Refugee-Host Integration through the Social Safety Net: Experimental Evidence from Ethiopia *

Dennis Egger† Alfredo Manfredini Böhm‡ Belay Mohammed§
Sandra V. Rozo¶ Daksh Walia||

Abstract

Most refugees worldwide are supported through parallel, aid-financed systems that exclude host communities. Using a randomized controlled trial among approximately 23,000 eligible refugee and host households in Ethiopia's urban public works program, we study whether integrating refugees into the national safety net can strengthen refugee-host social cohesion. For the first time, we experimentally vary the refugee-host composition of work teams, neighborhood-level exposure to mixed teams, and after-work social events. After sixteen months, host attitudes and behaviors toward refugees improve by 0.15 standard deviations in the two study sites where refugees and hosts had opportunities for inter-group contact. Where hosts received the same program benefits without opportunities for contact, social outcomes did not improve. Assignment to mixed teams increases the combined social index by an additional 0.07 standard deviations, while greater neighborhood exposure to mixed teams generates a 0.10 standard deviation spillover in social outcomes, with more favorable views of refugees even among non-beneficiaries. Refugees show little direct change in social outcomes, consistent with their already favorable attitudes toward hosts at baseline, but after-work socializing strengthens their economic ties with hosts. The program also raises incomes by 25 percent and broadly improves beneficiary well-being. Economic inclusion strengthens social cohesion, but only where it creates opportunities for inter-group contact.

Keywords: refugee inclusion, public works, social cohesion, Ethiopia
JEL Classification: J61, I38, D74

*We are thankful to Samuel Bryson and Valentina Daza, who provided excellent research assistance, and for the support from Erina Iwami, Ayuba Sani, Alemayehu Girma, Kiros Kinfe, Hiwot Mebrate, the operational team supporting this program. We thank the Ethiopian Ministry of Urban and Infrastructure as well as the Refugees and Returnees' Service for their partnership, and the team of Zerihun Associates for excellent research collaboration. We are grateful for comments from participants at the EGAP Pre-Analysis Meetings of 2023, the Migration and Development Conference at Paris School of Economics, Georgetown University, CEGA Training on Refugee Knowledge, University of Pennsylvania, Nova Africa, Bristol, Barcelona Summer Forum, WB African Chief Economist, WALES Conference, WAIS Conference, Queens Business School, and London Development Economics Conference. This study was pre-registered (AEARCTR-0012288) prior to data analysis and was approved by the Oxford IRB. We are also grateful for the funding support from the Prospects World Bank Trust Fund, IPA, J-PAL, RECIPE, the World Bank, and Hilton Foundation. All errors are due only to the authors.

†Oxford University. Email: dennis.egger@economics.ox.ac.uk
‡Independent Researcher. Email: a.manfredini.bohm@outlook.com
§Oxford University. Email: belay.yimer@economics.ox.ac.uk
¶Development Research Group, World Bank. Email: sandrarozo@worldbank.org
||Oxford University. Email: daksh.walia@economics.ox.ac.uk

I INTRODUCTION

Today, approximately 118 million people are forcibly displaced, and most refugees live in protracted displacement in low- and middle-income countries (UNHCR, 2024). For decades, the dominant response has been humanitarian assistance delivered through parallel, aid-financed systems that operate alongside, but separately from, the national safety nets serving host communities. As displacement has become protracted and aid budgets have contracted, this model has come under strain. Moreover, excluding poor host communities from refugee-targeted assistance can breed resentment toward refugees (Baseler et al., 2025; Tamim et al., 2025). In response, a small but growing set of host countries, backed by the Global Compact on Refugees (United Nations, 2018), are moving in the opposite direction, integrating refugees into national social protection programs alongside poor citizens and treating them as one vulnerable group among many.

Whether an integrated model strengthens or strains refugee-host relations is theoretically ambiguous. Serving both groups through a shared national program could foster cohesion by tying eligibility to need rather than nationality and by making refugees' contributions to the community visible. But it could equally provoke backlash if hosts perceive newcomers as competing for scarce benefits within their own safety net. Despite the policy shift underway, there is no experimental evidence on how integrating refugees into a national antipoverty program affects hosts' economic welfare or their relations with refugees. This paper provides such evidence.

We study this question in the context of Ethiopia's national public works and livelihoods antipoverty program, which for the first time integrated refugees and poor citizens at scale within the same national program. Our analysis asks four questions. First, does serving refugees and hosts through a shared safety net change how the two groups perceive and treat one another? Second, does engineered inter-group contact, created by randomly assigning refugees and hosts to work side by side in public works teams, generate additional cohesion gains beyond participation in the integrated pro-

gram? Third, do these effects reach beyond participants, shifting attitudes among neighbors who are exposed to mixed teams but are not themselves enrolled? Fourth, how large are the economic and well-being gains for each group, and does a safety net designed for poor citizens serve refugees as effectively as it serves hosts?

Our context is Ethiopia's Refugee and Host Integration through the Safety Net (RHISN) program, which for the first time supports refugees directly through the national safety net rather than through parallel humanitarian assistance. RHISN is embedded in the Urban Productive Safety Net, one of the largest urban social protection programs in Sub-Saharan Africa, which operates in 88 cities and reaches approximately 2.4 million beneficiaries nationwide, in addition to the over 8 million households served by its rural counterpart. Over the course of two waves, RHISN enrolled about 22,600 refugee and host households, thus reaching just over 4 percent of refugees hosted in Ethiopia. We evaluate the second wave of the program, which enrolled approximately 15,000 households.

RHISN delivers a two-year, high-intensity treatment package. In the first year, beneficiaries participate in labor-intensive public works for up to 25 hours per week and receive monthly wage payments of up to USD 133 in purchasing power parity terms, of which 20 percent must be saved in individual accounts. In parallel, beneficiaries engage in structured livelihood training and coaching through group sessions covering financial literacy, business planning, and job search strategies. In the second year, beneficiaries continue to receive livelihood coaching and are additionally provided with a one-time productive grant of approximately USD 600, or USD 2,460 in PPP terms (43% of control mean annual consumption), to support self-employment or labor market entry. The feature of RHISN most distinctive for our purposes is that it embeds a social cohesion experiment at scale. Refugees and hosts are randomly assigned to mixed or single-group public works teams, neighborhoods are randomly assigned higher or lower shares of mixed teams, and a random subset of teams is invited to after-work teas and dinners.

We study RHISN in four small towns and their adjacent refugee camps across four different regions of Ethiopia. The program is a substantial shock both at the household and at the local-economy level. The monthly wage earnings alone are equivalent to 28 percent of the mean monthly consumption of the control group. Across our four study sites, approximately 35 percent of all refugee and Ethiopian households in the area are enrolled in RHISN, and public works and grant payments represent roughly 40 percent of study-area GDP.

In this paper, we focus on the effects of approximately 12 to 16 months of exposure to public works and the associated training and coaching. All data were collected before the productive grant was disbursed, so our estimates isolate the effects of these components from those of the subsequent capital transfer.

We evaluate the program in Asayita (Afar), Dabat (Amhara), Bambasi (Benishangul-Gumuz), and Pugnido (Gambella). Together, these sites host approximately 120,000 refugees, 10 percent of all refugees in Ethiopia, alongside host towns with a combined population of roughly 100,000. The experimental design combines four sources of randomized variation. First, program intensity is randomized across 110 neighborhoods of approximately 2,000 residents each, with high-saturation neighborhoods receiving 25 percent more enrollment slots than a population-proportional quota and low-saturation neighborhoods 25 percent fewer (Egger et al., 2021). Second, within each neighborhood, enrollment is randomized at the household level among eligible households, where eligibility is universal for refugees and based on proxy-means targeting for hosts. Third, beneficiaries in Dabat and Asayita are randomly assigned to public works teams that are all-host, all-refugee, or mixed, with mixed teams composed of either equal shares of the two groups or a refugee minority. The share of beneficiaries assigned to mixed teams is itself cross-randomized across neighborhoods, generating community-level variation in exposure to refugees and hosts working together. Fourth, a random subset of teams is invited to after-work social activities in the form of shared teas and dinners. Each layer of the design maps to one of our questions. Neighborhood program intensity identifies local economic spillovers; the household

lottery identifies the direct effects of the program; team composition and the social activities identify the effects of inter-group contact and its depth; and the neighborhood share of mixed teams identifies whether greater community exposure to refugee-host collaboration shifts attitudes, including among non-beneficiaries.

We draw on three complementary sources of data. First, a comprehensive listing exercise conducted in 2023, before program implementation, enumerated all 23,000 eligible refugee and host households and established the sampling frame. Second, we field a baseline household survey between September and November 2023, before the start of public works and livelihood activities, and a midline survey in early 2025, approximately 12 to 16 months after program rollout and before the disbursement of the productive grant. Both surveys target approximately 12,000 households, evenly split between refugees and hosts, and collect detailed information on economic activity, consumption and assets, physical and mental health, female empowerment, social relations, and child well-being. Third, we conduct unannounced three-day visits to all 133 public works teams in Dabat, collecting structured observations of team dynamics and performance and running incentivized lab-in-the-field games that measure altruism, trust, and cooperation within and across group lines. Together, these data allow us to measure social cohesion not only in stated attitudes but also in observed behavior with real stakes.

We document three main results. First, integrating refugees into the national safety net improves social cohesion, but only for hosts and where there were opportunities for inter-group contact. Among beneficiaries, our combined index of social cohesion rises by 0.06 standard deviations relative to the control group, in line with the average effect of dedicated contact interventions (Lowe, 2025). These gains are driven entirely by hosts and appear in stated ougroup attitudes, support for refugee-inclusion policies, and incentivized behaviors, with host beneficiaries transferring substantially more to refugees in dictator games with real stakes. Geographically, these effects are concentrated in Dabat and urban Asayita, the two sites where refugees and hosts could participate side by side, where the index rises by 0.15 standard deviations. In Bambasi and

Pugnido, where distance, insecurity, and extreme heat prevented joint participation, hosts received the same economic benefits, yet their social outcomes did not change.

Second, inter-group mixing generates additional gains in social cohesion, and these gains extend beyond direct participants. Assignment to a mixed public works team improves the social outcomes combined index by a further 0.07 standard deviations, and neighborhoods randomly assigned a higher share of mixed teams gain another 0.10 standard deviations, 1.5 to 2 times the direct effect, with more favorable views of refugees even among non-beneficiaries. Refugees, who are already positively disposed toward hosts at baseline, show little attitudinal change from workplace contact alone. Their gains come instead from depth of contact; refugees in teams invited to after-work teas and dinners build significantly stronger economic ties with hosts. Partial integration carries risks, however, as refugees assigned to homogeneous teams in the mixing sites reduce their economic interactions with hosts.

The geographic distribution of the results helps us discriminate among mechanisms, which we examine separately for the direct and spillover effects. For the direct effects on host beneficiaries, income gains alone cannot be the explanation, since hosts outside Dabat and Asayita experienced the same economic benefits without social change. Information-based reciprocity also appears insufficient on its own. An information experiment that primes beneficiaries on the program's refugee-linked funding does not shift host attitudes, although a weak first stage prevents us from ruling this channel out entirely. Nor can we attribute the gains to better program implementation in these sites or to indirect exposure to refugees in the communities. Adjusting for how well the program was delivered does not close the gap across sites, and control households see refugees working in their neighborhoods just as beneficiaries do, yet their attitudes barely move. The evidence points to a simple explanation: hosts and refugees take part in the same program, and the income gains and goodwill it generates translate into cohesion only within that shared experience.

The spillover effects point instead to a response to being exposed to seeing refugees

working in the communities. They are too large to reflect diffusion through social networks alone, which would require neighbors to respond more strongly than participants themselves, and a substantial share of respondents' networks extends beyond their own neighborhood. They are concentrated among hosts, who live closest to the public works sites, and they appear as more favorable beliefs that refugees contribute positively to Ethiopia among beneficiaries and non-beneficiaries alike, consistent with residents updating their views as they observe refugees working in their communities.

Third, the program generates broad economic and well-being gains for both groups. Relative to control households, beneficiaries experience increases in annual household income of roughly 25 percent, driven primarily by public works earnings, a 19 percent increase in assets, and a 4 percent increase in consumption. Productive labor rises by approximately four weekly hours, almost entirely through public works, with limited crowding out concentrated in the lowest-paid jobs. We also find meaningful improvements in food security, reductions in psychological distress and depression, and gains in female empowerment, the latter largest among refugee women. Encouragingly, we also document early intergenerational impacts on beneficiary children, including better nutrition and health for both groups and fewer behavioral and emotional difficulties among host children. While economic gains are similar across groups, well-being gains are consistently larger for hosts, a pattern largely explained by differences in baseline conditions rather than by refugee status itself as documented by our random forest analysis. Finally, we find no economic spillovers at this stage of the program, which might change after the disbursal of the grant.

This study makes several contributions. First, it advances the literature on the effectiveness of policies that promote the self-reliance of displaced populations. While a growing body of economic research examines the impacts of displacement on hosting economies, existing evidence primarily focuses on cash-based humanitarian assistance, labor market integration in middle- and high-income countries, or refugee-only interventions (Becker, 2019; Verme and Schuettler, 2021; Bahar, Brough, and Peri, Bahar et al.; Rozo, 2025). In contrast, this study experimentally examines refugee inte-

gration in a national social program alongside host households, allowing us to assess how institutional integration—rather than parallel assistance—shapes economic outcomes, well-being, and social cohesion in a low-income, fragile setting. Moreover, the study allows to understand if hosts and refugees are affected in similar ways by anti-poverty programming or whether there is justification for additional targeted support for refugees.

Second, the paper contributes to the literature on intergroup contact and social cohesion by distinguishing between integration through shared institutions and integration through enforced interaction. While prior work has emphasized the role of intergroup contact in reducing prejudice (Allport, 1954; Rao, 2019; Scacco and Warren, 2018; Mousa, 2020; Lowe, 2021), existing evidence is typically based on short-term or small-scale interventions. This paper studies randomized, multi-month co-working relationships between refugees and hosts within a high-stakes economic setting and shows that inclusion within a shared safety net improves social cohesion even without direct mixing, while enforced mixing generates more limited and context-dependent effects. We additionally document that impacts of out-group exposure depend on the relative size of groups, and that after-work socializing has important impacts for refugee minority groups in our setting, where workplace contact alone did not shift refugee attitudes towards hosts substantially.

Third, the paper contributes to the growing literature using experimental evidence to study the effectiveness of social protection programs, by doing a comprehensive examination of a large-scale urban public work program that takes place in a fragile setting (Berg et al., 2012; Imbert and Papp, 2015a; Beegle et al., 2018; Bertrand et al., 2021; Olken and Hanna, 2025). We not only examine the direct effects of the program on labor outcomes, income, and consumption—outcomes traditionally studied—, but also examine how public works affects outcomes for which limited evidence is available such as mental and physical health, children well-being outcomes, social cohesion, female empowerment and financial inclusion (Bagga et al., 2024). Moreover, we complement evidence of a small number of studies that have examined the local gen-

eral equilibrium effects of similar programs using experimental variation (Egger et al., 2021; Muralidharan et al., 2023a; Franklin et al., 2024). We complement their work by examining not only market effects, but also the social cohesion and network externalities of this program.

Finally, the paper contributes to the literature on diversity and productivity by examining how refugee-host group composition affects public works performance. While most evidence on diversity comes from laboratory or educational settings, with few real-world exceptions (Hjort, 2014), this paper studies productivity, coordination, and social dynamics in diverse work teams operating at scale in a real professional setting. By separately varying on-the-job exposure and after-work social interaction, the paper provides new evidence on how diversity shapes cooperation, productivity, and social ties in real-world labor settings.

This paper informs policy debates on the design of responses to protracted forced displacement. Rather than creating parallel systems for refugees, the intervention studied here embeds forcibly displaced populations within an existing national safety net alongside vulnerable host households. This integrated host model may reduce administrative complexity and fiscal costs relative to stand-alone programs, while also reshaping the political economy of assistance by defining eligibility based on vulnerability rather than legal status. By jointly serving refugees and hosts, the model has the potential to mitigate perceptions of preferential treatment and directly foster social cohesion through shared participation in public programs. The evidence in this paper suggests that this approach can simultaneously advance welfare and social integration objectives.

II BACKGROUND

II.A Refugee Hosting and Policy Environment in Ethiopia

Ethiopia hosts one of the largest and longest-standing refugee populations in Africa and provides a particularly informative setting for studying development-oriented refugee policies, due to the scale and protracted nature of displacement and the coun-

try's recent legal reforms expanding refugee economic inclusion (Perez and Rozo, 2025). The country has historically maintained an open-door policy toward refugees fleeing protracted conflicts in neighboring countries, with the refugee population originating predominantly from South Sudan (40.5 percent), Somalia (33.4 percent), Eritrea (16.7 percent), and Sudan (8.4 percent) (RRS-UNHCR, 2025).

As of mid-2022, Ethiopia hosted approximately 865,000 refugees and asylum seekers, a figure that reached 1.1 million by 2025, making it the third-largest refugee-hosting country in Africa (RRS-UNHCR, 2025).[1] Children account for 60 percent of the refugee population (RRS-UNHCR, 2025). Displacement in Ethiopia is highly protracted, lasting 14 years on average (Wieser et al., 2024). Although a large refugee population lives in Addis Ababa, refugees are heavily concentrated in economically peripheral border regions—most notably Gambella, Somali, Benishangul-Gumuz, Afar, and Tigray. There, poverty rates are high, infrastructure is limited, and labor markets are predominantly informal (Wieser et al., 2024; Pape et al., 2018). Approximately 75 percent of refugees reside in 23 camps and settlement sites, 17 percent live in non-camp settlements, and only 8 percent reside in urban areas such as Addis Ababa; these locations exhibit weak market integration and limited access to wage employment, raising persistent concerns about economic self-reliance, social cohesion, and the sustainability of parallel service delivery systems (World Bank, 2020, 2024b).

In response, the Government of Ethiopia has implemented a series of legal and institutional reforms aimed at integrating refugees into national systems and promoting self-reliance. These reforms, formalized under the Comprehensive Refugee Response Framework and the 2019 Refugee Proclamation, expanded refugees' rights to work, move, and access services and were subsequently operationalized through directives launched that established formal pathways into wage employment, self-employment, and joint ventures (Perez and Rozo, 2025). This policy shift represents a departure from the earlier camp-based model and places Ethiopia among a small

[1] Uganda and Chad host the largest refugee populations in Africa, with over 1.8 million and 1.2 million refugees, respectively (UNHCR, 2024

set of low-income refugee-hosting countries that have formally expanded refugees' access to economic activities (World Bank, 2024b). However, evidence from Ethiopia points to a persistent gap between *de jure* rights and *de facto* economic integration: take-up of formal work and business permits remains limited, reflecting coordination challenges across institutions, constrained subnational administrative capacity, low awareness among refugees and employers, and weak incentives to formalize in labor markets characterized by high informality and limited economic opportunities (Perez and Rozo, 2025).

This context motivates the study of policies that integrate refugees into national welfare systems rather than relying on parallel refugee-specific programming. Ethiopia's Refugee and Host Integration through the Safety Net (RHISN) enrolls refugees and host households in shared public works and livelihood programs delivered through the national social protection system. This paper studies the effects of their participation on the socio-economic outcomes of refugee and host beneficiaries and their wider communities, shedding light on whether joint engagement in paid public works and in livelihood programs can translate into greater self-reliance and improved social cohesion.

II.B THE RHISN Program

RHISN is a large-scale social protection program designed to promote the economic and social integration of refugees and nearby Ethiopian urban communities. The program provides beneficiaries with public-works employment with livelihoods support and targeted over 22,600 beneficiary households in two phases until the end of 2025. RHISN is a relatively small sub-component of a larger Ethiopia's Urban Productive Safety Net and Jobs Project (UPSNJP). In 2022, UPSNJP operated in 88 cities and targets approximately 2.4 million beneficiaries nationwide (in addition to the over 8 million people served by its rural counterpart offering similarly comprehensive programs). [2]

[2]The government of Ethiopia has since expanded RHISN and the UPSNJP to other locations (World Bank, 2024a, 2025).

Location. In 2022, there were 23 refugee camps and settlements in six regions of Ethiopia: Afar, Amhara, Benishangul-Gumuz, Gambella, Somalia and Tigray. RHISN's Phase I was implemented in the Faafan zone of the Somali Regional State, starting in 2021. This paper focuses on the program's Phase II, which expands operations to four sites, in four different regions: Asayita in Afar, Dabat in Amhara, Bambasi in Benishangul-Gumuz, and Pugnido in Gambella (Figure 1). [3]

Each site consists of a refugee camp with between 15,000 and 60,000 refugees and a nearby host town, typically a small urban agglomeration with populations between 25,000 and 54,000. Refugee camps are between 2 to 10 kilometers away from the host town. Cities are divided into kebeles, the lowest administrative unit of the Ethiopian government, and kebeles are further divided into ketenas (neighborhoods of approximately 200 - 400 households and a population of $\approx$ 2,000). Refugee camps are administratively divided into zones, and zones are further subdivided into blocks. For the purpose of this study, we form block-groups, typically comprising several blocks and a similar population of $\approx$ 2,000 refugees). Baseline integration varies substantially across sites.

Dabat and Alemwach. The Alemwach refugee settlement was established on the outskirts of the small town of Dabat, in Northern Amhara, in June 2021 and hosts refugees fleeing political repression and indefinite national conscription in Eritrea. Eritrean refugees arrived gradually in Ethiopia, in the aftermath 1998-2000 Ethio-Eritrean border war. Originally hosted in camps in the Tigray Region, they were secondarily displaced when the Tigray camps were wound down, or destroyed during the Tigray War of 2020-2022 (Ludi and Tsionawit Gebre Yohannes, 2019; **?**). Refugees hosted in the Tigray camps were mostly Orthodox Christian from multiple ethno-linguistic groups of Eritrea, including a large majority of Tigray, a sizeable Kunama group and smaller groups of Tigre, Saho and Nara (Ludi and Tsionawit Gebre Yohannes, 2019). With the notable exception of the Kunama, many of these groups have had a

[3] Phase II also enrolled an additional batch of beneficiaries in Faafan zone; these are outside the scope of this evaluation and of this paper.

historically high propensity to resettlement or onward migration (Belloni, 2019). The distribution of primary languages within our refugee sample suggests that Alemwach inherited a similar ethnic makeup (Table 1). The Ethiopian government's July 2023 population projections, based on the 2007 Population and Housing Census, indicate that the host population of Dabat is predominantly Amharic-speaking and Orthodox Christian (Ethiopian Statistical Service, 2023). Alemwach differs from the other sites in our study in that it is not designed to be a camp with separate services and access restrictions, but a settlement, integrated with the town of Dabat. Many refugees in our sample live in Dabat town, not in Alemwach. Despite this, and despite sharing a common religion, relations between the refugee and host communities are not straightforward: the settlement is recent, refugees and hosts share neither ethnicity nor language, and tensions persist among the government of Ethiopia, that of Eritrea and Amhara nationalist forces in the aftermath of the Tigray war. Insecurity remains a major protection concern for refugees (UNHCR, 2026) in Dabat and indeed prevented us from conducting baseline data-collection there.

Pugnido. Pugnido refugee camp opened in 1993 on the site of a predecessor camp (1988–91), and hosts South Sudanese refugees—predominantly Nuer, with an Anuak minority—displaced by the second Sudanese civil war. The South Sudanese civil war and the resulting inflow of mostly Nuer refugees into Gambella prompted the opening of a second camp in 2015 - Pugnido II (Carver et al., 2019; Human Rights Watch, 2012). Ethnic belonging to Nuer or Anuak is more important here than being a refugee or an Ethiopian. Nuer and Anuak practice different livelihoods, speak different languages, and compete for political power and control of the land in the Gambella region. The Nuer practice migratory cattle husbandry and Anuak sedentary riverine agriculture; both are overwhelmingly Protestant and are affiliated to similar denominations as each other and their hosting population (Dereje Feyissa, 2011). The host area, Gog *woreda* in Gambella's Anywaa nationality zone, is populated mainly by Anuak farmers alongside a "highlander" minority that dominates trade and refugee-operation employment (Carver et al., 2019; Dereje Feyissa, 2011). Baseline relations are asym-

metric across refugee groups: there is qualitative evidence that Anuak refugees have better integrated over decades with the co-ethnic town through intermarriage, shared churches, and market participation, whereas Nuer refugees have reported isolation, insecurity outside the camp, and economic discrimination in local markets (Carver et al., 2019). Refugee inflows since 2013 have shifted the Anuak–Nuer demographic balance in Gambella region, in favour of the Nuer. The resulting tensions have erupted in episodes of intergroup violence including in and around Pugnido itself (Carver et al., 2019). Kinship ties link both Anuak and Nuer with neighbouring South Sudan, more strongly than national Ethiopian or South-Sudanese identity (Dereje Feyissa, 2011). This combined with the anecdotal evidence that refugee camps had or have a role in recruiting soldiers for the South Sudanese civil war, and with the banditry from South Sudan spilling over the border into violence in the area, make baseline relations among host and refugees tenser and more politicised in Pugnido than anywhere else in our sample (Gebresenbet, 2016; Carver et al., 2019).

Bambasi. Bambasi camp, opened in 2012, hosts an almost entirely Sudanese population displaced by the resumption of war in Sudan's Blue Nile state in late 2011. (UNHCR, 2018). The refugees are from a variety of ethnic groups including the Berta, Ingessana, Uduk among others and are predominantly Muslim. The refugee camp sits about a 10-kilometre drive east of Bambasi town and just a short walk south of Womba kebele, a small, peri-urban settlement that is one of the five kebeles comprising Bambasi Town. Besides an indigenous, Muslim, Berta population—sharing ethnic and linguistic roots with part of the refugee population— Bambasi town and surroundings are inhabited by Oromo and Amhara communities, established during resettlement programmes in the 1980s (Markakis, 2011; Fekadu Adugna and Ketema Debale, 2023). The Berta are one of the two more numerous indigenous populations of the Benishangul-Gumuz region. They traditionally practice agriculture, small-scale gold mining and artisanal crafts (Takele Merid, 2025). Despite its textured history of both forced and managed migration [4], recent relations between Ethiopians and refugees in

[4]In October 2025, Bambasi *woreda* was also hosting over 12,000 internally displaced people fleeing insecurity (UNHCR, 2025).

Bambasi have been peaceful.

Asayita. Asayita camp was formally established in 2007 for Eritrean Afar refugees. The Afar are a nomadic, pastoralist population, whose traditional lands straddle the Ethiopia-Eritrea border. After the 1998–2000 Ethio-Eritrean war many Eritrean Afar migrated Asayita town and its surrounding areas, seeking refugee status. Many lived among the community before the camp was established, and continue to do so. The camp sits 2.5–3 km from the town and movement restrictions tend to be loosely enforced. For many households the camp is primarily a ration-collection point (Dereje Feyissa and Dawud Mohamed, 2019). The refugees, drawn from the Assab, Massawa, and Dankalia coastal areas of Eritrea, are Muslim Afar pastoralists and fishers who share language, religion, and in many cases clan membership with the host population (Dereje Feyissa and Dawud Mohamed, 2019; IIED, 2023). Hosts in Asayita *woreda* are also Afar Muslim agro-pastoralists organised through clan-based land administration and customary institutions. Land belongs to a specific clan, but seasonal migration is normal in Afar culture and regulated through structures recognized across clan boundaries (Yazew and Kassa, 2024). Ethiopian Afar do not consider the Eritrean Afar refugees as foreigners, even though administratively the nationalities differ. Baseline integration is levels are ostensibly — and perhaps unsurprisingly — the highest here than in any of our four sites. Almost a third of Asayita refugees in our sample consider a host community member a close connection (Table 1). Roughly 28% of registered refugees lived outside the camps at the 2018 registration (Dereje Feyissa and Dawud Mohamed, 2019). Economic integration was also high at baseline. Host and refugees engaged in sharecropping, refugees worked as employees at the nearby Tendaho sugar plantation, and studied in local schools through to university (Dereje Feyissa and Dawud Mohamed, 2019).

Program Implementers The RHISN program is led by Ethiopia's Ministry of Urban and Infrastructure (MUI) through the Federal Project Coordination Unit (FPCU), with the Refugees and Returnees Service (RRS) supporting targeting and safeguards for refugees, and alignment with national refugee policies. A Steering Committee of

ministers and senior officials provides strategic oversight, while a Technical Working Group including MUI, FPCU, RRS, and the World Bank offers technical guidance. The World Bank financed the program and supported implementation oversight.

Program eligibility Host and refugee households were targeted separately. First, a quota system determined how many host-community households and how many refugee households were included in each area. Total beneficiary quotas for each refugee camp and host city were set ex-ante by the UPSNJP Steering Committee based on estimated demand. 58 percent of the beneficiary quota were planned to be Ethiopian host community households and 42 percent refugee households, though in practice these vary between 56 - 63% for hosts, and 37 - 44% for refugees. In both populations, households designated a single adult beneficiary to participate on their behalf.

Refugees. All refugee households listed in the official Refugees and Returnees Service registry including an adult able to participate in public works were eligible, with beneficiaries randomly selected up to site-specific quotas.[5]

Hosts. Host households were selected using proxy means test (PMT)-based targeting: 37 percent of host beneficiary slots were automatically assigned to the poorest households according to the PMT-based wealth score, and excluded from the randomized evaluation; the remaining 63 percent were randomly assigned among other eligible households (based on available slots for the program). [6]

Once the randomization was done, the lists were made publicly available and an appeal process was undertaken. Appeals were only possible to contest eligibility for automatic inclusion in the program (the poorest 37%) vs. eligibility for randomization and thus did not affect the exogenous nature of the random selection itself. Appeals could not alter the outcome of the randomization itself. In practice, appeals were rare.

[5]The registry was conducted by RRS. The research team cross-checked the list with the UNHCR refugee database to ensure it included only active refugees, and then returned it to RRS for final approval prior to randomization.

[6]The initial census data had to be digitized by the research team to conduct the PMT survey and run the randomization.

Program benefits and timeline The program was structured in two phases and planned to operate over a 24-month period, from December 2023 to December 2025. Over this period, the program was designed to deliver up to 24 monthly public works wage payments, 21 two-hour group-based livelihoods training and coaching sessions, and a one-time livelihoods grant.

Public works activities began in December 2023 and concluded in December 2025.[7] Livelihoods training and coaching began in April 2024 and ended in July 2025. Between April and September 2024, two sessions per month were conducted, followed by one session per month from October 2024 through July 2025. The one-time livelihoods grant was disbursed in April 2025.

In addition, refugee beneficiaries were issued a five-year residency permit, enabling legal participation in the labor market during and beyond the program period.

Figure 2 summarizes the timing and sequencing of program components. The two stages of implementation are described below.

Stage One (sixteen months). During the first sixteen months, beneficiaries were offered paid public works employment combined with livelihoods training and coaching.

Public works activities were organized in groups of approximately 30 individuals and were intended to include street cleaning, waste collection, urban greenery and tree planting, erosion control, and small-scale social infrastructure projects. In practice, approximately 80 percent of groups worked primarily on street cleaning and dry waste collection, and the only site where groups worked on urban greening and tree planting was Dabat. During active public works periods, participants could work up to 20 hours (up to 25 hours after September 2024) per week comprised of four days of five hours (up to five hours per day over five days after September 2024), subject to local implementation schedules and project availability. Wage payments were made monthly.

