaders, political expression reflects both citizen responsiveness and leaders' mobilization efforts. Instead, when identical materials are distributed directly to citizens, bypassing leaders, political expression does not increase. This interpretation explains why political mobilization responds only to leader involvement, without implying clientelistic transfers or the endogenous selection of leaders into treatment.

We now turn to political participation and voting. Table 4 shows the results. We begin by examining, in columns (1)–(2), the level of political participation among migrants and residents, as measured by the display of partisan political objects in their dwellings. This variable was measured through direct observation of visible political materials during interviews, including stickers, posters, T-shirts, and caps. Because such materials are frequently distributed at rallies or public events, this measure likely captures both exposure to campaign activity and willingness to display political affiliation. Display rates are low in control blocks, corresponding to 4.7% among migrants and 13.1% among residents.

Both treatments increase migrants' display of political objects by 2–3 percentage points, with no detectable effects among residents. The fact that both treatments have similar effects is consistent with this outcome capturing broad exposure to political activity among migrants. Participation in the program likely increased migrants' exposure to campaign activities by raising their social inclusion in the city, making them more likely to encounter and accept political materials distributed at public events or by local leaders, even in the absence of changes in political preferences. In line with this, Appendix D.1 shows that political objects are associated with multiple parties, indicating general exposure rather than partisan alignment.

Columns (3)–(8) relate to voter turnout and vote choice during the 2023 municipal elec-

tions. To build an objective indicator of voter turnout, we exploited the fact that, in Mozambique, voters' index fingers are marked with purple ink after casting a ballot to avoid multiple voting. We leveraged this institutional feature by deploying a large team of enumerators to canvass the city in the two days following the election and to record whether block leaders', migrants', and residents' fingers were inked. This measure allows us to capture high-cost political participation in response to campaign exposure and leader mobilization. Concerning vote choice, we rely instead on self-reports from survey data.

Turnout levels are high in the control group: 90% among leaders, 70% among migrants, and 82% among residents. Against this backdrop, we find that the leader treatment increases migrant turnout by 3.0 percentage points, a modest but meaningful effect given the already high baseline. This effect corresponds to an increase in turnout of 4.3% relative to the control group, robust to multiple hypothesis testing. The basic treatment also increases turnout by 2.2 percentage points, which is not statistically significant. The effects of both treatments for leaders and residents are imprecisely estimated and not statistically different from zero, likely due to limited variation at higher levels of baseline turnout.

Turning to vote choice in columns (4)–(6), we focus on self-reported voting for the nationally ruling party (FRELIMO) among migrants, residents, and migrant's origin contacts. For the latter, the sample of origin contacts is restricted to those living within the boundaries of districts with urban municipalities because no election occurred beyond this boundary.[13] We further restrict the sample to origin contacts living in the same province of Quelimane at the time of the election due to uncertainty about the exact district of residence when living outside the province. Unlike our turnout measure, which is based on direct observation of inked fingers, vote choice relies on self-reports and should, therefore, be interpreted with greater caution. Self-reported voting behavior may be affected by social desirability concerns, recall error, or strategic misreporting, particularly in politically polarized contexts. For this reason, we view these results as indicative of directional shifts in partisan alignment rather than precise estimates of vote shares, and we place greater weight on their consistency with the behavioral mobilization and turnout patterns documented above.

In line with the ruling party's stronger base in rural areas, 21.3% of origin contacts of

[13]This restriction reflects Mozambique's institutional structure, where local elections are held only in autonomous municipalities (*autarquias*), which are more urban. Other districts are administered by centrally appointed officials and do not hold local elections.

control migrants report having voted for FRELIMO, compared to 7.4% and 12.0% among migrants and residents in control blocks. We observe little response to treatments among migrants and residents. However, a striking pattern emerges for migrants' origin contacts. Under the leader treatment, origin contacts are 16 percentage points less likely to report voting for FRELIMO, implying increased support for the opposition. While this estimate is noisy and sensitive to multiple-hypothesis adjustments, its magnitude is large (76.5% relative to the control mean) and aligns with earlier evidence of information and migration spillovers to origin areas. In the longer run, we also observe that support for the party of the incumbent increased among leaders during the 2024 Presidential elections, more than a year after the end of the program (Appendix D.12).

The stronger political responses observed among migrants' origin contacts likely reflect a combination of information and selection mechanisms. Origin contacts receive information about urban policies primarily through migrants. Unlike migrants or city residents, origin contacts do not face immediate trade-offs related to urban redistribution or local political competition, allowing belief updating to translate more directly into expressed political preferences. In addition, urban integration policies are particularly relevant for origin households' future migration options, making political evaluations more responsive to perceived improvements in access and opportunity. These features help explain why political shifts are more pronounced among origin contacts, even though direct mobilization efforts are concentrated in the city.

Taken together, these results indicate that migrant integration policies can be politically feasible in dense urban settings, particularly when embedded in local governance structures and implemented during electorally salient periods. Rather than generating backlash, the program increased political engagement among migrants, strengthened leaders' mobilization capacity, and produced electoral returns without reducing participation among incumbent residents. Spillovers to migrants' origin areas indicate that the political consequences of urban integration extend beyond city boundaries.

6.4 Coordination versus political exchange

Given the large number of outcomes analyzed in previous sections, Appendix D.6 complements the outcome-by-outcome analysis with domain indices that aggregate related measures following Kling et al. (2007).[14] Across domains, both treatment arms generate positive effects, indicating that the integration program itself improves economic outcomes and shapes political behavior. Leader involvement, however, consistently amplifies these effects, particularly for indices related to employment and political mobilization in favor of the urban incumbent.

To assess whether this amplification operates through coordination or clientelistic exchanges, we implemented a SCA eliciting leaders' preferences for discretionary, excludable allocation relative to impersonal public good provision. Leaders were given 2,000 Mts (≈30 US$) and asked to choose between hiring, at their sole discretion, a resident of the block to serve as a "block leader assistant" for two months or allocating the same amount to a block-level cleaning service implemented by an external team, which removed discretion over beneficiary selection.[15] Favoring the assistant option grants leaders full control over the identity of the beneficiary and enables the provision of a private, excludable benefit. While this choice does not capture vote buying or explicit exchanges, selecting a privately chosen beneficiary, especially a relative or politically connected individual, reflects patronage behavior that can sustain clientelistic relationships.

Table 5 reports treatment effects on leaders' allocation choices, distinguishing between different forms of particularistic allocation. Column (1) examines whether leaders chose the assistant or the public good provision. In columns (2)–(5), we use information on the selected individuals and the tasks assigned to the assistant to characterize the nature of the appointment. Column (2) focuses on whether the leader selected a family member, thus capturing the propensity over nepotism. Column (3) focuses on whether the assistant previously held a leadership role in the block, thus capturing the selection of politically connected individuals, a choice that is consistent with clientelism. Finally, in column (5), we analyze whether

[14] Outcomes are standardized using the control-group mean and standard deviation, averaged to the block level, and summed within domains. Treatment effects are estimated using the same specification as for individual outcomes and are interpreted as equally weighted summary effects.

[15] The assistant received 1,000 Mts per month via mobile money and was issued an official certificate stamped by the Municipality confirming their role for two months. The cleaning took place approximately one week before the elections in most blocks, but due to logistical constraints, in a few blocks, cleaning occurred after the elections. See Appendix C for examples of the activities.

the assistant was tasked with helping the leader distribute funds, which could also indicate clientelistic exchanges. Together, these outcomes.

In the control group, 51% of leaders choose the private-benefit option, indicating that discretionary allocation is common even in the absence of the program. Both treatments reduce the probability of selecting an assistant, but the estimates are not statistically significant. Neither treatment significantly affects the likelihood of appointing family members, previously influential community figures, or assistants tasked with distributing funds. Point estimates are small relative to control means and do not display a consistent direction. These results suggest that program exposure did not increase particularistic allocation or targeting consistent with patronage.

Additional evidence from self-reported behavior (Appendix D.11) likewise shows no increase in payments between leaders and migrants, payments for job access, or vote buying. Reported levels of such practices are low across treatment and control groups, and estimated effects are small and statistically insignificant.

Heterogeneous treatment effects provide further insight into the mechanism (Appendix D.8). Amplification is strongest in contexts where leaders' intermediation capacity is likely to be most valuable, including blocks with higher migrant density and those led by more experienced leaders. In high-density areas, larger migrant populations increase demand for coordination and information, while experienced leaders are better positioned to leverage established networks and institutional knowledge to connect migrants to opportunities and services. By contrast, amplification does not depend systematically on leaders' partisan alignment or identity characteristics, and economic gains arise even when leaders are not closely tied to the incumbent. These patterns are difficult to reconcile with a clientelistic mechanism in which access to opportunities is contingent on political loyalty. Instead, they point to a coordination channel in which leaders reduce informational and matching frictions and translate program inputs into realized economic outcomes. Political gains for the incumbent appear to emerge as a by-product of successful integration rather than targeted exchanges for support.

7 Conclusion

This paper reports evidence from a randomized controlled trial conducted in Quelimane, Mozambique, evaluating the political and economic effects of a city-sponsored integration program targeting recent rural migrants. Involving local leaders fundamentally changed the effects of the program. Leader participation increased engagement and interactions with migrants, positioned leaders as key intermediaries, and translated program exposure into higher electoral participation and visible support for the local incumbent. Political effects extended beyond the city, shifting support among migrants' origin households. At the same time, leader involvement converted increased access to job information into a higher labor supply, improved perceptions of migrants, expanded mobile-money use and transfers, and induced additional rural–urban migration after the program ended. Together, these results show that integration policies can simultaneously reshape economic outcomes, political behavior, and urbanization dynamics when embedded in local governance structures.

These findings have broad implications for development policy. Urbanization and structural transformation are central to long-run development, yet in many low-income countries, national policies have often constrained urban growth. Our results show that city-level integration policies can be politically viable and even electorally advantageous when embedded in local governance structures, despite being misaligned with national political incentives. Contrary to concerns that migrants represent a political liability, we find that urban integration can expand political participation and generate support rather than backlash.

More broadly, our results underscore that the success of development policies depends not only on their economic content but also on their political embedding. When policies align local political incentives with development objectives, cities can become effective engines of structural change. Urbanization, in this sense, is not only an economic process but also a political one, in which local governments may be uniquely positioned to make it work.

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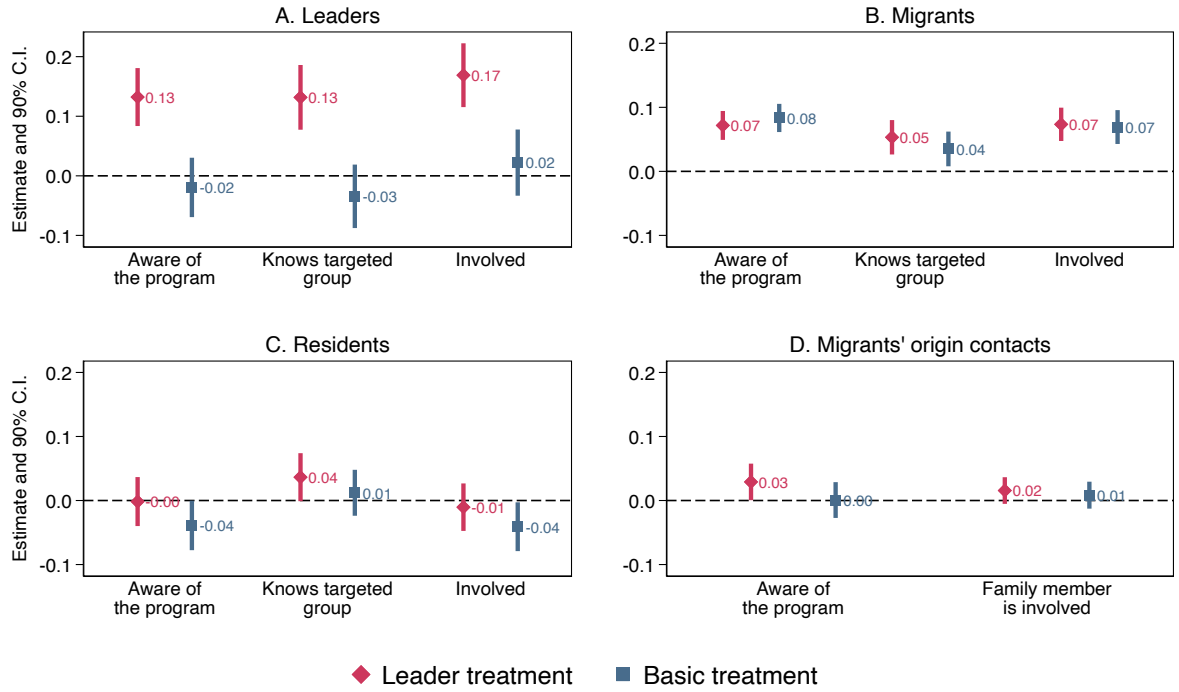
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Figure 1: Awareness of the program and participation



Note. Estimates based on OLS regressions using equation 1 applied to stacked post-intervention observations. Panel A reports results for block leaders; Panel B for sampled migrants; Panel C for sampled residents; and Panel D for migrants' origin contacts. Outcome variables are: (1) *Aware of the program* is an indicator variable equal to 1 if the respondent has heard about the program "Quelimane trabalha com todos", and 0 otherwise; (2) *Knows targeted group* is an indicator variable equal to 1 if the respondent reports that rural migrants were involved in the program, and 0 otherwise; (3) *Involved* is an indicator variable equal to 1 if the respondent reports having been involved in the program or if a family member was involved in the program, and 0 otherwise (in panel D, the outcome refers to family members only). All specifications include strata fixed effects and block- and individual-level controls (see Section 5). Standard errors are clustered at the block level; 90% confidence intervals are reported. Variable definitions are provided in Appendix C. Full results are reported in Appendix A.4.

Table 1: Urbanization and economic integration

	Migration responses			Financial inclusion and employment			
	Migrant remained in the city	Origin contact... intends to migrate	Origin contact... migrated to the city	Mobile money adoption	Any job opportunity	Worked	Hours worked
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(TL) Leader treatment	0.003	0.034***	0.043***	0.185*	0.071***	0.049***	0.427***
	(0.008)	(0.013)	(0.014)	(0.095)	(0.013)	(0.016)	(0.152)
(TB) Basic treatment	0.011	0.027**	0.039***	0.266***	0.082***	-0.001	-0.161
	(0.007)	(0.012)	(0.013)	(0.099)	(0.014)	(0.018)	(0.168)
Observations	3590	3792	2218	2856	6119	6118	6118
Adjusted R^2	0.051	0.027	0.012	0.182	0.163	0.093	0.119
Mean (control group)	0.955	0.115	0.092	2.537	0.234	0.529	4.556
TL = TB (p-value)	0.292	0.572	0.795	0.400	0.447	0.003	0.000
Outcome data	Endline	Pooled	Follow-up	Midline	Pooled	Pooled	Pooled
Romano-Wolf p-value TL	0.679	0.007	0.003	0.049	0.001	0.008	0.003
Romano-Wolf p-value TB	0.123	0.037	0.006	0.012	0.001	0.431	0.947

Note. Estimates based on OLS regressions using equation 1 applied to the data reported in the table footer (*outcome data*). Dependent variables by column: (1) *Migrant remained in the city* is an indicator variable for whether the respondent is in Quelimane at the time of the survey; (2) *Origin contact intends to migrate* is an indicator variable for whether the respondent reports intending to migrate; (3) *Origin contact migrated to the city* is an indicator variable for whether the respondent is living in Quelimane at the time of the survey; (4) *Mobile money adoption* is a variable counting the number of mobile money services that the respondent reports using; (5) *Any job opportunity* is an indicator variable for whether the respondent has heard of job opportunities in the previous 6 months; (6) *Worked* is an indicator variables for whether the respondent reports having worked in the previous day, and 0 otherwise; (7) *Hours worked* is a variable counting the number of hours in the previous day that the respondent reported being working. All specifications include block and individual controls (Section 5 provides the full list of controls), as well as strata fixed effects. Standard errors, reported in parentheses, are clustered at the block level. *** p<0.01, ** p<0.05, * p<0.1. *Romano-Wolf p-values* are the *p*-values adjusted for multiple hypothesis testing. Variable definitions are provided in Appendix C.