[7] Due to implementation constraints, beneficiaries in Bambasi received 23 public works payments and those in Pugnido received 21 payments. Transfer amounts in these locations were adjusted so that total public works transfers were equal in value to those received in other sites.

The number of eligible public work days (and thus payments) depended on the number of household members and varied between 1 and 4 weekly workdays (3 on average), remunerated at 150 ETB (5.5 USD PPP) per workday.[8] Households could thus earn up to 2,600 ETB per month (95 USD PPP) and 2,041 on average (75 USD PPP). Over the full 24-month public works cycle, total transfers amounted to a maximum of USD 3,030 PPP per household (and 2,378 USD PPP on average). Beneficiaries were required to save 20 percent of their public works earnings in a bank account.

In parallel, beneficiaries participated in 17 group-based livelihoods training and coaching sessions during Stage One, as part of a total of 21 sessions delivered over the full program period beginning in April 2024. Training sessions followed a structured curriculum focused on life skills, financial literacy, and introductory livelihood planning. Coaching sessions were more flexible and demand-driven, providing a forum for participants to clarify training content, raise questions, and receive targeted support, including assistance to households progressing more slowly through the program. While households could send different members to training and coaching sessions, program guidelines encouraged the household head to attend both.

Stage Two. After the initial sixteen months, the program expanded to include two additional components: (i) a one-time livelihoods grant of approximately USD 600 or 67,000 ETB at the time of grant disbursement (USD 2,462 PPP), and (ii) structured social cohesion–building activities, implemented through organized tea events within public works groups. The social cohesion activities included up to six monthly teas, one lunch and one annual cultural festival, intended to promote after-work socializing.[9]

This paper focuses exclusively on the first stage of the program.

[8] From September 2024, workdays increased to maximum 5 per week, and remuneration to 181 ETB per workday to account for an accelerated program timeline and inflation.

[9] The festival day program included music, drama and poetry performances, case story presentations, food, and handover of beneficiary profiles to the Labor and Skills office.

III DATA

The analysis draws on original household survey data collected alongside the rollout of RHISN, combined with administrative program records. Data collection comprises three components: i) a household listing conducted prior to program implementation for targeting and randomization purposes, ii) a baseline household survey administered before the start of public works activities, and iii) a midline household survey conducted during program implementation. These data sources are complemented with a comprehensive set of unannounced field visits to all public work groups in Dabat.

III.A Household Listing

Prior to the rollout of public works activities, a comprehensive household listing was conducted in all study sites to identify eligible refugee and host households. The listing captured approximately 10,000 refugee households and 15,000 host households. For the host communities, we first conducted a household census using a short screening survey to identify households eligible for the Proxy Means Test (PMT) survey. To qualify for the PMT survey, a household had to have resided in the area for at least six months and include at least one able-bodied adult willing to participate in public works. We then administered the PMT survey to all eligible households. The PMT questionnaire collected information on a range of poverty correlates, which were used to generate household welfare scores and rank households by vulnerability according to the Urban Productive Safety Net and Jobs Program. The most vulnerable households were automatically enrolled in the program, accounting for 37% of the quota for beneficiaries in the host community. Randomization was then conducted among the remaining eligible households to allocate the remaining 63% of program slots. For refugee populations, eligibility was based on administrative refugee registries maintained by the Refugees and Returnees Service, complemented by on the ground verification. The household listing defined the universe of eligible households and served as the sampling frame for randomization.

III.B Baseline Survey (September-November 2023)

From the population comprising all eligible households, but excluding those automatically enrolled, we drew a study sample of 12,000 households, roughly proportional to each site's quota. The baseline household survey was administered prior to the start of public works activities. The baseline survey collected detailed information on household demographics, education, labor supply, income sources, consumption, assets, food security, physical and mental health, social networks, attitudes toward outgroup, and women's economic participation and intra-household decision making. Baseline data are used to assess balance across treatment arms and to improve the precision of treatment effect estimates.

III.C Midline Survey (January-March 2025)

The midline household survey was conducted between January and March 2025, approximately 12 to 15 months after the start of public works activities. By the time of the midline survey, sampled households had been exposed to public works for over a year and approximately 12 months of livelihoods training and coaching but had not yet received the grant or participated in formal social cohesion activities.

The midline survey closely mirrors the baseline instrument and updates all major outcome domains augmenting it to children outcomes. It also includes modules on program awareness and participation, allowing measurement of compliance and treatment intensity.

III.D Public Work Group Observations (January-June 2025)

Our field team conducted unannounced visits of each of the 133 public work groups in Dabat. On average, each group includes approximately 27 beneficiaries, resulting in an overall sample of more than 3,500 individuals. The observations survey collected both quantitative and qualitative data, in the form of extended qualitative field notes. On day 1, we conducted a structured observation of a public work session that is conducted in a natural setting by a team of two enumerators capturing group- and individual-level productivity, member interactions, subgroup dynamics, task en-

gagement, motivation, teamwork, and interpersonal interactions. On days 2 and 3, we conducted "lab-in-the-field" games: i) the Engineering Challenge, which assesses productivity and cooperation, ii) the Deer–Partridge Game, which evaluates trust and social cohesion, and iii) the Dictator Game, which assesses altruistic preferences within and across teams. Lastly, we conducted a set of individual level interviews with group leaders and all team members collecting individual perceptions of randomly selected teammates' and the overall group's productivity, interaction, trust, empathy, motivation, etc.

III.E Sample and Timeline

The study sample comprises approximately 12,000 refugee and host households across the four refugee hosting regions of Ethiopia. The household listing and randomization preceded the baseline survey, which was fielded between September and November 2023 depending on site specific implementation schedules. The midline survey was conducted between January and March 2025. As a result, the midline analysis captures the effects of the first stage of the program, public works participation and livelihoods training and coaching, prior to the disbursement of the grant and the roll-out of structured social cohesion activities. Figure 3 illustrates the timeline and program implementation and the Online Supplementary Pre-Analysis report presents the descriptive statistics for all the family of outcomes that were specified in the pre-analysis plan.

IV EMPIRICAL STRATEGY

IV.A Randomization

Design overview. We evaluate the RHISN program using a two-stage clustered randomized controlled trial implemented in four refugee-hosting regions. In total, 25,000 households were listed as eligible for participation, including approximately 10,000 refugee households and 15,000 host households. The program aimed to enroll a total of 15,000 households, 8,500 among hosts and 6,500 among refugees. All listed refugee households entered into the treatment lottery. Eligible host households were ranked

on observable predictors of poverty using a proxy means test. The poorest 3,000 host households were automatically enrolled in the program. 5,500 household beneficiary slots were retained for the host treatment lottery.

Randomization proceeded in two stages. First, households eligible for the treatment lottery were randomly assigned to treatment — enrollment in the program — or control — exclusion from the program. Second, among treated beneficiaries, households were randomly allocated to public works teams that comprised hosts only, refugees only or a mix of hosts and refugees. In addition, the design cross-randomizes neighborhood-level program intensity and exposure to outgroup members, allowing us to identify both individual treatment effects and spillover effects operating through local saturation and inter-group contact.

Treatment arms. Randomization occurs at both the household and neighborhood levels, generating four cross-cutting sources of experimental variation:

1. **RHISN participation.** Eligible households are randomly selected for a bundled intervention comprising participation in remunerated public works, associated training/coaching, formal documentation to work legally, and subsequent business support (including grants). Let T_i denote an indicator equal to one if household i is assigned to receive RHISN.

2. **Inter-group contact through public work team composition.** RHISN beneficiaries are assigned to public works groups of approximately 30 members. Groups can be (i) refugee-only, (ii) host-only, (iii) *equal mixed* (approximately 15 refugees and 15 hosts), or (iv) *refugee-minority* (approximately 10 refugees and 20 hosts). Let MixedGroup_i indicate assignment to a mixed group (equal mixed or refugee-minority), and let EqualMixedGroup_i and $\text{RefugeeMinorityGroup}_i$ indicate assignment to the corresponding mixed-group type.

3. **After-work social cohesion activities.** A random subset of public works groups is invited—via public lottery—to six monthly teas, one lunch, and one annual graduation festival, activities intended to promote after-work socializing. Let

CohesionActivity$_i$ denote an indicator that beneficiary i is assigned to a group invited to these activities. This treatment was stratified across host-only, refugee-only and mixed groups to ensure balance.

4. **Cluster-level saturation and outgroup exposure.** We cross-randomize at the neighborhood level along two dimensions:

   (a) *High and low program saturation.* Overall program quotas across refugees and hosts are first divided to kebeles and camp zones proportionally to their population. Kebeles are further subdivided into ketenas (camps into block groups) and each of these neighborhoods has approximately 200 - 400 households and 100 - 2,000 residents. Across neighborhoods within each kebele/camp zone, we first compute a population-representative 'raw quota', and then randomly assign neighborhoods into high- and low-saturation: in high-saturation clusters, the number selected for RHISN is set to 125% of the quota; in low-saturation clusters it is 75% of the quota, implying a 65% increase in treatment intensity in high vs. low-saturation neighborhoods. Let HiSat$_c$ indicate high saturation in cluster c.

   (b) *High and low out-group exposure intensity.* In high-exposure clusters, two-thirds of beneficiaries are assigned to mixed groups; in low-exposure clusters, one-third are mixed. Let HiMix$_c$ indicate high out-group exposure in cluster c. Assignment of neighborhoods to high- and low- out-group exposure is stratified by high- vs. low- program saturation to ensure balance.

   Group assignments are constrained by geography and the need for beneficiaries to get to public work sites within reasonable time. Public works are organized at the kebele (camp zone) level. Refugee-only and host-only groups composed randomly with proportional representation from all ketena (block groups) within a kebele (zone). To generate mixed groups, each camp zone is assigned the geographically closest kebele, and mixed groups are again formed randomly with proportional representation of beneficiaries

from all neighborhoods within the matched kebele and camp zone, while ensuring overall mixing rations (50/50 for equal groups, approximately 2/3 hosts and 1/3 refugees for refugee-minority groups).

Together, these four components generate cross-randomized sources of variation that enable the identification of the direct treatment effects, mixing effects, and economic and social spillovers of the program. Figure 4 summarizes the two-stage empirical design and Figure 5 illustrates the location of host and refugee households assigned to treatment and control groups across the four study locations.

Implementation deviations relevant for analysis. Two field contingencies affected mixed implementation and baseline collection.

1. *Mixed-group implementation:* due to program implementation variation and security constraints, mixed-group exposure was only implemented fully in Dabat. In Asayita, mixing was only implemented for the $\approx$ 50% of refugees that live within the city, as opposed to the refugee camp due to high daytime temperatures (sometimes exceeding 40 degrees Celsius) and limited local transport availability between city and camp. In Bambasi, there is only one non-randomly chosen kebele — located between the camp and the city — and matched camp zone where refugees and hosts work in mixed teams. All others are not mixed due to distance and security concerns on the road linking camp and city. In Pugnido, mixing was not implemented due to heightened tensions between host community members and refugees and limited implementation capacity.[10] When presenting the effects of mixing and high-out-group exposure at neighborhood level, we restrict the sample to Dabat and refugees/hosts living within Asayita town.

2. *Baseline survey in Dabat:* due to conflict-related disruptions to field staff travel and communication shutdowns in the Amhara region continuing for several months,

[10]We note that this is an important finding in itself. An integrated social protection program with forced out-group exposure is only feasible when implementation is possible, i.e. when security can be guaranteed and when host-migrant tensions are low enough to feasibly integrate work teams.

baseline household survey data were not collected in Dabat, balance checks there rely on pre-intervention administrative data, PMT survey, and immutable characteristics.

IV.B Econometric specification

Direct program effects and saturation. Our primary estimand for the direct program effects is the Intention-To-Treat (ITT) effect of assignment to RHISN, while also controlling for the cross-randomized saturation assignment:

$$y_{ic} = \beta_0 + \beta_1 T_i + \beta_2 \text{HiSat}_{c(i)} + \gamma y_{ic}^{BL} + \alpha M_{ic}^{BL} + \alpha_s + \varepsilon_{ic}. \quad (1)$$

where y_{ic} is an outcome for individual/household i living in neighborhood/cluster c, T_i is an indicator for household i being assigned to beneficiary status, HiSat_c an indicator for neighborhood $c(i)$ where i resides being assigned to high-saturation status. α_s denote randomization-stratum fixed effects (i.e. eight fixed effects for the four camp for refugees and towns for hosts). y_{ic}^{BL} is the baseline value of the outcome when available, and M_{ic}^{BL} is an indicator for whether y_{ic}^{BL} is missing — with missing baseline outcomes imputed at the sample mean, as specified in the pre-analysis plan. Unless otherwise noted, inference is based on standard errors clustered at the neighborhood-level. Here, β_1 identifies the average effect of being assigned to RHISN and β_2 captures differences associated with assignment to a high-saturation cluster for households which are not RHISN beneficiaries.

We also estimate whether the effect of saturation differs across beneficiary and non-beneficiary households using the following specification:

$$y_{ic} = \theta_0 + \theta_1 T_i + \theta_2 \text{HiSat}_c \cdot \mathbf{1}\{T_i = 0\} + \theta_3 \text{HiSat}_c \cdot \mathbf{1}\{T_i = 1\} + \gamma y_{ic}^{BL} + \alpha M_{ic}^{BL} + \alpha_s + \varepsilon_{ic}. \quad (2)$$

Intergroup contact: mixed groups and outgroup-exposure clusters. We estimate the effects of mixing (which only occurs among treated units) and the impact of being in a high-mixing cluster (on both beneficiaries and non-beneficiaries) using the following specification:

$$y_{ic} = \beta_0 + \beta_1 T_i + \beta_2 \text{HiSat}_c + \delta_1 \text{MixedGroup}_i + \delta_2 \text{HiMix}_{c(i)} + \gamma y_{ic}^{BL} + \alpha M_{ic}^{BL} + \alpha_s + \varepsilon_{ic}. \quad (3)$$

where MixedGroup_i is an indicator for beneficiary i being assigned to a mixed group and $\text{HiMix}_{c(i)}$ is an indicator for the neighborhood c where i resides being assigned to high outgroup exposure. All other terms as above. δ_1 identifies the additional impact for beneficiaries assigned to mixed groups (relative to non-mixed groups), and δ_2 captures neighborhood-level spillovers from high outgroup exposure. For outcomes observed only among treated units (e.g., work-group measures), we estimate (3) restricting to $T_i = 1$ and omitting T_i.

We also examine whether the spillovers from mixing are different across beneficiaries and non-beneficiaries, using the following specification:

$$\begin{aligned} y_{ic} = \beta_0 + \phi_1 T_i + \phi_2 \text{HiSat}_c + \phi_1 \text{MixedGroup}_i + \phi_2 \text{HiSat}_c \cdot \mathbf{1}\{T_i = 0\} + \phi_3 \text{HiSat}_c \cdot \mathbf{1}\{T_i = 1\} \\ + \gamma y_{ic}^{BL} + \alpha M_{ic}^{BL} + \alpha_s + \varepsilon_{ic}. \end{aligned} \quad (4)$$

In secondary analyses we separate equal mixed groups from refugee-minority groups to test whether effects depend on the relative share of out-group members using the following specification:

$$\begin{aligned} y_{ic} = \beta_0 + \beta_1 T_i + \beta_2 \text{HiSat}_c + \theta_1 \text{EqualMixedGroup}_i + \theta_2 \text{RefugeeMinorityGroup}_i \\ + \delta_2 \text{HiMix}_c + \gamma y_{ic}^{BL} + \alpha M_{ic}^{BL} + \alpha_s + \varepsilon_{ic}. \end{aligned} \quad (5)$$

IV.C Internal Validity

This subsection assesses the internal validity of the experimental design by examining tracking rates, attrition patterns, covariate balance across experimental groups, and adjustments for multiple hypothesis testing. The research questions, empirical strategy, and primary and sec-

ondary outcome measures were fully pre-specified and pre-registered.[11] All analyses outlined in the pre-analysis plan are reported in the Online Appendix, while the main paper summarizes the key findings and validity checks.

Tracking rates. The RHISN baseline and midline household surveys aimed to reach a target sample of 9,652 and 13,618 households, respectively, from the main sample with effective replacements across the four primary study locations. Table A.1 shows the overall tracking rates achieved at baseline and midline of 77.9% and 83.9%, resulting in final tracked samples of 7,523 and 11,426 households, respectively. Out of the tracked baseline sample, 92.6% were successfully surveyed again at midline. We observe balance across treatment and control groups.[12]

Integrity of random assignment. We assess baseline balance across experimental arms using administrative and baseline survey measures of pre-treatment characteristics and outcomes. Balance tests are estimated using stratum fixed effects α_s and standard errors clustered at the neighborhood-level, matching the randomization unit. Due to the absence of baseline household survey data in Dabat, we conduct balance checks in that site using pre-intervention administrative data only (i.e., PMT/vulnerability measures and basic demographics) and immutable characteristics.

Tables A.2, A.3 and A.4 report balance tests for baseline demographics and main outcomes, respectively.[13] Across both tables, treatment and saturation coefficients are generally small and statistically insignificant. In all specifications, joint F-tests fail to reject the null that baseline covariates do not predict treatment assignment. Overall, these results confirm successful randomization and baseline balance across both household treatment status and neighborhood saturation.

Testing for non-random attrition. We test for differential attrition by estimating:

$$r_{ic} = \beta_0 + \beta_1 T_i + \beta_2 \text{HiSat}_c + \alpha_s + \varepsilon_{ic}, \tag{6}$$

$$r_{ic} = \beta_0 + \beta_1 T_i + \delta_1 \text{MixedGroup}_i + \beta_2 \text{HiSat}_c + \delta_2 \text{HiMix}_c + \alpha_s + \varepsilon_{ic}, \tag{7}$$

[11] This study was pre-registered in the AEA RCT Registry (AEARCTR-0012288).
[12] Tracking rates for more disaggregated groups are presented in the Online Appendix.
[13] These estimates exclude Dabat because conflict prevented baseline household data collection.

where r_{ic} is an indicator equal to one if unit i is observed at baseline but not at follow-up. Table A.5 presents the regression results showing no evidence of non-random attrition.

IV.D Program Compliance

Table 2 presents the estimates on program participation and compliance. The intervention achieved high compliance rates, with 91% reporting actual enrollment in the program, and 70% and 58% of treatment households reporting participation in public works and livelihood training, respectively.[14] The participation in livelihood trainings is promising for the development of business plans and future parts of the program which involve disbursement of large livelihoods grants to beneficiaries, this could potentially lead to medium-to long-run benefits of the program. However, there is considerable heterogeneity in implementation across groups. Host households are more likely than refugee households to take up the assigned treatment.

The high program take-up translated into a substantial increase in employment and income: beneficiaries logged an average of 4.65 weekly hours in public works and their household received USD 498 PPP in annualized public works income. These hours represent approximately one-third of the maximum eligible hours under the program, and in many cases other household members also participated in public works (contributing up to an additional 0.6 weekly hours). Weekly hours were not statistically different between refugee and host households.

We also observe small but statistically significant positive effects of high program saturation on overall take-up by beneficiaries, suggesting that greater exposure to other participating households may have reinforced individual attendance and engagement.

Who attended public works groups? Table A.6 presents the characteristics of household members participating in public works. Participants are predominantly household heads (81%), with an average age of 38 years. Women account for a substantial share of beneficiaries (65% overall), and there was a higher participation rate among hosts (67%) compared to refugees (62%). There are pronounced differences in educational attainment across groups, with refugees representing the more educationally vulnerable population.

[14] Some individuals may be enrolled in the program without attending public works if they were elderly or had disabilities.

V RESULTS ON ECONOMIC AND WELL-BEING OUTCOMES

We now turn to the direct program's effects on economic, well-being, and inter-generational household outcomes. All estimates are intent-to-treat (ITT) effects obtained from equation (1), using midline data collected between January and March 2025, approximately 16 months after program rollout. At this point, beneficiaries had completed 16 months of public works participation and approximately 20 livelihood training and coaching sessions, but had not yet received the productive grant. The estimates therefore capture the program's effects through its labor and training components alone, prior to any influence of the large capital transfer. The Online Supplementary Pre-Analysis Plan Report presents the ITT effects of RHISN across the primary and secondary outcomes as they were specified in the pre-analysis plan. In this section, we summarize the most important findings based on these analysis.

V.A Economic outcomes

Household labor supply. Table Table 3 reports the ITT effects of RHISN on household labor supply. Three findings stand out. First and foremost, the program creates employment rather than displacing existing work. Total productive labor increases by 4.17 hours per week, a 14% gain relative to the control mean, and this increase is fully explained by an additional 4.76 weekly hours in RHISN public works. Hours worked outside the program are statistically unchanged, indicating that public works draw primarily on previously underutilized labor rather than crowding out other employment. This is consistent with baseline labor market conditions. Control households supply only 29.7 productive hours per week across all members, and 53% report being unemployed, leaving substantial idle labor for the program to absorb.

Second, the limited displacement that does occur is concentrated among the lowest-paid jobs. Wage employment hours decline, but wage earnings remain unchanged, implying that households leave their least remunerative jobs rather than wage employment more broadly.[15] This adjustment is driven primarily by hosts, as refugees have relatively few private-sector jobs to leave. For refugees there is some reallocation toward household agriculture. Measured hourly

[15]Wage employment falls by 0.93 hours per week, a 10% reduction, while wage employment income is statistically unchanged (Panel A of Table 4). Appendix Table B.1 shows the reallocation directly. The overall effect on wage employment participation is zero because two offsetting flows occur: households with no wage work at baseline are 4.1 percentage points more likely to enter wage employment, while those whose baseline job paid below the median are 6.6 percentage points more likely to leave. The decline in hours is driven almost entirely by below-median earners.

wages increase by 21%, but this reflects earnings from public works since hourly wages outside the program are unchanged.[16]

Third, the program strengthens labor market attachment. Unemployment falls by 3.2 percentage points, while job-search hours decline by 18%. Both effects are larger for refugees, for whom RHISN often represents a first formal employment opportunity, although the differences relative to hosts are not statistically significant. The reduction in job search most likely reflects substitution from searching into program employment rather than withdrawal from the labor force.

Household balance sheet. Table 4 reports the ITT effects on the household balance sheet structuring outcomes in three panels. Panel A presents the results on income-related outcomes, panel B on assets, and panel C on consumption. Three main results can be highlighted.

First, household income increases by $565 USD PPP per year (a 25% rise over the control mean) reflecting the direct mechanical effect of the program payments rather than broader changes other types of economic activity.[17] One exception emerges in remittance behavior among refugees. Refugee beneficiaries experience a decline in net remittances ($-28.5 USD PPP), while hosts show no comparable change. The decline in remittances received by refugee households is consistent with a well-documented pattern in which public transfers partially crowd out informal private support. As program income raises household welfare, family members abroad revise downward their assessments of need and reduce transfers accordingly (Jensen, 2004; Albarran and Attanasio, 2003). For refugees in protracted displacement, remittances from abroad are often a primary lifeline, making their partial displacement by program income an important dimension of the overall transfer picture. The decline attenuates, but does not eliminate, the gross income gain from the program for refugee households.

Second, the program increased household assets by USD 207 PPP (19% relative to the control mean), largely through a USD 135 PPP increase in net financial savings. This increase is primarily mechanical, reflecting the program requirement that beneficiaries save 20% of their public works earnings in individual accounts, equivalent to roughly USD 100 PPP given average earnings of about USD 500 PPP, rather than an immediate change in saving behavior.

[16] The corresponding estimate excluding public works earnings is small and statistically insignificant.

[17] Other RHISN benefits include payments made directly to care providers or disabled beneficiaries who could not attend public works.

Nevertheless, this design feature may have important longer-run benefits. As illustrated in Table B.2 for many beneficiaries, particularly 40% of refugees and 10% of hosts, RHISN provides a first opportunity to own a formal savings account, and evidence suggests that access to formal savings can increase productive investment, improve resilience to shocks, and generate persistent improvements in financial behavior through habit formation and commitment mechanisms (Dupas and Robinson, 2013b,a; Ashraf et al., 2006; Schaner, 2018).

The asset response differs across groups in one important dimension. Household durables increase only among hosts, while refugees show no significant change, consistent with greater residential uncertainty leading refugees to favor more liquid and portable assets over durable goods (Chiovelli et al., 2021). By contrast, livestock and agricultural assets increase similarly for both groups, whereas there are no detectable effects on non-agricultural enterprise assets, housing value, or land value, likely because the productive grant had not yet been disbursed at the time of the midline survey.

Third, consumption increases by USD 226 PPP annually (4.2% relative to the control mean), with roughly 80% of the gain coming from higher food consumption, consistent with households using additional resources to meet unmet nutritional needs. Non-food consumption also rises modestly.

V.B Well-being outcomes

Table 5 reports ITT estimates of program effects on broader well-being outcomes. RHISN generates substantial improvements across multiple dimensions of household welfare that extend well beyond labor supply, income and consumption. Treated households exhibit significantly higher food security, reduced psychological distress, improved physical health, enhanced access to amenities, and greater female empowerment. We discuss each of these results in more detail below.

Food security. The food security index rises by 0.13 standard deviations overall, reflecting broad based improvements across all components (Table B.3). These improvements are notable given the severe baseline deprivation (as reported in the descriptive statistics of the Online Supplementary Pre-analysis Plan report). At baseline households were borrowing food, limiting portion sizes, and reducing daily meals on nearly every day of the week. The program reduces reliance on all major food coping strategies, including purchasing cheaper food,

borrowing food, restricting adult consumption to protect children, limiting portion sizes, and reducing the number of meals per day, and raises the probability of having enough food by 4.9 percentage points against a control mean of 42%.

The gains are substantially larger for host households than for refugees. Food security rises by 0.181 standard deviations for hosts, compared with 0.070 standard deviations for refugees, a statistically significant difference that mirrors the larger improvements in food consumption among hosts. A likely explanation is that refugees living in camps already received in-kind food assistance from humanitarian agencies, which provided a basic nutritional floor. As a result, additional RHISN income was likely spread across a wider range of unmet needs rather than being devoted primarily to food, limiting measured improvements in food security despite refugees' greater baseline deprivation.

Mental and physical health. RHISN substantially improves mental health (Table B.4). The mental health index increases by 0.146 standard deviations, with gains across all five components: depression (CES-D), life satisfaction, happiness, perceived stress, and locus of control. These improvements are consistent with evidence that higher incomes and greater food security reduce material stress and increase individuals' sense of control over their lives (Haushofer and Shapiro, 2016). The effects are significantly larger for host households (0.206 standard deviations) than for refugees (0.070 standard deviations), a pattern we examine further in the next section.

Physical health also improves, although more modestly (Table B.5). The health index rises by 0.060 standard deviations overall, with gains concentrated among host households (0.070 standard deviations), while the effect for refugees (0.042 standard deviations) is not statistically significant. A likely explanation is that refugees in camps already had relatively good access to healthcare and schooling through NGO-provided services, limiting the scope for additional improvements. This interpretation is consistent with evidence that camp-based refugees in Ethiopia have higher baseline healthcare utilization than nearby host communities (Wieser et al., 2024).

Amenities. The program improves perceptions of local amenities by 0.070 standard deviations, with gains concentrated in street cleanliness, green space, and trash odor, the dimensions most directly affected by RHISN public works (Table B.6). These improvements likely reflect

beneficiaries' direct involvement in infrastructure projects, which increased their awareness of and ownership over community improvements, even though the benefits were shared more broadly. The effects are driven entirely by host households, whose amenities index rises by 0.111 standard deviations, while the effect for refugees is small and statistically insignificant. A likely explanation is that host communities had infrastructure more amenable to cleaning and maintenance, whereas the more degraded physical environment of refugee camps limited the scope for measurable improvements in neighborhood quality.

Female empowerment. RHISN increases female empowerment by 0.149 standard deviations overall (Table B.7). The gains are larger for refugee women (0.195 standard deviations) than for host women (0.110 standard deviations), reversing the pattern observed for food security and mental health. Nearly all of the improvement is driven by a 0.402 standard deviation increase in resource control, measured through ownership of a personal bank account and mobile phone. These assets are directly linked to the program's payment mechanism, suggesting that transferring benefits through individual accounts strengthens women's control over household resources. The larger gains among refugee women are consistent with their lower baseline levels of financial inclusion, which left greater scope for improvement. The program also shifts refugee women's attitudes toward more egalitarian gender roles, but has no detectable effects on domestic conflict, controlling behaviours, or emotional violence at midline.

These midline improvements in financial access are likely to be particularly consequential for longer-run outcomes. Evidence shows that women's ownership of individual bank accounts and control over earned income generates durable improvements in labor supply, bargaining power, and gender norms well beyond the program period, with effects especially pronounced among women with initially low levels of economic participation (Field et al., 2021).

V.C Intergenerational effects

The welfare consequences of safety net programs extend beyond the direct beneficiaries to shape the developmental trajectories of their children. We examine this intergenerational dimension by analyzing a comprehensive module of children's outcomes covering physical health and nutrition, labor and time use, and socio-emotional development (Table 6). None of these outcomes were measured at baseline, so estimates represent the ITT effects excluding baseline covariates. We document improvements in child health and nutrition, no change in

child labor despite a substantial increase in adult labor supply, and meaningful reductions in behavioral and emotional difficulties among host children, with no corresponding gains for refugee children.

The three outcomes carry different degrees of novelty. Improvements in child health and nutrition confirm a well-established link between household cash transfers and children's physical well-being (Gertler, 2004; Haushofer and Shapiro, 2016). The null child labor result contrasts with evidence from public works programs in other settings (Shah and Steinberg, 2021) and is consistent with experimental evidence that cash transfers do not increase child work (Edmonds and Schady, 2012; Hizıroğlu Aygün et al., 2024). The socio-emotional findings are, to our knowledge, entirely new, with no prior experimental evidence linking public works participation to improvements in children's behavioral and emotional functioning as measured by a validated instrument. These gains are particularly consequential given the well-documented long-run effects of childhood behavioral and emotional difficulties on educational attainment and labor market outcomes (Currie and Stabile, 2006; Fletcher, 2014), suggesting that the program may generate human capital returns that extend well beyond the current period of observation.

Child health and nutrition. Panel A of Table 6 shows meaningful improvements in children's physical well-being. The child health index increases by 0.087 standard deviations overall, with similar gains for refugee and host children. The improvement is largely driven by better nutrition. The child nutrition index rises by 0.113 standard deviations overall, with larger gains for host children than for refugee children (0.156 versus 0.067 standard deviations). Children in treated households are also less likely to go to bed hungry, particularly among hosts, consistent with the improvements in household food security. Together, these findings suggest that households use the additional income to improve children's nutrition, consistent with evidence that cash transfers improve child health and reduce food insecurity (Gertler, 2004).

Child labor and time use. Panel B of Table 6 shows no evidence that the program increases child labour. Effects on child labour participation, hours of paid work, domestic chores, and time spent with adults are all small and statistically insignificant for both refugee and host households. Despite the increase in adult labour supply through public works, caregivers

absorb the additional work without relying on children or reducing parent-child interactions. This finding is consistent with evidence that income transfers reduce rather than increase child labour (Edmonds and Schady, 2012; Hizıroğlu Aygün et al., 2024), and contrasts with evidence from India's NREGS public works programme, where higher adult labour demand increased children's work and reduced school enrolment (Shah and Steinberg, 2021).

Socio-emotional development. Panel C of Table 6 shows improvements in children's socioemotional well-being, measured using the Strengths and Difficulties Questionnaire (SDQ) (Goodman, 1997). Among host children, the overall difficulties score falls by 0.362 points, indicating fewer behavioral and emotional problems. The gains are driven by reductions in hyperactivity and inattention, a dimension closely linked to later educational and labor market outcomes (Currie and Stabile, 2006; Fletcher, 2014). No statistically significant effects are detected for refugee children.

These findings are consistent with a parental mental health channel. Host adults experience larger improvements in food security and mental health, and the literature documents strong links between parental and child well-being (Bütikofer et al., 2024). The absence of comparable effects among refugee children may therefore reflect the smaller material and psychological gains experienced by their parents, limiting the transmission of benefits to children's development.

V.D Saturation Effects

At midline, we find no statistically significant neighborhood saturation effects after adjusting for multiple hypothesis testing as reported in the Online Supplementary PAP Report. Changes to these outcomes are expected after the grant disbursal which took place after midline data collection.

V.E Key Takeaways

The program generates broad and statistically significant improvements in economic welfare, reflected in higher labor supply, income, financial savings, and consumption. These economic improvements are accompanied by gains in individual well-being, including better food security, physical and mental health, and female empowerment, suggesting that the intervention strengthens both material and psychosocial resilience. A recent systematic review of experimental public works evaluations across LMICs finds that such well-being gains are observed

only in isolated cases, are not robust across programs, and suffer from significant measurement limitations (Bagga et al., 2024); the depth and consistency of effects documented here is therefore particularly notable.

Several findings also suggest that the program may have lasting effects beyond its implementation period. The mandatory savings component connects highly vulnerable households to the formal financial system, while the large gains in women's financial inclusion, particularly among refugee women, point to more persistent improvements in control over household resources and economic agency.

A striking result is that the program effects are systematically larger for host households than for refugees across economic, well-being, and child-related outcomes. We explore the determinants of these differences in section VII.

VI RESULTS ON SOCIAL OUTCOMES

This section examines the effects of the RHISN program over the social cohesion between refugees and hosts. We organize our discussion in two subsections. We first describe the direct effects of the RHISN program on social cohesion. This analysis informs how sharing institutions, in this case the RHISN program, affected overall social cohesion between groups. We then examine, separately, the social cohesion effects of deliberate intergroup contact, which was only randomized for RHISN participants. Our analysis is framed around the effects of the program on six pre-registered indices designed to capture distinct but complementary dimensions of refugee-host integration, which include:

1. *Outgroup Attitude Index:* an index of 12 items measuring comfort and trust in everyday interactions with the outgroup, including willingness to live as neighbors, share meals, allow children to attend the same school, work together, return lost valuables, provide emotional support, and general perceptions of refugees' contributions to the Ethiopian society.

2. *Policy Index (PA):* an index of six items measuring host support for refugee inclusion policies, including granting full citizenship, work permits, and freedom of movement, support for humanitarian and government spending on refugees, and perceptions of the net effect of refugees on Ethiopian society.

3. *Economic Interaction Index (EI):* an index of four items measuring market-based interactions with the outgroup, including the share of sales to outgroup members, co-ownership of businesses with outgroup members, the share of outgroup co-workers in paid employment, and the share of remittances sent to the outgroup.

4. *Social Interaction Index (SI):* an index of eight items measuring social ties with the outgroup, including the share of close relatives, close friends, good friends, economic relationships, and community group members from the outgroup, as well as sharing tea, and children's friendships and play with outgroup children.

5. *Social Cohesion Index (SC):* an index of six items measuring trust and pro-social behavior toward the outgroup, including hypothetical and incentivized donations to outgroup members, willingness to sign petitions supporting refugees, interpersonal trust, perceptions of refugee integration, and sense of national identity.

6. *Political Integration Index (PI):* an index of four items measuring civic and political engagement, including formal identity document ownership, holding a leadership role, citizen participation in community affairs, and trust and satisfaction with local government. Importantly, this index measures the in-group civic integration and is not measuring perceptions or behaviors toward the outgroup.

The *Outgroup* and *Policy* indexes are mostly composed of self-reported measures that might not reflect actual behaviors. For that reason, we also collected comprehensive information to construct the three additional indexes measuring actual behaviors toward the outgroup including the social and economic interactions, incentivized games, and willingness to sign petitions to support the outgroup. The last index, the *Political Integration* index, measures civic integration and is not directly capturing perceptions or behaviors toward the outgroup. Yet, it measures an important dimension of civic engagement. Together, these measures are intended to enable a robust overview of social cohesion between groups within hosting communities. For robustness, we also report the effects of the program of an aggregated *Combined* index that averages all the indexes described above.

VI.A Direct program effects

Table 7 reports ITT estimates of the direct effects of RHISN on social cohesion. Our most important result is that the program generates large and statistically significant improvements in the combined overall social index of 0.055 SD. These effects are predominantly driven by gains from hosts, who see improvements across outgroup attitudes, policy preferences, social and economic interactions, and prosocial behaviors. The single exception is the political integration index, where we observe improvements for refugees and hosts. In contrast, estimated effects for refugees are non statistically significant, and in fact, in most cases negative but small across indexes. Moreover, all the estimates are statistically different between refugees and hosts, with the exception of the economic interaction index. We explore potential explanations for the heterogeneous results by group in the section VIII.

The effect sizes for each of the indexes for the whole sample, ranging from 0.056 standard deviations on outgroup attitudes to 0.116 standard deviations on social cohesion among hosts, are broadly comparable to the average effect of roughly 0.08 standard deviations estimated across preregistered intergroup contact interventions (Lowe, 2025). Importantly, these effects were generated by program participation alone, without accounting yet for the deliberate engineering of intergroup contact, suggesting that institutional inclusion within a shared safety net is sufficient to shift host attitudes toward refugees across every measured dimension.

Upon examining what variables drive the changes observed in each of the indexes, we distinguish five additional results. First, the program induced sharp improvements in hosts' self-reported attitudes toward refugees (Table B.8). These gains reflect a true shift in attitudes rather than a mechanical consequence of joint participation in the program, as excluding the items most directly tied to program participation (such as having meals with the outgroup, willing to work together, or have them as supervisors) leaves the estimated effects essentially unchanged. The attitudinal gains are primarily driven by improvements in the willingness to rely on a refugee for emotional support, comfort with having children attend the same school as refugees, and the belief that refugees will return lost valuables if they find them.

Second, the attitudinal improvements for hosts translate into more social but not more economic interactions once the mechanical effects of the program are excluded (Table B.11 and

Table B.10).[18] These gains are entirely concentrated in children's social interactions, while the effects on adult social ties are small and statistically insignificant. The program, consequently, appears to improve intergroup social relations primarily for the next generation rather than among adult participants themselves. This pattern mirrors evidence from (Tamim et al., 2025), who find that refugee programs disproportionately affect children's cross-group social dynamics relative to those of adults, although in their setting the effect reflects a deterioration rather than an improvement in intergroup relations among children.

Third, RHISN recipients show higher support for policies related to refugee self-reliance, but these changes are not translated into actual political actions or mobilization. In particular, host households report greater support for refugee inclusion policies (Table B.9), such as gains of 0.113 SD in support for full citizenship, 0.094 SD for work permits, and 0.053 SD for freedom of movement. By contrast, we find no statistically significant effects on support for humanitarian or government spending on refugees, nor on perceptions of the net effect of refugees on Ethiopian society. These findings suggest that direct co-participation in a shared program can shift attitudes toward refugees' concrete inclusion rights and self-reliance, while deeper political and fiscal beliefs remain largely unchanged over the short run.

Fourth, we document large gains in hosts' self-reported and observed prosocial behaviors toward refugees (Table B.12). RHISN recipients score 0.061 SD higher on the social cohesion index than the control group, and this effect is driven entirely by host households, with no significant effects for refugees. Among hosts, significant gains are observed in interpersonal trust (0.060 standard deviations), perceptions of refugee integration (0.066 standard deviations), national identity (0.041 standard deviations), and importantly, hosts also transfer significantly more resources to refugees in the incentivized dictator game (0.661 standard deviations), providing evidence of changes in actual altruistic behaviors under real financial stakes rather than in stated preferences alone. At the same time, we detect no significant effect on willingness to sign petitions supporting refugees, consistent with the findings on policy attitudes, which suggest that the program shifts interpersonal attitudes and prosocial behavior toward refugees without translating into political mobilization on their behalf. Finally, the program does not shift refugees' sense of national identity toward Ethiopia, despite its scale and the depth of

[18]The effects of the program on the number of economic interactions is positive but becomes null once we exclude the share of out-group coworkers.

formal inclusion it provides. This suggests that economic integration alone may be insufficient to foster a sense of belonging in the host country, or that these outcomes require more time to evolve (Rozo, 2026).

Finally, political integration is the only dimension of social cohesion for which the program generates significant improvements among both refugees and hosts, with no statistically significant difference across groups (Table B.13). The alternative index, which excludes community meeting attendance and formal ID ownership to address potential mechanical program-driven increases, yields nearly identical effects for both groups, confirming that the gains reflect genuine improvements in civic engagement rather than program mechanics. Significant effects are observed for both groups in citizen participation, political actions, and trust in local government, suggesting that direct engagement with a government-delivered program encourages civic participation and strengthens institutional trust symmetrically across refugees and hosts. This convergence stands in sharp contrast to the pattern observed for all other social cohesion indices, where gains are concentrated almost entirely among host households.

VI.B Intergroup mixing effects: Implementation in Dabat and Urban Asayita

The RHISN program provides a rare experimental setting in which to test the contact hypothesis at scale. Program participants from refugee and host communities work side by side on shared infrastructure projects, creating conditions that closely approximate those identified as optimal for prejudice reduction (Allport, 1954). Participation in the same public works group establishes equal status among members, the shared objective of completing community projects provides a common goal, the collaborative nature of the work requires sustained intergroup cooperation, and the government administration of the program provides institutional authority and legitimacy. By the time of the midline survey, participants had accumulated approximately 70 weeks of joint activity at roughly five hours per week, representing a substantially higher dosage of structured intergroup exposure than any prior contact experiment we are aware of in a fragile or conflict-affected setting (Mousa, 2020; Lowe, 2021).

Dabat and urban Asayita. Intergroup mixing was fully implemented as designed in Dabat and urban Asayita. While in Asayita it was only partially implemented between refugees living outside the camps and nearby hosts. In the rest of Asayita, the geographic distance between the refugee camp and the host community, together with extreme temperatures, prevented

joint participation. In Pugnido, elevated intergroup tensions following enrollment made joint public works groups infeasible, and in Bambasi, the long distance between the camp and the host community, together with severe road insecurity, prevented mixing. Despite these constraints, we successfully randomized 180 groups into refugee-only, host-only, and mixed compositions across Dabat and urban Asayita, preserving the core features of the experimental design and providing sufficient variation to estimate the effects of intergroup contact on our pre-specified outcomes.

Direct program effects - Dabat and Urban Asayita. We compare the direct program effects for the full sample, the subsample not randomized to mixing (mainly from Bambasi and Asayita), and the Dabat and urban Asayita subsample randomized to mixing in **??**. The positive direct social effects of RHISN in the full sample are largely driven by the subsample randomized to mixing and by hosts (Panel C). Within this subsample, effects are positive and larger across all indexes for hosts, while those for refugees are small and statistically insignificant. The concentration of positive effects in areas where mixing was randomized, and where refugees worked alongside hosts in local communities, suggests that these effects may be driven by hosts' exposure to refugees working in their communities. In non-mixed areas, by contrast, refugees mostly worked within camps and were therefore less visible to host communities.[19] We explore this hypothesis in greater detail in Section VIII.

Intergroup mixing effects. We examine the ITT estimates of working in mixed and non-mixed public works groups using equation 3. Table 8 illustrates the results splitting the analysis by non-mixing effects,additional effects of mixing, and High mixing saturation spillovers.

Assignment to a mixed public work group improves the overall combined social index by an additional 0.07 SD beyond the baseline program effect, and these effects are predominantly driven by improvements in economic interactions (Table 8). The effects are positive and large for refugees and hosts, but statistically significant for hosts only. The magnitude of this effect is consistent with the broader experimental evidence on intergroup contact. A recent meta-analysis of 41 pre-registered contact experiments finds that structured intergroup contact improves outgroup attitudes by approximately 0.10 SD on average (Lowe, 2025), placing our estimates squarely in line with the most effective interventions documented to date. Our setting

[19]With the exception of Bambasi where non-random mixed groups were formed in public works but they worked in a site between the camps and the cities so exposure was not as large.

is distinctive, however, in that contact occurs through a large scale government-administered public works program and in that it is a prolonged intervention of almost two years (Mousa, 2020; Rao, 2019).

For mixed hosts, the program improves outgroup attitudes, and economic and social interactions. For refugees the program improves economic interactions only. The uncovered effects in social and economic interactions are, however, largely mechanical. Decomposing each index reveals that the gains are driven by interactions with public works co-workers from the outgroup directly and dissipate once co-worker ties are excluded, indicating that mixed group assignment generates cross-group contact within the program rather than broader integration into outgroup social and economic networks (Table C.2 and Table C.3).

Finally, within the sample randomized to mixing, the effects of the program among individuals assigned to non-mixed groups are positive overall and for hosts, but negative for refugees. The negative effects for refugees are driven by reductions in economic interactions with the outgroup. We hypothesize that this may reflect either fewer opportunities to interact with hosts relative to refugees assigned to mixed groups, or frustration from observing other refugees leave the camps to work alongside hosts in local communities. We explore these hypotheses in greater detail in Section VIII.

Intergroup mixing saturation effects. The program induces positive spillovers on social cohesion that are almost 1.5 to 2 times as large as the direct and intergroup mixing effects. The effects are largely driven by gains in outgroup attitudes and economic interactions. That neighborhood-level exposure to mixed public works groups is associated with attitude gains nearly 1.5 times larger those from individual assignment is difficult to reconcile with a simple social multiplier, where each treated individual persuades several neighbors. While such a multiplier is possible in principle, the size of the effect seems large for an indirect channel. A more plausible interpretation is that high neighborhood saturation shifts perceived social norms around intergroup engagement. When a visible share of community members participate in mixed groups through a formal government program, this may alter what residents believe others in their community think and do, a channel that could operate more broadly than the attitude updating that occurs through individual experience alone. We explore this potential channel in section VIII. Prior contact experiments have documented limited evidence

of spillover effects beyond the individuals directly encountered during the intervention (Lowe, 2025; Mousa, 2020). The saturation effects we observe are consistent with the possibility that community-level exposure to structured contact may reshape local norms.

Varying intensity and depth of intergroup contact. Our experimental design included two additional variations that allow us to assess the role of intensity and depth of intergroup contact for shaping social cohesion between groups. We vary contact *intensity* by comparing mixed groups randomly assigned to equal or refugee-minority composition, estimating equation (5), and vary contact *depth* by cross-randomizing mixed groups to social tea events, where group members spent time together outside of work. Neither margin induces changes in the combined social index that are statistically significant (Table 9). Yet, the effects are large and have opposite signs, with the core of variation mainly driven by the effects induced in the refugee group. For refugees, increasing contact intensity through more equal group composition generally reduces social cohesion, while deepening contact through social events improves it dramatically. These effects are driven primarily by changes in economic interactions and suggest that intergroup gatherings outside of structured work are largely beneficial for building economic relations potentially because they allow the space to chat and built these relationships.

VII EXPLORING HETEROGENEOUS EFFECTS

Our main results point to substantial heterogeneity in both the direct and intergroup treatment effects, particularly between refugee and host groups. In this section, we examine two questions. First, what explains treatment effect heterogeneity? In particular, is household type (refugee or host) itself the main source of heterogeneity, or do the observed differences reflect variation in program implementation and other baseline characteristics? Do the same households benefit most across economic, well-being, and social cohesion outcomes, or do different households benefit across these domains? Second, we examine whether refugees' connectedness to local markets and host communities is associated with heterogeneity in program effects.

We use a causal forest to answer these questions because the object of interest is not a a small set of pre-specified interactions, but treatment effect heterogeneity across a high-dimensional set of baseline characteristics. Specifically, we estimate a fixed-effects-adjusted causal forest over baseline moderators. We implement the causal forest following Athey and Wager (2019)

and Wager and Athey (2018), incorporating fixed effects for Region × HH-Type through cross-fold residualization (Kattenberg et al., 2023). Here, HH-Type distinguishes refugee and host households, while Region identifies our four study locations.

The forest is trained on a rich set of pre-treatment capturing seven dimensions of heterogeneity including context (study region, refugee or host household type, and neighborhood saturation), demographics, livelihoods sources, social capital and connectedness, economic conditions, mental health, and the baseline value of the outcome under study. These variables characterize households' initial economic conditions and exposure to the program, allowing the forest to identify the baseline profiles most predictive of treatment effect heterogeneity. The resulting model ranks the importance of each baseline characteristic and estimates household-level conditional average treatment effects (CATEs). Appendix D reports the complete list of moderators.

VII.A What explains treatment effect heterogeneity?

The mechanical role of implementation fidelity. Figure D.1 illustrates the baseline characteristics that are most predictive of treatment-effect heterogeneity across all outcomes. Heterogeneity in program implementation is driven primarily by geographic location, with study region emerging as the strongest predictor of public works participation, hours worked, and RHISN income.[20] Beyond program implementation, however, no single baseline characteristic consistently predicts larger treatment effects across outcomes. Instead, different characteristics emerge as important for different outcomes.

The high relevance of geographic location in predicting RHISN implementation raises the question of whether regional differences in outcomes reflect differences in program implementation fidelity. Figure D.2 examines this possibility by normalizing regional treatment effects by the region-specific predicted public works participation effect. Regional differences remain pronounced after this normalization, and the relative performance of regions continues to vary substantially across economic, well-being, and social cohesion outcomes. Thus, differences in program implementation fidelity alone cannot account for the regional heterogeneity in downstream effects.

[20]Similar geographic variation has been documented in other public employment programs, where local implementation capacity and labor market conditions determine what the program ultimately delivers (Imbert and Papp, 2015b; Muralidharan et al., 2023b; Bagga et al., 2024).

The relevance of initial endowments. Once delivery is held roughly fixed by normalizing it using the regional implementation CATE, households with stronger initial endowments are better able to translate the program into outcome gains more effectively. The characteristics associated with larger gains, however, vary by outcome. Figure D.3 shows that households with higher baseline schooling, stronger labor market attachment, and greater economic resources experience larger gains in income, consumption, and assets, while stronger pre-existing social connectedness predicts larger improvements in social outcomes. By contrast, the gender of the intended beneficiary is the dominant predictor of female empowerment, highlighting the importance of targeting women as direct beneficiaries.

Overall, these findings suggest that households with stronger initial endowments are better able to translate the program into economic gains, consistent with the complementarities between transfers, human capital, and market access documented in the anti-poverty literature (Banerjee et al., 2015; Bandiera et al., 2017; Blattman et al., 2014; Balboni et al., 2022). They also echo the targeting tradeoff emphasized by Haushofer et al. (2022) whereby the households with the largest predicted gains are not necessarily the most deprived.

VII.B Exploring the role of connectedness

Refugees mechanically face greater barriers to connectedness than hosts. These barriers may be particularly salient for refugees living in camps or those with weaker ethnic ties to host communities. We explore whether connectedness shapes program effectiveness along two dimensions: (i) camp versus town residence and (ii) ethnic proximity to hosts. Our results, presented in Appendix D.A, provide little evidence that treatment effects systematically vary along either margin.

VIII POTENTIAL MECHANISMS

Our analysis of the program's effects on social cohesion points to three main results. First, the program itself, even without experimentally induced inter-group contact, improves the combined social index by 0.06 SD, with gains concentrated among hosts and in mixed areas. Second, inter-group mixing generates an additional 0.07 SD increase in the index, roughly as large as the program's direct effect, with positive effects for both refugees and hosts. Third, the program generates positive spillovers on the index of 0.10 SD, 1.5 to 2 times as large as the direct and inter-group mixing effects, with positive effects for both beneficiaries and non-

beneficiaries. In this section, we explore the potential drivers of these effects.

VIII.A Rationalizing the program's effects for refugees

Direct Effects— For refugees, the direct effects of the program on the combined social index are close to zero in both the full sample and the subsample not randomized to inter-group mixing. One explanation is that refugees already had high levels of social cohesion toward hosts at baseline, leaving limited scope for further improvement. Figure E.1 supports this interpretation, showing high baseline values across the social cohesion indexes for refugees.

The results differ within the subsample randomized to mixed or homogeneous groups. Refugees assigned to homogeneous groups experience negative effects on the combined social index, driven primarily by reductions in economic interactions with hosts. Two mechanisms may explain this result. First, refugees may replace some relationships with hosts with new relationships with refugees formed through the public works groups. Second, refugees assigned to homogeneous groups may become frustrated when they observe other refugees leaving the camps to work with hosts in local communities.

Additional Inter-group Mixing Effects— For refugees, the additional effects of inter-group mixing on the combined social index are positive but imprecisely estimated, with gains driven primarily by increased economic interactions. Our experimental variation in the intensity and depth of contact helps identify which types of contact matter for refugees (Table 9). While increasing the intensity of contact through equal sized groups does not improve social cohesion for refugees, increasing its depth through social events outside working hours produces a positive and statistically significant effect on the combined social index, again driven by increased economic interactions. These results suggest that deeper, informal interactions may be particularly effective at strengthening refugees' economic relationships with hosts.

In summary, refugees are already positively disposed toward hosts at baseline, leaving relatively limited scope for the program alone to improve social cohesion. Inter-group mixing, however, generates additional gains, showing that direct contact can further strengthen social outcomes. More equal group composition does not generate further gains, while deeper contact through informal social interactions does. Taken together, these results suggest that for refugees, the depth of inter-group contact matters more than its intensity.

VIII.B Rationalizing the social cohesion program's effects for hosts

Direct effects— The bulk of RHISN's direct effects on social outcomes are driven by hosts in the subsample randomized to inter-group mixing in Dabat and urban Asayita. These locations differ from the other study sites along several dimensions. Program implementation was more faithful to the original design, with greater program awareness (Table E.1), and refugee settlements were closer to the main host communities than in Bambasi and Pugnido, making inter-group mixing feasible in the first place (Table 1 and Figure 5). Hosts in Dabat also seem more welcoming toward refugees to begin with. Table E.2 compares social outcomes across regions using respondents in low-saturation control areas, given the lack of baseline data in Dabat, and shows substantially higher host social outcomes in Dabat than in the other three regions. In Asayita, hosts and refugees sre largely co-ethnic Afar, so their closeness comes from shared ethnicity, language, and clan rather than from the attitudes we measure here. Although low-saturation-control areas outcomes may be affected by treatment spillovers, the estimated spillovers are much smaller than the gap between Dabat and the other sites. The fact that the program is more effective where hosts were more welcoming, is also in line with our heterogeneity results (Section VII) where we show that hosts who start with stronger social connectedness gain more from the program.

The heterogeneous effects presented in Section VII show that differences in program implementation fidelity do not fully account for the estimated social effects of RHISN. Although implementation was stronger in Dabat and urban Asayita, conditioning for program fidelity does not account for the differences in social outcomes across sites. Exposure alone is also unlikely to explain these effects. Both beneficiaries and controls live in the same neighborhoods and are exposed to refugees working in their communities, yet only beneficiaries move, so passive community exposure alone cannot be the driver. However, it remains a possibility that treated individuals working in homogeneous groups in Dabat and Asayita might have had a more direct exposure to refugees in the program, just by the nature of program co-participation and because in this region mixed groups worked in the same city as homogeneous groups.

Two other complementary explanations may also account for the documented effects. First, the economic gains generated by RHISN may improve hosts' attitudes and behaviors toward refugees. Consistent with this explanation, Beltramo et al. (2026) show in Mozambique that ex-

perimentally improving financial security among refugees and hosts increases social cohesion, while subsequent losses in financial security reverse these gains. We look at this by asking whether better economic conditions go together with better social outcomes. Figure E.1 shows a positive association between baseline assets and social outcomes, including among hosts. But this is only a baseline correlation, so it cannot tell us whether wealthier hosts are simply more open to refugees to begin with, or whether the extra income from the program is what moves them. Income effects, however, cannot be the whole story because hosts outside Dabat and Asayita received income but showed no social effects, and normalizing regional effects by program delivery does not close that gap (Section VII). Income can still be a real contributor, but it only appears to convert into cohesion where hosts and refugees are in the same program in a salient way.

Second, reciprocity may play a role. Hosts who are beneficiaries may recognize that RHISN and its associated investments are available to them because of the presence of refugees, and that may be generating more positive attitudes and behaviors toward them. Consistent with this mechanism, Baseler et al. (2025) show that cash transfers to hosts in Uganda and Kenya, explicitly linked to the refugee response, increase support for refugee integration. Reciprocity could also go beyond recognizing the sources of funding and might be related with co-participation in refugee inclusive institution; it could come from receiving a program that includes refugees. We test the reciprocity channel experimentally by priming a random sample of beneficiaries about the source of RHISN funding and its direct link to the presence of refugees. The treatment raised beneficiaries' awareness of the program and had a positive but imprecise effect on the belief that RHISN is there because of refugees (Table E.3), and we find no evidence that it improves hosts' attitudes toward refugees (Table E.4). So giving hosts this information does not move their attitudes, but because the first stage is weak we cannot fully rule reciprocity out. Hosts may already have known why RHISN is in their area, or the goodwill may come from taking part in a shared program rather than from the information itself.

In sum, after ruling out program fidelity, passive exposure, and income on its own, what remains is co-participation in a shared refugee-inclusive program, in places where hosts were already relatively open to refugees, working through the direct exposure it brings, the goodwill of a shared program, or income that improves relations only in this setting.

Additional Inter-group Mixing Effects— For inter-group mixing, we also document additional gains in social cohesion among hosts, driven primarily by improvements in economic interactions and outgroup attitudes (Table 8). However, experimentally increasing either the intensity of contact through more equal group composition or its depth through social events generates no additional gains for hosts (Table 9).

VIII.C Rationalizing the Spillover Effects

We find positive social spillovers from living in locations randomized to a higher share of mixed groups. These effects are similar for beneficiaries and non-beneficiaries, although imprecisely measured for the latter, suggesting that they are not mechanically driven by direct participation in the program.

Two complementary mechanisms could explain these effects. First, network effects may spread the benefits of inter-group contact beyond those directly exposed, as new relationships between beneficiaries and refugees extend to other members of their social networks. The magnitude of the spillovers, which are nearly twice as large as the direct and additional mixing effects, makes network effects alone a less likely explanation. It would require individuals not directly exposed to the program to respond more strongly than those who participated in it. Moreover, it is more unlikely given that at least 25 to 50 percent of networks in Asayita and Dabat for the people in our sample live outside the individual's neighborhood (Table E.6). Yet, we cannot completely rule it out and it might be a contributing factor.

Second, social norms may change as people observe refugees working alongside hosts and contributing to their communities, leading to more favorable social outcomes toward refugees. We find compelling evidence in support of this exposure hypothesis. The social spillover effects are predominantly driven by hosts (Table E.C), who live closer to the public works sites (Figure V.2) and are therefore more likely to observe refugees working in their communities. When we decompose the social indexes, we find large and statistically significant spillover effects, for both beneficiaries and non-beneficiaries, on perceptions that refugees contribute positively to Ethiopia (Table V.7). Together, these results suggest that exposure to refugees working and contributing to local communities may explain large part of the positive spillover effects.

IX CONCLUSION

This paper examines the midline effects of a flagship national anti-poverty public works program that integrates refugees into the same program as Ethiopian nationals, providing the same benefits and opportunities to work alongside one another. For approximately sixteen months before our midline survey, hosts and refugees participated in paid public works and coaching and skills training. Our experimental design allows us to separately examine the direct effects of the program, the additional effects of inter-group mixing within public works, and local spillover effects. We measure impacts on economic and well-being outcomes and on a comprehensive set of social outcomes, including self-reported measures and incentivized games. The program was implemented in four refugee-hosting regions of Ethiopia. Due to distance, weather, and conflict, inter-group mixing was only feasible in Dabat and urban Asayita, comprising 180 randomized public works groups and roughly 5,000 study households.

Our most important finding is that, at midline, the program directly improved hosts' attitudes and behaviors toward refugees, with additional gains from inter-group mixing roughly as large as the direct effect. We also find positive spillovers among residents of high-mixing neighborhoods that are about 1.5 to 2 times the size of the direct effect. These gains are comparable to those of dedicated contact interventions, but here they are generated through an anti-poverty program and appear not only in self-reported outcomes but also in incentivized behavior with real stakes, with hosts transferring significantly more resources to refugees in dictator games. The social effects are concentrated among hosts and in the areas where inter-group mixing was operationally feasible. For refugees, who were already positively disposed toward hosts at baseline, the direct social effects are close to zero, although the program improves their political and community engagement. Deeper contact appears more consequential for refugees than workplace contact alone. After-work social events generate sizable gains in the combined social index, driven primarily by greater economic interactions with hosts. Finally, in the full sample, the program also generates broad improvements in economic and well-being outcomes, including income, consumption, mental health, child nutrition, and female empowerment. These well-being gains are generally larger for hosts, and we find no evidence of economic spillovers at this stage of the program.

The concentration of the social effects in urban Asayita and Dabat, and among hosts, is con-

sistent with several forces working together. First, these are the settings where baseline relations between refugees and hosts were relatively more favorable, and our heterogeneity results show that hosts with stronger initial social connectedness gain more from the program. Second, the direct gains do not require assignment to a mixed team, since host beneficiaries improve even when assigned to host-only groups. Rather, they arise among beneficiaries participating in a refugee-inclusive program in the two sites where refugees were also visible participants in the same local program. Differences in implementation fidelity and income gains alone do not explain this pattern, and simple passive exposure cannot account for the beneficiary-control difference within these communities. This leaves participation in a shared refugee-inclusive program as a central part of the explanation. It may operate through greater exposure to refugees generated by program participation, goodwill from benefiting from a program that also includes refugees, or economic gains that improve attitudes specifically in this shared setting, although we cannot fully distinguish among these pathways.

Third, the neighborhood spillovers are most consistent with broader exposure to refugees working and contributing alongside hosts. Hosts live closer to public works sites, and those in high-mixing neighborhoods become more likely to report that refugees contribute positively to Ethiopia, whether or not they themselves participate in the program. Networks may transmit some of these gains, but network diffusion alone is difficult to reconcile with spillovers that are larger than the direct effects.

We document that economic inclusion improves social cohesion when it takes place in settings that create opportunities for inter-group contact. This scope condition is visible within our own study. In Bambasi and Pugnido, where opportunities for meaningful refugee-host contact through the program were limited or absent, RHISN generated economic gains but social outcomes did not improve. By contrast, in Dabat and urban Asayita, directly mixing refugees and hosts generates additional social gains, while greater neighborhood exposure to mixed groups produces positive spillovers beyond those who participate directly. Past evidence on public works programs has focused almost exclusively on their economic and well-being effects. We provide, to our knowledge, the first experimental evidence from varying refugee-host composition within work teams in a national safety net. Our findings show that these government programs, which are widely implemented in developing countries where most refugees are

hosted, can also be used to foster inter-group contact and strengthen social cohesion where conditions allow it.

This paper also shows that integrating refugees into the same social programs available to national citizens can do more than reduce duplication of effort. When integration creates opportunities for contact and makes refugees' contributions visible, it can also strengthen social cohesion between groups. This contrasts with parallel, aid-financed systems that serve refugees separately and may generate resentment among excluded hosts. In our setting, the largest gains emerge where communities can observe refugees contributing alongside hosts, and this visibility improves views of refugees even among individuals who are not program beneficiaries.