Table 2: Interaction between leaders and citizens

	Leader's personal ties to migrants in the block		Citizen interaction with their block leaders					
			Knows the leader		Contacted the leader		Sought assistance for job	
	Any tie	Share known	Migrant	Resident	Migrant	Resident	Migrant	Resident
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(TL) Leader treatment	0.083**	0.061***	0.054**	0.071	0.036***	0.049**	0.012***	0.016**
	(0.036)	(0.017)	(0.026)	(0.045)	(0.010)	(0.020)	(0.003)	(0.007)
(TB) Basic treatment	0.035	0.010	-0.038	-0.043	0.009	-0.004	0.009**	0.004
	(0.038)	(0.019)	(0.028)	(0.045)	(0.010)	(0.022)	(0.003)	(0.006)
Observations	820	846	2856	753	6119	1574	6093	1566
Adjusted R^2	0.073	0.123	0.237	0.197	0.054	0.082	0.009	0.030
Mean (control group)	0.674	0.162	0.539	0.583	0.098	0.165	0.012	0.007
TL = TB (p-value)	0.171	0.011	0.001	0.008	0.008	0.015	0.373	0.121
Outcome data	Pooled	Pooled	Midline	Midline	Pooled	Pooled	Pooled	Pooled
Romano-Wolf p-value TL	0.012	0.002	0.023	0.087	0.001	0.032	0.002	0.032
Romano-Wolf p-value TB	0.389	0.852	0.202	0.328	0.500	0.913	0.030	0.913

Note. Estimates based on OLS regressions using equation 1 applied to the data reported in the table footer (*outcome data*). Outcomes in columns (3)–(4) are available only in the midline survey. None of the specifications includes the baseline value of the dependent variable since they were not collected at baseline. Dependent variables by column: (1) *Any tie* is an indicator variable for whether the leader knows any rural migrants living in their block, and 0 otherwise; (2) *Share known* is the share of sampled migrants that the leader knows (the leader was shown the list of sampled migrants and asked to indicate those personally known); (3)-(4) *Knows leader* is an indicator variable equal to 1 if the respondent correctly identifies the name of their block leader, and 0 otherwise; (5)-(6) *Contacted leader* is an indicator variable for whether the respondent reports having approached the block leader at least once since the previous survey waves; (7)-(8) *Resorted to leader for a job* is an indicator variable for whether the respondent reports having contacted or paid the leader for a job in the previous year. All specifications include block and individual controls (Section 5 provides the full list of controls), as well as strata fixed effects. Standard errors, reported in parentheses, are clustered at the block level. *** p<0.01, ** p<0.05, * p<0.1. *Romano-Wolf p-values* are the *p*-values adjusted for multiple hypothesis testing. Variable definitions are provided in Appendix C.

Table 3: Mobilization in support of the incumbent

	Mobilization of citizens for block meeting		Express support for the incumbent	
	Reported capacity	Realized	Observed stickers distributed by... leaders	field team
	(1)	(2)	(3)	(4)
(TL) Leader treatment	0.120**	0.655*	0.042**	0.006
	(0.052)	(0.394)	(0.017)	(0.026)
(TB) Basic treatment	0.082	0.677*	-0.011	-0.014
	(0.053)	(0.404)	(0.018)	(0.026)
Observations	444	444	444	444
Adjusted R^2	0.132	0.043	0.709	0.518
Mean (control group)	0.630	1.473	0.431	0.652
TL = TB (p-value)	0.472	0.960	0.010	0.442
Outcome data	Endline	Endline	Endline	Endline
Romano-Wolf p-value TL	0.067	0.079	0.052	0.964
Romano-Wolf p-value TB	0.277	0.244	0.857	0.857

Note. Estimates based on OLS regressions using equation 1 applied to the data reported in the table footer (*outcome data*). The measurement is available for all blocks with a leader. Column (1) refers to leaders, columns (2) and (4) to migrants, columns (3) and (5) to residents, and column (6) to migrant's origin contacts. Dependent variables by column: (3) *Reports cyclists* is an indicator variable equal to 1 if the list left with leaders for cyclist mobilization contains any names at the time of collection, and 0 otherwise; (4) *Observed # mobilized* is the number of cyclists mobilized by leaders and observed by the enumerator; (5) *Stickers from leader* is the percentage of brown stickers (out of the 40 stickers distributed to the leader) found hanging on households' doors during the enumerators' visit to the blocks; (6) *Stickers from field team* is the percentage of pink stickers (out of the number of stickers distributed in each block) found hanging at households' doors during the enumerators' visit to the blocks. All specifications include block and individual controls (Section 5 provides the full list of controls), as well as strata fixed effects. Standard errors, reported in parentheses, are clustered at the block level. *** p<0.01, ** p<0.05, * p<0.1. *Romano-Wolf p-values* are the *p*-values adjusted for multiple hypothesis testing. Variable definitions are provided in Appendix C.

Table 4: Political participation and voting

	Display of partisan political objects		Municipal elections: Turnout			Municipal elections: Self-reported voting for national incumbent (FRELIMO)		
	Migrants	Residents	Leaders	Migrants	Residents	Migrants	Residents	Origin contacts
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(TL) Leader treatment	0.017***	0.007	0.045	0.028*	-0.047	-0.006	0.052	-0.142***
	(0.006)	(0.014)	(0.034)	(0.017)	(0.034)	(0.013)	(0.037)	(0.053)
(TB) Basic treatment	0.024***	-0.002	0.021	0.022	0.026	-0.013	0.036	-0.085
	(0.007)	(0.014)	(0.034)	(0.018)	(0.033)	(0.013)	(0.036)	(0.057)
Observations	6117	1571	412	3333	811	2092	534	400
Adjusted R^2	0.042	0.037	-0.035	0.079	0.038	0.034	0.159	0.147
Mean (control group)	0.047	0.083	0.906	0.698	0.821	0.074	0.119	0.196
TL = TB (p-value)	0.256	0.585	0.476	0.747	0.034	0.567	0.664	0.315
Outcome data	Pooled	Pooled	Election	Election	Election	Endline	Endline	Endline
Romano-Wolf p-value TL	0.011	0.627	0.158	0.090	0.132	0.685	0.140	0.006
Romano-Wolf p-value TB	0.001	0.619	0.470	0.214	0.404	0.278	0.273	0.092

Note. Estimates based on OLS regressions using equation 1 applied to the data reported in the table footer (*outcome data*). Column (1) refer to leaders, columns (2) and (4) to migrants, columns (3) and (5) to residents, and column (6) to migrant's origin contacts. Dependent variables by column: (1)-(2) *display of partisan political objects* is an indicator variable equal to 1 if, during the survey interview, the enumerator observed any objects with political content held by the respondent, and 0 otherwise; (3)–(5) *electoral turnout* is an indicator variable for whether the respondent's finger was marked with purple ink during the enumerators' visit in the two days following the 2023 municipal elections (see Section 6.3 for details about this measure); (6)-(8) *self-reported voting for national incumbent (FRELIMO)* is an indicator variable equal to 1 if the respondent reports having voted for FRELIMO in the 2023 municipal elections, and 0 otherwise. In columns (6)–(8), the sample is restricted to respondents who reported having voted. In column (8), the sample is further restricted to respondents living in districts including municipalities and within the same province of Quelimane. All specifications include block and individual controls (Section 5 provides the full list of controls), as well as strata fixed effects. Standard errors, reported in parentheses, are clustered at the block level. *** p<0.01, ** p<0.05, * p<0.1. *Romano-Wolf p-values* are the *p*-values adjusted for multiple hypothesis testing. Variable definitions are provided in Appendix C.

Table 5: Leader behavior in the allocation of funds

	Chose assistant over public good	Chose assistant and selected person is...		
		A relative	A previous block leader	Helping with fund allocation
	(1)	(2)	(3)	(4)
(TL) Leader treatment	-0.081	-0.046	-0.001	-0.004
	(0.057)	(0.056)	(0.054)	(0.038)
(TB) Basic treatment	-0.063	-0.032	0.028	-0.004
	(0.062)	(0.057)	(0.058)	(0.041)
Observations	442	442	442	442
Adjusted R^2	0.096	0.100	0.106	0.161
Mean (control group)	0.510	0.372	0.317	0.145
TL = TB (p-value)	0.756	0.787	0.599	1.000
Romano-Wolf p-value TL	0.261	0.666	0.990	0.990
Romano-Wolf p-value TB	0.554	0.853	0.853	0.898

Note. Estimates based on OLS regressions using equation 1. Outcomes are based on the SCA described in Section 6.4. Dependent variables by columns: (1) *Chose assistant over public good* is an indicator variable equal to 1 if the leader chose the assistant in the allocation of funds activity, and 0 otherwise; (2) *Chose assistant and selected person is a relative* is an indicator variable equal to 1 if the leader selected the assistant and the selected person is a relative, and 0 otherwise; (3) *Chose assistant and selected person is a previous block leader* is an indicator variable equal to 1 if the leader selected the assistant and the selected person is someone who previously held a leadership position in the block, and 0 otherwise; (4) *Chose assistant and selected person is helping with fund allocation* is an indicator variable equal to 1 if the leader selected the assistant and the selected person is assigned a task to help the leader with the allocation of funds within the block, and 0 otherwise. All specifications include block and individual controls, as well as strata fixed effects. Section 5 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. See Appendix A.1 for the timing of activities.

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

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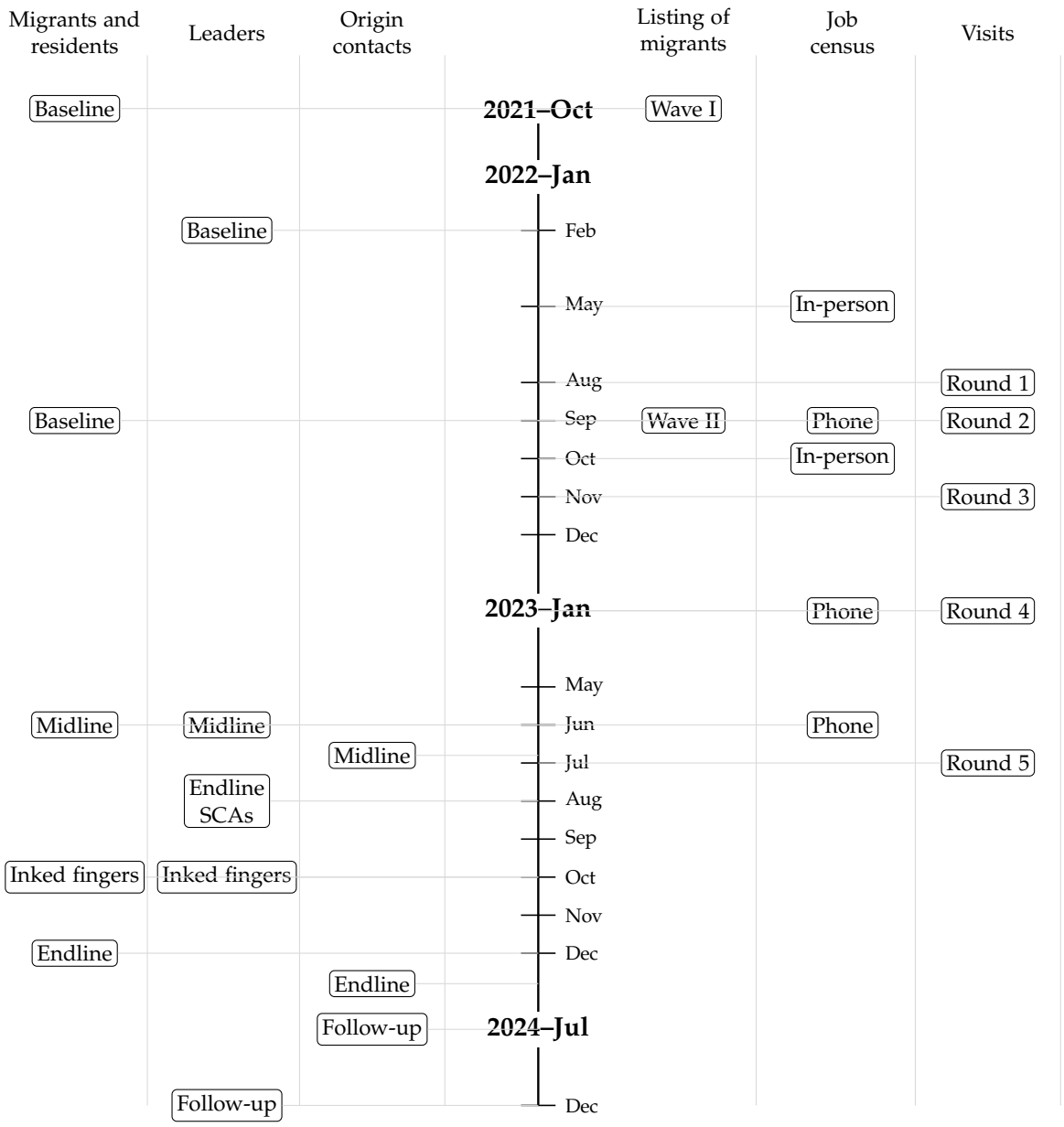
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A Details about the program implementation

A.1 Timeline of activities

Figure A1 provides the timeline of activities, including both the program intervention and the measurement activities.

Figure A1: Timeline of measurements and of program implementation



Note. Elements to the left of the temporal scale indicates activities related to measurements, while elements to the right indicates activities related to the implementation of the program. Details about measurement activities are provided in Section 4. Note that, under *Migrants and residents*, the second baseline activity is restricted to the migrants listed in the second wave of the migrant listing. Details about the program and its implementation are provided in Section 3 and Appendix A.2.

A.2 Modules of the integration program

Job matching module. The main focus of the five face-to-face home visits was job matching. To this end, prior to each visit, we gathered information on the supply of job opportunities suitable for recent rural migrants. To gather this information, we implemented two censuses of job opportunities by visiting every household and establishment in the city before the first and third visits. Figure A2 maps all attempted households and establishments and the locations of identified job opportunities. The censuses were supplemented by three rounds of phone-based follow-up interviews with previously identified employers, which allowed us to update job availability prior to the second, fourth, and fifth visits. Table A1 reports descriptive statistics for the job offers collected in each round.

Prior to each visit, program participants were matched with job opportunities based on elicited job preferences and provided with the names and phone numbers of potential employers. Each participant received three contacts during the first visit, up to seven (depending on availability) during the second visit, and up to ten during the third, fourth, and fifth visits. During the final two visits, implementers further facilitated matching by contacting employers directly during the home visit and scheduling interview dates. At the end of each visit, participants were sent a text message containing the contact details of the relevant employers.

To enhance engagement, during the fourth visit, the participant was shown a short video featuring a recently arrived migrant describing a successful experience with the intervention. Two versions of the video (one featuring a female migrant and one featuring a male migrant) were produced, but only one, randomly selected through the survey platform, was shown to the participant. The script of the video is as follows:

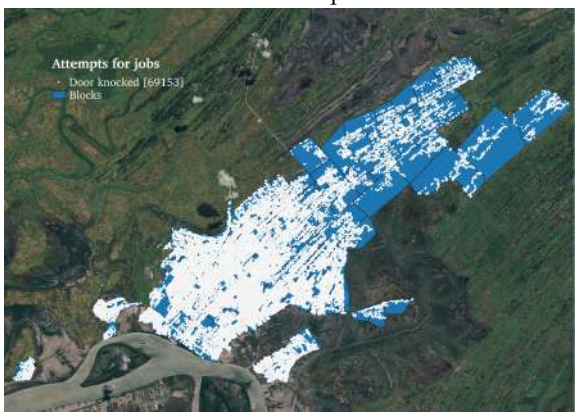
> Good morning/afternoon, My name is [name], and I was born in [district]. I moved to Quelimane in [year] for [reason]. In September 2021, I joined the "Quelimane trabalha com todos" integration program in collaboration with the Quelimane Municipality. Since then, I have received three home visits from the program team. During these visits, I was provided with contacts of people potentially looking for employment. However, the first number I called did not 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 program truly works, you just need to be persistent. There are jobs and opportunities in the city for everyone. Keep trying!