An important caveat is the difficulty of implementing inter-group contact at scale in settings with low implementation capacity. In our study, mixing was only feasible where refugees and hosts lived relatively close to one another and where relations were already comparatively good. Where antagonism was more severe, as in Pugnido, mixing could not be implemented at all. The settings that may benefit most from engineered contact may therefore also be those where it is hardest to deliver. Even where mixing was feasible, preserving the experimental design required sustained support from the research and operational teams, raising questions about replicability in settings without such implementation capacity. Partial mixing may also carry risks. Refugees assigned to homogeneous groups while others worked alongside hosts experienced declines in economic interactions with hosts. Finally, our evidence comes from four purposively selected sites hosting protracted refugee populations in small Ethiopian towns and captures outcomes roughly sixteen months into the program, so the extent to which these findings generalize beyond these settings remains an open question.

Moving forward, we will examine the endline effects of the program. Our midline results capture the effects of public works and training before the disbursement of the large productive grant, allowing the endline to show whether a substantial capital injection amplifies these effects and to provide an additional test of the income channel. Our mechanisms make different predictions. If income improves social cohesion only within the shared institutional context, the grant, which is delivered directly to households rather than through shared work, should generate little additional social cohesion on its own. If co-participation and exposure are the

main drivers, the social gains should persist after public works and payments end. If instead they depend on the continued flow of resources, they may fade once transfers stop. The durability of these gains, and the mechanisms that sustain them, is therefore a central question for the next stage of this research agenda.

References

Albarran, P. and O. P. Attanasio (2003). Limited commitment and crowding out of private transfers: Evidence from a randomised experiment. *The Economic Journal 113*(486), C77–C85.

Allport, G. W. (1954). *The Nature of Prejudice*. Cambridge, MA: Addison-Wesley.

Ashraf, N., D. Karlan, and W. Yin (2006). Tying Odysseus to the mast: Evidence from a commitment savings product in the Philippines. *Quarterly Journal of Economics 121*(2), 635–672.

Athey, S. and G. Imbens (2016). Recursive partitioning for heterogeneous causal effects. *Proceedings of the National Academy of Sciences 113*(27), 7353–7360.

Athey, S., J. Tibshirani, and S. Wager (2019). Generalized random forests.

Athey, S. and S. Wager (2019). Estimating treatment effects with causal forests: An application. *Observational studies 5*(2), 37–51.

Bagga, A., M. Holmlund, N. Khan, S. Mani, E. Mvukiyehe, and P. Premand (2024). Do public works programs have sustained impacts? a review of experimental studies from lmics. *The World Bank Research Observer*, lkae011.

Bahar, D., R. Brough, and G. Peri. Refugees' economic integration. *Journal of Economic Literature*.

Balboni, C., O. Bandiera, R. Burgess, M. Ghatak, and A. Heil (2022). Why do people stay poor? *The Quarterly Journal of Economics 137*(2), 785–844.

Bandiera, O., R. Burgess, N. Das, S. Gulesci, I. Rasul, and M. Sulaiman (2017). Labor markets and poverty in village economies. *The Quarterly Journal of Economics 132*(2), 60.

Banerjee, A., E. Duflo, N. Goldberg, D. Karlan, R. Osei, W. Pariente, J. Shapiro, B. Thuysbaert, and C. Udry (2015, May). A multifaceted program causes lasting progress for the very poor: Evidence from six countries. *Science 348*(6236), 1260799–1260799.

Baseler, T., T. Ginn, R. Hakiza, H. Ogude-Chambert, and O. Woldemikael (2025). Can redistribution change policy views? aid and attitudes toward refugees. *Journal of Political Economy 133*(9), 2874–2917.

Becker, A. (2019). On the Economic Origins of Restrictions on Women's Sexuality. Working Paper.

Beegle, K., A. Coudouel, and E. Monsalve (2018). Realizing the Full Potential of Social Safety Nets in Africa. Technical report, International Bank for Reconstruction and Development / The World Bank, Washington DC.

Belloni, M. (2019). *The Big Gamble: The Migration of Eritreans to Europe*. University of California Press. Open access (Luminos).

Beltramo, T., F. Nimoh, M. O'Brien, and S. Sequeira (2026). Financial security, climate shocks and social cohesion. Working Paper.

Berg, E., S. Bhattacharyya, R. Durgam, and M. Ramachandra (2012). Can rural public works affect agricultural wages? evidence from india.

Bertrand, M., B. Crépon, A. Marguerie, and P. Premand (2021). Do workfare programs live up to their promises? experimental evidence from cote d'ivoire. Technical report, National Bureau of Economic Research.

Blattman, C., N. Fiala, and S. Martinez (2014, May). Generating Skilled Self-Employment in Developing Countries: Experimental Evidence from Uganda*. *The Quarterly Journal of Economics 129*(2), 697–752.

Bütikofer, A., R. Ginja, K. Karbownik, and F. Landaud (2024). (breaking) intergenerational transmission of mental health. *Journal of Human Resources 59*(S), S108–S137.

Carver, F., Fana Gebresenbet, and D. Naish (2019). 2018–2019 refugee and host community context analysis: Gambella region report. Technical report, Overseas Development Institute / Danish Refugee Council.

Chiovelli, G., S. Michalopoulos, E. Papaioannou, and S. Sequeira (2021). Forced displacement and human capital: Evidence from separated siblings.

Currie, J. and M. Stabile (2006). Child mental health and human capital accumulation: The case of ADHD. *Journal of Health Economics 25*(6), 1094–1118.

Dereje Feyissa (2011). *Playing Different Games: The Paradox of Anywaa and Nuer Identification Strategies in the Gambella Region, Ethiopia*. Berghahn Books.

Dereje Feyissa and Dawud Mohamed (2019). 2018–2019 refugee and host community context analysis: Afar regional report. Technical report, Overseas Development Institute / Danish Refugee Council.

Dupas, P. and J. Robinson (2013a). Savings constraints and microenterprise development: Evidence from a field experiment in kenya. *American Economic Journal: Applied Economics 5*(1), 163–192.

Dupas, P. and J. Robinson (2013b). Why don't the poor save more? evidence from health savings experiments. *American Economic Review 103*(4), 1138–1171.

Edmonds, E. V. and N. Schady (2012). Poverty alleviation and child labor. *American Economic Journal: Economic Policy 4*(4), 100–124.

Egger, D., D. Auer, and J. Kunz (2021). Effects of Migrant Networks on Labor Market Integration, Local Firms and Employees. pp. 51.

Ethiopian Statistical Service (2023, July). Population size of towns by sex, region, zone and weredas: July 2023.

Fekadu Adugna and Ketema Debale (2023). Conflict and displacement in Ethiopia: the case of Benishangul-Gumuz regional sate and Konso zone, Southern Nations, Nationalities and Peoples region. Technical report, EU Trust Fund for Africa (Horn of Africa Window) Research and Evidence Facility.

Field, E., R. Pande, N. Rigol, S. Schaner, and C. Troyer Moore (2021). On her own account: How strengthening women's financial control impacts labor supply and gender norms. *American Economic Review 111*(7), 2342–2375.

Fletcher, J. M. (2014). The effects of childhood ADHD on adult labor market outcomes. *Health Economics 23*(2), 159–181.

Franklin, S., C. Imbert, G. Abebe, and C. Mejia-Mantilla (2024). Urban public works in spatial equilibrium: Experimental evidence from ethiopia. *American Economic Review*.

Gebresenbet, F. (2016). Land acquisitions, the politics of dispossession, and state-remaking in Gambella, western Ethiopia. *Africa Spectrum 51*(1), 5–28.

Gertler, P. (2004). Do conditional cash transfers improve child health? evidence from PROGRESA's control randomized experiment. *American Economic Review 94*(2), 336–341.

Goodman, R. (1997). The Strengths and Difficulties Questionnaire: A research note. *Journal of Child Psychology and Psychiatry 38*(5), 581–586.

Haushofer, J., P. Niehaus, C. Paramo, E. Miguel, and M. W. Walker (2022). Targeting impact versus deprivation. Technical report, National Bureau of Economic Research.

Haushofer, J. and J. Shapiro (2016). The short-term impact of unconditional cash transfers to the poor: Experimental evidence from Kenya. *Quarterly Journal of Economics 131*(4), 1973–2042.

Hizıroğlu Aygün, A., M. G. Kırdar, M. Koyuncu, and Q. Stoeffler (2024). Keeping refugee children in school and out of work: Evidence from the world's largest humanitarian cash transfer program. *Journal of Development Economics 168*.

Hjort, J. (2014). Ethnic divisions and production in firms. *The Quarterly Journal of Economics 129*(4), 1899–1946.

Human Rights Watch (2012). "waiting here for death": Forced displacement and "villagization" in Ethiopia's Gambella region. Technical report, Human Rights Watch.

IIED (2023). Protracted displacement in an urban world: Eritrean refugees in Ethiopia. Technical report, International Institute for Environment and Development.

Imbert, C. and J. Papp (2015a). Labor market effects of social programs: Evidence from india's employment guarantee. *American Economic Journal: Applied Economics 7*(2), 233–263.

Imbert, C. and J. Papp (2015b). Labor market effects of social programs: Evidence from India's employment guarantee. *American Economic Journal: Applied Economics 7*(2), 233–263.

Jensen, R. T. (2004). Do private transfers "displace" the benefits of public transfers? Evidence from South Africa. *Journal of Public Economics 88*(1–2), 89–112.

Kattenberg, M., B. Scheer, and J. Thiel (2023). Causal forests with fixed effects for treatment effect heterogeneity in difference-in-differences. Technical report, CPB Netherlands Bureau for Economic Policy Analysis.

Knaus, M. C., M. Lechner, and A. Strittmatter (2021). Machine learning estimation of heterogeneous causal effects: Empirical monte carlo evidence. *The Econometrics Journal 24*(1), 134–161.

Lowe, M. (2021). Types of contact: A field experiment on collaborative and adversarial caste integration. *American Economic Review 111*(6), 1807–1844.

Lowe, M. (2025). Has intergroup contact delivered? *Annual Review of Economics 17*, 321–344.

Ludi, E. and Tsionawit Gebre Yohannes (2019). Refugee and host community context analysis: Tigray region report. Technical report, Overseas Development Institute / Danish Refugee Council. Fieldwork 2018–2019; published 2021. [Verify public ODI URL.].

Markakis, J. (2011). *Ethiopia: The Last Two Frontiers*. Woodbridge: James Currey. See p. 226 on resettlement sites in Assosa and Bambasi.

Mousa, S. (2020). Building social cohesion between christians and muslims through soccer in post-ISIS Iraq. *Science 369*(6505), 866–870.

Muralidharan, K., P. Niehaus, and S. Sukhtankar (2023a). General equilibrium effects of (improving) public employment programs: Experimental evidence from india. *Econometrica 91*(4), 1261–1295.

Muralidharan, K., P. Niehaus, and S. Sukhtankar (2023b). General Equilibrium Effects of (Improving) Public Employment Programs: Experimental Evidence From India. *Econometrica 91*(4), 1261–1295.

Olken, B. A. and R. Hanna (Eds.) (2025). *Social Protection Handbook*. MIT Press. Forthcoming.

Pape, U. J., B. Petrini, and S. A. Iqbal (2018). Informing durable solutions by micro-data: A skills survey for refugees in ethiopia. Technical report, World Bank Group.

Perez, A. M. and S. V. Rozo (2025). From policy to practice: Lessons from the implementation of the refugee work rights policy in ethiopia. (11254).

Rao, G. (2019). Familiarity does not breed contempt: Generosity, discrimination, and diversity in delhi schools. *American Economic Review 109*(3), 774–809.

Rozo, S. (2026). Regularization and the formation of social capital: Evidence from forced migrants in colombia. *Policy Research Working Paper. World Bank N. 11427*.

Rozo, S. V. (2025, March). Refugees and other forcibly displaced populations. *VoxDevLit 14*(1).

RRS-UNHCR (2025). Ethiopia: Refugee and asylum seeker population statistics. Technical report, RRS and UNHCR. Administrative data, Ethiopia.

Scacco, A. and S. S. Warren (2018). Can social contact reduce prejudice and discrimination? evidence from a field experiment in nigeria. *American Political Science Review 112*(3), 654–677.

Schaner, S. (2018). The persistent power of behavioral change: Long-run impacts of temporary savings subsidies for the poor. *American Economic Journal: Applied Economics 10*(3), 67–100.

Shah, M. and B. M. Steinberg (2021). Workfare and human capital investment: Evidence from india. *Journal of Human Resources 56*(2), 403–428.

Takele Merid (2025). Memories of slavery and the slave trade among the Berta of Western Ethiopia: Past experiences and current perceptions. In A. Meckelburg, G. Bonacci, S. Tolino, and Ahmed Hassen (Eds.), *Slavery and the Slave Trade in Ethiopian Studies*, Volume 12 of *Supplement to Aethiopica: International Journal of Ethiopian and Eritrean Studies*, pp. 109–124. Wiesbaden: Harrassowitz Verlag. Open access (CC BY-SA 4.0).

Tamim, A., E. C. Smith, B. Palmer, E. Miguel, S. Leone, S. V. Rozo, and S. Stillman (2025). Housing subsidies for refugees: Experimental evidence on life outcomes and social integration in jordan. *National Bureau of Economic Research, Working Paper 33408*.

UNHCR (2018). Bambasi refugee camp profile. Technical report, UNHCR.

UNHCR (2024). Global trends: Forced displacement in 2023. Technical report, UNHCR.

UNHCR (2025, 10). Protection monitoring and solutions (PMS) report: Benishangul-Gumuz region, Bambasi woreda. *EU IM3 Project, Ethiopia*. Prepared with Plan International; funded by the European Union. Data collected October 2025; 96 key informant interviews with IDPs across IDP Site 01 and IDP Site 02, Bambasi Woreda.

UNHCR (2026, March). Protection monitoring survey (pms) report: Amhara region, aftit and alemwach settlements.

United Nations (2018). Global compact on refugees. New York: United Nations, A/73/12 (Part II).

Verme, P. and K. Schuettler (2021). The impact of forced displacement on host communities: A review of the empirical literature in economics. *Journal of Development Economics 150*, 102606.

Wager, S. and S. Athey (2018). Estimation and inference of heterogeneous treatment effects using random forests. *Journal of the American Statistical Association 113*(523), 1228–1242.

Wieser, C., Wondimagegn Mesfin, Fikirte Girmachew, J. A. Lebow, and Manex Bule (2024). Expanding development approaches to refugees and their hosts in ethiopia. Report, World Bank and UNHCR Joint Data Center on Forced Displacement.

World Bank (2020). Informing durable solutions for refugees in ethiopia. Technical report, World Bank.

World Bank (2024a). Ethiopia - urban productive safety net and jobs project: Second additional financing.

World Bank (2024b). Forced displacement and development responses in ethiopia. Technical report, World Bank.

World Bank (2025). Ethiopia - urban productive safety net and jobs project: Additional financing. Technical report, World Bank Group.

Yazew, B. T. and G. Kassa (2024). Social structure and clan group networks of Afar pastorals along the lower Awash valley. *Humanities and Social Sciences Communications 11*, 812.

X Figures and Tables

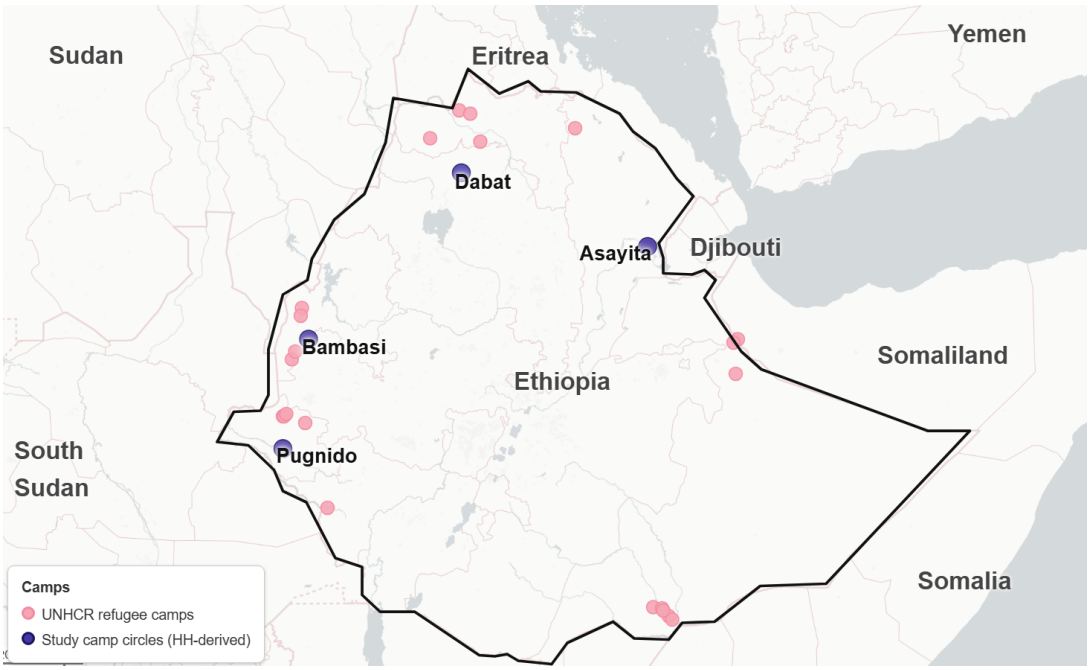


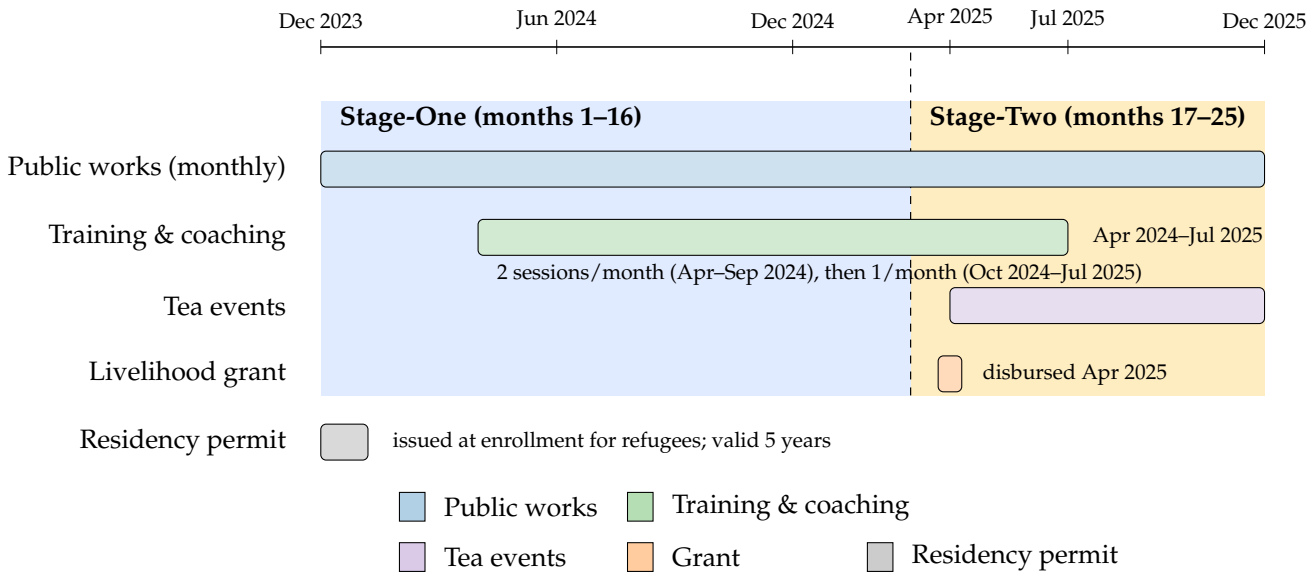
Figure 1. Study Locations in Ethiopia

Table 1. Baseline Characteristics of RHISN Study Sites: Host Towns and Refugee Camps

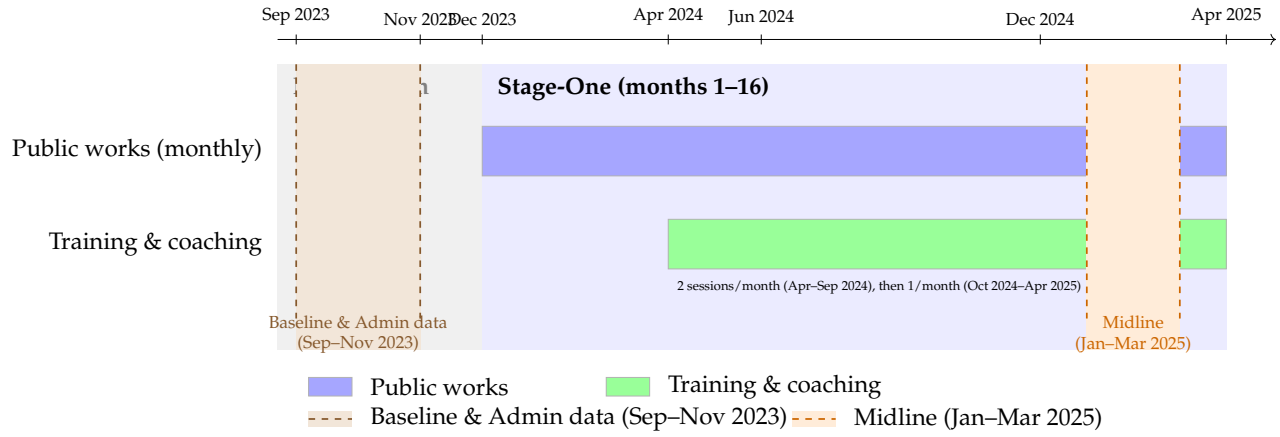
	Asayita		Bambasi		Dabat (Alemwach)		Pugnido	
	Asayita town	**Asayita camp**	**Bambasi town**	**Bambasi camp**	**Dabat town**	**Alemwach camp**	**Pugnido town**	**Pugnido I camp**
Population size	41,494	25,741	27,889	20,307	29,545	21,743	14,859	51,117
Share of female	48%	50%	49%	50%	53%	45%	51%	55%
Share of working age (15 to 64)		53%		46%		62%		49%
Year of camp establishment		2007		2012		2021		1993
Camp distance from the town		3 km		10 km		2 km		5–15 km*
Average temperature		25°C to 35°C		18°C to 25°C		14°C to 20°C		26°C to 40°C
Average altitude		300 meters		1,668 meters		2,596 meters		475 meters
Weekly hours worked (household)	43.7	26.5	53.2	33.9	NA	NA	29.7	12.7
Unemployment rate	41.6%	40.8%	39.4	53.8%	NA	NA	56.9%	68.2%
Refugee country of origin		Eritrea (100%)		Sudan (99.8%)		Eritrea (100%)		South Sudan (100%)
Language (primary)	Amharic (62.9%) Afar (36.0%) Other (1.1%)	Afar (98.5%) Other (1.5%)	Oromo (41.2%) Amharic (34.5%) Berta (14.4%) Other (10.1%)	Ruxan / Rutan (69.5%) Arabic (16.8%) Berta (12.2%) Other (1.5%)	Amharic (100%)	Tigrigna (89.9%) Saho (4.5%) Kunama (4.2%) Other (0.5%)	Agnuak (96.0%) Amharic (2.8%) Other (1.2%)	Nuer (75.4%) Agnuak (22.5%) Other (2.1%)
Religion	Muslim (91.3%) Orth. Christian (8.1%) Other (0.6%)	Muslim (99.7%) Other (0.3%)	Muslim (60.4%) Orth. Christian (24.5%) Protestant (14.6%)	Muslim (99.4%) Other (0.6%)	Orth. Christian (93.8%) Muslim (6.2%)	Orth. Christian (81.8%) Muslim (11.1%) Other (2.8%)	Protestant (88.9%) Catholic (2.5%) Orth. Christian (6.6%) Other (2.0%)	Protestant (76.2%) Catholic (8.1%) Orth. Christian (6.4%) Other (9.3%)
Share with outgroup friends	1.2%	29.7%	0.5%	0.9%	NA	NA	1.7%	12.9%

Notes: Weekly hours worked, unemployment, primary language, religion, and the share with outgroup friends are based on the RHISN baseline household survey. For Dabat, primary language and religion are taken from the midline survey because no baseline survey was conducted. All other indicators are compiled from UNHCR databases and reports, government publications, and other credible sources. The share with outgroup friends captures whether a respondent had at least one relationship with an outgroup member, including a close friend, other good friend, relative, or economic contact; the indicator equals one if the respondent reported at least one such relationship and zero otherwise. *Pugnido I camp comprises two sections: the Anuak section, located approximately 5 km from Pugnido town, and the Nuer section, approximately 15 km away and separated from the town by a forest. NA indicates that information was not available. Host population estimates are from Ethiopian Statistical Service (ESS) projections for July 2023. Refugee population figures are based on UNHCR registrations as of June 2023. Pugnido II camp (established in 2015), which hosts South Sudanese refugees, was not part of the RHISN impact evaluation. Orth. Christian denotes Orthodox Christian.

Figure 2. RHISN Program Stages, Benefits and Timeline

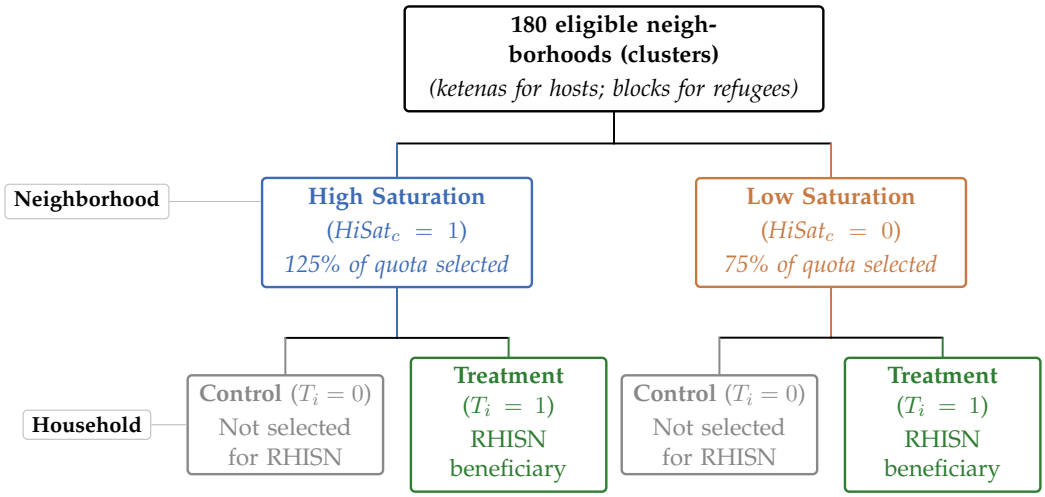


Notes: Month 1 corresponds to December 2023 and month 25 to December 2025. Training/coaching ran from April 2024 (month 5) to July 2025 (month 20), with two sessions per month during April–September 2024 and one session per month during October 2024–July 2025 (21 sessions total). The grant was disbursed in April 2025 (month 17). Public works transfers totaled approximately USD 1,444 PPP per household; beneficiaries were required to save 20 percent of earnings in a formal account. The livelihood grant corresponds to a one time transfer of USD 600 PPP per household. Six tea gatherings, one lunch, and one graduation cultural event were implemented to promote social cohesion among members of public works groups.

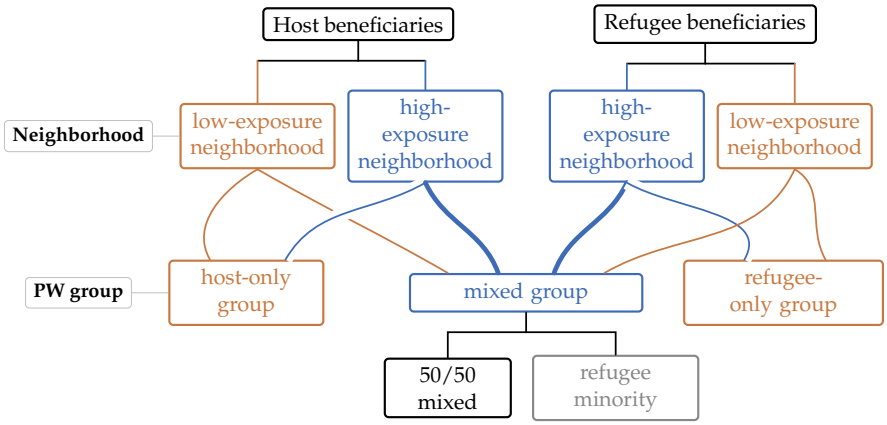


Notes: By the time of the midline survey, sampled households had been exposed to roughly 16 months of public works participation and 12 months of livelihoods training and coaching, but had not yet received the grant or participated in formal social cohesion activities. Administrative data includes the refugee and host listing and the proxy mean test survey collected by the Ethiopian Government.

Figure 3. Stage One Program Timeline and Data Collection Windows



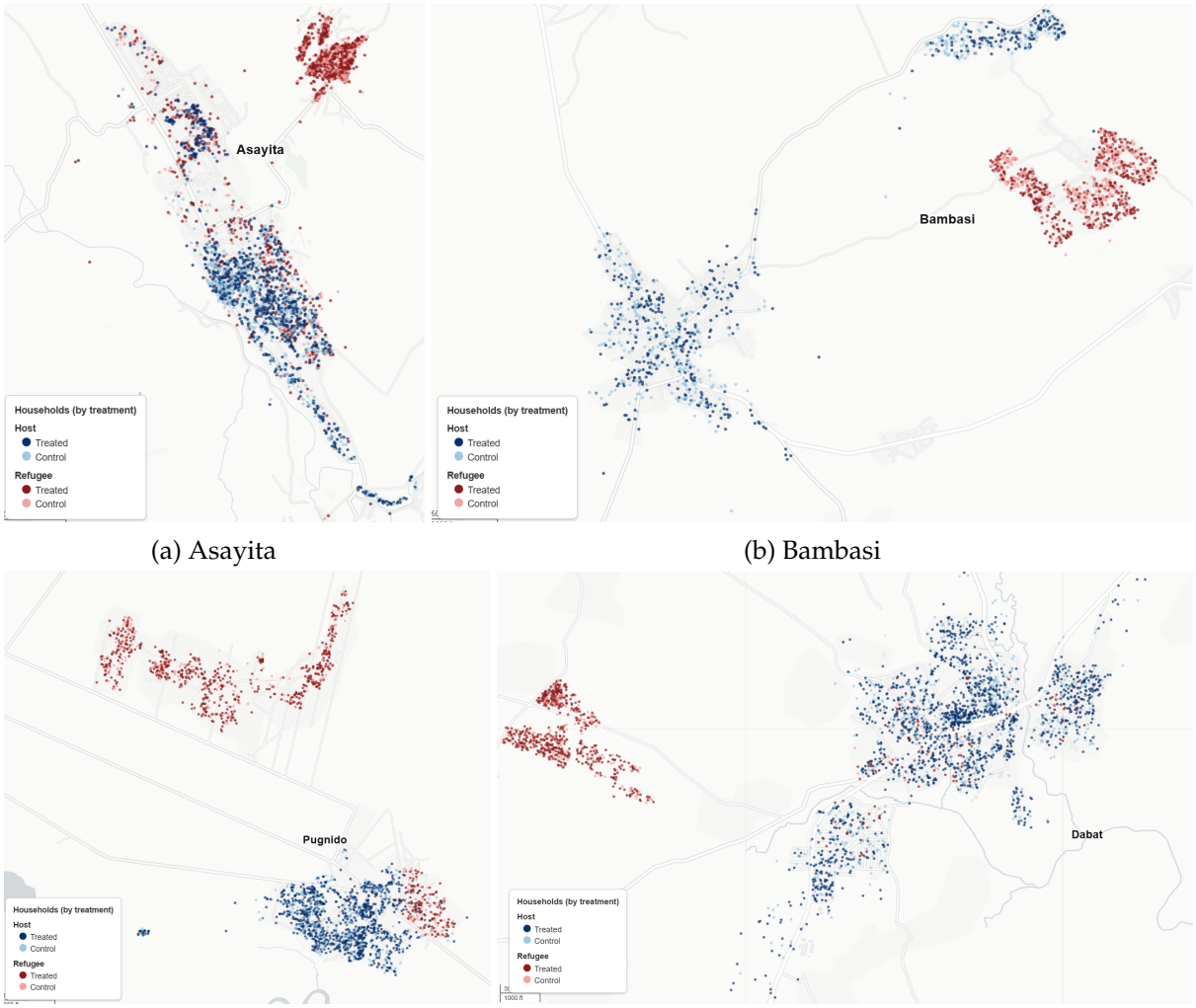
Panel A: Stage One — Selection of Beneficiaries



Panel B: Stage Two — Assignment of Beneficiaries to Public Works Groups

Notes: In the complete empirical design each neighborhood is cross-randomized across two dimensions: program saturation (high vs. low) and mixing exposure (high vs. low). Within each neighborhood, individuals are selected into control and treatment groups, and treatment beneficiaries are assigned to a public works group that is either homogeneous (host or refugee only) or mixed (50/50 or refugee minority), such that together these assignments satisfy the neighborhood-level allocations. This process was done in two stages: In stage one individuals were selected for treatment (Panel A), and in stage two beneficiaries are selected into public works groups (Panel B).

Figure 4. RHISN Experimental Design



(a) Asayita (b) Bambasi

(c) Pugnido (d) Dabat

Figure 5. Geographic Distribution of Host and Refugee Households by Treatment Assignment in the Four Study Locations.

Notes: Each panel illustrates the geographic distribution of host and refugee households assigned to treatment and control.

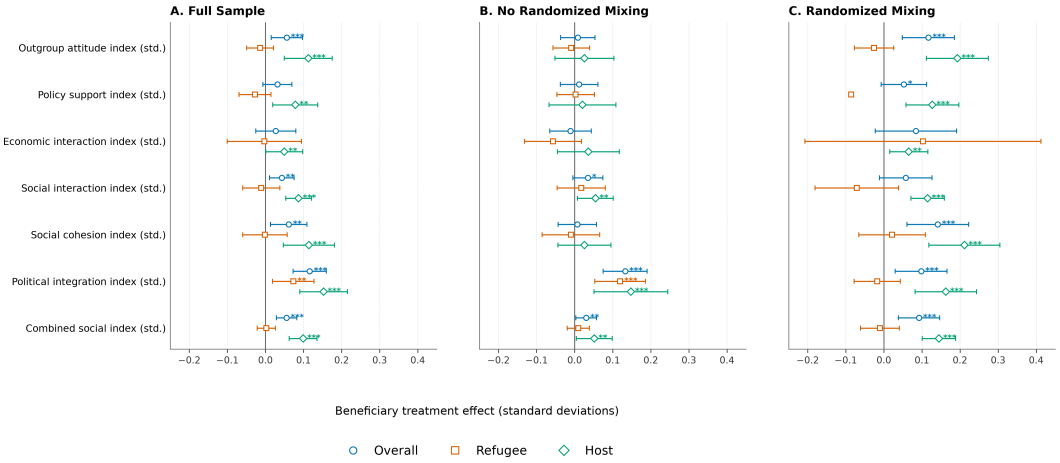


Figure 6. RHISN Direct Social Effects: Full Sample and Samples With and Without Randomized Intergroup Mixing

Notes: The figure reports the treatment effect on beneficiaries enrolled in the RHISN program for different social outcomes. We report the results for the full sample, and then separately for the areas where randomized mixing was implemented and where it was not implemented. All index components have been redefined so that they move in the same direction, with higher values reflecting better outcomes. All indices have been standardized using the overall control group mean. The indices are constructed as follows. **OA** (Outgroup Attitude): comfort and trust in everyday interactions with the outgroup, including living proximity, working relations, children's contact, and perceived community contributions. **PA** (Policy Attitudes): support for refugee inclusion policies, including citizenship, freedom of movement, work permits, and public spending on refugees. **EI** (Economic Interaction): market-based interactions with the outgroup, including sales and working together. **SI** (Social Interaction): social ties with the outgroup, including friendships, community group participation, shared activities, and children's play. **SC** (Social Cohesion): trust and pro-social behavior toward the outgroup, including interpersonal trust, charitable donations, perceived integration, and national identity. **PI** (Political Integration): civic and political engagement, including identity document ownership, community leadership, civic participation, and trust in local government. Combined social index is constructed as the average of all these indices. The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2. RHISN Program Compliance

		Control	Treatment Effects			Test	HighSat Spillover		
Outcome	N	Mean	Overall (β_1)	Refugee ($\beta_{1,R}$)	Host ($\beta_{1,H}$)	$\beta_{1,R} = \beta_{1,H}$	Overall (β_2)	Beneficiary (θ_3)	Control (θ_2)
(1) **1**(Enrolled in RHISN)	11986	0	0.904***	0.875***	0.928***	0.006	-0.0113	-0.0218	0.011
			(0.0108)	(0.0153)	(0.0119)		(0.00889)	(0.0143)	(0.0108)
(2) **1**(HH participates in Public Works)	11426	0.089	0.703***	0.69***	0.713***	0.559	0.047***	0.0725***	-0.00739
			(0.0215)	(0.0178)	(0.0362)		(0.0112)	(0.0209)	(0.0325)
(3) **1**(HH participates in livelihood training)	11426	0.099	0.578***	0.524***	0.622***	0.039	0.0484**	0.0829***	-0.0251
			(0.0241)	(0.0299)	(0.0365)		(0.0186)	(0.0307)	(0.0327)
(4) Weekly PW Eligible Hours (HH)	11418	0	12.3***	14***	11***	0.0000	0.294*	0.278	0.327
			(0.274)	(0.226)	(0.294)		(0.177)	(0.299)	(0.28)
(5) Weekly public work hours (intended beneficiary)	10097	0.361	4.65***	4.58***	4.72***	0.86	0.813**	1.24**	-0.171
			(0.395)	(0.507)	(0.592)		(0.359)	(0.553)	(0.553)
(6) Weekly public work hours (other HH members)	11426	0.17	0.609***	0.411***	0.764***	0.056	0.141	0.178	0.0631
			(0.0998)	(0.0726)	(0.169)		(0.144)	(0.201)	(0.141)
(7) Eligible RHISN income (USD PPP, annualized)	11418	0	1,234***	1,399***	1,101***	0.0000	29.4*	27.8	32.7
			(27.4)	(22.6)	(29.4)		(17.7)	(29.9)	(28)
(8) RHISN income (USD PPP, annualized)	11426	49.9	498***	478***	513***	0.653	68.9**	114**	-26.3
			(39.8)	(51.3)	(59.6)		(27.9)	(47.7)	(54.6)
(9) **1**(Aware of RHISN)	1106	0.506	0.335***	0.339***	0.331***	0.91	0.0198	0.0735**	-0.088
			(0.0358)	(0.0398)	(0.0556)		(0.0345)	(0.035)	(0.0666)

Notes: The table reports the treatment effect on beneficiaries enrolled in the RHISN program. Rows (1), (2), (3) and (9) are dummy variables indicating realized treatment (rather than assignment), participation in public works in the past year, participation in the RHISN livelihood training in the past year, and having heard of the RHISN program, respectively. Rows (4-6) are defined as the amount of weekly public work hours the household is eligible for, and the actual intended beneficiary and household weekly labor supply in public works. Rows (7-8) are defined as the amount of public work income the household is eligible for and the annual household employment income from public works. They are reported in real USD PPP and winsorized at 98%. The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets, and square brackets report q-values adjusted for False Discovery Rate. *** p<0.01, ** p<0.05, * p<0.1.

Table 3. Direct and Saturation Effects of RHISN: Labor Supply

		Control	Treatment Effects			Test	HighSat Spillover		
Outcome	N	Mean	Overall (β_1)	Refugee ($\beta_{1,R}$)	Host ($\beta_{1,H}$)	$\beta_{1,R} = \beta_{1,H}$	Overall (β_2)	Beneficiary (θ_3)	Control (θ_2)
(1) Total productive hours	11426	29.7	4.17***	4.38***	3.91***	0.733	1.08	1.63	-0.108
			(0.718)	(0.782)	(1.16)		(1.38)	(1.58)	(1.65)
(2) Public works hours (RHISN)	11426	0.482	4.76***	4.58***	4.91***	0.671	0.813**	1.22**	-0.0666
			(0.403)	(0.485)	(0.613)		(0.324)	(0.542)	(0.586)
(3) Productive hours (excl. public works)	11426	29.3	-0.594	-0.211	-1.01	0.511	0.266	0.414	-0.0501
			(0.632)	(0.627)	(1.04)		(1.42)	(1.6)	(1.56)
(4) Wage employment hours	11426	9.31	-0.927**	-0.565	-1.25**	0.323	-1.04	-1.14	-0.827
			(0.365)	(0.406)	(0.566)		(0.73)	(0.724)	(1)
(5) Hours in HH agriculture	11426	8.23	0.798**	0.835**	0.764	0.915	0.986	0.87	1.23
			(0.351)	(0.378)	(0.545)		(0.856)	(0.883)	(1.12)
(6) Hours in non-ag self-employment	11426	11.7	-0.493	-0.43	-0.632	0.823	0.351	0.674	-0.34
			(0.463)	(0.566)	(0.71)		(1.28)	(1.44)	(1.27)
(7) HH hourly wage	5989	63.5	13.1***	16.2***	11.9***	0.528	-1.04	-2.17	3.68
			(3.26)	(5.52)	(3.98)		(3.32)	(3.66)	(6.36)
(8) HH hourly wage (excl. public works)	2876	60.8	1.09	6.13	-0.512	0.411	0.718	-1.78	4.87
			(3.13)	(7.37)	(3.4)		(3.69)	(4.12)	(5.94)
(9) Job search hours	11426	2.4	-0.428***	-0.537***	-0.346**	0.394	-0.157	-0.203	-0.0572
			(0.112)	(0.167)	(0.15)		(0.221)	(0.233)	(0.301)
(10) **1**(Unemployment)	11426	0.529	-0.0319***	-0.0445***	-0.0226	0.297	-0.0367	-0.0419	-0.0254
			(0.0106)	(0.0159)	(0.0138)		(0.0303)	(0.0328)	(0.0303)

Notes: The table reports the treatment effect on beneficiaries enrolled in the RHISN program. Total productive hours is defined as the total household weekly labor supply and is the sum of rows (2) and (3), where (2) is defined as the household weekly labor supply in public works employment. Productive hours (excl. public works) is the sum of rows (4-6), i.e. defined as the total household weekly labor supply in non-public works employment, in pastoralist or agricultural activities, and self-employment (non-agriculture). Row (9) is the weekly hours spent actively searching for a job and row (10) is an unemployment dummy variable. The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Direct and Saturation Effects of RHISN: Household Balance Sheet

		Control	Treatment Effects			Test	HighSat Spillover		
Outcome	N	Mean	Overall (β_1)	Refugee ($\beta_{1,R}$)	Host ($\beta_{1,H}$)	$\beta_{1,R} = \beta_{1,H}$	Overall (β_2)	Beneficiary (θ_3)	Control (θ_2)
Panel A: Income									
(1) Total income (USD PPP, annualized)	11426	2,245	565***	576***	541***	0.754	-4.38	34.1	-86.5
			(56.8)	(85.1)	(75.8)		(89.3)	(105)	(113)
(2) RHISN income and benefits	11426	62.9	588***	615***	567***	0.614	61.4*	98.1*	-16.8
			(47.6)	(70)	(63.7)		(31)	(54.8)	(61.8)
(3) Total income (excl. RHISN)	11426	2,182	-24.5	-43.4	-26.5	0.81	-65.3	-62.8	-70.7
			(34.9)	(53.2)	(46.3)		(85)	(89.6)	(103)
(4) Wage employment income (excl. RHISN)	11426	603	-35.4	-37	-37.5	0.991	-78.1*	-101**	-29.2
			(23.8)	(29.4)	(36.5)		(47)	(48.1)	(67)
(5) Agriculture & livestock income	11426	146	2.08	-4.61	4.23	0.542	15.4	14.7	16.8
			(7.69)	(8.51)	(11.8)		(14.1)	(14.8)	(20.5)
(6) Non-ag enterprise profits	11426	407	-4.54	-8.44	-5.01	0.904	-9.94	8.98	-50.3
			(15.1)	(13.3)	(25.2)		(39.7)	(40.9)	(42.7)
(7) Aid and transfers (excl. RHISN)	11426	838	17.7	36.1	3.43	0.418	13.9	15.4	10.6
			(18.8)	(38.7)	(11.5)		(44.1)	(47.4)	(46.6)
(8) Net remittances	11426	173	-9.54	-28.5**	4.34	0.055	-4.41	0.42	-14.7
			(8.37)	(12.3)	(11.8)		(14.7)	(17.1)	(18.9)
(9) Rental income	11426	14.9	1.69	1.05	2.35	0.513	-0.049	-0.141	0.147
			(1.06)	(0.845)	(1.79)		(2.31)	(2.42)	(2.78)
Panel B: Assets									
(10) Total assets, excl. land/housing (USD PPP)	11426	1,113	207***	177***	234***	0.315	39.1	84.8	-58.5
			(30.2)	(28.1)	(49.9)		(58.5)	(68.1)	(62.9)
(11) Productive assets (liv, agri, ent)	11426	356	42.4**	31.8**	50.9*	0.54	26	41.1	-6.39
			(16.4)	(14.9)	(27.4)		(42.5)	(45.9)	(45.4)
(12) Household durables	11426	748	37.3**	-0.491	69.8***	0.036	1.13	23.3	-46.1
			(16.9)	(21.5)	(25.5)		(39.5)	(46)	(38.3)
(13) Net financial savings	11426	9.31	135***	145***	128***	0.41	15.7	24.2	-2.57
			(11.1)	(11.9)	(17.3)		(13)	(15.7)	(20.7)
(14) Land and housing value	8120	72	2.3	1.8	2.77	0.85	-1.38	0.0821	-4.89
			(2.83)	(2.05)	(4.7)		(7.91)	(8.1)	(9.49)
Panel C: Consumption									
(15) Total consumption (USD PPP, annualized)	11426	5,388	226***	220***	256***	0.763	-57.5	-96.8	26.2
			(60.8)	(79.4)	(89.8)		(166)	(184)	(176)
(16) Food consumption	11426	4,360	181***	185***	201***	0.871	-58.5	-100	30.7
			(49.8)	(68.9)	(71.7)		(147)	(161)	(155)
(17) Non-food consumption	11426	1,029	49.3***	40.1	55.7**	0.664	0.614	3.63	-5.83
			(17.9)	(25.2)	(25.4)		(29.3)	(33.3)	(38.2)
(18) Durables	11426	420	23.3**	16.3	29.2**	0.51	-7	0.37	-22.7
			(9.25)	(15.9)	(11.4)		(14.3)	(15.3)	(18.9)
(19) Non-food non-durables	11426	576	20.9**	20.9*	20.5	0.982	11.6	6.46	22.5
			(10.4)	(12.1)	(15.8)		(19.8)	(22.7)	(24.6)

Notes: The table reports the treatment effect on beneficiaries enrolled in the RHISN program. All outcomes are reported in real USD PPP and winsorized at 98%. **Panel A:** Total income is the sum of rows (2) and (3), i.e. the sum of annual employment income and cash and in-kind benefits from RHISN as well as total income excluding RHISN. Total income (excl. RHISN) is the sum of rows (4-9). **Panel B:** Total assets is the sum of rows (11-13). **Panel C:** Total consumption is the sum of rows (16) and (17). Non-food consumption is the sum of rows (18-19). The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 5. Direct and Saturation effects of RHISN: Well-being Outcomes

		Control	Treatment Effects			Test	HighSat Spillover		
Outcome	N	Mean	Overall (β_1)	Refugee ($\beta_{1,R}$)	Host ($\beta_{1,H}$)	$\beta_{1,R} = \beta_{1,H}$	Overall (β_2)	Beneficiary (θ_3)	Control (θ_2)
(1) Food security index (std.)	11426	0	0.131***	0.0671**	0.181***	0.01	0.0331	0.0454	0.00702
			(0.0232)	(0.0293)	(0.0326)		(0.0515)	(0.0576)	(0.0559)
(2) Health index (std.)	11426	0	0.0601***	0.0424	0.0697**	0.54	0.0161	0.0535	-0.0637
			(0.0227)	(0.0296)	(0.0333)		(0.0354)	(0.0366)	(0.0514)
(3) Mental health index (std.)	11426	0	0.146***	0.0704**	0.206***	0.005	0.0679	0.0992	0.00114
			(0.0271)	(0.0295)	(0.0374)		(0.0622)	(0.0692)	(0.0628)
(4) CES-D score (1-4)	11422	3.26	0.0639***	0.0249*	0.0948***	0.003	0.018	0.0319	-0.0118
			(0.0127)	(0.0134)	(0.0184)		(0.0289)	(0.0329)	(0.0277)
(5) **1**(CES-D Depression Risk)	11422	0.441	-0.0546***	-0.0313**	-0.0737***	0.03	-0.0136	-0.0281	0.0176
			(0.0103)	(0.012)	(0.0152)		(0.023)	(0.0257)	(0.0237)
(6) Amenities index (std.)	11426	0	0.0703***	0.0145	0.111***	0.018	-0.0272	-0.0451	0.0111
			(0.0208)	(0.0286)	(0.0289)		(0.0516)	(0.056)	(0.0539)
(7) Female empowerment index (std.)	11424	0	0.427***	0.425***	0.427***	0.97	-0.00155	0.0248	-0.0577
			(0.0231)	(0.0354)	(0.031)		(0.037)	(0.0438)	(0.0418)
(8) Resource control (0-1, only fem.)	7511	0.627	0.155***	0.22***	0.0984***	0.0000	0.0143	0.0125	0.0182
			(0.012)	(0.0155)	(0.0107)		(0.0124)	(0.0156)	(0.0233)
(9) Decision control (0-1, only fem.)	4235	0.858	0.00935	0.0189*	-0.000918	0.214	0.0103	0.0153	-0.000369
			(0.00804)	(0.0103)	(0.0121)		(0.0142)	(0.0155)	(0.0183)

Notes: The table reports the treatment effect on beneficiaries enrolled in the RHISN program. All outcomes have been redefined so that they move in the same direction, with higher values reflecting better outcomes. The exception is (5) CES-D Depression, which is a dummy variable indicating depression (i.e. higher value reflecting a worse outcome). All indices have been standardized using the overall control group mean. The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. The indices are constructed as follows. Food security (1): frequency of buying less expensive food, borrowing food, limiting portion sizes, adults restricting consumption for children, and reducing daily meals. Health (2): self-rated health, number of hospital visits, number of work days missed, number of times needed but did not seek medical care, any major health issue, annual health expenditure. Mental health (3): CES-D score, life satisfaction, happiness, perceived stress, and locus of control. Amenities (6): sewerage quality, street cleanliness, green space, public toilets, drainage and trash smells, crime and safety. Female empowerment (7): resource and decision control, domestic discord, gender attitudes, controlling behaviors, emotional violence, employment outside HH. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 6. Direct and Saturation Effects of RHISN: Children Outcomes

Outcome	N	Control Mean	Treatment Effects: Overall (β_1)	Refugee ($\beta_{1,R}$)	Host ($\beta_{1,H}$)	Test: $\beta_{1,R} = \beta_{1,H}$	HighSat Spillover: Overall (β_2)	Beneficiary (θ_3)	Control (θ_2)
Panel A: Child Health									
(1) Child health index (std.)	8572	0	0.09***	0.07**	0.10***	0.561	0.03	0.02	0.05
			(0.02)	(0.04)	(0.03)		(0.04)	(0.04)	(0.06)
(2) Number of symptoms	8572	1.72	-0.0517	0.0251	-0.126*	0.114	0.0635	0.00597	0.188*
			(0.0497)	(0.0648)	(0.0704)		(0.0759)	(0.0838)	(0.104)
(3) Share symptoms treated	5372	0.662	0.0215*	0.0168	0.0256	0.713	-0.00348	-0.0037	-0.00304
			(0.0121)	(0.0163)	(0.0175)		(0.0208)	(0.0239)	(0.0265)
(4) Child nutrition index (std.)	8572	0	0.11***	0.07**	0.16***	0.042	0.02	0.03	-0.00
			(0.02)	(0.03)	(0.03)		(0.05)	(0.05)	(0.07)
(5) **1**(Any child health issue)	8572	0.631	-0.0112	0.00321	-0.0246	0.266	0.00459	-0.00439	0.024
			(0.013)	(0.0184)	(0.0169)		(0.0161)	(0.0189)	(0.0223)
(6) Child not hungry to bed (7 days)	8572	6.69	0.0637***	0.0412	0.0839***	0.259	0.0357	0.0317	0.0444
			(0.0197)	(0.0286)	(0.0247)		(0.031)	(0.0332)	(0.0405)
Panel B: Child Labor									
(7) Child labor and chores (hours)	8572	2.33	-0.0286	-0.0618	0.0173	0.599	0.14	0.159	0.0985
			(0.0752)	(0.126)	(0.0821)		(0.122)	(0.142)	(0.13)
(8) Child labor (hours)	8572	0.43	0.0208	-0.00137	0.0425	0.389	0.00873	0.015	-0.00473
			(0.0257)	(0.0304)	(0.0409)		(0.0422)	(0.0477)	(0.0458)
(9) Child labor (hours, alternate)	7983	0.74	-0.0452	-0.0382	-0.0476	0.967	0.034	0.176	-0.273
			(0.11)	(0.186)	(0.128)		(0.129)	(0.146)	(0.168)
(10) Dom. work and chores (hours)	8572	1.9	-0.0494	-0.0604	-0.0252	0.792	0.131	0.144	0.103
			(0.0653)	(0.12)	(0.0597)		(0.1)	(0.12)	(0.109)
(11) Child labor income (USD PPP)	7983	0.61	-0.0406	0.0359	-0.113	0.261	0.0306	0.0187	0.0563
			(0.0645)	(0.0835)	(0.103)		(0.0793)	(0.0885)	(0.133)
Panel C: Child Development									
(12) SDQ scale (0-40)	8556	9.96	-0.0932	0.156	-0.362*	0.082	-0.0838	-0.191	0.147
			(0.16)	(0.224)	(0.195)		(0.434)	(0.466)	(0.45)
(13) Emotional symptoms (0-10)	8508	2.3	-0.0653	-0.0181	-0.127	0.376	-0.00441	-0.0571	0.109
			(0.0636)	(0.0904)	(0.0833)		(0.154)	(0.162)	(0.17)
(14) Conduct problems (0-10)	8507	1.71	0.0212	0.122*	-0.0778	0.023	-0.131	-0.157	-0.0752
			(0.0488)	(0.0622)	(0.0615)		(0.12)	(0.132)	(0.124)
(15) Hyperactivity/Inattention (0-10)	8516	3.59	-0.0484	0.0395	-0.132**	0.051	0.114	0.073	0.201
			(0.0467)	(0.0654)	(0.0578)		(0.121)	(0.131)	(0.128)
(16) Peer problems (0-10)	8548	2.39	0.0162	0.0472	-0.0231	0.368	-0.0533	-0.0517	-0.0566
			(0.0398)	(0.0579)	(0.0525)		(0.109)	(0.115)	(0.119)
(17) Prosocial behaviour (0-10)	8522	6.31	0.0304	0.00617	0.0708	0.564	0.142	0.13	0.169
			(0.0574)	(0.0726)	(0.0857)		(0.137)	(0.142)	(0.171)
(18) Child/adult time	8572	9.82	-0.0166	-0.0689	-0.00634	0.884	-0.649	-0.636	-0.676
			(0.22)	(0.253)	(0.348)		(0.584)	(0.59)	(0.734)

Notes: The table reports the treatment effect on beneficiaries enrolled in the RHISN program. None of the outcomes presented in this table were measured at baseline. Outcomes are standardized using the overall control group mean. **Panel A:** Child health index is the average of rows (2-4). Rows (2), (3) and (5) refer to the past month. Child nutrition index (4) is the average of five indicators for child food security in the past week. **Panel B:** Child labor and chores (7) is the sum of rows (8) and (10). Row (11) is defined as the contribution of child labor to household income in the last month. **Panel C:** Rows (12-17) show the results for the Strengths and Difficulties Questionnaire (SDQ), with SDQ scale (12) showing the total difficulties score (Scale 0-40) - the sum of rows (13-16). Child/adult time (18) is the number of hours the caregiver spent with the child in the last week. The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 7. Direct and Saturation Effects of RHISN: Social Outcomes

		Control	Treatment Effects			Test	HighSat Spillover		
Outcome	N	Mean	Overall (β_1)	Refugee ($\beta_{1,R}$)	Host ($\beta_{1,H}$)	$\beta_{1,R} = \beta_{1,H}$	Overall (β_2)	Beneficiary (θ_3)	Control (θ_2)
(1) Outgroup attitude index (std.)	11426	0	0.0561***	-0.014	0.113***	0.001	0.0238	0.0608	-0.0549
			(0.0208)	(0.0181)	(0.0321)		(0.0529)	(0.0541)	(0.0633)
(2) Policy support index (std.)	11426	0	0.0315	-0.0274	0.0785**	0.005	-0.0233	-0.0116	-0.0481
			(0.0193)	(0.0215)	(0.0301)		(0.0449)	(0.0445)	(0.0571)
(3) Economic interaction index (std.)	11426	0	0.0271	-0.00282	0.0495**	0.345	0.0159	0.0264	-0.00652
			(0.0269)	(0.0497)	(0.0247)		(0.033)	(0.0421)	(0.0361)
(4) Social interaction index (std.)	11426	0	0.0428**	-0.0108	0.0872***	0.001	-0.0554	-0.0353	-0.0983**
			(0.0164)	(0.0249)	(0.0173)		(0.0394)	(0.043)	(0.0377)
(5) Social cohesion index (std.)	11426	0	0.0614**	-0.00133	0.114***	0.012	0.0286	0.0602	-0.0388
			(0.0244)	(0.0298)	(0.0342)		(0.0623)	(0.068)	(0.0642)
(6) Political integration index (std.)	11426	0	0.116***	0.0731**	0.153***	0.062	0.0534	0.0535	0.0533
			(0.0223)	(0.0278)	(0.032)		(0.0381)	(0.0426)	(0.0451)
(7) Combined social index (std.)	11426	0	0.0555***	0.00251	0.099***	0.0000	0.00659	0.0247	-0.0321
			(0.0137)	(0.0123)	(0.0186)		(0.0242)	(0.0258)	(0.0298)

Notes: The table reports the treatment effect on beneficiaries enrolled in the RHISN program. All index components have been redefined so that they move in the same direction, with higher values reflecting better outcomes. All indices have been standardized using the overall control group mean. The indices are constructed as follows. **OA** (Outgroup Attitude): comfort and trust in everyday interactions with the outgroup, including living proximity, working relations, children's contact, and perceived community contributions. **PA** (Policy Attitudes): support for refugee inclusion policies, including citizenship, freedom of movement, work permits, and public spending on refugees. **EI** (Economic Interaction): market-based interactions with the outgroup, including sales and working together. **SI** (Social Interaction): social ties with the outgroup, including friendships, community group participation, shared activities, and children's play. **SC** (Social Cohesion): trust and pro-social behavior toward the outgroup, including interpersonal trust, charitable donations, perceived integration, and national identity. **PI** (Political Integration): civic and political engagement, including identity document ownership, community leadership, civic participation, and trust in local government. Combined social index is constructed as the average of the indices in rows (1)–(6). The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 8. Mixing effects of RHISN by household type: Social Outcomes - Dabat and Urban Asayita

		Control Mean		Non-Mixing Effects			Additional Effects of Mixing			Test	HighMix Spillover		
Outcome	N	Refugee	Hosts	Overall (β_1)	Ref. ($\beta_{1,R}$)	Host ($\beta_{1,H}$)	Overall (δ_1)	Ref. ($\delta_{1,R}$)	Host ($\delta_{1,H}$)	$\delta_{1,R} = \delta_{1,H}$	Overall (δ_2)	Benefic (ϕ_3)	Control (ϕ_2)
(1) Outgroup attitude index (std.)	4964	0.406	0.204	0.09**	-0.03	0.13**	0.06	0.01	0.10**	0.208	0.14*	0.15*	0.12
				(0.04)	(0.05)	(0.06)	(0.04)	(0.06)	(0.04)		(0.08)	(0.09)	(0.09)
(2) Policy support index (std.)	4964	0.407	0.179	0.04	-0.09	0.11**	0.02	0.01	0.04	0.406	0.03	0.03	0.00
				(0.03)	(0.06)	(0.04)	(0.03)	(0.03)	(0.03)		(0.05)	(0.05)	(0.06)
(3) Economic interaction index (std.)	4964	0.457	-0.353	-0.05*	-0.12*	-0.03	0.29***	0.42**	0.21***	0.259	0.08*	0.12**	-0.03
				(0.03)	(0.07)	(0.02)	(0.06)	(0.19)	(0.03)		(0.04)	(0.06)	(0.07)
(4) Social interaction index (std.)	4964	0.981	-0.143	0.05*	-0.05	0.09***	0.02	-0.03	0.06**	0.138	0.07	0.07	0.07
				(0.03)	(0.04)	(0.03)	(0.03)	(0.05)	(0.03)		(0.04)	(0.05)	(0.05)
(5) Social cohesion index (std.)	4964	-0.159	0.272	0.12***	-0.01	0.18***	0.05	0.06	0.07	0.951	0.14	0.14	0.14
				(0.04)	(0.05)	(0.06)	(0.03)	(0.06)	(0.04)		(0.13)	(0.13)	(0.14)
(6) Political integration index (std.)	4964	-0.071	0.035	0.10**	-0.04	0.18***	-0.01	0.05	-0.04	0.137	0.10	0.10	0.11
				(0.04)	(0.04)	(0.05)	(0.03)	(0.05)	(0.04)		(0.07)	(0.07)	(0.09)
(7) Combined social index (std.)	4964	0.337	0.032	0.06**	-0.05**	0.11***	0.07***	0.08	0.07***	0.789	0.10***	0.11***	0.08
				(0.03)	(0.02)	(0.03)	(0.02)	(0.06)	(0.02)		(0.04)	(0.04)	(0.05)