Figure A2: Geographical distribution of job offers

A. First in-person round

Doors attempted

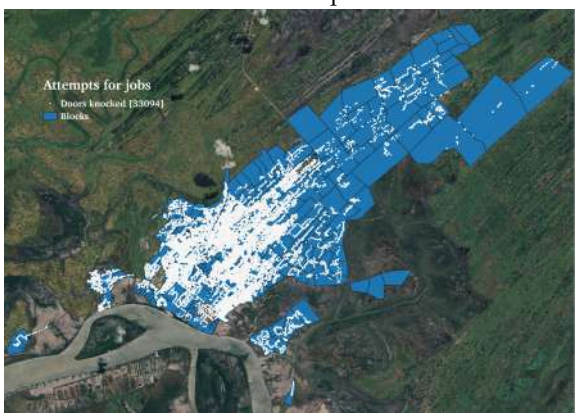


Job offers

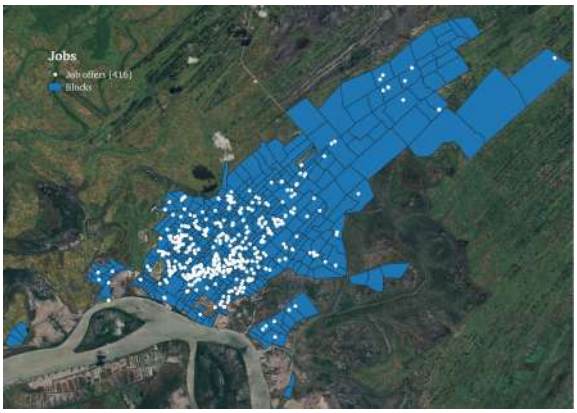


B. Second in-person round

Doors attempted



Job offers



Note. The figures show the geographical distribution of the job census activities. *Doors attempted* refer to households and establishments visited to collect information about job opportunities. *Job offers* refer to the locations of identified job opportunities. The timing of each activity is reported in Figure A1.

Mobile money module. This module introduced migrants to mobile money through a structured presentation on Mozambique's leading service, M-PESA. Facilitators provided step-by-step guidance on account setup and use, including securing accounts with PINs, avoiding fraud, depositing and withdrawing cash through agents, and checking balances (see panel A of Figure A3). The module emphasized everyday uses of mobile money for income management, such as receiving work payments and paying for airtime, transportation, utilities, and

Table A1: Job opportunities for migrants

	By round of visit					Total
	1	2	3	4	5	
Collection of job offers						
Doors attempted	69150	952	33094	1462	1171	105829
Doors with answer	36227	952	26660	1403	983	66225
Job offers	499	149	416	310	208	1582
Job offers composition (%)						
Domestic worker	58.1	23.5	53.8	44.2	52.9	50.3
Farming, livestock, or fishing	16.0	4.7	15.6	15.5	10.1	14.0
Construction	14.0	4.0	14.9	12.3	15.9	13.2
Vendor/trader	3.8	5.4	4.3	5.2	5.8	4.6
Guard/security	6.6	.	3.6	2.6	7.7	5.0
Handyman	1.8	1.3	1.7	1.3	2.9	1.8
Barber/hairdresser	1.4	0.0	2.2	1.9	1.9	1.6
Mechanic	1.2	.	2.4	1.0	1.4	1.5
Driver	0.8	0.7	1.7	0.3	2.9	1.2
Clerical/administrative work	1.0	0.0	1.4	0.6	0.5	0.9
Tailor/seamstress	0.4	.	1.0	1.0	0.5	0.7
Poster and leaflet distributor	2.2	.	0.2	.	.	1.3
Shoemaker	0.2	.	0.5	.	0.5	0.4
Other informal services	.	.	.	.	0.5	0.5
Port laborer	0.4	.	.	.	.	0.4
Mobile money agent	0.2	.	.	.	.	0.2

Note. The table shows statistics about the job opportunities for migrants, as gathered from the censuses of job opportunities and the follow-up phone surveys with the employers. The timing of each activity is reported in Figure A1.

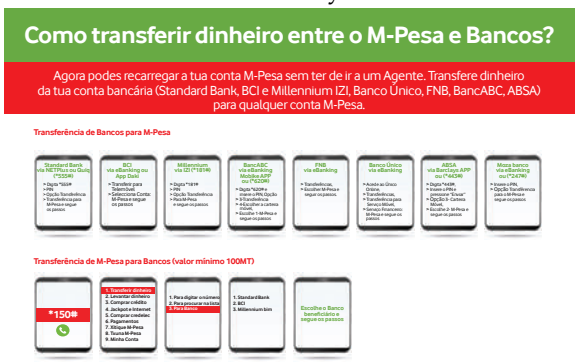
small purchases, and explained transaction fees and costs relative to cash. It also focused on remittances, providing guidance on sending transfers to origin households, planning transfer timing, and using mobile money in emergencies. To encourage active use, participants in the third visit received 20 Meticais ($\approx$ US$ 0.30), which they were instructed to cash and transfer to a family member in their home district, both to incentivize account opening and to allow migrants to trial remittances using mobile money.

City module. Over five home visits, facilitators began each interaction with a city integration module, providing practical administrative and institutional information about living in Quelimane. During the first two visits, the module consisted of a presentation covering municipal services and institutions, including access to schooling and healthcare; procedures for obtaining citizen cards, residence documentation, and voter registration; key public infrastructure; local cultural practices; neighborhood-level governance; and channels for contacting local authorities (see panel B of Figure A3 for an example). From the third visit onward, the same content was embedded in the survey platform and delivered as an interactive quiz, with respondents first prompted to answer and then shown the correct response. When delivered

with leader involvement, block leaders were explicitly introduced as municipal representatives, formalizing a point of contact that is often opaque to new arrivals.

Figure A3: Examples of content provided in the mobile money and city modules

A. Mobile money module



B. City module



Note. Panel A provides an extract from the presentation provided to participants as part of the mobile money module. Panel B shows instead an extract from the presentation provided to participants as part of the city module.

A.3 Instructions for engagement with the block leaders

For migrants in the leader treatment group, at the beginning of the conversations, the implementing team informed the participants about the name of the block leader, the fact that the leader supports the program and will be present during the visits whenever possible, and provided the contact phone number of the leader. The team proceeded to show a video on a tablet with a short message from the leader, expressing support for the program and encouraging migrants to join the program. The script of the video is as follows:

> I am [name] and I have been the leader in block [block] 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 the city. Over the past year, interviews have been conducted with people who recently arrived, with my support. You are one of these individuals. Over the next six months, we will carry out monthly visits to better understand your main challenges, follow up on job opportunities, and help you better understand how you can support your family and friends more easily. I personally invite you to join "Quelimane trabalha com todos." We hope to support you during this transition period into the city.

In the fourth round of visits, all leaders were asked to emphasize in their videos the relevance of participating in elections. The content and framing of such messages were left to

their discretion, with most leaders delivering a political message related to the approaching municipal elections. The exact instructions provided to the leaders were as follows:

> Hi, my name is [enumerator name]. 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 are here once again to ask for your cooperation. We came to you last year to request that you 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 encouraging 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; and 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.

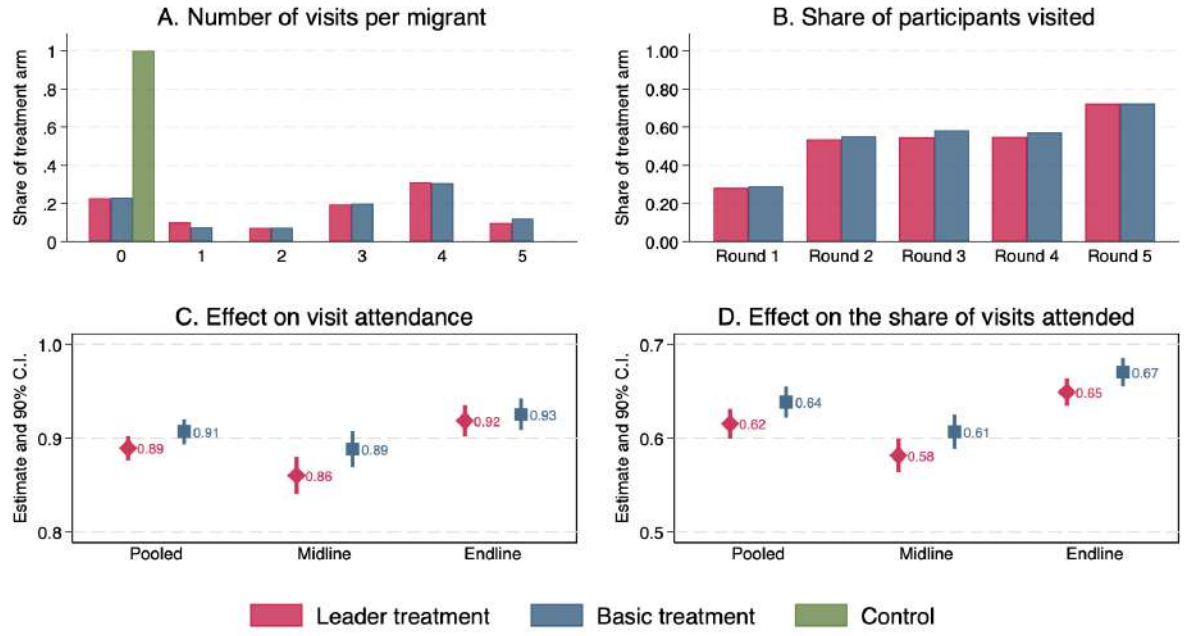
During all visits, the leader's support was reinforced throughout the different modules by providing supportive statements. During the city module, "I remind you that this program is sponsored by your block leader [name]. We count on his/her support, and you can turn to him/her whenever you have any struggles in the block. You can reach him/her at: [contact numbers]." During the job matching module, "once more, I remind you that this program has the support of the block leader [name]. All these offers have been approved and made available by him." Finally, during the module on mobile money, "this presentation is also supported by the block leader [name]. He acknowledges and promotes the benefits of adhering to mobile money." At the end of the visit, the implementing team was instructed to deliver the contact card of the block leader to the participant.

A.4 Program implementation and perceived participation

Using administrative data about the implementation of the program, Figure A4 shows the number of visits received by each sampled migrant in each treatment group (panel A) and the share of sampled migrants who participated in the visits during each round of visits

(panel B). Panels C and D use this information to estimate treatment effects using equation (1) on an indicator variable for whether, between two surveys, the respondent has received and attended a visit and on the cumulative share of visits received and attended at the time of the survey. Table A2 provides correlates of take-up of the integration program in the sample of migrants in the leader and basic treatment groups. Table A3 complements these results using survey data and focusing on self-reported awareness of the program and the targeted group, as well as perceived involvement in the program. The estimates provided in the table are the basis for Figure 1 in the main text. Panel A includes all data points for surveys, while panel B restricts the sample to midline surveys.

Figure A4: Take-up of the integration program



Note. Statistics based on administrative data from the implementation of the program. Panel A reports the number of visits received by each sampled migrant, by treatment group. Panel B reports the share of sampled migrants that participated in the visit during each round of visits. In Round 1, we removed from the sample the migrants that were listed during the second wave of listing because they could have not participated in the round of visits. Panels C and D provide estimates of treatment effects based on OLS regressions using equation 1 applied to stacked post-intervention observations or separately for the midline and endline observations. The outcome variables in panels C and D are described in Appendix C. All specifications include strata fixed effects and block- and individual-level controls (see Section 5). Standard errors are clustered at the block level; 90% confidence intervals are reported. Variable definitions are provided in Appendix C. Details about the program and its implementation are provided in Section 3.

Table A2: Determinants of take-up in the leader and basic treatment groups

	Visit attendance			Number of visits attended		
	(1)	(2)	(3)	(4)	(5)	(6)
Age	0.00		-0.00	0.00*		0.00*
	(0.00)		(0.00)	(0.00)		(0.00)
Male	-0.00		-0.00	-0.00		-0.00
	(0.01)		(0.01)	(0.01)		(0.01)
Primary schooling	-0.07***		-0.07***	-0.09***		-0.09***
	(0.01)		(0.01)	(0.01)		(0.01)
Muslim	-0.01		-0.02	-0.03		-0.03
	(0.02)		(0.02)	(0.02)		(0.02)
Other religion	-0.00		-0.00	-0.01		-0.01
	(0.02)		(0.02)	(0.01)		(0.01)
Second listing wave	0.19***		0.18***	0.16***		0.16***
	(0.01)		(0.01)	(0.01)		(0.01)
Block population	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Age leader		-0.00	-0.00		-0.00*	-0.00
		(0.00)	(0.00)		(0.00)	(0.00)
Male leader		0.03	0.02		0.00	-0.00
		(0.02)	(0.02)		(0.02)	(0.02)
Primary schooling leader		-0.02	-0.02		-0.04*	-0.04*
		(0.02)	(0.02)		(0.02)	(0.02)
Muslim leader		0.02	0.02		-0.00	0.00
		(0.03)	(0.03)		(0.02)	(0.02)
Other religion leader		-0.00	-0.01		-0.01	-0.02
		(0.02)	(0.02)		(0.02)	(0.02)
Observations	2404	2391	2391	2404	2391	2391
Adjusted R^2	0.172	0.095	0.173	0.177	0.098	0.180
Controls:						
Household/block characteristics	Yes	-	Yes	Yes	-	Yes
Block leader characteristics	-	Yes	Yes	-	Yes	Yes

Note. Estimates based on OLS regressions with outcomes measured at baseline and the sample restricted to observations in the leader and basic treatment groups. Dependent variables by column: (1)-(3) *visit attendance* is an indicator variable for whether the respondent has received and attended a visit from the integration program; (4)–(6) *number of visits attended* is the total number of visits received and attended. Standard errors are clustered at the block level. Details about the program and its implementation are provided in Section 3.