Notes: The table reports the effect of treatment and mixing on beneficiaries enrolled in the RHISN program in Amhara (Dabat) and parts of Afar (Asayita), where mixing was implemented properly. Estimates use double and debiased machine learning. All index components have been redefined so that they move in the same direction, with higher values reflecting better outcomes. All indices have been standardized using the overall control group mean. The indices are constructed as follows. **OA** (Outgroup Attitude): comfort and trust in everyday interactions with the outgroup, including living proximity, working relations, children's contact, and perceived community contributions. **PA** (Policy Attitudes): support for refugee inclusion policies, including citizenship, freedom of movement, work permits, and public spending on refugees. **EI** (Economic Interaction): market-based interactions with the outgroup, including sales and working together. **SI** (Social Interaction): social ties with the outgroup, including friendships, community group participation, shared activities, and children's play. **SC** (Social Cohesion): trust and pro-social behavior toward the outgroup, including interpersonal trust, charitable donations, perceived integration, and national identity. **PI** (Political Integration): civic and political engagement, including identity document ownership, community leadership, civic participation, and trust in local government. Combined social index is constructed as the average of the indices in rows (1)–(6). The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 9. Varying the Depth and Intensity of Intergroup Contact
Dabat and Urban Asayita - Mixed Groups Only

Outcome	N	Control Mean	Treatment Effect Benefic. (β_1)	Overall		Refugee		Host	
Panel A. Intensity: Equal versus Minority Groups									
				Mixed	Equal × Mixed	Mixed	Equal × Mixed	Mixed	Equal × Mixed
(1) Outgroup attitude index (std.)	4703	0.277	0.10**	0.13***	-0.10*	0.11	-0.12	0.13**	-0.05
			(0.04)	(0.04)	(0.06)	(0.08)	(0.07)	(0.05)	(0.08)
(2) Policy support index (std.)	4703	0.261	0.05	0.05	-0.05	0.01	-0.01	0.06	-0.03
			(0.04)	(0.03)	(0.03)	(0.07)	(0.07)	(0.04)	(0.04)
(3) Economic interaction index (std.)	4703	-0.058	-0.06	0.36***	-0.08	0.89***	-0.58***	0.16***	0.12**
			(0.04)	(0.07)	(0.11)	(0.09)	(0.20)	(0.03)	(0.05)
(4) Social interaction index (std.)	4703	0.265	0.04	0.06**	-0.05	0.10	-0.12	0.05	0.01
			(0.03)	(0.03)	(0.06)	(0.07)	(0.10)	(0.03)	(0.04)
(5) Social cohesion index (std.)	4703	0.114	0.12***	0.06	0.01	0.07	-0.00	0.06	0.04
			(0.04)	(0.04)	(0.05)	(0.07)	(0.08)	(0.05)	(0.06)
(6) Political integration index (std.)	4703	-0.004	0.11***	0.02	-0.04	0.10	-0.06	-0.02	-0.02
			(0.04)	(0.04)	(0.05)	(0.10)	(0.09)	(0.04)	(0.06)
(7) Combined social index (std.)	4703	0.143	0.06**	0.11***	-0.05	0.21***	-0.15**	0.07***	0.01
			(0.03)	(0.02)	(0.04)	(0.05)	(0.06)	(0.02)	(0.03)
Panel B. Depth: Effects of Social Events									
				Mixed	Mixed × SCA	Mixed	Mixed × SCA	Mixed	Mixed × SCA
(1) Outgroup attitude index (std.)	4703	0.277	0.09**	0.06	0.04	0.00	0.13	0.11*	-0.01
			(0.04)	(0.05)	(0.06)	(0.06)	(0.15)	(0.06)	(0.06)
(2) Policy support index (std.)	4703	0.261	0.05	0.01	0.08*	-0.01	0.12	0.03	0.05
			(0.04)	(0.03)	(0.04)	(0.03)	(0.10)	(0.04)	(0.05)
(3) Economic interaction index (std.)	4703	-0.058	-0.05	0.29***	0.13	0.41	0.56**	0.22***	-0.02
			(0.03)	(0.08)	(0.09)	(0.25)	(0.26)	(0.04)	(0.06)
(4) Social interaction index (std.)	4703	0.265	0.03	0.04	-0.00	0.03	-0.04	0.05*	0.00
			(0.03)	(0.04)	(0.07)	(0.07)	(0.13)	(0.03)	(0.07)
(5) Social cohesion index (std.)	4703	0.114	0.11***	0.07	-0.01	0.02	0.26	0.11**	-0.13
			(0.04)	(0.04)	(0.09)	(0.06)	(0.18)	(0.05)	(0.10)
(6) Political integration index (std.)	4703	-0.004	0.13***	-0.03	0.14	0.00	0.33	-0.04	0.07
			(0.04)	(0.03)	(0.10)	(0.06)	(0.20)	(0.04)	(0.11)
(7) Combined social index (std.)	4703	0.143	0.06**	0.07***	0.06	0.07	0.23**	0.08***	-0.00
			(0.03)	(0.03)	(0.04)	(0.08)	(0.09)	(0.02)	(0.04)

Notes: Panel A examines variation in the intensity of intergroup contact. Equal groups are mixed public works groups in which refugees and hosts each make up roughly half of the members. Minority groups are mixed groups in which refugees are minority of members. Panel B examines variation in the depth of intergroup contact. The social cohesion activity (SCA) consists of facilitated teas and shared meals after work; the comparison is among mixed public works groups only.

Appendices

Note on Appendix Coverage

This Appendix includes tables and figures referenced by number in the main text. Additional internal validity checks, robustness checks for direct program and mixing effects, summary statistics, and other supporting analyses are available in the Supplementary Online Appendix.

Contents

A Tracking, Balance, Attrition, and Compliance Analyses **77**

B Direct Effects: Additional Supporting Evidence **82**

B.A Economic Effects 82

B.B Social Effects 89

C Mixing Effects: Additional Supporting Evidence **97**

D Causal Forest Estimation and Baseline Moderators **101**

D.A Exploring the Role of Connectedness 105

E Mechanism: Additional Supporting Evidence **108**

E.A Reciprocity Channel 114

E.B Income Effects 118

E.C Network Effects 120

A Tracking, Balance, Attrition, and Compliance Analyses

Table A.1. Tracking Rates - Full Sample

	Baseline	Endline 1	BL found at EL1
All	77.9%	83.9%	92.6%
Treatment	77.8%	84.2%	92.3%
Control	78.2%	83.3%	92.9%
p-value (T-C)	0.728	0.148	0.457
N Target Sample	9,652	13,618	7,523
N Final Surveyed Sample	7,523	11,426	6,963

Notes: This table shows panel retention with treatment assignment and household type. The p-values are obtained by estimating $\text{Surveyed}_h = \alpha_s + \beta\,\text{Beneficiary}_h + \varepsilon_h$. Standard errors are clustered at the neighborhood level. The unit of observation is the household. The observations come from the main sample with effective replacements that (i) were surveyed at baseline, excluding Dabat because it was not possible to collect baseline data due to conflict and social-political conditions, (ii) were surveyed at midline, and (iii) were surveyed at both baseline and midline.

Table A.2. Balance Test - Baseline Demographics
Full Sample

Variable	N	Control Mean	Beneficiary	High-Sat.
Panel A: All households				
Beneficiaries number (PMT)	11,423	3.479	-0.036	0.147
			(0.052)	(0.099)
HH size (PMT)	11,408	3.630	-0.045	0.154
			(0.053)	(0.105)
HH head's age (PMT)	11,424	38.415	-0.043	0.546
			(0.221)	(0.565)
Female head (PMT)	11,426	0.538	-0.000	-0.012
			(0.008)	(0.017)
HH number of children	11,426	1.256	-0.035	0.074
			(0.028)	(0.053)
Respondent's age	11,426	40.357	-0.230	0.108
			(0.251)	(0.545)
Respondent is female	11,426	0.596	-0.008	-0.010
			(0.008)	(0.018)
Respondent is married	11,426	0.622	0.010	0.003
			(0.011)	(0.017)
Orthodox Christian	11,426	0.352	-0.003	0.008
			(0.005)	(0.016)
Country of birth: Ethiopia	11,426	0.580	-0.001	-0.001
			(0.002)	(0.002)
Panel B: Host only				
PMT score	6,332	9.745	0.018*	-0.028
			(0.009)	(0.028)
PMT rank	6,332	133.700	-4.590	10.558
			(3.662)	(20.948)
Panel C: Refugee only				
Vulnerability score	5,094	3.906	-0.040	-0.009
			(0.030)	(0.132)
Year since arrival in ETH	4,935	20.269	-0.003	0.033
			(0.130)	(0.246)
Pre-arrival job: agri./livstk.	4,930	0.633	0.013	-0.011
			(0.013)	(0.032)
Pre-arrival job: unemployed	4,930	0.122	0.004	-0.008
			(0.011)	(0.028)
Pre-arrival job: security	4,930	0.043	-0.010	0.020*
			(0.007)	(0.010)
Number rooms in home country	4,930	1.758	-0.045	0.140*
			(0.034)	(0.072)
Father's job: agri./livstk.	4,930	0.806	0.014	0.001
			(0.013)	(0.024)
Father's job: unemployed	4,930	0.028	0.004	0.002
			(0.004)	(0.016)
Father's job: security	4,930	0.045	-0.003	0.012
			(0.007)	(0.010)
Monthly income in home country (USD PPP)	3,633	372.769	-59.902	32.996
			(72.415)	(76.542)
Pre-arrival wealth status	4,930	3.023	0.002	-0.016
			(0.046)	(0.100)
Wealth status now vs. before	4,930	3.347	-0.050	-0.035
			(0.049)	(0.105)
Joint F-Test (Prob > F)			0.642	0.732

Notes: This table presents treatment balance results for all pre-baseline covariates, using all households with available data in our sample. The second column reports the control and low-saturation group mean while the third and fourth columns report the results of estimating: $Variable_h = \alpha_s + \beta Beneficiary_h + \gamma HighSaturation_h + \epsilon_h$. $Beneficiary_h$ is an indicator equal to 1 if household h is a beneficiary of the program; $HighSaturation_h$ is an indicator equal to 1 if the household is in a high-saturated location; α_s is the randomization stratum fixed effects and standard errors, ϵ_h are clustered at the neighborhood level. Clustered standard errors are reported in round brackets. Joint F-test (Prob > F) indicates the p-value of the orthogonality joint test, which is obtained by running the treatment variable on a set of core household baseline characteristics (Beneficiaries number (PMT), HH Size (PMT), HH head's age (PMT), Female head (PMT), Respondent's age, and Respondent is female) and then testing the joint hypothesis that all coefficients are equal to zero (b1 = b2 = b3 = ... = b16 = 0). *** p<0.01, ** p<0.05, * p<0.1.

Table A.3. Balance test - Baseline demographics (Mixing)

Variable	N	Control Mean	Mixed Group	High-Mix.
Panel A: All households				
Beneficiaries number (PMT)	3,072	3.553	0.059	-0.219
			(0.063)	(0.173)
HH size (PMT)	3,068	3.710	0.055	-0.261
			(0.065)	(0.179)
HH head's age (PMT)	3,073	37.309	0.080	-0.668
			(0.430)	(1.195)
Female head (PMT)	3,073	0.525	0.019	0.039
			(0.017)	(0.030)
HH number of children	3,073	1.023	0.037	-0.134
			(0.048)	(0.082)
Respondent's age	3,073	38.974	0.209	-0.876
			(0.349)	(0.973)
Respondent is female	3,073	0.559	0.002	0.046
			(0.023)	(0.036)
Respondent is married	3,073	0.542	-0.007	-0.002
			(0.028)	(0.035)
Orthodox Christian	3,073	0.918	0.013	-0.025
			(0.009)	(0.024)
Country of birth: Ethiopia	3,073	0.754	0.001	-0.005
			(0.005)	(0.005)
Panel B: Host only				
PMT score	2,048	9.425	0.001	-0.025
			(0.012)	(0.042)
PMT rank	2,048	178.677	0.185	-8.675
			(5.419)	(35.254)
Panel C: Refugee only				
Vulnerability score	1,025	4.067	0.053	-0.222
			(0.053)	(0.214)
Year since arrival in ETH	990	17.165	0.274	0.374
			(0.208)	(0.265)
Pre-arrival job: agri./livstk.	996	0.324	0.024	0.043
			(0.030)	(0.039)
Pre-arrival job: unemployed	996	0.159	-0.031*	0.033
			(0.018)	(0.032)
Pre-arrival job: security	996	0.212	-0.007	-0.048*
			(0.021)	(0.025)
Number rooms in home country	996	1.876	0.004	0.135
			(0.067)	(0.112)
Father's job: agri./livstk.	996	0.694	0.017	0.049
			(0.032)	(0.046)
Father's job: unemployed	996	0.006	-0.010	-0.001
			(0.006)	(0.006)
Father's job: security	996	0.171	-0.003	-0.033
			(0.018)	(0.039)
Monthly income in home country (USD PPP)	714	406.715	-27.257	-18.042
			(37.020)	(60.901)
Pre-arrival wealth status	996	2.576	-0.057	0.245
			(0.082)	(0.195)
Wealth status now vs. before	996	3.365	-0.010	0.161
			(0.068)	(0.111)
Joint F-Test (Prob > F)			0.554	0.226

Notes: This table reports treatment balance results for the mixing sample that was surveyed at midline. Treated households were randomized into mixed and non-mixed groups and the mixing saturation varies across locations. The second column reports the non-mixed group and low-mixing (mixing) location mean while the third and fourth columns report the results of estimating: $Outcome_h = \alpha_s + \beta MixedGroup_h + \gamma HighMixing_h + \epsilon_h$. $MixedGroup_h$ is an indicator equal to 1 if household h is a beneficiary of the program and assigned to a mixed group; $HighMixing_h$ is an indicator equal to 1 if the household is in a high-mixed location; α_s is the randomization stratum fixed effects and standard errors, ϵ_h are clustered at the neighborhood level. Standard errors are reported in round brackets. Joint F-test (Prob > F) indicates the p-value of the orthogonality joint test, which is obtained by running the mixing variable on a set of core household baseline characteristics (Beneficiaries number (PMT), HH Size (PMT), HH head's age (PMT), Female head (PMT), Respondent's age, and Respondent is female) and then testing the joint hypothesis that all coefficients are equal to zero (b1 = b2 = b3 = ... = b20 = 0). *** p<0.01, ** p<0.05, * p<0.1.

Table A.4. Balance Test - Baseline Primary Outcomes

Outcome	N	Control Mean	Beneficiary	High-Sat.
Total assets (USD PPP)	6,963	1103.226	35.899	29.476
			(38.187)	(80.897)
Total income (USD PPP)	6,963	1448.705	-71.906*	76.232
			(37.629)	(73.996)
Total consumption (USD PPP)	6,963	5157.025	113.053	127.913
			(116.311)	(175.433)
Financial access index	6,963	0.234	-0.000	0.001
			(0.006)	(0.015)
Self-employment revenue (USD PPP)	6,963	789.538	-36.355	-8.727
			(35.716)	(83.685)
Hours worked (HH)	6,963	40.263	0.303	-0.040
			(0.977)	(2.152)
Food security index (std.)	6,962	0.029	-0.001	0.063
			(0.021)	(0.045)
Health index (std.)	6,963	0.116	0.001	-0.020
			(0.025)	(0.036)
Mental health index (std.)	6,963	-0.022	0.010	0.035
			(0.029)	(0.040)
Aspirations index (std.)	6,963	-0.110	0.015	0.060
			(0.030)	(0.044)
Social connections	6,963	2.950	0.018	-0.029
			(0.071)	(0.172)
Outgroup attitude index (std.)	6,963	0.020	0.029	0.062
			(0.024)	(0.052)
Policy support index (std.)	6,963	0.144	0.005	0.046
			(0.023)	(0.043)
Economic interaction index (std.)	6,963	-0.195	0.019	0.009
			(0.022)	(0.049)
Social interaction index (std.)	6,963	-0.193	0.030	-0.018
			(0.020)	(0.045)
Social cohesion index (std.)	6,963	0.033	0.054**	-0.022
			(0.027)	(0.057)
Language proficiency index (std.)	4,053	-0.205	-0.024	0.057
			(0.034)	(0.084)
Migrated elsewhere	6,963	0.440	-0.000	0.001
			(0.003)	(0.002)
Amenities index (std.)	6,963	0.033	-0.003	0.054
			(0.039)	(0.062)
Female empowerment index (std.)	6,963	0.114	0.017	-0.040
			(0.028)	(0.047)
Child education index (std.)	6,234	-0.031	0.055*	-0.098
			(0.030)	(0.068)
Joint F-Test (Prob > F)			0.562	0.638

Notes: This table reports treatment balance results for the 6,963 households surveyed both at baseline and midline, and for all primary outcomes as specified in the pre-analysis plan (PAP). Following primary outcomes from the PAP were not measured at baseline and are not reported in this table: Political integration index, Child health index, Child labor hours, Child development. The second column reports the control and low-saturation group mean while the third and fourth columns report the results of estimating: $Outcome_h = \alpha_s + \beta Beneficiary_h + \gamma HighSaturation_h + \epsilon_h$. $Beneficiary_h$ is an indicator equal to 1 if household h is a beneficiary of the program; $HighSaturation_h$ is an indicator equal to 1 if the household is in a high-saturated location; α_s is the randomization stratum fixed effects and standard errors, ϵ_h are clustered at the neighborhood level. Standard errors are reported in round brackets. Joint F-test (Prob > F) indicates the p-value of the orthogonality joint test, which is obtained by running the treatment variable on all primary outcomes and then testing the joint hypothesis that all coefficients are equal to zero (b1 = b2 = b3 = ... = b21 = 0). *** p<0.01, ** p<0.05, * p<0.1.

Table A.5. Testing for Non-Random Attrition

Variable	N	Control Mean	Beneficiary	High-Sat.
Baseline	9,652	0.764	-0.007	0.014
			(0.013)	(0.017)
Midline	13,618	0.818	0.011	0.003
			(0.008)	(0.017)
BL found at ML	7,523	0.918	-0.006	0.009
			(0.006)	(0.011)

Notes: The coefficients are obtained by estimating $Variable_h = \alpha_s + \beta Beneficiary_h + \gamma HighSaturation_h + \epsilon_h$. $Beneficiary_h$ is an indicator equal to 1 if household h is a beneficiary of the program; $HighSaturation_h$ is an indicator equal to 1 if the household is in a high-saturated location; α_s is the randomization stratum fixed effects and standard errors, ϵ_h are clustered at the neighborhood level. Standard errors are reported in round brackets. The unit of observation is the household. Baseline tracks survey completion at baseline of the whole sample (main sample and replacement sample). Midline tracks survey completion at midline of the whole sample. BL found at ML tracks ML survey completion of those surveyed at BL. Midline tracking, captures tracking of households at midline for the primary target sample.

Table A.6. Characterizing the Household Members Attending Public Works

	Overall		Host		Refugee	
	(1) Benefic	(2) All	(3) Benefic	(4) All	(5) Benefic	(6) All
Refugee Household	0.412	0.465	0.000	0.000	1.000	1.000
Female	0.648	0.564	0.670	0.580	0.618	0.546
Age	38.285	33.664	37.016	33.731	40.092	33.587
Household Head	0.811	0.394	0.776	0.412	0.881	0.370
Household Head's Spouse	0.111	0.213	0.129	0.206	0.076	0.222
Household Head's Child	0.046	0.216	0.055	0.195	0.028	0.244
No Education	0.390	0.313	0.279	0.223	0.547	0.416
Upto Primary Schooling	0.276	0.267	0.267	0.231	0.288	0.307
Secondary Schooling or More	0.307	0.403	0.423	0.523	0.142	0.266
Observations	6161	29713	3620	15897	2541	13816

Notes: This table reports the characteristics of household members who attended public works under the RHISN program in the host and refugee groups.

B Direct Effects: Additional Supporting Evidence

B.A Economic Effects

Table B.1. Effects on Wage Employment and Wages

	Non-PW Hourly Wage		Wage Employment			Wage Employment Hours	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	Baseline wage heterogeneity	All	No baseline employment	Baseline wage heterogeneity	Baseline employment	Baseline wage heterogeneity
RHISN beneficiary	1.087		0.008	0.041**		-2.302**	
	(3.129)		(0.011)	(0.016)		(1.146)	
RHISN beneficiary × below-median baseline wage		6.014			-0.066*		-4.038**
		(8.402)			(0.037)		(2.021)
RHISN beneficiary × above-median baseline wage		-0.424			0.030		-0.443
		(8.120)			(0.034)		(1.506)
Observations	2,876	804	11,426	5,350	1,612	1,613	1,612

Notes: Standard errors are reported in parentheses. Columns labelled "Baseline wage heterogeneity" estimate heterogeneous effects by the household's baseline wage category. These specifications are therefore restricted to households reporting wage employment at baseline. $*p < 0.10$, $**p < 0.05$, and $***p < 0.01$.

Table B.2. Main treatment effects of RHISN: Financial Assets

		Control	Treatment Effects			Test	HighSat Spillover		
Outcome	N	Mean	Overall (β_1)	Refugee ($\beta_{1,R}$)	Host ($\beta_{1,H}$)	$\beta_{1,R} = \beta_{1,H}$	Overall (β_2)	Beneficiary (θ_3)	Control (θ_2)
(1) Net financial savings (USD PPP)	11426	9.31	135***	145***	128***	0.41	15.7	24.2	-2.57
			(11.1)	(11.9)	(17.3)		(13)	(15.7)	(20.7)
			[0.000]	[0.000]	[0.000]	[0.533]	[1.000]	[0.506]	[1.000]
(2) Total savings (USD PPP)	11426	116	139***	147***	131***	0.44	12.6	24.1*	-11.8
			(10.7)	(12.3)	(16.1)		(8.82)	(12.2)	(17.7)
			[0.000]	[0.000]	[0.000]	[0.533]	[0.962]	[0.346]	[1.000]
(3) Bank balance (USD PPP)	11426	105	136***	146***	128***	0.355	13	24.1**	-10.8
			(10.3)	(11.9)	(15.4)		(8.2)	(11.5)	(16.8)
			[0.000]	[0.000]	[0.000]	[0.533]	[0.962]	[0.346]	[1.000]
(4) Total lent (USD PPP)	11426	11.4	2.09**	0.775	3.12**	0.139	-0.278	-0.0446	-0.777
			(0.82)	(0.817)	(1.35)		(1.28)	(1.54)	(1.8)
			[0.010]	[0.407]	[0.020]	[0.385]	[1.000]	[1.000]	[1.000]
(5) Total debt (USD PPP)	11426	107	2.85	2.15	2.88	0.933	-2.25	0.261	-7.61
			(4.64)	(4.57)	(7.46)		(9.68)	(10.4)	(13.1)
			[0.220]	[0.667]	[0.305]	[1.000]	[1.000]	[1.000]	[1.000]
(6) HH debt (USD PPP)	11426	22.9	-3.05**	-0.528	-5.32**	0.096	-0.722	-1.69	1.34
			(1.54)	(1.59)	(2.38)		(3.2)	(3.54)	(3.79)
			[0.029]	[0.684]	[0.021]	[0.344]	[1.000]	[1.000]	[1.000]
(7) Total owed (USD PPP)	11426	84.2	5.15	1.88	7.72	0.453	-1.05	2.37	-8.37
			(4.15)	(3.66)	(6.87)		(9.78)	(10.7)	(12)
			[0.089]	[0.667]	[0.110]	[0.533]	[1.000]	[1.000]	[1.000]
(8) **1**(Bank account)	11426	0.67	0.232***	0.393***	0.103***	0.0000	0.02**	0.0107	0.0398
			(0.022)	(0.0219)	(0.0104)		(0.0101)	(0.0185)	(0.0299)
			[0.000]	[0.000]	[0.000]	[0.000]	[0.962]	[1.000]	[1.000]
(9) **1**(Mobile account)	11426	0.029	-0.00031	0.000792	-0.00115	0.74	-0.000981	0.00342	-0.0104*
			(0.00307)	(0.00332)	(0.00483)		(0.00463)	(0.00522)	(0.00593)
			[0.373]	[0.684]	[0.322]	[0.974]	[1.000]	[1.000]	[1.000]
(10) **1**(Loan group participation)	11426	0.159	0.185***	0.071***	0.277***	0.0000	0.00217	0.0205	-0.0369
			(0.031)	(0.0215)	(0.0442)		(0.0134)	(0.028)	(0.046)
			[0.000]	[0.002]	[0.000]	[0.000]	[1.000]	[1.000]	[1.000]

Notes: The table reports the treatment effect on beneficiaries enrolled in the RHISN program. All monetary outcomes are reported in real USD PPP terms and winsorized at 98%. Net financial savings is defined as the net total of total savings (2) and total debt (4). Total savings is the sum of households' bank balance (3) and total amount lent (4) (total cash/value of goods sent that is expected to be paid back). Total debt is the sum of total household debt (6) (commercial lenders and moneylenders) and total amount owed (7) (total cash/value of goods received that is expected to be paid back). Bank account (8), mobile account (9) and loan group participation (10) are dummy variables. The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table B.3. Main treatment effects of RHISN: Food Security

		Control	Treatment Effects			Test	HighSat Spillover		
Outcome	N	Mean	Overall (β_1)	Refugee ($\beta_{1,R}$)	Host ($\beta_{1,H}$)	$\beta_{1,R} = \beta_{1,H}$	Overall (β_2)	Beneficiary (θ_3)	Control (θ_2)
(1) Food security index (std.)	11426	0	0.131***	0.0671**	0.181***	0.01	0.0331	0.0454	0.00702
			(0.0232)	(0.0293)	(0.0326)		(0.0515)	(0.0576)	(0.0559)
			[0.000]	[0.056]	[0.000]	[0.036]	[1.000]	[1.000]	[1.000]
(2) Less expensive food (days)	11421	4.83	0.307***	0.196***	0.396***	0.031	0.0888	0.135	-0.00942
			(0.0497)	(0.0588)	(0.0703)		(0.122)	(0.132)	(0.135)
			[0.000]	[0.011]	[0.000]	[0.036]	[1.000]	[1.000]	[1.000]
(3) Borrow food (days)	11401	5.54	0.178***	0.0927*	0.248***	0.036	0.0568	0.0763	0.0154
			(0.0374)	(0.0528)	(0.0514)		(0.0794)	(0.0852)	(0.0939)
			[0.000]	[0.066]	[0.000]	[0.036]	[1.000]	[1.000]	[1.000]
(4) Limit portion size (days)	11413	4.98	0.24***	0.108*	0.342***	0.018	0.111	0.111	0.11
			(0.0526)	(0.0631)	(0.0751)		(0.103)	(0.116)	(0.121)
			[0.000]	[0.066]	[0.000]	[0.036]	[1.000]	[1.000]	[1.000]
(5) Adults restrict for children (days)	11407	5.38	0.218***	0.105	0.301***	0.036	-0.0291	0.00811	-0.108
			(0.0455)	(0.0679)	(0.0634)		(0.09)	(0.103)	(0.102)
			[0.000]	[0.079]	[0.000]	[0.036]	[1.000]	[1.000]	[1.000]
(6) Reduce meals per day (days)	11409	5.14	0.237***	0.105*	0.337***	0.009	0.0819	0.0875	0.0699
			(0.0469)	(0.0586)	(0.0648)		(0.102)	(0.116)	(0.107)
			[0.000]	[0.066]	[0.000]	[0.036]	[1.000]	[1.000]	[1.000]
(7) **1**(Enough food)	11426	0.423	0.0494***	0.0292**	0.0658***	0.078	0.0198	0.0231	0.0126
			(0.011)	(0.0119)	(0.0169)		(0.0228)	(0.0257)	(0.0262)
			[0.000]	[0.056]	[0.000]	[0.044]	[1.000]	[1.000]	[1.000]

Notes: The table reports the treatment effect on beneficiaries enrolled in the RHISN program. All outcomes have been redefined so that they move in the same direction, with higher values reflecting better outcomes. Food security index (1) is constructed as the weighted average of rows (2-6) and standardized by the overall control group mean. Rows (2-6) are defined as the number of days in the last week when the household did not have to do each. Row (7) is a dummy indicated if the household had enough food or money to buy food in the last 7 days. The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table B.4. Main treatment effects of RHISN: Mental Health

		Control	Treatment Effects			Test	HighSat Spillover		
Outcome	N	Mean	Overall (β_1)	Refugee ($\beta_{1,R}$)	Host ($\beta_{1,H}$)	$\beta_{1,R} = \beta_{1,H}$	Overall (β_2)	Beneficiary (θ_3)	Control (θ_2)
(1) Mental health index (std.)	11426	0	0.146***	0.0704**	0.206***	0.005	0.0679	0.0992	0.00114
			(0.0271)	(0.0295)	(0.0374)		(0.0622)	(0.0692)	(0.0628)
			[0.000]	[0.079]	[0.000]	[0.018]	[1.000]	[0.561]	[1.000]
(2) CES-D score (1-4)	11422	3.26	0.0639***	0.0249*	0.0948***	0.003	0.018	0.0319	-0.0118
			(0.0127)	(0.0134)	(0.0184)		(0.0289)	(0.0329)	(0.0277)
			[0.000]	[0.127]	[0.000]	[0.018]	[1.000]	[0.561]	[1.000]
(3) Life satisfaction (1-10)	11413	6.55	0.28***	0.155	0.383***	0.069	0.171	0.227	0.0517
			(0.067)	(0.0944)	(0.0818)		(0.136)	(0.145)	(0.154)
			[0.000]	[0.154]	[0.000]	[0.053]	[1.000]	[0.561]	[1.000]
(4) Happiness index (1-7)	11422	5.39	0.125***	0.0775	0.162***	0.267	0.0668	0.0874	0.0228
			(0.0392)	(0.0547)	(0.0524)		(0.1)	(0.108)	(0.101)
			[0.000]	[0.167]	[0.000]	[0.097]	[1.000]	[0.561]	[1.000]
(5) Stress scale (1-5, PSS-4)	11425	3.21	0.0573***	0.0283	0.0812***	0.076	0.0653	0.0855*	0.022
			(0.0157)	(0.0204)	(0.0216)		(0.0417)	(0.0489)	(0.0366)
			[0.000]	[0.167]	[0.000]	[0.053]	[1.000]	[0.561]	[1.000]
(6) Locus of control (0-1)	11426	0.648	0.0254***	0.00844	0.0393***	0.021	-0.0103	-0.00754	-0.0163
			(0.00694)	(0.00935)	(0.00946)		(0.0135)	(0.0144)	(0.0173)
			[0.000]	[0.230]	[0.000]	[0.036]	[1.000]	[0.561]	[1.000]
(7) **1**(CES-D Depression Risk)	11422	0.441	-0.0546***	-0.0313**	-0.0737***	0.03	-0.0136	-0.0281	0.0176
			(0.0103)	(0.012)	(0.0152)		(0.023)	(0.0257)	(0.0237)
			[0.000]	[0.079]	[0.000]	[0.039]	[1.000]	[0.561]	[1.000]

Notes: The table reports the treatment effect on beneficiaries enrolled in the RHISN program. The outcomes have been redefined so that they move in the same direction, with higher values reflecting better outcomes. Mental health index (1) is constructed as the average of rows (2-6) and is standardized by the overall control group mean. CES-D score (2) is constructed as the average of how often during the last week the intended beneficiary felt or behaved in a certain way, using 10 out of the 20 indicators from the CES-D Scale (Scale 1-4). Life satisfaction (3) reflects overall satisfaction with life (Scale 1-10). Happiness score (4) reflects current happiness (Scale 1-7). Stress scale (5) is constructed as the average of how often in the last month the intended beneficiary felt stressed out, using the Perceived Stress Scale-4 (PSS-4) (Scale 1-5). Locus of control (6) is constructed as the average of two dummy indicators indicating sense of control over destiny. The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table B.5. Main treatment effects of RHISN: Health