Table A3: Awareness and program participation

	Leaders			Migrants			Residents			Origin contacts	
	Aware of the program	Knows targeted group	Involved	Aware of the program	Knows targeted group	Involved	Aware of the program	Knows targeted group	Involved	Aware of the program	Family member is involved
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
A. Pooled observations											
(TL) Leader treatment	0.138***	0.132***	0.155***	0.076***	0.053***	0.068***	-0.002	0.044*	-0.014	0.036**	0.015
	(0.029)	(0.032)	(0.031)	(0.014)	(0.016)	(0.016)	(0.023)	(0.022)	(0.024)	(0.017)	(0.012)
(TB) Basic treatment	-0.017	-0.032	0.015	0.088***	0.039**	0.074***	-0.031	0.021	-0.038	0.008	0.011
	(0.031)	(0.032)	(0.032)	(0.013)	(0.016)	(0.016)	(0.023)	(0.021)	(0.024)	(0.017)	(0.012)
Observations	846	846	976	6118	6110	7264	1574	1574	1990	3874	5038
Adjusted R^2	0.188	0.225	0.190	0.180	0.114	0.164	0.361	0.165	0.203	0.099	0.097
Mean (control group)	0.729	0.329	0.408	0.703	0.281	0.519	0.592	0.199	0.379	0.219	0.142
TL = TB (p-value)	0.000	0.000	0.000	0.383	0.360	0.715	0.208	0.304	0.283	0.123	0.789
B. Pre-election observations											
(TL) Leader treatment	0.191***	0.116**	0.166***	0.203***	0.073***	0.169***	0.010	0.099***	-0.020	0.093***	0.033***
	(0.058)	(0.049)	(0.049)	(0.026)	(0.020)	(0.024)	(0.043)	(0.030)	(0.031)	(0.023)	(0.012)
(TB) Basic treatment	-0.054	-0.039	0.015	0.200***	0.056***	0.159***	-0.023	0.020	-0.047	0.054**	0.020
	(0.060)	(0.043)	(0.051)	(0.025)	(0.020)	(0.024)	(0.041)	(0.026)	(0.032)	(0.022)	(0.012)
Observations	369	369	487	2855	2849	3632	753	753	995	1532	2518
Adjusted R^2	0.178	0.141	0.110	0.183	0.093	0.195	0.171	0.137	0.076	0.068	0.051
Mean (control group)	0.603	0.167	0.278	0.478	0.199	0.355	0.320	0.081	0.216	0.104	0.056
TL = TB (p-value)	0.000	0.002	0.004	0.899	0.430	0.640	0.429	0.005	0.388	0.098	0.318

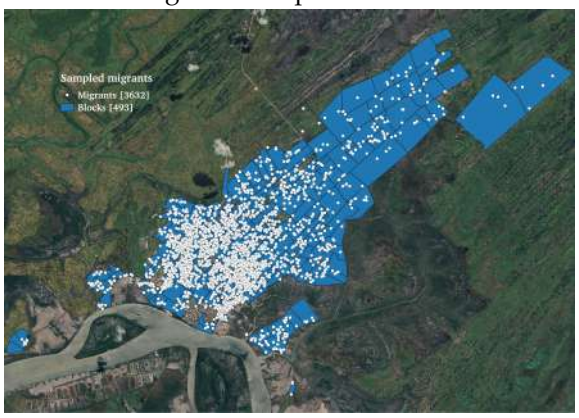
Note. Treatment effect estimates from OLS regressions based on equation 1. Panel A reports results using stacked post-intervention observations. Panel B reports results from the pre-election survey wave. Columns (1) to (3) present results for block leaders; Columns (4) to (6) report results for sampled migrants; Columns (7) to (9) report result for sampled residents; Columns (10) to (11) report results for migrant's origin contacts. Outcome variables are: (1), (4), (7), (10) *aware of the program* is an indicator variable equal to 1 if the respondent has heard about the program "Quelimane trabalha com todos", and 0 otherwise; (2), (5), (8) *knows targeted group* is an indicator variable equal to 1 if the respondent reports that rural migrants were involved in the program, and 0 otherwise; (3), (6), (9), (11) *involved* is an indicator variable equal to 1 if the respondent reports having been involved in the program or if a family member was involved in the program, and 0 otherwise (in column (11), the outcome refers to family members only). All specifications include strata fixed effects and block- and individual-level controls (see Section 5). Standard errors, reported in parentheses, are clustered at the block level. Variable definitions are provided in Appendix C.

B Sampling and randomization

The sampling procedure for rural migrants is described in Section 3. Panel A in Figure B1 displays the geographical distribution of the sampled migrants. Randomization was performed within strata composed of up to three blocks. Two variables were used to compute the stratification metric. First, the neighborhood of the block, i.e, the administrative division containing groups of blocks defined by the municipality. The city comprises a total of 54 neighborhoods. Second, the number of migrants in the block, defined by the number of migrants in the first round of listing. Panel B in Figure B1 shows the treatment allocation by block. Tables B1–B3 provide summary statistics and balance checks for the observable characteristics of all actors. Table B4 provides tests for selective attrition by treatment. We do not observe any significant differences in attrition rates across treatment arms.

Figure B1: Sampling and treatment allocation

A. Migrants sampled at baseline



B. Treatment allocation by block



Note. Panel A shows the location of sampled migrants sampled. Panel B shows the treatment allocation for each block. See Section 3.2 for details on how sampling and treatment allocation.

Table B1: Descriptive statistics – blocks and block leaders

	Mean (control)	Any treatment	TL	TB	*p*-value	N
	(1)	(2)	(3)	(4)	(5)	(6)
A. Block characteristics						
Block population	143.80	-12.83	-5.36	-20.34**	0.09	493
	[117.45]	(8.05)	(8.44)	(9.78)		
# sampled migrants	4.78	-0.08	-0.00	-0.16	0.22	493
	[2.63]	(0.08)	(0.10)	(0.10)		
Ilegal construction	0.42	0.02	0.02	0.02	0.89	482
	[0.50]	(0.04)	(0.05)	(0.05)		
# of taxi drivers	25.34	-8.06	-7.22	-8.97*	0.21	418
	[96.44]	(5.83)	(7.06)	(5.28)		
Distance to school	1.61	-0.05	-0.07	-0.03	0.72	483
	[0.84]	(0.07)	(0.08)	(0.08)		
Distance to water fountain	1.55	-0.01	-0.12	0.11	0.12	465
	[1.16]	(0.10)	(0.12)	(0.12)		
B. Leader characteristics						
Age	49.73	-1.38	-0.85	-1.95	0.37	455
	[12.10]	(1.12)	(1.27)	(1.38)		
Male	0.67	-0.02	-0.03	-0.01	0.82	441
	[0.47]	(0.05)	(0.05)	(0.06)		
Married/cohabiting	0.71	0.00	0.02	-0.02	0.72	482
	[0.45]	(0.04)	(0.05)	(0.05)		
Catholic	0.67	-0.02	-0.01	-0.03	0.84	482
	[0.47]	(0.05)	(0.05)	(0.05)		
Literate	0.78	-0.03	-0.00	-0.06	0.41	441
	[0.42]	(0.04)	(0.05)	(0.05)		
Primary schooling	0.56	0.02	0.00	0.03	0.82	455
	[0.50]	(0.05)	(0.06)	(0.06)		
Own dwelling	0.95	0.01	-0.00	0.02	0.78	441
	[0.23]	(0.02)	(0.03)	(0.03)		
Years in position (leader)	3.59	0.32	0.16	0.48	0.61	441
	[3.99]	(0.41)	(0.47)	(0.49)		
Aims at higher office position	0.55	-0.05	-0.04	-0.05	0.65	432
	[0.50]	(0.05)	(0.06)	(0.06)		
Participated in a rally/campaign	0.70	-0.09*	-0.12**	-0.06	0.12	439
	[0.46]	(0.05)	(0.06)	(0.06)		
Assists citizens with voting	0.31	-0.03	-0.01	-0.05	0.61	432
	[0.46]	(0.05)	(0.06)	(0.05)		
Likes migrants	0.77	0.06	0.04	0.08	0.27	431
	[0.42]	(0.04)	(0.05)	(0.05)		
Gov. is helping the poor	0.02	0.02	0.01	0.02	0.58	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 B2: Descriptive statistics – migrants and residents

	Mean (control)	Any treatment	TL	TB	*p*-value	N
	(1)	(2)	(3)	(4)	(5)	(6)
A. Migrant characteristics						
Age	24.30	-0.20	-0.20	-0.20	0.77	3582
	[8.40]	(0.28)	(0.32)	(0.32)		
Male	0.66	-0.03*	-0.03	-0.02	0.24	3632
	[0.48]	(0.02)	(0.02)	(0.02)		
Married/cohabiting	0.37	0.00	-0.00	0.01	0.89	3626
	[0.48]	(0.02)	(0.02)	(0.02)		
Number of children	1.16	-0.04	-0.02	-0.05	0.69	3507
	[1.68]	(0.05)	(0.06)	(0.06)		
Catholic	0.59	0.02	0.03	0.02	0.34	3515
	[0.49]	(0.02)	(0.02)	(0.02)		
Literate	0.66	0.02	0.03	0.01	0.35	3609
	[0.47]	(0.02)	(0.02)	(0.02)		
Primary schooling	0.53	-0.01	-0.02	0.00	0.63	3628
	[0.50]	(0.02)	(0.02)	(0.02)		
No occupation	0.31	0.03*	0.06**	0.01	0.04	2312
	[0.46]	(0.02)	(0.02)	(0.02)		
Visited district since migration	0.25	-0.00	0.01	-0.01	0.71	3626
	[0.43]	(0.02)	(0.02)	(0.02)		
Moved to work	0.50	-0.01	-0.02	-0.00	0.73	3632
	[0.50]	(0.02)	(0.02)	(0.02)		
Moved for family	0.25	0.01	0.00	0.01	0.78	3625
	[0.43]	(0.02)	(0.02)	(0.02)		
Struggling w/ finding a job	0.33	0.04*	0.04*	0.03	0.17	3632
	[0.47]	(0.02)	(0.02)	(0.02)		
Struggling w/ financial constraints	0.18	-0.01	-0.00	-0.01	0.81	3632
	[0.38]	(0.01)	(0.02)	(0.02)		
Contacted local leader	0.24	0.04	0.05	0.03	0.58	2105
	[0.63]	(0.04)	(0.05)	(0.04)		
B. Resident characteristics						
Age	34.25	-0.71	-0.69	-0.73	0.73	991
	[14.68]	(0.88)	(1.01)	(1.00)		
Male	0.40	0.03	0.05	0.02	0.44	995
	[0.49]	(0.03)	(0.04)	(0.04)		
Married/cohabiting	0.46	0.05	0.04	0.06*	0.20	993
	[0.50]	(0.03)	(0.03)	(0.03)		
Number of children	2.33	-0.11	-0.13	-0.08	0.69	994
	[2.25]	(0.13)	(0.15)	(0.16)		
Catholic	0.63	0.01	0.02	-0.00	0.77	991
	[0.48]	(0.03)	(0.04)	(0.04)		
From Quelimane	0.61	0.01	0.03	-0.01	0.45	995
	[0.49]	(0.03)	(0.04)	(0.04)		
Literate	0.82	0.03	0.05*	0.01	0.18	978
	[0.39]	(0.02)	(0.03)	(0.03)		
Primary schooling	0.33	-0.02	-0.01	-0.03	0.57	994
	[0.47]	(0.03)	(0.03)	(0.03)		
Contacted local leader	0.40	0.13**	0.12*	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 B3: Descriptive statistics – origin contacts

	Mean (control)	Any treatment	TL	TB	p-value	N
	(1)	(2)	(3)	(4)	(5)	(6)
Age	33.21	-0.82**	-0.50	-1.10***	0.02	2252
	[8.57]	(0.34)	(0.41)	(0.38)		
Male	0.01	0.00	0.00	0.00	0.55	2321
	[0.10]	(0.00)	(0.01)	(0.00)		
Married/cohabiting	0.51	-0.00	0.00	-0.01	0.85	2289
	[0.50]	(0.02)	(0.02)	(0.02)		
Number of children	2.24	0.10	0.31*	-0.09	0.03	1535
	[2.57]	(0.15)	(0.16)	(0.17)		
Catholic	0.62	0.00	-0.00	0.01	0.89	2286
	[0.49]	(0.02)	(0.03)	(0.02)		
Literate	0.89	-0.00	0.00	-0.00	0.95	1539
	[0.31]	(0.01)	(0.02)	(0.02)		
Primary schooling	0.32	0.00	0.02	-0.01	0.31	2383
	[0.47]	(0.02)	(0.02)	(0.02)		
Primary occupation: none	0.17	0.01	0.02	0.01	0.71	2383
	[0.37]	(0.02)	(0.02)	(0.02)		
Primary occupation: student	0.07	0.01	0.01	0.01	0.45	2383
	[0.25]	(0.01)	(0.01)	(0.01)		

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: Attrition rates and selective attrition, by survey round and sample

	Midline				Endline				Follow-up	
	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: dummy variable equal to 1 if the respondent was not found at the follow-up; (8) Attrition from baseline to follow-up: dummy variable equal to 1 if the respondent was not found at the follow-up. All specifications include block and individual controls, as well as strata fixed effects. Section 5 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.

C Definition of outcome variables

Table C1 provides a detailed definition of all outcome variables used in the paper.

Table C1: Definition of outcome variables

Topic	Variable name and description
Awareness and program participation	**Aware of the program.** Indicator equal to 1 if the respondent reports having heard of the program "Quelimane trabalha com todos," without being given any description, and zero otherwise. Self-reported. Collected in the midline and endline surveys, but not collected at baseline.
	Knows targeted group. Indicator equal to 1 if the respondent reports that rural migrants were involved in the program, and zero otherwise. Asked conditional on reporting awareness of the program and set to zero otherwise. Self-reported. Collected in the midline and endline surveys, but not collected at baseline.
	Involved. Indicator equal to 1 if the respondent or a family member reports having been involved in the program, and zero otherwise. Asked conditional on program awareness and set to zero otherwise. Self-reported. Collected in the midline and endline surveys, but not collected at baseline.
	Program take-up. We build two indicators using administrative data from program implementation. *Visit attendance* is an indicator variable for whether the respondent has received and attended a visit since the previous survey interview. *Share of visits attended* is the (cumulative) share of visits received and attended at the time of the survey (the share is computed out of 4 visits at midline and out of 5 visits at endline). The share is adjusted accounting for the fact that migrants listed in the first wave of listings could have attended 5 visits, while those listed in the second wave of listings could have attended 4 visits.
Migration	**Migrant remained in the city.** Indicator equal to 1 if the migrant respondent is still living in Quelimane at the time of the second post-baseline survey wave, and zero otherwise. Constructed from field records and survey responses.
	Origin contact intends to migrate. Indicator equal to 1 if an origin contact reports being *"very likely"* or *"likely"* to move to Quelimane within the next year, and zero otherwise. Based on a four-category self-reported likelihood question. Collected in the midline and endline surveys, but not collected at baseline.
	Origin contact migrated to the city. Indicator equal to 1 if the origin contact lives in Quelimane at the time of the survey. This is measured at the follow-up survey, one year after the end of the program.
Financial inclusion	**Mobile money adoption.** Number of mobile money services used by the respondent, constructed by summing selected options from a multiple-response question. Collected in the first post-baseline survey wave only. Not collected at baseline.
Employment	**Any job opportunity.** Indicator equal to 1 if the respondent reports having heard of a job opportunity in the previous 12 months, and zero otherwise. Self-reported. Collected in the midline and endline surveys, but not collected at baseline.
	Worked. Indicator equal to 1 if the respondent worked on the day prior to the interview, constructed using information from self-reported time use. Collected at baseline (only for migrants listed in the first wave of listing), midline and endline.
	Hours worked. Total hours worked on the day prior to the interview, constructed using information from self-reported time use. Collected at baseline (only for migrants listed in the first wave of listing), midline and endline.