		Control	Treatment Effects			Test	HighSat Spillover		
Outcome	N	Mean	Overall (β_1)	Refugee ($\beta_{1,R}$)	Host ($\beta_{1,H}$)	$\beta_{1,R} = \beta_{1,H}$	Overall (β_2)	Beneficiary (θ_3)	Control (θ_2)
(1) Health index (std.)	11426	0	0.0601***	0.0424	0.0697**	0.54	0.0161	0.0535	-0.0637
			(0.0227)	(0.0296)	(0.0333)		(0.0354)	(0.0366)	(0.0514)
			[0.049]	[1.000]	[0.071]	[1.000]	[1.000]	[0.785]	[1.000]
(2) Self-rated health (likert, 1-4)	11426	3.59	0.0275*	-0.000356	0.0486***	0.074	0.00895	0.0285	-0.0328
			(0.0145)	(0.0203)	(0.0183)		(0.024)	(0.0244)	(0.0332)
			[0.109]	[1.000]	[0.058]	[0.209]	[1.000]	[0.963]	[1.000]
(3) Number of health visits (month)	11426	0.781	-0.0206	-0.0218	-0.0141	0.887	-0.0364	-0.0698**	0.0347
			(0.0269)	(0.0388)	(0.0373)		(0.03)	(0.0319)	(0.0568)
			[0.287]	[1.000]	[0.253]	[1.000]	[1.000]	[0.274]	[1.000]
(4) Work/school days missed (month)	11180	1.71	-0.212**	-0.0445	-0.335**	0.068	0.0536	-0.0712	0.323
			(0.0844)	(0.0822)	(0.135)		(0.138)	(0.141)	(0.211)
			[0.049]	[1.000]	[0.058]	[0.209]	[1.000]	[1.000]	[1.000]
(5) Healthcare not accessed (month)	11426	0.689	-0.0671*	-0.0524	-0.0779	0.744	0.00888	-0.0132	0.0559
			(0.0384)	(0.0583)	(0.0524)		(0.0603)	(0.0667)	(0.0826)
			[0.116]	[1.000]	[0.128]	[1.000]	[1.000]	[1.000]	[1.000]
(6) **1**(Major health issues)	11392	0.287	-0.0116	-0.0121	-0.0108	0.944	-0.0117	-0.0206	0.00722
			(0.00978)	(0.0128)	(0.0143)		(0.0243)	(0.0256)	(0.027)
			[0.171]	[1.000]	[0.247]	[1.000]	[1.000]	[1.000]	[1.000]
(7) Annual health expenditure (USD PPP)	11426	130	-1.03	11.9	-11.6	0.06	-1.64	-5.29	6.13
			(6.56)	(9.78)	(7.74)		(8.97)	(8.71)	(14.8)
			[0.497]	[1.000]	[0.128]	[0.209]	[1.000]	[1.000]	[1.000]

Notes: The table reports the treatment effect on beneficiaries enrolled in the RHISN program. Health index (1) is constructed as the average of rows (2-7) and standardized using the overall control group mean. For the index construction, rows (3-6) were redefined so that higher values reflect better outcomes (e.g. higher value reflects less days missed). Self-rated health (2) shows the self-described health status (1 - Very Poor 4 - Good). Rows (3-5) are defined as the number of visits to a hospital/clinic, number of days of work/housework missed due to poor health, and the number of times the intended beneficiary felt they should go to the doctor but did not in the last month. Row (6) shows a dummy variable indicating any major health issue in the past year. Health expenditure (7) is the total annual healthcare expenditure reported in real USD PPP and winsorized at 98%. The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table B.6. Main treatment effects of RHISN: Amenities

Outcome	N	Control Mean	Treatment Effects			Test	HighSat Spillover		
			Overall (β_1)	Refugee ($\beta_{1,R}$)	Host ($\beta_{1,H}$)	$\beta_{1,R} = \beta_{1,H}$	Overall (β_2)	Beneficiary (θ_3)	Control (θ_2)
(1) Amenities index (std.)	11426	0	0.0703***	0.0145	0.111***	0.018	-0.0272	-0.0451	0.0111
			(0.0208)	(0.0286)	(0.0289)		(0.0516)	(0.056)	(0.0539)
			[0.003]	[1.000]	[0.001]	[0.074]	[1.000]	[1.000]	[1.000]
(2) Sewerage quality (likert, 1-5)	11085	2.94	0.0482	0.00636	0.0795*	0.201	-0.00175	-0.0474	0.0947
			(0.0296)	(0.0358)	(0.0446)		(0.0576)	(0.0609)	(0.0684)
			[0.093]	[1.000]	[0.067]	[0.205]	[1.000]	[1.000]	[1.000]
(3) Street cleanliness (likert, 1-5)	11372	3.44	0.15***	0.0856**	0.2***	0.026	-0.0102	-0.0202	0.0112
			(0.0257)	(0.0382)	(0.0334)		(0.0565)	(0.0599)	(0.0663)
			[0.000]	[0.308]	[0.000]	[0.074]	[1.000]	[1.000]	[1.000]
(4) Green space (likert, 1-5)	10478	2.78	0.0798***	0.047	0.104***	0.23	-0.000443	-0.0222	0.0457
			(0.0248)	(0.0306)	(0.0358)		(0.0813)	(0.0867)	(0.087)
			[0.004]	[0.850]	[0.006]	[0.205]	[1.000]	[1.000]	[1.000]
(5) Public toilets (likert, 1-5)	9619	2.53	0.0195	-0.0344	0.0591	0.085	-0.0481	-0.0632	-0.0154
			(0.0275)	(0.0385)	(0.038)		(0.0767)	(0.081)	(0.0813)
			[0.296]	[1.000]	[0.091]	[0.119]	[1.000]	[1.000]	[1.000]
(6) Drain smells (likert, 1-5)	11426	3.02	-0.0127	-0.0384	-0.000189	0.377	0.0155	0.00284	0.0424
			(0.0212)	(0.0326)	(0.0284)		(0.057)	(0.0635)	(0.0559)
			[0.296]	[1.000]	[0.331]	[0.232]	[1.000]	[1.000]	[1.000]
(7) Trash smells (likert, 1-5)	10899	3.29	0.0867**	-0.0163	0.166***	0.01	-0.0665	-0.0655	-0.0685
			(0.034)	(0.0541)	(0.0438)		(0.0827)	(0.0892)	(0.0908)
			[0.015]	[1.000]	[0.001]	[0.074]	[1.000]	[1.000]	[1.000]
(8) Crime and safety index (std.)	11426	0	0.0133	0.0497	-0.0129	0.276	-0.0326	-0.0228	-0.0534*
			(0.0258)	(0.0535)	(0.0213)		(0.0239)	(0.0319)	(0.0295)
			[0.296]	[1.000]	[0.200]	[0.205]	[1.000]	[1.000]	[1.000]

Notes: The table reports the treatment effect on beneficiaries enrolled in the RHISN program. All outcomes have been redefined so that they move in the same direction, with higher values reflecting better outcomes. Amenities index (1) is constructed as the average of rows (2-8) and is standardized by the overall control group mean. Rows (2-7) show the reported neighborhood quality as measured by different indicators (1 - Very Poor 5 - Very Good). Crime and safety index (8) constructed as the average of the number of times the respondent has been a victim of a list of 7 crimes or attempted crimes in the past year, and is standardized by the overall control group mean. The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table B.7. Main treatment effects of RHISN: Female Empowerment

		Control	Treatment Effects			Test	HighSat Spillover		
Outcome	N	Mean	Overall (β_1)	Refugee ($\beta_{1,R}$)	Host ($\beta_{1,H}$)	$\beta_{1,R} = \beta_{1,H}$	Overall (β_2)	Beneficiary (θ_3)	Control (θ_2)
(1) Female empowerment index (std.)	11424	0	0.427***	0.425***	0.427***	0.97	-0.00155	0.0248	-0.0577
			(0.0231)	(0.0354)	(0.031)		(0.037)	(0.0438)	(0.0418)
			[0.000]	[0.000]	[0.000]	[1.000]	[1.000]	[1.000]	[1.000]
(2) Female empowerment index alt. (std.)	11424	0	0.0235	0.0273	0.017	0.794	-0.0116	0.00575	-0.0488
			(0.0191)	(0.0308)	(0.0245)		(0.0324)	(0.0346)	(0.0424)
			[0.324]	[0.398]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
(3) Resource control (std., only fem.)	7511	0	0.402***	0.57***	0.255***	0.0000	0.037	0.0323	0.047
			(0.0312)	(0.0401)	(0.0277)		(0.0321)	(0.0405)	(0.0603)
			[0.000]	[0.000]	[0.000]	[0.000]	[1.000]	[1.000]	[1.000]
(4) Decision control (std., only fem.)	4235	0	0.0356	0.0718*	-0.0035	0.214	0.0392	0.0584	-0.00141
			(0.0306)	(0.0391)	(0.0462)		(0.0542)	(0.0591)	(0.0696)
			[0.324]	[0.130]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
(5) Domestic discord (std.)	7173	0	-0.0312	-0.0472	-0.0197	0.582	-0.0159	0.0123	-0.0764
			(0.0245)	(0.0394)	(0.0309)		(0.0396)	(0.0449)	(0.0461)
			[0.324]	[0.254]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
(6) Gender own attitudes (std.)	11423	0	0.0346**	0.0486*	0.0211	0.437	0.00587	0.0152	-0.014
			(0.0174)	(0.026)	(0.0239)		(0.0272)	(0.0307)	(0.037)
			[0.110]	[0.130]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
(7) Gender community attitudes (std.)	11423	0	-0.0197	-0.0416	0.00038	0.261	0.00943	0.0217	-0.0168
			(0.0182)	(0.0292)	(0.0232)		(0.0247)	(0.0282)	(0.0347)
			[0.324]	[0.191]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
(8) Controlling behaviors (std., only fem.)	7202	0	0.0137	0.0218	0.0053	0.727	-0.0476	-0.0243	-0.0972*
			(0.0232)	(0.0384)	(0.0278)		(0.0486)	(0.0523)	(0.0538)
			[0.624]	[0.455]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
(9) Emotional violence (std., only fem.)	291	0	0.0297	-0.108	0.11	0.483	0.0386	0.0886	-0.0311
			(0.148)	(0.266)	(0.16)		(0.14)	(0.215)	(0.178)
			[0.624]	[0.455]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
(10) Employment outside HH (std.)	10552	0	0.739***	0.696***	0.777***	0.364	-0.00891	0.0232	-0.078
			(0.0427)	(0.0676)	(0.0579)		(0.0458)	(0.0619)	(0.0714)
			[0.000]	[0.000]	[0.000]	[1.000]	[1.000]	[1.000]	[1.000]
(11) Girls attend school (std.)	5664	0	0.0265	0.0194	0.0362	0.749	-0.00104	0.0124	-0.0315
			(0.0264)	(0.0402)	(0.0339)		(0.0344)	(0.0389)	(0.0542)
			[0.327]	[0.455]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]

Notes: The table reports the treatment effect on beneficiaries enrolled in the RHISN program. All outcomes have been redefined so that they move in the same direction, with higher values reflecting better outcomes, and have been standardized by the overall control group mean. Female empowerment index (1) is constructed as the average of rows (3-6) and (8-10). Female empowerment index alt. (2) is an alternative index that excludes rows (3) and (10), which include bank account ownership and public work employment, i.e. possible mechanical RHISN-driven increases in female empowerment. Resource control (3), decision control (4), controlling behaviors (8) and emotional violence (9) are restricted to female respondents. Rows (3), (4), (6) and (7) are averages of a set of dummy variables indicating resource control (bank account and phone ownership), decision control (e.g. saving, children's disciplining) and gender attitudes (e.g. whether wife has right to express opinions if she disagrees). Rows (5), (8) and (9) are averages of variables indicating the frequency of certain behaviors/incidences (1 - often, 4 - never). This includes domestic discord (arguments over e.g. spending, children, alcohol), controlling behaviors (e.g. accusing of being unfaithful), and emotional violence (e.g. insulting). Row (10) is defined as the number of women above the age of 18, who have paid wage employment outside of the household. Row (11) indicates whether all girls (6–17) in the household are attending school. The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** p<0.01, ** p<0.05, * p<0.1.

B.B Social Effects

Table B.8. Main treatment effects of RHISN: Outgroup Attitudes Index

		Control	Treatment Effects			Test	HighSat Spillover		
Outcome	N	Mean	Overall (β_1)	Refugee ($\beta_{1,R}$)	Host ($\beta_{1,H}$)	$\beta_{1,R} = \beta_{1,H}$	Overall (β_2)	Beneficiary (θ_3)	Control (θ_2)
(1) Outgroup attitude index (std.)	11426	0	0.0561***	-0.014	0.113***	0.001	0.0238	0.0608	-0.0549
			(0.0208)	(0.0181)	(0.0321)		(0.0529)	(0.0541)	(0.0633)
			[0.033]	[0.697]	[0.003]	[0.009]	[1.000]	[0.796]	[1.000]
(2) Outgroup attitude index alt. (std.)	11426	0	0.0311	-0.0371*	0.0856**	0.002	0.0192	0.055	-0.0573
			(0.0213)	(0.0193)	(0.0328)		(0.0555)	(0.0565)	(0.0663)
			[0.213]	[0.679]	[0.022]	[0.009]	[1.000]	[0.796]	[1.000]
(3) Job rank diff.	1161	-0.041	-0.011	-0.0952	0.0604	0.366	-0.189*	-0.192	-0.181
			(0.084)	(0.13)	(0.113)		(0.11)	(0.125)	(0.157)
			[0.573]	[0.697]	[0.271]	[0.255]	[1.000]	[0.796]	[1.000]
(4) Neighbors comfort (likert, 1-5)	11426	4.11	0.00793	0.00127	0.0133	0.783	0.000645	0.0103	-0.0199
			(0.0216)	(0.0327)	(0.0288)		(0.0518)	(0.0514)	(0.0653)
			[0.544]	[1.000]	[0.271]	[0.431]	[1.000]	[1.000]	[1.000]
(5) Offspring marriage (likert, 1-5)	11426	3.55	0.0234	-0.0273	0.0646	0.125	-0.0611	-0.0064	-0.178**
			(0.0315)	(0.0404)	(0.0442)		(0.0661)	(0.0689)	(0.0824)
			[0.376]	[0.697]	[0.089]	[0.130]	[1.000]	[1.000]	[0.850]
(6) Dinner together (freq., 1-5)	11426	2.13	0.108***	0.0245	0.175***	0.015	-0.0357	0.00494	-0.122*
			(0.0338)	(0.0518)	(0.0332)		(0.0621)	(0.0666)	(0.0713)
			[0.012]	[0.792]	[0.000]	[0.028]	[1.000]	[1.000]	[1.000]
(7) Returning valuables (likert, 1-4)	11426	2.52	0.0169	-0.0445**	0.0663**	0.002	0.0578	0.0637*	0.0453
			(0.0192)	(0.0219)	(0.0286)		(0.0369)	(0.0375)	(0.0533)
			[0.376]	[0.679]	[0.029]	[0.009]	[1.000]	[0.796]	[1.000]
(8) Emotional support (likert, 1-4)	11426	2.78	0.0192	-0.029	0.0593***	0.003	0.0155	0.0315	-0.0185
			(0.0161)	(0.0196)	(0.0212)		(0.0342)	(0.0384)	(0.0371)
			[0.284]	[0.679]	[0.017]	[0.009]	[1.000]	[0.796]	[1.000]
(9) Work with (likert, 1-5)	11426	4.11	0.0744***	0.0334	0.107***	0.098	0.0525	0.0707*	0.0137
			(0.0214)	(0.0331)	(0.0291)		(0.0416)	(0.0421)	(0.0555)
			[0.010]	[0.695]	[0.003]	[0.126]	[1.000]	[0.796]	[1.000]
(10) Work under supervision (likert, 1-5)	11426	3.95	0.0608**	0.0377	0.0791**	0.395	0.0453	0.0675	-0.00181
			(0.0238)	(0.0355)	(0.0333)		(0.0515)	(0.0511)	(0.0674)
			[0.037]	[0.695]	[0.029]	[0.255]	[1.000]	[0.796]	[1.000]
(11) Local police (likert, 1-5)	11426	3.78	0.037	0.0241	0.0464	0.661	0.0637	0.0976	-0.00866
			(0.026)	(0.0346)	(0.0376)		(0.0575)	(0.061)	(0.0687)
			[0.213]	[0.697]	[0.111]	[0.380]	[1.000]	[0.796]	[1.000]
(12) Same school (likert, 1-5)	11426	4.42	0.036**	0.0137	0.0534**	0.254	0.00781	0.0308	-0.0412
			(0.0173)	(0.0241)	(0.025)		(0.0339)	(0.0352)	(0.0428)
			[0.085]	[0.749]	[0.038]	[0.222]	[1.000]	[0.796]	[1.000]
(13) Refugee effect in Ethiopia (likert, 1-3)	11426	2.57	0.00357	-0.0203	0.0235	0.053	-0.0321	-0.0237	-0.0502
			(0.0118)	(0.0129)	(0.0183)		(0.0324)	(0.033)	(0.0375)
			[0.544]	[0.679]	[0.111]	[0.076]	[1.000]	[0.796]	[1.000]
(14) Activities easy together (0-5)	5094	2.38	-0.0632*	-0.0632*	0.	0.	0.142	0.154	0.109
			(0.0371)	(0.0371)	(0.)		(0.173)	(0.177)	(0.181)
			[0.166]	[0.679]	[0.]	[0.]	[1.000]	[0.796]	[1.000]

Notes: The table reports the treatment effect on beneficiaries enrolled in the RHISN program. The outcomes have been redefined so that they move in the same direction, with higher values reflecting better outcomes. Outgroup attitude index (1) is constructed as the average of rows (3-14) and is standardized using the overall control group mean. Outgroup attitude index alt. (2) is an alternative index that excludes rows (6), (9) and (10), i.e. possible mechanical RHISN-driven increases in positive attitudes towards the outgroup. Rows (4-5) and (9-12) are defined as the degree of comfort with i) being neighbors with, ii) offspring marry someone from, iii) working with, iv) working under supervision of, v) accepting as a local police officer, and vi) attendance at the same school of children from the ingroup with someone from the outgroup (Scale 1-5). Row (6) is a dummy variable indicating if the intended beneficiary had dinner/tea with someone from the outgroup in the past year. Rows (7-8) are defined as the extent to which the intended beneficiary agrees that someone from the outgroup would return something valuable that they left behind and that they could lean on someone from the outgroup during difficult times (Scale 1-4). Row (13) is defined as the opinion on the net effect of refugees in Ethiopia (Scale 1-3). Row (14) is defined as the sum of number of things that are easy to do between hosts & refugees out of market exchange, labor mobility, use of financial services, social participation and sharing local resources. The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table B.9. Main treatment effects of RHISN: Policy Support Index

		Control	Treatment Effects			Test	HighSat Spillover		
Outcome	N	Mean	Overall (β_1)	Refugee ($\beta_{1,R}$)	Host ($\beta_{1,H}$)	$\beta_{1,R} = \beta_{1,H}$	Overall (β_2)	Beneficiary (θ_3)	Control (θ_2)
(1) Policy support index (std.)	11426	0	0.0315	-0.0274	0.0785**	0.005	-0.0233	-0.0116	-0.0481
			(0.0193)	(0.0215)	(0.0301)		(0.0449)	(0.0445)	(0.0571)
			[0.271]	[0.763]	[0.035]	[0.018]	[1.000]	[1.000]	[1.000]
(2) Full citizenship (likert, 1-5)	11426	4.06	0.0533**	-0.0197	0.113***	0.005	-0.0253	0.000672	-0.0808
			(0.0235)	(0.0374)	(0.028)		(0.05)	(0.0519)	(0.0641)
			[0.214]	[0.953]	[0.001]	[0.018]	[1.000]	[1.000]	[1.000]
(3) Work permit (likert, 1-5)	11426	4.06	0.0392	-0.0288	0.0942**	0.021	0.00919	0.00979	0.00791
			(0.0273)	(0.0347)	(0.0397)		(0.0463)	(0.0468)	(0.0651)
			[0.299]	[0.953]	[0.041]	[0.036]	[1.000]	[1.000]	[1.000]
(4) Free mobility (likert, 1-5)	11426	4.31	0.0333	0.00819	0.0534*	0.284	-0.0209	-0.0232	-0.016
			(0.0205)	(0.0291)	(0.0305)		(0.0394)	(0.0396)	(0.0536)
			[0.271]	[0.953]	[0.092]	[0.139]	[1.000]	[1.000]	[1.000]
(5) Humanitarian spending (likert, 1-5)	11426	4.58	-0.00539	-0.0301*	0.0146	0.133	-0.0111	-0.00699	-0.0197
			(0.0157)	(0.017)	(0.0242)		(0.031)	(0.0303)	(0.0432)
			[0.486]	[0.763]	[0.226]	[0.097]	[1.000]	[1.000]	[1.000]
(6) Government spending (likert, 1-5)	11426	4.31	0.0239	-0.00894	0.0497	0.127	0.000805	0.0122	-0.0236
			(0.02)	(0.0202)	(0.0326)		(0.0417)	(0.0414)	(0.0547)
			[0.331]	[0.953]	[0.118]	[0.097]	[1.000]	[1.000]	[1.000]
(7) Refugee effect in Ethiopia (likert, 1-3)	11426	2.57	0.00357	-0.0203	0.0235	0.053	-0.0321	-0.0237	-0.0502
			(0.0118)	(0.0129)	(0.0183)		(0.0324)	(0.033)	(0.0375)
			[0.486]	[0.763]	[0.158]	[0.056]	[1.000]	[1.000]	[1.000]

Notes: The table reports the treatment effect on beneficiaries enrolled in the RHISN program. All outcomes have been redefined so that they move in the same direction, with higher values reflecting better outcomes. Policy support index (1) is constructed as the average of rows (2-7) and is standardized using the overall control group mean. Rows (2-6) show refugee attitudes as reflected in the level of agreement with each pro-refugee action (e.g. granting work permits to refugees) (1 - Strongly Disagree 5 - Strongly Agree). Row (7) is the opinion on the net effect of refugees on Ethiopia's society (Scale 1-3). The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table B.10. Main treatment effects of RHISN: Economic Interaction Index

		Control	Treatment Effects			Test	HighSat Spillover		
Outcome	N	Mean	Overall (β_1)	Refugee ($\beta_{1,R}$)	Host ($\beta_{1,H}$)	$\beta_{1,R} = \beta_{1,H}$	Overall (β_2)	Beneficiary (θ_3)	Control (θ_2)
(1) Economic interaction index (std.)	11426	0	0.0271	-0.00282	0.0495**	0.345	0.0159	0.0264	-0.00652
			(0.0269)	(0.0497)	(0.0247)		(0.033)	(0.0421)	(0.0361)
			[1.000]	[1.000]	[0.108]	[1.000]	[1.000]	[1.000]	[1.000]
(2) Economic interaction index alt. (std.)	11426	0	-0.00701	-0.0283	0.00898	0.243	0.00895	0.0159	-0.00585
			(0.0155)	(0.0247)	(0.0202)		(0.0296)	(0.0328)	(0.0329)
			[1.000]	[1.000]	[0.933]	[0.946]	[1.000]	[1.000]	[1.000]
(3) Number of econ. int. with outgroup (0-4)	11426	0.107	0.00384	0.00641	0.00214	0.754	-0.00542	-0.00388	-0.00869
			(0.00711)	(0.00777)	(0.0113)		(0.0115)	(0.0126)	(0.0135)
			[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
(4) Share of sales to outgroup	3418	0.383	0.0067	0.00407	0.00854	0.77	-0.0195	-0.0161	-0.0275
			(0.00764)	(0.0113)	(0.0103)		(0.0168)	(0.0173)	(0.0205)
			[1.000]	[1.000]	[0.933]	[1.000]	[1.000]	[1.000]	[1.000]
(5) Share of outgroup co-workers	6654	0.107	-0.00623	-0.122***	0.0407***	0.0000	0.0125	0.00886	0.0252
			(0.0176)	(0.0363)	(0.00953)		(0.0129)	(0.0175)	(0.0217)
			[1.000]	[0.012]	[0.000]	[0.000]	[1.000]	[1.000]	[1.000]
(6) Share of outgroup co-workers excl. public works	3576	0.106	-0.00192	-0.0485**	0.0144***	0.008	0.00212	-0.0091	0.0197
			(0.00759)	(0.0229)	(0.00492)		(0.00816)	(0.0109)	(0.0124)
			[1.000]	[0.159]	[0.016]	[0.029]	[1.000]	[1.000]	[1.000]
(7) Share of outgroup co-workers in public works	4020	0.113	-0.0133	-0.0233	-0.00961	0.7	0.0102	0.00948	0.0285
			(0.0161)	(0.0301)	(0.0188)		(0.0205)	(0.0209)	(0.0345)
			[1.000]	[1.000]	[0.933]	[1.000]	[1.000]	[1.000]	[1.000]
(8) Share of remittances to outgroup	8509	0.099	-0.0011	-0.00356	0.0012	0.662	0.00213	0.00578	-0.00552
			(0.00452)	(0.0106)	(0.00285)		(0.00901)	(0.0105)	(0.00834)
			[1.000]	[1.000]	[0.933]	[1.000]	[1.000]	[1.000]	[1.000]

Notes: The table reports the treatment effect on beneficiaries enrolled in the RHISN program. Economic interaction index (1) is constructed as the average of rows (3), (4), (5) and (8), and is standardized by the overall control group mean. Economic interaction index alt. (2) is an alternative index that includes row (6) instead of row (5), i.e. excludes co-working with the outgroup in public works (possible mechanical RHISN-driven increases in interactions). Row (3) shows the number of economic interactions with the outgroup, as measured by whether the host/refugee respondent visited the refugee camp/nearby town within the last month to i) work for a firm, ii) look for work, iii) sell goods, and/or iv) buy goods. Row (4) is defined as the share of total earnings from selling crops, livestock or animal products earned from selling to the outgroup. Row (5) is defined as the share of co-workers in paid employment positions that are from the outgroup, with (6) and (7) showing that share for non-public works and public works positions. Row (8) is defined as the share of remittances sent to the outgroup. The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table B.11. Main treatment effects of RHISN: Social Interaction

		Control	Treatment Effects			Test	HighSat Spillover		
Outcome	N	Mean	Overall (β_1)	Refugee ($\beta_{1,R}$)	Host ($\beta_{1,H}$)	$\beta_{1,R} = \beta_{1,H}$	Overall (β_2)	Beneficiary (θ_3)	Control (θ_2)
(1) Social interaction index (std.)	11426	0	0.0428**	-0.0108	0.0872***	0.001	-0.0554	-0.0353	-0.0983**
			(0.0164)	(0.0249)	(0.0173)		(0.0394)	(0.043)	(0.0377)
			[0.049]	[1.000]	[0.000]	[0.005]	[0.841]	[1.000]	[0.075]
(2) Social interaction index alt. (std.)	11393	0	0.0181	-0.0133	0.0447***	0.036	-0.0402	-0.0274	-0.0674**
			(0.0135)	(0.0228)	(0.0155)		(0.0358)	(0.0394)	(0.0337)
			[0.281]	[1.000]	[0.009]	[0.078]	[0.897]	[1.000]	[0.092]
(3) Share close relatives	10544	0.041	0.0000	-0.0000836	0.000398	0.929	-0.0057	-0.00553	-0.00608
			(0.00232)	(0.00535)	(0.00108)		(0.00446)	(0.00487)	(0.00504)
			[0.776]	[1.000]	[0.355]	[0.591]	[0.841]	[1.000]	[0.160]
(4) Share close friends	9611	0.061	-0.00275	-0.00767	0.00175	0.133	-0.00183	0.00174	-0.00962
			(0.00306)	(0.00582)	(0.0023)		(0.00668)	(0.00771)	(0.00593)
			[0.467]	[1.000]	[0.250]	[0.153]	[1.000]	[1.000]	[0.136]
(5) Share good friends	6979	0.06	-0.00194	-0.007	0.00226	0.287	-0.00671	-0.00363	-0.0129
			(0.00391)	(0.00854)	(0.00168)		(0.00805)	(0.00871)	(0.00946)
			[0.578]	[1.000]	[0.151]	[0.285]	[0.897]	[1.000]	[0.150]
(6) Share economic relationships	4353	0.092	0.00244	-0.000692	0.00464	0.66	-0.00503	0.00147	-0.0186*
			(0.00508)	(0.0118)	(0.00286)		(0.011)	(0.0131)	(0.0109)
			[0.578]	[1.000]	[0.100]	[0.493]	[1.000]	[1.000]	[0.136]
(7) Share community group members	1331	0.028	0.00919	0.0143	0.0075	0.795	-0.0129**	-0.0128	-0.0133
			(0.00815)	(0.0255)	(0.00653)		(0.00631)	(0.00784)	(0.0103)
			[0.355]	[1.000]	[0.189]	[0.546]	[0.766]	[1.000]	[0.156]
(8) **1**(Tea with outgroup)	11426	0.459	0.0449***	0.00535	0.0771***	0.001	-0.0307	-0.0158	-0.0626**
			(0.0121)	(0.0151)	(0.0141)		(0.0206)	(0.0222)	(0.0255)
			[0.003]	[1.000]	[0.000]	[0.005]	[0.841]	[1.000]	[0.075]
(9) **1**(Children friends with)	9041	0.48	0.0282**	-0.000783	0.0534***	0.027	-0.0236	-0.00824	-0.0567**
			(0.0122)	(0.0185)	(0.0157)		(0.0274)	(0.032)	(0.0242)
			[0.054]	[1.000]	[0.003]	[0.078]	[0.897]	[1.000]	[0.075]
(10) **1**(Children play with)	9356	0.494	0.0279**	0.00873	0.0459***	0.127	-0.0227	-0.014	-0.0413
			(0.0121)	(0.0176)	(0.0168)		(0.028)	(0.0322)	(0.0265)
			[0.054]	[1.000]	[0.011]	[0.153]	[0.897]	[1.000]	[0.136]

Notes: The table reports the treatment effect on beneficiaries enrolled in the RHISN program. Social interaction index (1) is constructed as the average of rows (3-10) and is standardized by the overall control group mean. Social interaction index alt. (2) is an alternative index that excludes rows (7) and (8), i.e. possible mechanical RHISN-driven increases in social interactions with the outgroup. Rows (3-7) are defined as the share of relatives, close friends, good friends, economic relationships and community group members from the outgroup. Rows (8-10) are dummy variables indicating whether they had tea with the outgroup in the past year, and whether their children are friends with or play with children from the outgroup. The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table B.12. Main treatment effects of RHISN: Social Cohesion