(continued on next page)

Topic	Variable name and description
Interaction between leaders and citizens	**Leader's personal ties to migrants in the block**. We consider two measures. First, *any tie* is an indicator equal to 1 if the leader reports personally knowing at least one migrant living in the block, and zero otherwise. Self-reported. Collected in the midline and endline surveys, but not collected at baseline. Second, the *share known* is the share of sampled migrants in the block whose names the leader identifies as personally known from a pre-filled list. The variable ranges from 0 to 1. Collected in the midline and endline surveys. Not collected at baseline.
	Citizen interaction with block leaders. We consider three measures. First, *knows the leader* is an indicator equal to 1 if the respondent correctly names the current block leader, and zero otherwise. Leader names were verified against field records. Self-reported. Collected in the first post-baseline survey wave only due to a field error. Not collected at baseline. Second, *contacted the leader* is an indicator equal to 1 if the respondent reports having contacted the block leader between survey waves, and zero otherwise. Self-reported. Collected in the midline and endline surveys, but not collected at baseline. Third, *sought the leader for job assistance* is an indicator equal to 1 if the respondent reports having contacted or paid the leader in the previous year to obtain a job, and zero otherwise. Self-reported. Collected in the midline and endline surveys, but not collected at baseline.
Mobilization, political participation and voting	**Mobilization of citizens for block meeting**. We consider two measures based on the behavioral mobilization exercise described in Section 4. *Reported capacity* is an indicator equal to 1 if the list provided by the leader contains at least one verified bicycle taxi driver eligible for mobilization in political rallies, and zero otherwise. *Realized* is the count of bicycle taxi drivers who attended the field team's confirmation visit following leader mobilization. Constructed from attendance rosters. These variables are collected only in the endline survey. Panel A in Figure C1 provides an example.
	Express support for the incumbent. We consider two measures. First, *observed stickers distributed by the leader* is the share of the 40 brown stickers distributed to the leader that were observed hanging on doors in the block two weeks later. Second, *observed stickers distributed by field team* is the share of pink stickers observed hanging on doors relative to the total number of sampled respondents (migrants and residents) in the block. These variables are collected only in the endline survey. Panels B in Figure C1 shows the stickers that were distributed to block leaders.
	Display of partisan political objects. Indicator equal to 1 if the field administrator observed any political objects (e.g., hats, t-shirts, posters, pins) in the respondent's dwelling, and zero otherwise. Observational measure collected in the midline and endline surveys, but not collected at baseline.
	Turnout. Indicator equal to 1 if the respondent's finger was marked with purple ink during post-election visits, and zero otherwise. Based on direct observation following the 2023 municipal elections, collected in October 2023.
	Self-reported voting for national incumbent (FRELIMO). Indicator equal to 1 if the respondent reports having voted for FRELIMO in the 2023 municipal elections, and zero otherwise. Missing for respondents who report not having voted. Collected in the endline survey.

(continued on next page)

Topic	Variable name and description
Leader behavior in the allocation of funds	These variables relate to the fund allocation SCA described in Section 6.4. *Chose assistant over public good* is an indicator variable equal to 1 if the leader chose the assistant in the allocation of funds activity, and 0 otherwise. *Selected person is a relative* is an indicator variable equal to 1 if the leader selected the assistant and the selected person is a relative, and 0 otherwise. *Selected person is a previous block leader* is an indicator variable equal to 1 if the leader selected the assistant and the selected person is someone who previously held a leadership position in the block, and 0 otherwise. *Selected person is helping with fund allocation* is an indicator variable equal to 1 if the leader selected the assistant and the selected person is assigned a task to help the leader with the allocation of funds within the block, and 0 otherwise. Panels C and D in Figure C1 provides examples of the activities carried out during the SCA.

Note. Refer to Appendix A.1 for details on the timing of measurements.

Figure C1: Examples from SCAs related to mobilization and fund allocation

A. Mobilized cyclists for political rally



B. Sticker distributed by leaders



C. Formalization of the assistant



D. Cleaning of the block



Note. Panel A shows an example for the mobilization for block meeting activity. Panels B shows the stickers that were distributed to the block leaders. The same sticker, but with a pink background was distributed to migrants by the field team. Panel C provides an example of the formalization of the assistant in the fund allocation SCA. Panel D provides an example of the cleaning of the block in the same SCA. Details about the measurement are provided in Table C1.

D Additional analysis

D.1 Detailed results on political objects

Table D1 shows the treatment effects on the holding of partisan political objects. In addition to the two main parties (RENAMO and FRELIMO), we also consider the Movimento Democrático de Moçambique (MDM), an opposition political party founded in 2009 from a split within RENAMO, and positioned as a reformist, centrist party with a strong urban base.

Table D1: Display of partisan political objects, by political party

	Migrants			Residents		
	RENAMO	FRELIMO	MDM	RENAMO	FRELIMO	MDM
	(1)	(2)	(3)	(4)	(5)	(6)
(TL) Leader treatment	0.010**	0.006	-0.000	0.011	-0.010	-0.004*
	(0.005)	(0.004)	(0.001)	(0.010)	(0.011)	(0.003)
(TB) Basic treatment	0.017***	0.005	-0.001	-0.006	-0.001	-0.000
	(0.006)	(0.004)	(0.001)	(0.010)	(0.011)	(0.003)
Observations	6117	6117	6117	1571	1571	1571
Adjusted R^2	0.044	0.027	0.007	0.081	0.002	0.022
Mean (control group)	0.030	0.017	0.003	0.045	0.039	0.006
TL = TB (p-value)	0.231	0.723	0.569	0.117	0.465	0.167

Note. Estimates are based on OLS regressions. The specification follows columns (1)–(2) of Table 4, with outcomes disaggregated by party. Additional details about the dependent variables are presented in Appendix C. All specifications include block and individual controls, as well as strata fixed effects. Section 5 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.

D.2 Views and attitudes about migrants

Table D2 shows the treatment impacts on self-reported views about migrants. Columns (1)–(3) focus on whether migrants are unfairly treated, while columns (4)–(6) focus on whether migrants are considered positive for the community.

Table D2: Views about migrants

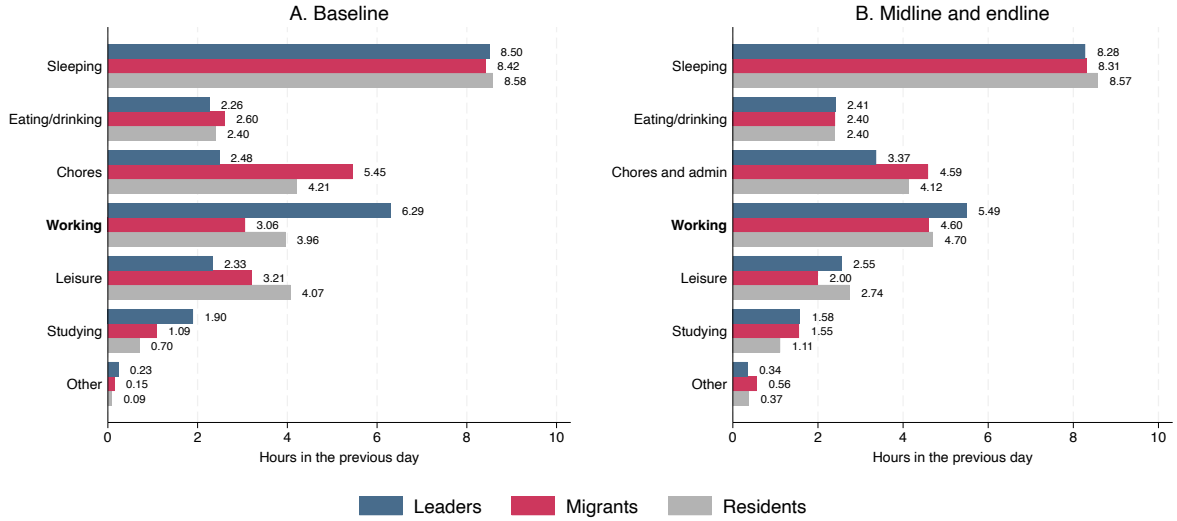
	Migrants unfairly treated			Migrants are positive		
	Leader (1)	Migrant (2)	Resident (3)	Leader (4)	Migrant (5)	Resident (6)
(TL) Leader treatment	0.095**	0.021	0.015	0.095	-0.015	0.131***
	(0.048)	(0.017)	(0.022)	(0.064)	(0.026)	(0.044)
(TB) Basic treatment	0.023	0.008	0.009	0.087	-0.008	0.034
	(0.049)	(0.019)	(0.021)	(0.065)	(0.026)	(0.046)
Observations	347	1623	704	357	2747	721
Adjusted R^2	0.089	0.047	0.021	0.111	0.090	0.086
Mean (control group)	0.109	0.083	0.037	0.496	0.415	0.401
TL = TB (p-value)	0.161	0.499	0.812	0.897	0.791	0.033

Note. Estimates based on OLS regressions. All columns use equation 1, and include data from the midline survey. 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 C1. All specifications include block and individual controls, as well as strata fixed effects. Section 5 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.

D.3 Time use

Figure D1 provides descriptive statistics for time use on the day prior to the interview for leaders, migrants, and residents. Time use was collected by asking the respondents to report the activities performed during each of the 24 hours of the day. This information provides the basis for computing whether a respondent worked and for how many hours, which are discussed in Section 6.1. Table D3 shows the treatment effects on the probability of working and on the number of hours worked among leaders and residents.

Figure D1: Time use



Note. *Leisure* includes spare time, time for personal care, and time with family or friends. *Other* includes administrative tasks, community activities, and time spent commuting or traveling. Details about the measurement and how the samples were selected are provided in Section 4.

Table D3: Employment among different actors

	Gained job	Lost job	Total hours working	Leader		Resident	
	Migrant	Migrant	Migrant	Worked	Hours worked	Worked	Hours worked
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(TL) Leader treatment	0.036***	-0.026***	110.512*	0.022	0.166	0.013	0.165
	(0.012)	(0.008)	(61.008)	(0.037)	(0.360)	(0.031)	(0.283)
(TB) Basic treatment	0.004	-0.000	-91.332	0.003	0.144	0.017	0.196
	(0.012)	(0.008)	(61.165)	(0.040)	(0.385)	(0.031)	(0.294)
Observations	6114	6114	6114	846	846	1574	1574
Adjusted R^2	0.185	0.075	0.226	0.119	0.170	0.053	0.065
Mean (control group)	0.115	0.079	2555.429	0.643	5.292	0.573	4.633
TL = TB (p-value)	0.007	0.001	0.001	0.632	0.954	0.895	0.919
Outcome data	Pooled	Pooled	Pooled	Pooled	Pooled	Pooled	Pooled

Note. Estimates based on OLS regressions. All columns use equation 1, employing the midline and endline (stacked regressions). We include the lagged values of any of the presented variables. Columns (1)-(3) display results for migrants; (4) and (6) display results for leaders; columns (5) and (7) display results for residents. Dependent variables by column: (1) , and 0 otherwise; (2) , and 0 otherwise; (3) , and 0 otherwise; (4)-(5) *Worked*: is an indicator variables for whether the respondent reports having worked in the previous day, and 0 otherwise; (6)-(7) *Hours worked*: is a variable counting the number of hours in the previous day that the respondent reported being working. Additional details about the dependent variables are in the Appendix in Table C1. All specifications include block and individual controls, as well as strata fixed effects. Section 5 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.

D.4 Monetary transfers and remittances

Table D4 shows the treatment effects on the probability of sending or receiving transfers among migrants and their origin contacts. Information about migrants is available only at midline, while for origin contacts, it is available in both post-intervention surveys.

Table D4: Monetary transfers from/to migrants

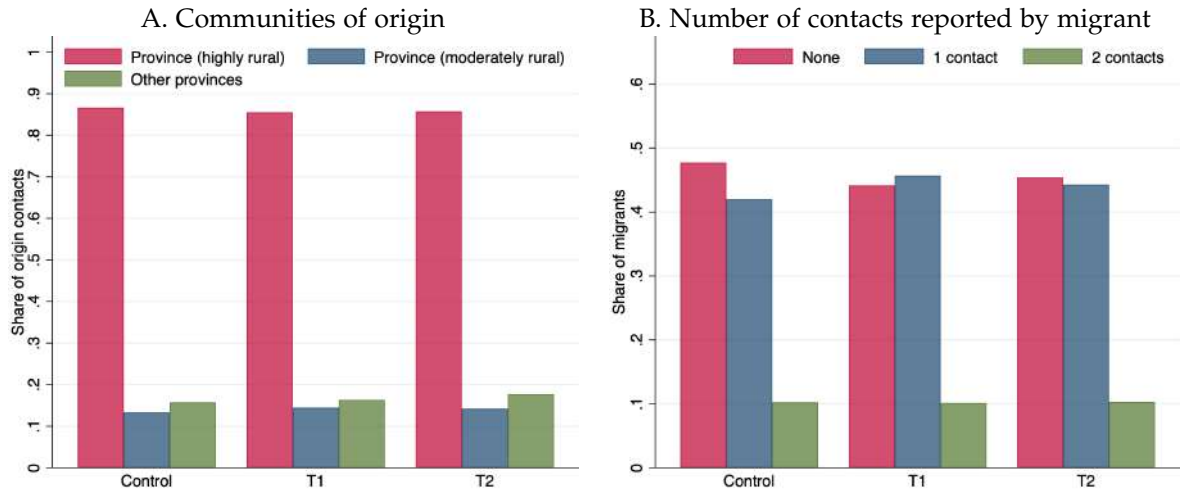
	Migrants → outside the city (reported by migrants)		Origin contacts → outside their district (reported by origin contacts)			
	Sent	Received	Sent	Received	Sent	Received
	(1)	(2)	(3)	(4)	(5)	(6)
(TL) Leader treatment	0.021	0.007	0.023	0.069*	-0.017	-0.029
	(0.019)	(0.018)	(0.040)	(0.038)	(0.028)	(0.034)
(TB) Basic treatment	0.050**	0.011	0.046	0.074*	0.005	-0.012
	(0.020)	(0.019)	(0.040)	(0.038)	(0.028)	(0.031)
Observations	2856	2856	986	990	1575	1574
Adjusted R^2	0.061	0.049	0.077	0.018	0.194	0.172
Mean (control group)	0.242	0.183	0.486	0.418	0.481	0.460
TL = TB (p-value)	0.155	0.870	0.591	0.906	0.453	0.612
Outcome data	Midline	Midline	Midline	Midline	Endline	Endline

Note. Estimates based on OLS regressions using equation 1. Columns (1)–(4) present results at midline, columns (5)–(6) present results for the endline. We did not collect lagged values of any of the presented variables. Columns (1)–(2) display results for migrants, who were asked report whether they sent or received transfers with family or friends living outside the city in the previous 30 days. Columns (3)-(6) display results for origin contacts, who were asked to report whether they sent or received transfers with family or friends living outside their own district. Dependent variables by column: (1), (3) and (5) *sent* is an indicator variable equal to 1 if the respondent has sent any mobile money transfers in the previous 30 days, and 0 otherwise; (2), (4) and (6) *received* is an indicator variable equal to 1 if the respondent has received any mobile money transfers in the previous 30 days, and 0 otherwise. Additional details about the dependent variables are in the Appendix in Table C1. All specifications include block and individual controls, as well as strata fixed effects. Section 5 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.

D.5 Origin communities of migrants

Figure D2 provides descriptive statistics on the origin of the sampled migrants and the sample of origin contacts (see Section 4 for details about the sampling). For comparison, we report the distribution for each treatment arm. Panel A focuses on the district of origin of migrants, distinguishing between highly rural and moderately rural districts within the same province, as well as districts in other provinces. Panel B focuses on the number of contacts reported by each migrant. Table D5 shows the observable determinants of reporting origin contacts among baseline migrants and the number of contacts reported. Differences are due to the fact that not all migrants have contacts in their origin community that can be reached over the phone. In addition, migrants who are male, more educated, not Muslim, and who moved to the city more recently (as captured by being listed in the second wave of listings) have a higher chance of reporting an origin contact. To test whether these differences influence our main results, in Table D6, we estimate treatment effects on the outcomes for origin contacts, weighting each regression for the inverse of the number of listed origin contacts per migrant (i.e., weighting down observations in which a migrant reported two contacts). Results are robust to this test.

Figure D2: Migrants' origin and contacts



Note. Panel A was constructed using data from the baseline survey wave with sampled migrants. *Province* indicates districts within the same province of Quelimane (Zambezia province). *Highly rural* indicates districts with population densities lower than 99 per km^2. *Moderately rural* indicates districts with population densities between 100 and 299 per km^2. *Other provinces* include all provinces in Mozambique except Zambezia. Panel B shows the number of origin contacts reported by each sampled migrant. Details about how the sample was selected are provided in Section 4.

Table D5: Determinants of reporting origin contacts

	Gave contact (1)	Number of contacts (2)
Age	-0.00	-0.00
	(0.00)	(0.00)
Male	0.08***	0.09***
	(0.02)	(0.02)
Primary schooling	-0.16***	-0.22***
	(0.02)	(0.02)
Muslim	-0.06**	-0.05
	(0.03)	(0.04)
Other religion	0.03	0.04*
	(0.02)	(0.02)
Second listing wave	0.13***	0.19***
	(0.02)	(0.02)
Block population	-0.00	-0.00
	(0.00)	(0.00)
Observations	3632	3632
Adjusted R^2	0.138	0.166

Note. Estimates based on OLS regressions with outcomes measured at baseline and the sample restricted to observations in the leader and basic treatment groups. Dependent variables by column: (1)-(3) *visit attendance* is an indicator variable for whether the respondent has received and attended a visit from the integration program; (4)–(6) *number of visits attended* is the total number of visits received and attended. Standard errors are clustered at the block level. Details about the program and its implementation are provided in Section 3.

Table D6: Robustness to selection of origin contacts - weighted regressions

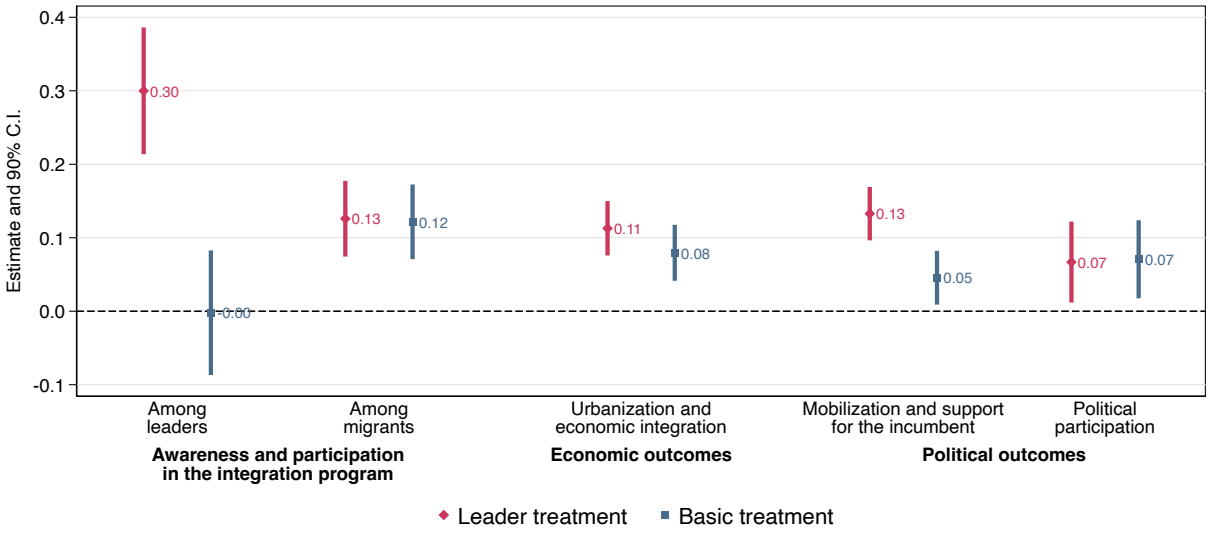
	Figure 1		**Table 1**		**Table 4**
	Awareness of the program	Family member is involved	Intends to migrate	Migrated to the city	Voted FRELIMO
	(1)	(2)	(3)	(4)	(5)
(TL) Leader treatment	0.041**	0.007	0.038**	0.028*	-0.145**
	(0.019)	(0.007)	(0.015)	(0.016)	(0.058)
(TB) Basic treatment	0.007	-0.001	0.034**	0.046***	-0.097
	(0.019)	(0.007)	(0.014)	(0.015)	(0.060)
Observations	3403	3393	3325	1945	351
Adjusted R^2	0.110	0.040	0.037	0.019	0.137
Mean (control group)	0.220	0.026	0.110	0.094	0.223
TL = TB (p-value)	0.088	0.262	0.763	0.242	0.383
Outcome data	Pooled	Pooled	Pooled	Follow-up	Endline

Note. Estimates based on OLS regressions using equation 1, weighted using the inverse of the number of origin contacts per migrant in the dataset as weight. Refer to Appendix C for the definition of dependent variables. All specifications include block and individual controls (Section 5 provides the full list of controls), as well as strata fixed effects. Standard errors, reported in parentheses, are clustered at the block level. *** p<0.01, ** p<0.05, * p<0.1.

D.6 Outcome aggregation

We complement the analysis of individual outcomes with an analysis of aggregated outcomes. Because outcomes are measured for different actors with varying sample sizes and representativeness, aggregation is conducted at the block level. For each outcome, we standardize the variable using the mean and standard deviation of the control group in the relevant sample and period, yielding z-scores. We then average these standardized outcomes within each block and period, producing up to two post-baseline observations per block–outcome pair. Following Kling et al. (2007), we construct indices by summing all non-missing z-scores and estimate treatment effects using equation (1) at the block level. Because some indices combine outcomes from different actors without population weights, estimated effects should be interpreted as equally weighted average effects. Index composition and estimates are reported in Figure D3.

Figure D3: Outcome aggregation at block-level



Note. The figure provides estimates of treatment effects based on OLS regressions using equation 1 applied to stacked post-intervention observations (if data are available for both periods) or for the midline or endline observations. The outcome variables are indices built using the procedure described in Appendix D.6. Indices in the category of *awareness and participation in the integration program* include the variables presented in panel A and panel B of Figure 1. The index of *economic outcomes* includes all the outcomes presented in Table 1. For *political outcomes*, the index of *mobilization and support for the incumbent* includes all outcomes in Tables 2 and 3, as well as the self-reported voting for FRELIMO in Table 4. The index *political participation* includes the holding of partisan political objects and turnout during the municipal elections presented in Table 4. All specifications include strata fixed effects and block- and individual-level controls (see Section 5). In all specifications, individual-level controls are the average value within the block. Standard errors are clustered at the block level; 90% confidence intervals are reported.

D.7 Treatment intensity

Table D7 reports the IV estimates of the effect of program participation. Estimates are based on IV regressions where the endogenous take-up of the program (measured by the number of visits attended by a migrant) is instrumented with the treatment indicators (TL and TB). The specifications are otherwise similar to equation (1). The coefficients can be interpreted as local average treatment effects (LATE) of attending an additional program visit for compliers, i.e., migrants whose participation responds to the randomized assignment to the leader or basic treatment. These estimates capture the causal effect of program intensity among individuals induced to participate by the experimental design, rather than average effects for all eligible migrants.

Table D7: IV regressions with treatment intensity for outcomes of migrants

	Share of TL visits attended		**Share of TB visits attended**		Test of equality (p-value)	N
	Coeff.	S.E.	Coeff.	S.E.		
	(1)	(2)	(3)	(4)	(5)	(6)
Aware of the program	0.09***	0.03	0.11***	0.03	0.00	6118
Knows targeted group	0.06*	0.03	0.03	0.03	0.22	6110
Involved	0.07**	0.03	0.10***	0.03	0.01	7264
In the city	0.00	0.01	0.02	0.01	0.19	3590
Mobile money adoption	0.20	0.17	0.29*	0.17	0.21	2856
Job opportunities	0.09***	0.03	0.11***	0.03	0.00	6119
Worked	0.06**	0.03	0.00	0.03	0.05	6118
Hours worked	0.51*	0.27	-0.16	0.26	0.03	6118
Knows the leader	0.05	0.05	-0.08*	0.05	0.02	2856
Contacted the leader	0.05***	0.02	0.00	0.02	0.01	6119
Sought job assistance	0.02***	0.01	0.01	0.01	0.02	6093
Campaign exposure	0.02*	0.01	0.03**	0.01	0.04	6117
Voting FRELIMO	-0.01	0.02	-0.02	0.02	0.49	2094
Electoral turnout	0.05	0.03	0.03	0.03	0.31	3333

Note. Estimates based on IV regressions replacing treatment indicators in equation 1 with the number of visits attended for each program modality, instrumented with the treatment indicators. Dependent variables are the outcome variables for migrants presented in Tables 1–4. All specifications include block and individual controls (Section 5 provides the full list of controls), as well as strata fixed effects. Standard errors, reported in parentheses, are clustered at the block level. *** p<0.01, ** p<0.05, * p<0.1. Variable definitions are provided in Appendix C.

D.8 Heterogeneous treatment effects

Tables D8–D13 provide estimates of heterogeneous effects using pre-specified dimensions. Heterogeneous treatment effects are estimated using equation (1) and by interacting the treatment indicators with the variable measuring the dimension of heterogeneity. Each table presents the estimates of the main effects and the interacted effect.

Table D8: Heterogeneous effects by timing of migrants' arrival in the city

Outcome variable	TL		TL×months since arrival		TB		TB×months since arrival		N
	Coeff. (1)	S.E. (2)	Coeff. (3)	S.E. (4)	Coeff. (5)	S.E. (6)	Coeff. (7)	S.E. (8)	(9)
A. Migrants									
In the city	-0.00	0.00	0.00	0.00	-0.00	0.00	0.00	0.00	3234
MM adoption	0.20*	0.11	0.05	0.09	0.34***	0.11	0.17*	0.10	2833
Job opportunities	0.07***	0.01	-0.01	0.01	0.08***	0.01	-0.03*	0.02	6073
Worked	0.05***	0.02	0.02	0.02	0.00	0.02	-0.01	0.02	6072
Hours worked	0.43***	0.15	0.32**	0.15	-0.14	0.17	-0.01	0.14	6072
Knows the leader	0.06**	0.03	0.01	0.02	-0.04	0.03	-0.01	0.02	2833
Contacted the leader	0.04***	0.01	-0.01	0.01	0.01	0.01	-0.01	0.01	6073
Sought job assistance	0.01***	0.00	-0.01**	0.00	0.01**	0.00	-0.00	0.00	6047
Campaign exposure	0.02***	0.01	0.01	0.01	0.02***	0.01	-0.00	0.01	6071
Voting FRELIMO	-0.00	0.01	-0.00	0.02	-0.01	0.01	-0.02	0.02	2073
Electoral turnout	0.03*	0.02	-0.01	0.02	0.02	0.02	-0.02	0.02	3012
B. Origin contacts									
Intends to move	0.03**	0.01	-0.01	0.01	0.03**	0.01	-0.02	0.01	3652
Migrated to city	0.07***	0.02	-0.05***	0.02	0.04**	0.02	-0.00	0.02	2094
Voting FRELIMO	-0.08	0.06	-0.09	0.07	-0.08	0.06	-0.01	0.07	380

Note. Estimates based on OLS regressions using equation 1. *Months since arrival* is a continuous variable measuring the number of months since the migrant arrived in the city. The variable was standardized by subtracting the mean and dividing by the standard deviation. *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 5 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 D9: Heterogeneous effects by gender of the leader

Outcome variable	TL		TL×male		TB		TB×male		
	Coeff. (1)	S.E. (2)	Coeff. (3)	S.E. (4)	Coeff. (5)	S.E. (6)	Coeff. (7)	S.E. (8)	N (9)
A. Leaders									
Any tie to migrants	0.09**	0.04	-0.02	0.04	0.05	0.04	-0.03	0.05	773
% of migrants known	0.06***	0.02	-0.01	0.02	0.01	0.02	0.01	0.02	798
Reports cyclists	0.13**	0.05	0.01	0.06	0.12**	0.06	0.03	0.07	399
Observed # mobilized	0.70	0.42	-0.92*	0.52	0.84*	0.46	-0.05	0.58	399
Stickers (leaders)	0.04**	0.02	0.01	0.02	-0.02	0.02	0.01	0.02	399
Stickers (field team)	0.01	0.03	-0.01	0.03	-0.01	0.03	-0.00	0.03	399
Electoral turnout	0.02	0.04	0.05	0.05	0.01	0.04	0.09*	0.05	372
Chose assistant	-0.06	0.06	-0.04	0.08	-0.03	0.07	-0.10	0.08	397
B. Migrants									
In the city	0.01	0.01	-0.00	0.01	0.02*	0.01	0.01	0.01	3261
MM adoption	0.19*	0.10	-0.31**	0.13	0.26**	0.11	-0.20	0.13	2596
Job opportunities	0.08***	0.01	0.02	0.02	0.08***	0.01	0.02	0.02	5555
Worked	0.04**	0.02	0.04*	0.02	0.00	0.02	0.04	0.02	5554
Hours worked	0.42***	0.16	0.13	0.17	-0.07	0.18	0.25	0.19	5554
Knows the leader	0.03	0.02	-0.00	0.03	-0.02	0.03	0.03	0.03	2596
Contacted the leader	0.03***	0.01	-0.00	0.01	0.01	0.01	0.01	0.01	5555
Sought job assistance	0.01***	0.00	0.00	0.00	0.01**	0.00	-0.00	0.00	5533
Campaign exposure	0.01**	0.01	0.00	0.01	0.02***	0.01	0.01	0.01	5553
Voting FRELIMO	-0.00	0.01	0.03**	0.01	-0.02	0.01	0.04**	0.02	1920
Electoral turnout	0.02	0.02	-0.00	0.02	0.03	0.02	0.02	0.02	3038
C. Residents									
Knows the leader	0.07	0.05	-0.00	0.05	-0.02	0.05	0.02	0.06	670
Contacted the leader	0.05**	0.02	0.05*	0.03	-0.00	0.02	0.06**	0.03	1405
Sought job assistance	0.02**	0.01	0.00	0.01	0.00	0.01	0.00	0.01	1398
Campaign exposure	-0.01	0.02	0.04**	0.02	0.01	0.02	0.04*	0.02	1402
Voting FRELIMO	0.04	0.04	-0.07	0.04	0.04	0.04	-0.02	0.05	476
Electoral turnout	-0.03	0.04	-0.01	0.04	0.04	0.04	-0.03	0.04	721
D. Origin contacts									
Intends to move	0.03*	0.01	0.01	0.01	0.03**	0.01	-0.01	0.02	3445
Migrated to city	0.03**	0.02	0.02	0.02	0.03**	0.01	0.01	0.02	2020
Voting FRELIMO	-0.12**	0.05	0.11*	0.06	-0.07	0.05	-0.04	0.07	373

Note. Estimates based on OLS regressions using equation 1. *Male* is an indicator variable equal to one if the leader is male, and 0 if female. *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 5 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 D10: Heterogeneous effects by leader's age

	TL		TL×age		TB		TB×age		
Outcome variable	Coeff. (1)	S.E. (2)	Coeff. (3)	S.E. (4)	Coeff. (5)	S.E. (6)	Coeff. (7)	S.E. (8)	N (9)
A. Leaders									
Any tie to migrants	0.08**	0.04	0.07*	0.04	0.03	0.04	-0.02	0.04	820
% of migrants known	0.06***	0.02	-0.02	0.02	0.01	0.02	0.00	0.02	846
Reports cyclists	0.12**	0.05	0.01	0.06	0.08	0.05	0.05	0.06	444
Observed # mobilized	0.68*	0.40	-0.38	0.40	0.70*	0.41	-0.29	0.42	444
Stickers (leaders)	0.04**	0.02	0.01	0.02	-0.01	0.02	-0.00	0.02	444
Stickers (field team)	0.01	0.03	-0.07***	0.03	-0.01	0.03	-0.07**	0.03	444
Electoral turnout	0.04	0.03	0.10**	0.04	0.02	0.03	0.07**	0.04	412
Chose assistant	-0.08	0.06	-0.07	0.07	-0.06	0.06	0.03	0.07	442
B. Migrants									
In the city	0.00	0.01	-0.00	0.01	0.01	0.01	-0.00	0.01	3569
MM adoption	0.17*	0.09	0.15	0.13	0.27***	0.10	0.27**	0.13	2837
Job opportunities	0.07***	0.01	-0.00	0.01	0.08***	0.01	0.00	0.02	6081
Worked	0.05***	0.02	0.03	0.02	-0.00	0.02	0.04*	0.02	6080
Hours worked	0.41***	0.15	0.20	0.18	-0.16	0.17	0.21	0.20	6080
Knows the leader	0.06**	0.03	-0.02	0.03	-0.03	0.03	0.04	0.03	2837
Contacted the leader	0.04***	0.01	0.00	0.01	0.01	0.01	-0.01	0.01	6081
Sought job assistance	0.01***	0.00	0.00	0.00	0.01**	0.00	0.00	0.00	6055
Campaign exposure	0.02***	0.01	0.00	0.01	0.02***	0.01	0.00	0.01	6079
Voting FRELIMO	-0.00	0.01	-0.01	0.01	-0.01	0.01	0.00	0.02	2086
Electoral turnout	0.03	0.02	0.02	0.02	0.02	0.02	0.04*	0.02	3314
C. Residents									
Knows the leader	0.07	0.05	0.04	0.05	-0.04	0.05	0.01	0.05	743
Contacted the leader	0.05**	0.02	-0.01	0.02	-0.01	0.02	0.01	0.03	1554
Sought job assistance	0.01**	0.01	0.02**	0.01	0.00	0.01	0.01	0.01	1546
Campaign exposure	0.00	0.01	0.02	0.01	-0.00	0.01	0.04**	0.02	1551
Voting FRELIMO	0.06	0.04	-0.06	0.04	0.03	0.04	-0.06	0.04	531
Electoral turnout	-0.04	0.04	-0.01	0.04	0.04	0.03	-0.02	0.03	801
D. Origin contacts									
Intends to move	0.03***	0.01	-0.00	0.01	0.03**	0.01	-0.01	0.02	3767
Migrated to city	0.04***	0.01	-0.03*	0.01	0.04***	0.01	-0.03*	0.02	2206
Voting FRELIMO	-0.16***	0.05	0.11**	0.05	-0.09*	0.06	0.11	0.07	406

Note. Estimates based on OLS regressions using equation 1. *Leader's age* is a continuous variable for age of the leader in years. The variable was standardized by subtracting the mean and dividing by the standard deviation. *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 5 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 D11: Heterogeneous effects by leader's experience

	TL		TL×years in position		TB		TB×years in position		
Outcome variable	Coeff. (1)	S.E. (2)	Coeff. (3)	S.E. (4)	Coeff. (5)	S.E. (6)	Coeff. (7)	S.E. (8)	N (9)
A. Leaders									
Any tie to migrants	0.08**	0.04	-0.03	0.05	0.03	0.04	-0.09*	0.04	782
% of migrants known	0.06***	0.02	-0.03	0.02	-0.00	0.02	-0.00	0.02	807
Reports cyclists	0.14***	0.05	0.04	0.07	0.13**	0.06	0.05	0.06	399
Observed # mobilized	0.71*	0.42	0.20	0.64	0.84*	0.45	-0.46	0.40	399
Stickers (leaders)	0.04**	0.02	-0.02	0.02	-0.02	0.02	-0.02	0.02	399
Stickers (field team)	0.01	0.03	-0.02	0.04	-0.01	0.03	0.03	0.03	399
Electoral turnout	0.02	0.04	0.08*	0.05	0.02	0.04	0.01	0.04	372
Chose assistant	-0.06	0.06	-0.00	0.07	-0.03	0.07	0.08	0.06	397
B. Migrants									
In the city	0.01	0.01	0.00	0.01	0.02**	0.01	0.00	0.01	3284
MM adoption	0.17	0.10	-0.25*	0.14	0.22**	0.11	-0.03	0.11	2616
Job opportunities	0.08***	0.01	0.00	0.02	0.08***	0.01	-0.01	0.02	5597
Worked	0.04**	0.02	-0.03	0.02	0.00	0.02	-0.01	0.02	5596
Hours worked	0.44***	0.16	-0.22	0.21	-0.08	0.18	-0.23	0.16	5596
Knows the leader	0.04*	0.02	0.07**	0.03	-0.01	0.02	0.05*	0.03	2616
Contacted the leader	0.03***	0.01	0.02	0.02	0.01	0.01	0.01	0.01	5597
Sought job assistance	0.01***	0.00	0.01	0.01	0.01***	0.00	0.01	0.01	5575
Campaign exposure	0.02**	0.01	0.02*	0.01	0.02***	0.01	-0.00	0.01	5595
Voting FRELIMO	-0.00	0.01	0.02	0.02	-0.02*	0.01	0.03**	0.02	1925
Electoral turnout	0.03	0.02	0.01	0.02	0.03	0.02	0.01	0.02	3061
C. Residents									
Knows the leader	0.09*	0.05	0.11**	0.06	-0.00	0.05	0.00	0.04	676
Contacted the leader	0.05**	0.02	0.01	0.03	0.00	0.02	-0.04	0.02	1419
Sought job assistance	0.02**	0.01	-0.00	0.01	0.00	0.01	0.00	0.01	1412
Campaign exposure	-0.00	0.02	0.01	0.02	0.01	0.02	-0.02	0.02	1416
Voting FRELIMO	0.04	0.04	-0.02	0.05	0.04	0.04	-0.03	0.05	479
Electoral turnout	-0.03	0.04	-0.05	0.04	0.03	0.04	0.01	0.03	730
D. Origin contacts									
Intends to move	0.02*	0.01	-0.04**	0.02	0.03**	0.01	-0.03***	0.01	3471
Migrated to city	0.03**	0.01	-0.00	0.02	0.03**	0.01	-0.02	0.01	2038
Voting FRELIMO	-0.11**	0.05	-0.05	0.08	-0.05	0.05	-0.07	0.07	378

Note. Estimates based on OLS regressions using equation 1. *Years in position* is a continuous variable counting the number of years that the leader has been leading the block. The variable was standardized by subtracting the mean and dividing by the standard deviation. *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 5 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 D12: Heterogeneous effects by leader's support for incumbent's party

	TL		TL×declared supporter		TB		TB×declared supporter		
Outcome variable	Coeff. (1)	S.E. (2)	Coeff. (3)	S.E. (4)	Coeff. (5)	S.E. (6)	Coeff. (7)	S.E. (8)	N (9)
A. Leaders									
Any tie to migrants	0.07*	0.04	0.03	0.05	0.05	0.05	-0.04	0.05	820
% of migrants known	0.06***	0.02	-0.01	0.02	0.01	0.03	-0.01	0.03	846
Reports cyclists	0.07	0.06	0.12*	0.07	0.01	0.06	0.13**	0.06	444
Observed # mobilized	0.71*	0.38	-0.11	0.45	0.47	0.37	0.38	0.48	444
Stickers (leaders)	0.03*	0.02	0.02	0.02	-0.02	0.02	0.02	0.02	444
Stickers (field team)	-0.01	0.03	0.04	0.03	-0.04	0.03	0.05*	0.03	444
Electoral turnout	0.03	0.04	0.02	0.04	-0.02	0.04	0.07*	0.04	412
Chose assistant	-0.09	0.06	0.03	0.07	-0.13*	0.07	0.12*	0.07	442
B. Migrants									
In the city	-0.00	0.01	0.02**	0.01	0.01	0.01	0.01	0.01	3590
MM adoption	0.05	0.11	0.32***	0.12	0.17	0.11	0.21*	0.11	2856
Job opportunities	0.06***	0.02	0.03*	0.02	0.08***	0.02	0.01	0.02	6119
Worked	0.07***	0.02	-0.04*	0.02	-0.01	0.02	0.02	0.02	6118
Hours worked	0.48***	0.17	-0.10	0.20	-0.21	0.20	0.08	0.20	6118
Knows the leader	0.08***	0.03	-0.05*	0.03	-0.05	0.04	0.01	0.04	2856
Contacted the leader	0.04***	0.01	-0.01	0.01	0.02	0.01	-0.01	0.01	6119
Sought job assistance	0.01***	0.00	0.00	0.00	0.01**	0.00	0.00	0.00	6093
Campaign exposure	0.02**	0.01	0.00	0.01	0.02***	0.01	-0.00	0.01	6117
Voting FRELIMO	-0.00	0.02	-0.02	0.02	-0.02	0.02	0.01	0.02	2092
Electoral turnout	0.01	0.02	0.04*	0.02	-0.01	0.02	0.07***	0.02	3333
C. Residents									
Knows the leader	0.09*	0.05	-0.04	0.05	-0.08*	0.05	0.06	0.05	753
Contacted the leader	0.04*	0.02	0.02	0.02	-0.00	0.02	-0.00	0.03	1574
Sought job assistance	0.02**	0.01	-0.00	0.01	0.01	0.01	-0.00	0.01	1566
Campaign exposure	0.01	0.02	-0.01	0.02	-0.00	0.02	-0.00	0.02	1571
Voting FRELIMO	0.05	0.04	0.00	0.04	0.02	0.04	0.03	0.04	534
Electoral turnout	-0.07*	0.04	0.05	0.04	-0.01	0.04	0.07*	0.04	811
D. Origin contacts									
Intends to move	0.04**	0.02	-0.01	0.02	0.02	0.01	0.01	0.02	3792
Migrated to city	0.04***	0.02	0.01	0.02	0.05***	0.02	-0.02	0.02	2218
Voting FRELIMO	-0.20***	0.06	0.15**	0.06	-0.12*	0.06	0.09	0.06	406

Note. Estimates based on OLS regressions using equation 1. *Declared supporter* is a dummy variable equal to 1 if the leader reports being a RENAMO supporter, and 0 otherwise. *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 5 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 D13: Heterogeneous effects by migrant population in the block

	TL		TL×migrant number		TB		TB×migrant number		
Outcome variable	Coeff. (1)	S.E. (2)	Coeff. (3)	S.E. (4)	Coeff. (5)	S.E. (6)	Coeff. (7)	S.E. (8)	N (9)
A. Leaders									
Any tie to migrants	0.08**	0.04	0.03	0.04	0.04	0.04	-0.00	0.04	820
% of migrants known	0.06***	0.02	0.01	0.02	0.01	0.02	-0.01	0.02	846
Reports cyclists	0.12**	0.05	-0.00	0.06	0.08	0.05	-0.07	0.05	444
Observed # mobilized	0.67*	0.39	-0.52	0.43	0.70*	0.40	-0.09	0.47	444
Stickers (leaders)	0.04**	0.02	-0.00	0.02	-0.01	0.02	0.04**	0.02	444
Stickers (field team)	0.01	0.03	-0.01	0.03	-0.01	0.03	0.04*	0.02	444
Electoral turnout	0.05	0.04	0.00	0.03	0.02	0.03	-0.01	0.04	412
Chose assistant	-0.08	0.06	-0.06	0.06	-0.06	0.06	0.01	0.06	442
B. Migrants									
In the city	0.00	0.01	0.01	0.01	0.01	0.01	0.01	0.01	3590
MM adoption	0.18*	0.09	0.01	0.10	0.27***	0.10	0.04	0.10	2856
Job opportunities	0.07***	0.01	0.02	0.01	0.08***	0.01	-0.01	0.01	6119
Worked	0.05***	0.02	-0.01	0.02	-0.00	0.02	-0.01	0.02	6118
Hours worked	0.43***	0.15	-0.10	0.15	-0.15	0.17	-0.11	0.16	6118
Knows the leader	0.06**	0.03	-0.02	0.03	-0.04	0.03	-0.03	0.03	2856
Contacted the leader	0.04***	0.01	0.00	0.01	0.01	0.01	0.01	0.01	6119
Sought job assistance	0.01***	0.00	-0.00	0.00	0.01***	0.00	-0.00	0.00	6093
Campaign exposure	0.02***	0.01	-0.01	0.01	0.02***	0.01	-0.01	0.01	6117
Voting FRELIMO	-0.01	0.01	-0.00	0.01	-0.01	0.01	0.01	0.01	2092
Electoral turnout	0.03*	0.02	0.02	0.02	0.02	0.02	0.02	0.02	3333
C. Residents									
Knows the leader	0.07	0.05	-0.01	0.04	-0.04	0.04	-0.03	0.04	753
Contacted the leader	0.05**	0.02	0.01	0.02	-0.00	0.02	0.01	0.02	1574
Sought job assistance	0.02**	0.01	-0.01	0.01	0.01	0.01	0.01	0.01	1566
Campaign exposure	0.01	0.01	0.00	0.02	-0.00	0.01	0.01	0.01	1571
Voting FRELIMO	0.05	0.04	0.03	0.03	0.03	0.04	-0.00	0.03	534
Electoral turnout	-0.05	0.03	0.05	0.03	0.02	0.03	0.04	0.03	811
D. Origin contacts									
Intends to move	0.03***	0.01	0.02**	0.01	0.02**	0.01	0.02**	0.01	3792
Migrated to city	0.04***	0.01	-0.00	0.01	0.04***	0.01	-0.00	0.01	2218
Voting FRELIMO	-0.12**	0.05	-0.12**	0.06	-0.07	0.05	-0.10	0.07	406

Note. Estimates based on OLS regressions using equation 1. *Migrant number* is a continuous variable counting the number of migrants sampled in the block. The variable was standardized by subtracting the mean and dividing by the standard deviation. *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 5 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.

D.9 Selection of control variables

We verify the robustness of main estimates to the selection of controls by implementing the PDSL procedure of Belloni et al. (2013). Table D14 shows a comparison of treatment effects using both the set of controls used in the main text (*post-model selection*) and the PDSL procedure. The controls included in the procedure are the following:

- **Block characteristics** include stratum indicator variables, indicators for the presence of illegal construction in the block, categorical variables for the number of migrants sampled at baseline and the approximate block population, the number of bicycle taxi drivers, and the distances to the nearest school, market, and water fountain.
- **Leader characteristics** include *demographics and wealth variables*, including indicators for sex, marital status, illiteracy, less than primary education, primary education, Catholic religion, and Muslim religion, categorical variables for age (in years) and household size, and an indicator for ownership of the dwelling; *political variables*, including indicators for membership in the RENAMO party at baseline, voting for RENAMO in the 2018 and 2019 elections, favorable attitudes toward migrants, and employment status, and categorical variable for years served as block leader.
- **Migrant characteristics** include *demographics and wealth variables*, including indicators for sex, marital status, illiteracy, less than primary education, primary education, Catholic religion, Muslim religion, unemployment at baseline, student status at baseline, employment at baseline, migration to Quelimane for work, categorical variables for age (in years), number of children, and household size, indicators for renting or owning the dwelling, housing materials (concrete walls, concrete floors, zinc roof), asset ownership (bank account, radio, television, mattress, fan, motorcycle, refrigerator, phone, bicycle, solar panel), and housing quality index constructed as the average of three indicators (concrete walls, concrete floors, zinc roof); and *migration variables*, including indicators for whether the main difficulty at baseline was finding a job or making friends, for feeling a strong connection to Quelimane at baseline, and for perceived discrimination at baseline; trust index constructed as the average of four indicators (trust in market sellers, the Mozambican president, the provincial government, and the mayor of Quelimane).

Migrant characteristics are included only in the PDSL procedure for migrant-level outcomes. All continuous variables are standardized and included in levels and squared terms. To ensure the same sample size in the post–model selection and PDSL procedures, missing values are set to zero, and an indicator for missingness is included for each variable.

Table D14: Comparison with Post-Double Selection LASSO

	Post-Model Selection				Post-Double Selection LASSO				
	TL		TB		TL		TB		
	Coeff. (1)	S.E. (2)	Coeff. (3)	S.E. (4)	Coeff. (5)	S.E. (6)	Coeff. (7)	S.E. (8)	N (9)
A. Leaders									
Any tie to migrants	0.08**	0.04	0.04	0.04	0.08**	0.03	0.05	0.03	820
% of migrants known	0.06***	0.02	0.01	0.02	0.06***	0.02	0.02	0.02	846
Reports cyclists	0.12**	0.05	0.08	0.05	0.12***	0.04	0.08*	0.04	444
Observed # mobilized	0.66*	0.39	0.68*	0.40	0.69**	0.33	0.66**	0.32	444
Stickers (leaders)	0.04**	0.02	-0.01	0.02	0.04***	0.01	-0.01	0.01	444
Stickers (field team)	0.01	0.03	-0.01	0.03	0.01	0.02	-0.02	0.02	444
Electoral turnout	0.04	0.03	0.02	0.03	0.04	0.03	0.01	0.03	412
Chose assistant	-0.08	0.06	-0.06	0.06	-0.04	0.05	-0.05	0.05	442
B. Migrants									
In the city	0.00	0.01	0.01	0.01	-0.00	0.01	0.01	0.01	3590
MM adoption	0.18*	0.09	0.27***	0.10	0.22**	0.09	0.25**	0.11	2856
Job opportunities	0.07***	0.01	0.08***	0.01	0.07***	0.01	0.08***	0.01	6119
Worked	0.05***	0.02	-0.00	0.02	0.04***	0.02	-0.01	0.02	6118
Hours worked	0.62***	0.18	-0.14	0.19	0.59***	0.18	-0.14	0.18	3742
Knows the leader	0.05**	0.03	-0.04	0.03	0.04*	0.02	-0.02	0.02	2856
Contacted the leader	0.04***	0.01	0.01	0.01	0.04***	0.01	0.01	0.01	6119
Sought job assistance	0.01***	0.00	0.01**	0.00	0.01***	0.00	0.01**	0.00	6093
Campaign exposure	0.02***	0.01	0.02***	0.01	0.02***	0.01	0.02***	0.01	6117
Voting FRELIMO	-0.01	0.01	-0.01	0.01	-0.00	0.01	-0.01	0.01	2092
Electoral turnout	0.03*	0.02	0.02	0.02	0.02	0.02	0.02	0.02	3333
C. Residents									
Knows the leader	0.07	0.04	-0.04	0.04	0.07**	0.03	-0.03	0.04	753
Contacted the leader	0.05**	0.02	-0.00	0.02	0.05**	0.02	-0.01	0.02	1574
Sought job assistance	0.02**	0.01	0.00	0.01	0.02**	0.01	0.01	0.01	1566
Campaign exposure	0.01	0.01	-0.00	0.01	0.01	0.01	-0.00	0.01	1571
Voting FRELIMO	0.05	0.04	0.04	0.04	0.04	0.03	0.02	0.03	534
Electoral turnout	-0.05	0.03	0.03	0.03	-0.06**	0.03	0.03	0.03	811
D. Origin contacts									
Intends to move	0.03***	0.01	0.03**	0.01	0.03***	0.01	0.02*	0.01	3792
Migrated to city	0.04***	0.01	0.04***	0.01	0.05***	0.01	0.04***	0.01	2218
Voting FRELIMO	-0.15***	0.05	-0.09	0.05	-0.08*	0.05	0.01	0.05	406

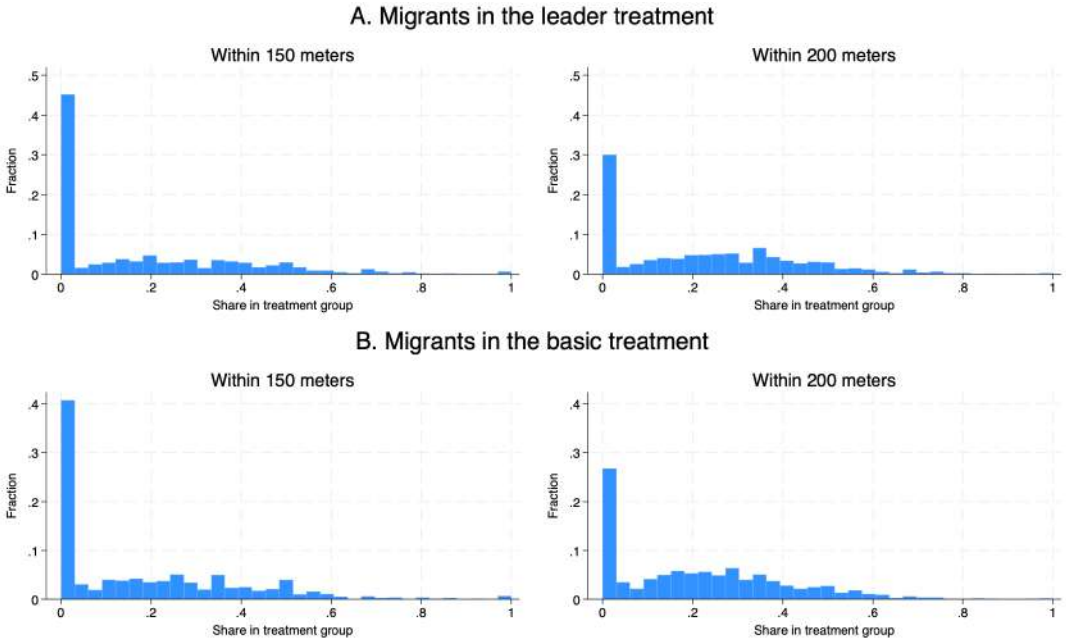
Note. Estimates based on OLS regression using equation 1. *TL* refers to leader treatment, *TB* refers to basic treatment (see section B of the main text). Standard errors are reported in columns (2),(4),(6) and (8) and clustered at the block level. In columns (1)-(4), the specifications are constant across outcome variables (see section 5). In columns (5)-(8), the specifications are outcome-specific and include individual and block-level controls, selected using the PDSL procedure of Belloni et al. (2013). The full list of control variables included in the procedure are presented in Appendix D.9.

D.10 Testing for spatial contamination

We exploit the fact that a higher local concentration of treated migrants is expected to increase the likelihood of spillovers. Because this concentration is driven by the randomization of blocks and by the location of migrants within each block, local variation in the concentration

of treated migrants at small distances from a respondent can be reasonably assumed to be random. We use precise geolocation data on migrants' dwellings and compute the fraction of treated migrants within a given distance of a respondent. We consider two distance thresholds (150m and 200m). Figure D4 shows the distribution of these measures in the control group.

Figure D4: Concentration of treated migrants from control group respondents



Note. The figures show the distribution of the fraction of all sampled migrants within a given distance of a control-group respondent that are part of the leader treatment (panel A) and the basic treatment (panel B). The fractions are computed using information about the geolocation of respondents' dwellings. Distances include only sampled migrants, and exclude distance from leaders or residents in blocks of different treatment arms.

Table D15 presents two distinct analyses. First, in columns (1)–(5), we restrict the sample to control-group migrants and estimate a version of equation (1) in which treatment indicators are replaced by the share of nearby migrants assigned to either the leader or the basic treatment. We report the coefficients and standard errors on these indicators. Any significant coefficient suggests potential spillovers from treated blocks to the control groups. Second, in columns (6)–(10), we estimate equation (1) while controlling for the share of nearby migrants assigned to either the leader or the basic treatment. We report estimates of treatment effects for the leader and basic treatments. The presence of spillover effects should influence the estimates of treatment effects compared to those estimated without controlling for potential spillover effects. Results point toward the lack of spillovers originating from treatments.

Table D15: Treatment spillovers

	Spillovers in the control group					Treatment effects controlling for potential spillovers				
	% migrants in TL		% migrants in TB			TL		TB		
Outcome variable	Coeff. (1)	S.E. (2)	Coeff. (3)	S.E. (4)	N (5)	Coeff. (6)	S.E. (7)	Coeff. (8)	S.E. (9)	N (10)
A. Distance bound of 150m										
Aware of the program	0.06	0.06	-0.06	0.06	5909	0.07***	0.02	0.06***	0.02	5909
Knows targeted group	-0.01	0.07	-0.09	0.05	5901	0.03	0.02	0.02	0.02	5901
Involved	-0.06	0.06	-0.01	0.06	7022	0.08***	0.02	0.06***	0.02	7022
In the city	-0.05	0.04	-0.04	0.03	3469	0.00	0.01	-0.00	0.01	3469
Mobile money adoption	-0.27	0.44	-0.18	0.46	2761	0.34***	0.12	0.46***	0.13	2761
Job opportunities	0.00	0.07	-0.08	0.06	5910	0.05***	0.02	0.08***	0.02	5910
Worked	-0.09	0.06	-0.03	0.07	5909	0.06***	0.02	-0.01	0.02	5909
Hours worked	-0.46	0.72	-0.53	0.69	5909	0.63***	0.19	-0.16	0.21	5909
Knows the leader	-0.12	0.10	0.08	0.09	2761	0.09***	0.03	-0.08**	0.03	2761
Contacted the leader	-0.01	0.05	-0.01	0.05	5910	0.04***	0.01	0.02	0.01	5910
Sought job assistance	-0.01	0.02	-0.00	0.01	5885	0.02***	0.00	0.01***	0.01	5885
Campaign exposure	-0.02	0.02	0.03	0.03	5909	0.02**	0.01	0.03***	0.01	5909
Voting FRELIMO	-0.00	0.07	0.05	0.06	2029	-0.02	0.02	-0.01	0.02	2029
Electoral turnout	-0.02	0.10	-0.09	0.09	3219	0.01	0.02	0.02	0.02	3219
B. Distance bound of 200m										
Aware of the program	-0.01	0.06	-0.05	0.06	5909	0.07***	0.02	0.07***	0.02	5909
Knows targeted group	0.01	0.09	-0.10	0.06	5901	0.03*	0.02	0.03	0.02	5901
Involved	-0.05	0.07	-0.02	0.06	7022	0.08***	0.02	0.06***	0.02	7022
In the city	-0.04	0.06	-0.04	0.05	3469	0.00	0.01	0.00	0.01	3469
Mobile money adoption	0.02	0.44	-0.17	0.53	2761	0.28**	0.12	0.44***	0.13	2761
Job opportunities	0.01	0.09	-0.12	0.07	5910	0.05***	0.02	0.08***	0.02	5910
Worked	-0.04	0.11	-0.03	0.09	5909	0.05***	0.02	-0.02	0.02	5909
Hours worked	-0.11	1.07	-0.73	0.89	5909	0.53***	0.19	-0.24	0.20	5909
Knows the leader	-0.13	0.11	0.08	0.11	2761	0.07**	0.03	-0.08**	0.03	2761
Contacted the leader	0.03	0.05	-0.00	0.06	5910	0.04***	0.01	0.02	0.01	5910
Sought job assistance	-0.00	0.02	0.00	0.01	5885	0.01***	0.00	0.01**	0.00	5885
Campaign exposure	0.02	0.03	-0.00	0.04	5909	0.02**	0.01	0.03***	0.01	5909
Voting FRELIMO	-0.00	0.10	0.00	0.07	2029	-0.01	0.01	-0.02	0.02	2029
Electoral turnout	-0.01	0.10	-0.07	0.11	3219	0.01	0.02	0.02	0.02	3219

Note. Estimates based on OLS regression using equation 1 where treatment indicators are replaced by the share of nearby migrants assigned to either the leader or the basic treatment. In columns (1)–(5), we consider nearby migrants using a distance threshold of 100 meters from the respondent's dwelling, while in columns (6)–(10), we consider 300 meters. Distances are computed using the exact geolocation of each respondent's dwelling. All specifications include block and individual controls, as well as strata fixed effects. Section 5 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.

D.11 Self-reported measures of clientelism and vote-buying

Table D16 shows treatment effects on a range of private transfers, including vote buying, brokerage payments for accessing benefits, and payments for employment, which may reflect clientelistic exchange, patronage, or corruption. Outcomes in columns (1)–(2) are reported by leaders, while outcomes in columns (3)–(5) are reported by migrants.

Table D16: Private transfers between migrants and leaders

	Leaders		Migrants		
	Received transfer	Vote buying	Made transfer to leader	Paid leaders for job offers	Vote buying
	(1)	(2)	(3)	(4)	(5)
(TL) Leader treatment	-0.006	-0.012	0.000	0.001	0.022
	(0.010)	(0.013)	(0.003)	(0.002)	(0.017)
(TB) Basic treatment	0.000	0.005	-0.001	-0.000	-0.015
	(0.009)	(0.015)	(0.003)	(0.002)	(0.016)
Observations	819	838	3249	6103	3183
Adjusted R^2	0.041	0.016	0.011	0.011	0.145
Mean (control group)	0.019	0.029	0.007	0.006	0.137
TL = TB (p-value)	0.502	0.260	0.853	0.570	0.034
Outcome data	Pooled	Pooled	Endline	Pooled	Endline

Note. Estimates based on OLS regressions using equation 1. Columns (1)-(2) refers to answers from leaders, while columns (3)-(5) refer to answers from migrants. Dependent variables by columns: (1) *Received transfer* is an indicator variable equal to 1 if the leader reports to have received any monetary or in-kind transfer from citizens in its role as block leader in the previous 2 months, and 0 otherwise; (2) *Vote buying - leader* is an indicator variable equal to 1 if the leader reports to have offered a monetary or in-kind transfer to any citizen to vote for their party, and 0 otherwise; (3) *Made transfer to leader* is an indicator variable equal to 1 if the migrant reports to have made any monetary or in-kind transfer to the block leader in the previous 2 months, and 0 otherwise; (4) *Paid leaders for job offers* is an indicator variable equal to 1 if the migrant reports to have paid a local leader to obtain job offers in the previous 12 months, and 0 otherwise; (5) *Vote buying - migrant* is an indicator variable equal to 1 if the migrant reports to have received a monetary or in-kind transfer to vote for a specific party, and 0 otherwise. All specifications include block and individual controls, as well as strata fixed effects. Section 5 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.

D.12 Leader political behavior following the program

Table D17 shows the results for the leader's political behavior. Columns (1)–(2) show results on voting behavior during the 2019 Presidential elections, before the start of the program. Columns (3)–(7) instead show the longer-run effects on leaders' political behavior using data collected more than one year after the program ended and after the 2024 national elections. Data originate from a phone survey conducted after these national elections in Mozambique. Columns (3)–(4) focus on whether the leader was a member of a party, and in particular, of RENAMO. Columns (5)–(7) show self-reported voting during the 2024 Presidential elections. In addition to RENAMO and FRELIMO, during these elections, voters could vote for

PODEMOS, an opposition party originating from factions within FRELIMO.

Table D17: Leaders' political behavior during Presidential elections

	Vote (2019 elections)		Party membership		Vote (2024 elections)		
	RENAMO	FRELIMO	Any	RENAMO	RENAMO	FRELIMO	PODEMOS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(TL) Leader treatment	-0.013	0.018	0.028	0.041	0.121*	-0.021	-0.090
	(0.043)	(0.015)	(0.047)	(0.050)	(0.073)	(0.022)	(0.075)
(TB) Basic treatment	0.041	-0.012	-0.073	-0.060	0.139**	-0.031	-0.103
	(0.042)	(0.012)	(0.052)	(0.054)	(0.070)	(0.022)	(0.071)
Observations	487	487	354	352	319	319	319
Adjusted R^2	0.342	-0.026	0.067	0.050	0.086	0.082	0.063
Mean (control group)	0.698	0.012	0.858	0.840	0.394	0.038	0.558
TL = TB (p-value)	0.192	0.033	0.074	0.075	0.810	0.457	0.861
Outcome data	Baseline	Baseline	Follow-up	Follow-up	Follow-up	Follow-up	Follow-up

Note. Estimates based on OLS regressions using equation 1. Outcomes in columns (1)–(5) are measured in the follow-up, while outcomes in columns (6)–(7) are measured at endline (see Appendix A.1). We do not include the lagged values of any of the dependent variables. Dependent variables by columns: (1)–(2) *Vote (2019 Presidential elections)* are indicator variables equal to 1 if the respondent reports to have voted for the corresponding party in the 2019 Presidential elections, and 0 otherwise; (3)-(4) *Party membership* are indicator variables equal to 1 if the respondent reports to be affiliated to any political party or RENAMO, and 0 otherwise; (5)–(7) *Vote (2024 Presidential elections)* are indicator variables equal to 1 if the respondent reports to have voted for the corresponding party in the 2024 Presidential elections, and 0 otherwise. All specifications include block and individual controls, as well as strata fixed effects. Section 5 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.