		Control	Treatment Effects			Test	HighSat Spillover		
Outcome	N	Mean	Overall (β_1)	Refugee ($\beta_{1,R}$)	Host ($\beta_{1,H}$)	$\beta_{1,R} = \beta_{1,H}$	Overall (β_2)	Beneficiary (θ_3)	Control (θ_2)
(1) Social cohesion index (std.)	11426	0	0.0614**	-0.00133	0.114***	0.012	0.0286	0.0602	-0.0388
			(0.0244)	(0.0298)	(0.0342)		(0.0623)	(0.068)	(0.0642)
			[0.055]	[1.000]	[0.005]	[0.029]	[1.000]	[0.895]	[1.000]
(2) Social cohesion index alt. (std.)	11426	0	0.0586**	-0.0159	0.122***	0.001	0.0555	0.102*	-0.0444
			(0.0229)	(0.0265)	(0.0315)		(0.0535)	(0.0589)	(0.0531)
			[0.055]	[1.000]	[0.002]	[0.008]	[1.000]	[0.247]	[1.000]
(3) Hypothetical: share donated (0-100)	11425	34.6	0.93	0.0116	1.64*	0.147	-0.252	-0.201	-0.359
			(0.59)	(0.667)	(0.9)		(1.61)	(1.74)	(1.84)
			[0.134]	[1.000]	[0.032]	[0.119]	[1.000]	[1.000]	[1.000]
(4) Incentivized: share donated (0-100)	11426	26.8	0.238	-0.299	0.661**	0.053	0.024	0.04	-0.0101
			(0.249)	(0.388)	(0.306)		(0.62)	(0.695)	(0.653)
			[0.218]	[1.000]	[0.026]	[0.071]	[1.000]	[1.000]	[1.000]
(5) **1**(Sign petitions refugees)	11426	0.745	0.013	0.0231**	0.00485	0.285	-0.0166	-0.0212	-0.00677
			(0.00903)	(0.0102)	(0.0137)		(0.0332)	(0.0341)	(0.0357)
			[0.146]	[0.288]	[0.100]	[0.136]	[1.000]	[1.000]	[1.000]
(6) Trust scale (likert, 1-4)	11425	2.64	0.0165	-0.0333	0.06**	0.008	0.0319	0.0642*	-0.037
			(0.0176)	(0.0244)	(0.0243)		(0.0359)	(0.0369)	(0.0461)
			[0.218]	[1.000]	[0.020]	[0.029]	[1.000]	[0.247]	[1.000]
(7) Integration perception (likert, 1-4)	11426	3	0.0374**	0.00511	0.0663**	0.074	0.0586*	0.0947***	-0.0184
			(0.0182)	(0.0229)	(0.0252)		(0.0331)	(0.034)	(0.0429)
			[0.092]	[1.000]	[0.019]	[0.080]	[1.000]	[0.052]	[1.000]
(8) National identity	11426	4.25	0.0274*	0.00788	0.0413**	0.319	0.00303	0.0164	-0.0255
			(0.016)	(0.0295)	(0.0161)		(0.038)	(0.0388)	(0.0458)
			[0.126]	[1.000]	[0.019]	[0.136]	[1.000]	[1.000]	[1.000]

Notes: The table reports the treatment effect on beneficiaries enrolled in the RHISN program. All outcomes have been redefined so that they move in the same direction, with higher values reflecting better outcomes. Social cohesion index (1) is constructed as the average of rows (3-7) and is standardized by the overall control group mean. Social cohesion index alt. (2) is an alternative index that excludes rows (4-5), since these outcomes were not measured at baseline. Rows (3) and (4) are defined as the amount of money donated to the outgroup in the hypothetical and incentivized dictator games. Row (5) is a dummy variable indicating willingness to sign a petition to support refugees. Trust scale (6) and integration perception (7) are defined as self-reported trust in the outgroup and self-reported integration of refugees (Scale 1-4: 1 - Strongly Disagree 4 - Strongly Agree). National identity (8) is defined as the self-reported feeling of connection to Ethiopia, reflecting how close the intended beneficiary feels to Ethiopia and how often they feel like an outsider. The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table B.13. Main treatment effects of RHISN: Political Integration

Outcome	N	Control Mean	Treatment Effects: Overall (β_1)	Refugee ($\beta_{1,R}$)	Host ($\beta_{1,H}$)	Test: $\beta_{1,R} = \beta_{1,H}$	HighSat Spillover: Overall (β_2)	Beneficiary (θ_3)	Control (θ_2)
(1) Political integration index (std.)	11426	0	0.116***	0.0731**	0.153***	0.062	0.0534	0.0535	0.0533
			(0.0223)	(0.0278)	(0.032)		(0.0381)	(0.0426)	(0.0451)
			[0.000]	[0.016]	[0.000]	[0.387]	[1.000]	[1.000]	[1.000]
(2) Political integration index alt. (std.)	11426	0	0.103***	0.0884***	0.116***	0.543	0.0427	0.054	0.0184
			(0.0229)	(0.0329)	(0.0318)		(0.0397)	(0.0444)	(0.0451)
			[0.000]	[0.016]	[0.001]	[1.000]	[1.000]	[1.000]	[1.000]
(3) **1**(Any formal ID)	11426	0.978	0.00745***	-0.000509	0.014***	0.001	0.0048	0.00188	0.011
			(0.0026)	(0.000624)	(0.0044)		(0.00519)	(0.00514)	(0.00734)
			[0.004]	[0.228]	[0.003]	[0.010]	[1.000]	[1.000]	[1.000]
(4) **1**(Leadership role)	11426	0.033	0.00589	0.00581	0.00589	0.992	0.00519	0.00783	-0.000454
			(0.00368)	(0.00597)	(0.00475)		(0.00501)	(0.00612)	(0.00619)
			[0.040]	[0.201]	[0.123]	[1.000]	[1.000]	[1.000]	[1.000]
(5) Citizen participation index (std.)	11426	0	0.0899***	0.0773***	0.102***	0.494	0.0173	0.0312	-0.0123
			(0.0192)	(0.0202)	(0.0305)		(0.0489)	(0.0541)	(0.0494)
			[0.000]	[0.002]	[0.002]	[1.000]	[1.000]	[1.000]	[1.000]
(6) Political awareness (likert, 1-5)	10197	2.17	0.0203	0.0106	0.0265	0.756	0.00339	0.0168	-0.0254
			(0.0251)	(0.0404)	(0.0315)		(0.0612)	(0.0652)	(0.0653)
			[0.144]	[0.313]	[0.192]	[1.000]	[1.000]	[1.000]	[1.000]
(7) Political discussions (freq., 1-5)	11426	1.47	0.0337	0.0312	0.0336	0.952	-0.00895	-0.00527	-0.0168
			(0.0212)	(0.0217)	(0.0332)		(0.0575)	(0.0593)	(0.0709)
			[0.040]	[0.099]	[0.163]	[1.000]	[1.000]	[1.000]	[1.000]
(8) Political actions (0-1)	11426	0.283	0.0492***	0.0389***	0.0594***	0.167	0.0104	0.0127	0.00532
			(0.00742)	(0.00967)	(0.0112)		(0.0189)	(0.0215)	(0.0184)
			[0.000]	[0.002]	[0.000]	[0.803]	[1.000]	[1.000]	[1.000]
(9) Political engagement (0-1)	11426	0.173	0.0127***	0.0141***	0.0122	0.834	0.00715	0.00954	0.00206
			(0.00466)	(0.00444)	(0.00766)		(0.0137)	(0.0149)	(0.0136)
			[0.005]	[0.007]	[0.070]	[1.000]	[1.000]	[1.000]	[1.000]
(10) Trust/satisfied: local government (likert, 1-5)	11426	3.13	0.101***	0.0757**	0.124***	0.353	0.0352	0.0303	0.0457
			(0.0269)	(0.0326)	(0.0399)		(0.037)	(0.0413)	(0.05)
			[0.000]	[0.023]	[0.003]	[1.000]	[1.000]	[1.000]	[1.000]

Notes: The table reports the treatment effect on beneficiaries enrolled in the RHISN program. All outcomes have been redefined so that they move in the same direction, with higher values reflecting better outcomes. Political integration index (1) is constructed as the average of rows (3-5) and (10) and is standardized by the overall control group mean. Political integration index alt. (2) is an alternative index that excludes row (3) and attendance in community meetings as a component of political actions in row (8), i.e. possible mechanical RHISN-driven increases in political integration. Rows (3) and (4) are dummy variables indicating having any formal ID and holding a position of responsibility in a camp or city. Citizen participation index (5) is an index constructed as the average of rows (6-9), and is standardized by the overall control group mean. Political awareness (6) and discussions (7) reflect understanding of and frequency of discussing political issues in Ethiopia (Scale 1-5). Political actions (8) and engagement (9) are averages of a set of dummies reflecting having taken political action or contacted politicians/leaders to discuss problems in the past year. Row (10) reflects trust and satisfaction with the local government (Scale 1-5). The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table B.14. Direct and Saturation Effects of RHISN: Social Outcomes
Dabat and Urban Asayita

		Control	Treatment Effects			Test	HighSat Spillover		
Outcome	N	Mean	Overall (β_1)	Refugee ($\beta_{1,R}$)	Host ($\beta_{1,H}$)	$\beta_{1,R} = \beta_{1,H}$	Overall (β_2)	Beneficiary (θ_3)	Control (θ_2)
(1) Outgroup attitude index (std.)	4974	0.277	0.117***	-0.0255	0.193***	0.0000	0.0405	0.0714	-0.0389
			(0.0349)	(0.0264)	(0.0414)		(0.0723)	(0.0732)	(0.0904)
(2) Policy support index (std.)	4974	0.261	0.0524*	-0.0859	0.127***	0.	-0.0439	-0.0159	-0.116*
			(0.0305)	(0.0000)	(0.0354)		(0.0437)	(0.0444)	(0.0631)
(3) Economic interaction index (std.)	4974	-0.058	0.0841	0.103	0.0655**	0.813	0.037	0.0611	-0.0251
			(0.0545)	(0.158)	(0.0256)		(0.0398)	(0.0547)	(0.0586)
(4) Social interaction index (std.)	4974	0.265	0.0575	-0.0711	0.115***	0.003	-0.0299	-0.021	-0.053
			(0.0352)	(0.056)	(0.0223)		(0.0498)	(0.056)	(0.0598)
(5) Social cohesion index (std.)	4974	0.114	0.142***	0.0216	0.212***	0.005	0.0194	0.0478	-0.0538
			(0.0414)	(0.0446)	(0.0477)		(0.104)	(0.11)	(0.103)
(6) Political integration index (std.)	4974	-0.004	0.098***	-0.0175	0.163***	0.001	0.0848	0.118**	-0.00111
			(0.0347)	(0.031)	(0.0412)		(0.0561)	(0.0565)	(0.0738)
(7) Combined social index (std.)	4974	0.143	0.0921***	-0.0103	0.145***	0.0000	0.017	0.0425	-0.0485
			(0.0277)	(0.0261)	(0.0224)		(0.0363)	(0.0384)	(0.0454)

Notes: The table reports the treatment effect on beneficiaries enrolled in the RHISN program, restricted to the Amhara (Dabat) and Afar (Asayita) subsample, where mixing was implemented properly. All index components have been redefined so that they move in the same direction, with higher values reflecting better outcomes. All indices have been standardized using the overall control group mean. The indices are constructed as follows. **OA** (Outgroup Attitude): comfort and trust in everyday interactions with the outgroup, including living proximity, working relations, children's contact, and perceived community contributions. **PA** (Policy Attitudes): support for refugee inclusion policies, including citizenship, freedom of movement, work permits, and public spending on refugees. **EI** (Economic Interaction): market-based interactions with the outgroup, including sales and working together. **SI** (Social Interaction): social ties with the outgroup, including friendships, community group participation, shared activities, and children's play. **SC** (Social Cohesion): trust and pro-social behavior toward the outgroup, including interpersonal trust, charitable donations, perceived integration, and national identity. **PI** (Political Integration): civic and political engagement, including identity document ownership, community leadership, civic participation, and trust in local government. Combined social index is constructed as the average of the indices in rows (1)–(6). The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** p<0.01, ** p<0.05, * p<0.1.

C Mixing Effects: Additional Supporting Evidence

Table C.1. Mixing effects of RHISN by household type: Outgroup Attitudes Index (DDML)

		Control	Overall	Mixing Effects			Test	HighMix Spillover		
Outcome	N	Mean	Benefic (β_1)	Overall (δ_1)	Refugee ($\delta_{1,R}$)	Host ($\delta_{1,H}$)	$\delta_{1,R} = \delta_{1,H}$	Overall (δ_2)	Benefic (ϕ_3)	Control (ϕ_2)
(1) Outgroup attitude index (std.)	4694	0.277	0.10**	0.07*	0.02	0.11**	0.243	0.14*	0.15*	0.12
			(0.04)	(0.04)	(0.06)	(0.05)		(0.08)	(0.09)	(0.09)
(2) Outgroup attitude index alt. (std.)	4694	0.284	0.07*	0.05	0.00	0.09*	0.196	0.14	0.16*	0.10
			(0.04)	(0.04)	(0.05)	(0.05)		(0.09)	(0.09)	(0.09)
(3) Job rank diff.	480	-0.139	0.00354	-0.0955	0.0494	-0.115	0.518	0.114	0.0839	0.0769
			(0.133)	(0.114)	(0.165)	(0.142)		(0.152)	(0.174)	(0.232)
(4) Neighbors comfort (likert, 1-5)	4694	4.28	0.0428	0.00644	-0.0477	0.0348	0.156	0.0724	0.0686	0.0774
			(0.0379)	(0.0355)	(0.0317)	(0.0484)		(0.0689)	(0.0717)	(0.0852)
(5) Offspring marriage (likert, 1-5)	4694	3.74	0.0842*	0.00834	-0.1	0.0796	0.073	0.141	0.181	0.0657
			(0.0476)	(0.0569)	(0.08)	(0.0617)		(0.114)	(0.118)	(0.146)
(6) Dinner together (freq., 1-5)	4694	2.39	0.185***	0.0227	-0.0651	0.076	0.241	0.161	0.174	0.138
			(0.0669)	(0.0633)	(0.122)	(0.0557)		(0.11)	(0.124)	(0.142)
(7) Returning valuables (likert, 1-4)	4694	2.59	0.0399	0.0392	0.0253	0.0472	0.658	0.0935	0.121*	0.0428
			(0.0414)	(0.0257)	(0.0333)	(0.0356)		(0.0591)	(0.0639)	(0.0716)
(8) Emotional support (likert, 1-4)	4694	2.88	0.0412	0.061**	0.0384	0.0756*	0.539	0.113*	0.152**	0.0345
			(0.029)	(0.0301)	(0.0388)	(0.0416)		(0.0673)	(0.0724)	(0.0739)
(9) Work with (likert, 1-5)	4694	4.24	0.0775*	0.115***	0.126**	0.122***	0.956	0.0467	0.0279	0.0901
			(0.0409)	(0.0341)	(0.0608)	(0.0414)		(0.0599)	(0.0612)	(0.0876)
(10) Work under supervision (likert, 1-5)	4694	4.1	0.0693	0.0808*	0.0866	0.0834*	0.963	0.0634	0.0266	0.134*
			(0.0425)	(0.0419)	(0.0876)	(0.0481)		(0.073)	(0.0793)	(0.0781)
(11) Local police (likert, 1-5)	4694	3.98	0.0347	0.0629	-0.0131	0.111**	0.146	-0.0125	-0.0344	0.0321
			(0.048)	(0.0422)	(0.0658)	(0.0545)		(0.0871)	(0.1)	(0.0994)
(12) Same school (likert, 1-5)	4694	4.55	0.0068	0.06**	0.0669*	0.0589	0.852	-0.0107	-0.0206	0.00749
			(0.0297)	(0.0274)	(0.0396)	(0.0376)		(0.0444)	(0.045)	(0.0572)
(13) Refugee effect in Ethiopia (likert, 1-3)	4694	2.74	0.0513***	-0.0276	-0.0289	-0.0235	0.915	0.0877**	0.0859**	0.0958**
			(0.0173)	(0.0183)	(0.0246)	(0.0243)		(0.0372)	(0.0429)	(0.0422)
(14) Activities easy together (0-5)	1617	2.72	-0.0911	0.0566	0.0566	0.	0.	-0.0219	0.0204	-0.0973
			(0.0731)	(0.0817)	(0.0817)	(0.)		(0.346)	(0.366)	(0.335)

Notes: The table reports the effect of treatment and mixing on beneficiaries enrolled in the RHISN program in Amhara (Dabat) and parts of Afar (Asayita), where mixing was implemented properly. The outcomes have been redefined so that they move in the same direction, with higher values reflecting better outcomes. Outgroup attitude index (1) is constructed as the average of rows (3-14) and is standardized using the overall control group mean. Outgroup attitude index alt. (2) is an alternative index that excludes rows (6), (9) and (10), i.e. possible mechanical RHISN-driven increases in positive attitudes towards the outgroup. Rows (4-5) and (9-12) are defined as the degree of comfort with i) being neighbors with, ii) offspring marry someone from, iii) working with, iv) working under supervision of, v) accepting as a local police officer, and vi) attendance at the same school of children from the ingroup with someone from the outgroup (Scale 1-5)n . Row (6) is a dummy variable indicating if the intended beneficiary had dinner/tea with someone from the outgroup in the past year. Rows (7-8) are defined as the extent to which the intended beneficiary agrees that someone from the outgroup would return something valuable that they left behind and that they could lean on someone from the outgroup during difficult times (Scale 1-4). Row (13) is defined as the opinion on the net effect of refugees in Ethiopia (Scale 1-3). Row (14) is defined as the sum of number of things that are easy to do between hosts & refugees out of market exchange, labor mobility, use of financial services, social participation and sharing local resources. The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table C.2. Mixing effects of RHISN by household type: Social Interaction Index (DDML)

		Control	Overall	Mixing Effects			Test	HighMix Spillover		
Outcome	N	Mean	Benefic (β_1)	Overall (δ_1)	Refugee ($\delta_{1,R}$)	Host ($\delta_{1,H}$)	$\delta_{1,R} = \delta_{1,H}$	Overall (δ_2)	Benefic (ϕ_3)	Control (ϕ_2)
(1) Social interaction index (std.)	4694	0.267	0.04	0.03	0.01	0.05**	0.533	0.06	0.06	0.05
			(0.03)	(0.03)	(0.06)	(0.02)		(0.04)	(0.05)	(0.06)
(2) Social interaction index alt. (std.)	4684	0.252	-0.01	0.02	0.02	0.03	0.812	0.03	0.03	0.04
			(0.03)	(0.02)	(0.04)	(0.03)		(0.04)	(0.05)	(0.05)
(3) Share close relatives	4299	0.072	-0.00841	-0.00242	0.00258	-0.00317	0.574	0.00464	0.00385	0.00582
			(0.00904)	(0.0031)	(0.0092)	(0.00211)		(0.00393)	(0.00544)	(0.00901)
(4) Share close friends	3746	0.106	-0.0057	0.000937	0.000	0.0019	0.859	0.00469	0.00523	0.00251
			(0.00614)	(0.00557)	(0.0141)	(0.00313)		(0.00624)	(0.00731)	(0.0101)
(5) Share good friends	2568	0.118	-0.0103*	0.00394	0.00818	0.00257	0.863	0.000	0.00587	-0.0107
			(0.00555)	(0.0102)	(0.0318)	(0.00267)		(0.00505)	(0.0078)	(0.00896)
(6) Share economic relationships	1460	0.187	0.00292	0.00965	0.0103	0.0075*	0.747	0.000516	-0.00586	0.0172
			(0.00956)	(0.0091)	(0.0292)	(0.00441)		(0.0111)	(0.0134)	(0.0147)
(7) Share community group members	925	0.028	0.00307	0.0399***	0.0788	0.0291***	0.304	0.000	0.00424	-0.0145
			(0.00821)	(0.0129)	(0.0492)	(0.00889)		(0.00706)	(0.0105)	(0.0142)
(8) **1**(Tea with outgroup)	4694	0.542	0.0843***	0.00472	-0.0314	0.0255	0.106	0.0427	0.0524	0.0208
			(0.0215)	(0.0195)	(0.0317)	(0.0191)		(0.0321)	(0.0339)	(0.0468)
(9) **1**(Children friends with)	3599	0.575	0.025	0.025	0.0153	0.0338	0.622	0.0279	0.0363	0.0115
			(0.0202)	(0.0197)	(0.0277)	(0.0262)		(0.0362)	(0.0407)	(0.0364)
(10) **1**(Children play with)	3709	0.605	0.0354*	0.0115	0.00529	0.0169	0.791	0.0516	0.043	0.0688*
			(0.0201)	(0.0203)	(0.0329)	(0.0261)		(0.0349)	(0.0397)	(0.036)

Notes: The table reports the effect of treatment and mixing on beneficiaries enrolled in the RHISN program in Amhara (Dabat) and parts of Afar (Asayita), where mixing was implemented properly. Social interaction index (1) is constructed as the average of rows (3-10) and is standardized by the overall control group mean. Social interaction index alt. (2) is an alternative index that excludes rows (7) and (8), i.e. possible mechanical RHISN-driven increases in social interactions with the outgroup. Rows (3-7) are defined as the share of relatives, close friends, good friends, economic relationships and community group members from the outgroup. Rows (8-10) are dummy variables indicating whether they had tea with the outgroup in the past year, and whether their children are friends with or play with children from the outgroup. The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table C.3. Mixing effects of RHISN by household type: Economic Interaction (DDML)

		Control	Overall	Mixing Effects			Test	HighMix Spillover		
Outcome	N	Mean	Benefic (β_1)	Overall (δ_1)	Refugee ($\delta_{1,R}$)	Host ($\delta_{1,H}$)	$\delta_{1,R} = \delta_{1,H}$	Overall (δ_2)	Benefic (ϕ_3)	Control (ϕ_2)
(1) Economic interaction index (std.)	4694	-0.057	-0.06	0.31***	0.49**	0.21***	0.25	0.07	0.12*	-0.03
			(0.04)	(0.07)	(0.25)	(0.03)		(0.04)	(0.06)	(0.07)
(2) Economic interaction index alt. (std.)	4694	-0.053	-0.01	-0.00	-0.02	0.01	0.566	0.01	-0.01	0.04
			(0.02)	(0.02)	(0.05)	(0.02)		(0.04)	(0.04)	(0.05)
(3) Number of econ. int. with outgroup (0-4)	4694	0.054	-0.0107	0.000	-0.0154	0.00663	0.132	-0.00604	-0.0216	0.0266
			(0.0092)	(0.00773)	(0.0119)	(0.00979)		(0.0186)	(0.0162)	(0.0238)
(4) Share of sales to outgroup	1395	0.328	0.00414	-0.0025	0.0625**	-0.03*	0.007	0.0454	0.0465	0.0443
			(0.0145)	(0.0163)	(0.0301)	(0.0166)		(0.03)	(0.0291)	(0.0451)
(5) Share of outgroup co-workers	3448	0.087	-0.0539**	0.16***	0.233**	0.124***	0.295	0.0351**	0.0513**	-0.0374
			(0.0223)	(0.0314)	(0.105)	(0.0161)		(0.0163)	(0.0227)	(0.0336)
(6) Share of outgroup co-workers excl. public works	1456	0.086	-0.00667	0.000	-0.0785	0.0136	0.112	0.00136	0.00549	-0.00325
			(0.00866)	(0.0159)	(0.0588)	(0.0124)		(0.00953)	(0.0177)	(0.0121)
(7) Share of outgroup co-workers in public works	2603	0.102	-0.0794***	0.194***	0.265**	0.155***	0.303	0.0419*	0.0426*	-0.0255
			(0.0218)	(0.0341)	(0.106)	(0.0184)		(0.0229)	(0.0231)	(0.0625)
(8) Share of remittances to outgroup	3740	0.136	0.0124	-0.00705	-0.00537	-0.00663	0.972	0.00992	0.00529	0.019**
			(0.00823)	(0.00524)	(0.0143)	(0.00441)		(0.0082)	(0.0105)	(0.00934)

Notes: The table reports the effect of treatment and mixing on beneficiaries enrolled in the RHISN program in Amhara (Dabat) and parts of Afar (Asayita), where mixing was implemented properly. Economic interaction index (1) is constructed as the average of rows (3), (4), (5) and (8), and is standardized by the overall control group mean. Economic interaction index alt. (2) is an alternative index that includes row (6) instead of row (5), i.e. excludes co-working with the outgroup in public works (possible mechanical RHISN-driven increases in interactions). Row (3) shows the number of economic interactions with the outgroup, as measured by whether the host/refugee respondent visited the refugee camp/nearby town within the last month to i) work for a firm, ii) look for work, iii) sell goods, and/or iv) buy goods. Row (4) is defined as the share of total earnings from selling crops, livestock or animal products earned from selling to the outgroup. Row (5) is defined as the share of co-workers in paid employment positions that are from the outgroup, with (6) and (7) showing that share for non-public works and public works positions. Row (8) is defined as the share of remittances sent to the outgroup. The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** p<0.01, ** p<0.05, * p<0.1.

D Causal Forest Estimation and Baseline Moderators

This appendix provides additional details on the causal forest specification used to study treatment effect heterogeneity.

A conventional subgroup analysis would require selecting a small number of interactions ex ante or estimating a large number of separate interaction models. The former risks overlooking interactions among household type, geographic location, baseline economic conditions, and social connectedness, while the latter raises concerns about multiple hypothesis testing and limited power. We therefore use causal forests, which employ sample splitting and honest estimation to flexibly identify treatment effect heterogeneity while limiting overfitting (Athey and Imbens, 2016). The estimator extends random forests to estimate conditional average treatment effects (CATEs) nonparametrically (Wager and Athey, 2018; Athey et al., 2019). Monte Carlo evidence suggests that causal machine learning methods perform well in recovering heterogeneous treatment effects when treatment assignment and outcome processes are flexible (Knaus et al., 2021).

We estimate the generalized causal forest proposed by Athey and Wager (2019) and Wager and Athey (2018). To account for the experimental design, we first residualize both the treatment indicator and the outcome with respect to Region $\times$ HH-Type fixed effects using cross-fold residualization following Kattenberg et al. (2023). Here, HH-Type distinguishes refugee and host households, while Region denotes the four study locations. The causal forest is then estimated using these residualized variables together with a rich set of baseline moderators.

For each outcome, the forest estimates household-specific conditional average treatment effects (CATEs), ranks the baseline characteristics most predictive of treatment effect heterogeneity, and identifies the household profiles expected to benefit most from the intervention. The forest is trained using seven groups of pre-treatment moderators:

1. **Program context:** household type (refugee or host), study region, and neighborhood saturation assignment.
2. **Demographic characteristics:** intended beneficiary age, sex, marital status, educational attainment, and an indicator for the presence of children in the household.
3. **Livelihood characteristics:** baseline employment status, self-employment status, and total household labor supply.
4. **Social capital and connectedness:** language proficiency index, number of close friends, and the share of social ties belonging to the outgroup.
5. **Economic conditions:** household consumption relative to the local median, log baseline consumption, log baseline household income, and log baseline household assets.
6. **Health and psychosocial status:** baseline mental health index.
7. **Baseline outcome:** the pre-treatment value of the outcome under study.

Together, these variables capture households' initial demographic, economic, social, and geographic conditions, allowing the forest to flexibly identify the baseline characteristics associated with treatment effect heterogeneity.



Figure D.1. Causal Forest: Moderators Predictive Relevance

Notes: This figure reports the variation explained by different baseline moderators in the fixed-effects-adjusted causal forest across outcomes. Rows are baseline moderators grouped by moderator family, columns are outcomes grouped by outcome family, and darker cells indicate that the moderator receives greater importance in predicting treatment-effect heterogeneity for that outcome.

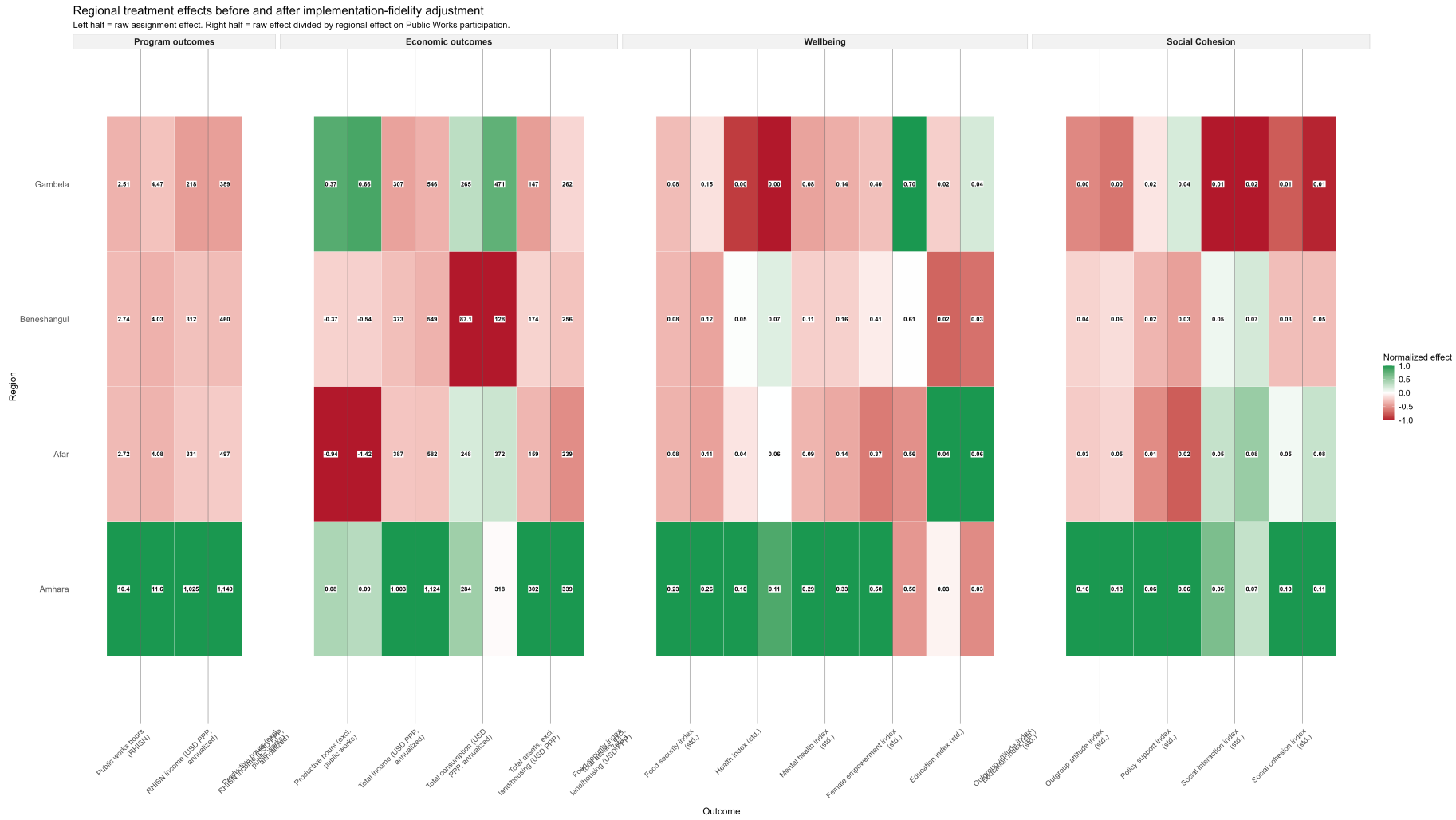


Figure D.2. Regional variation in predicted effects, before and after normalizing by program delivery

Notes: Each panel reports predicted regional CATEs by outcome. The left panel reports raw predicted effects. The right panel divides each region's outcome CATE by that region's public-works participation CATE, so that regions are compared at a common level of program delivery. Outcomes whose regional dispersion falls under the normalization are those for which regional differences in predicted effects track regional differences in delivery. Outcomes whose dispersion rises vary across regions for reasons the delivery margin does not capture. The first stage relates four regional means and the ratio uses two estimated predictions, so the exercise is a descriptive normalization rather than an instrumental variables estimand.

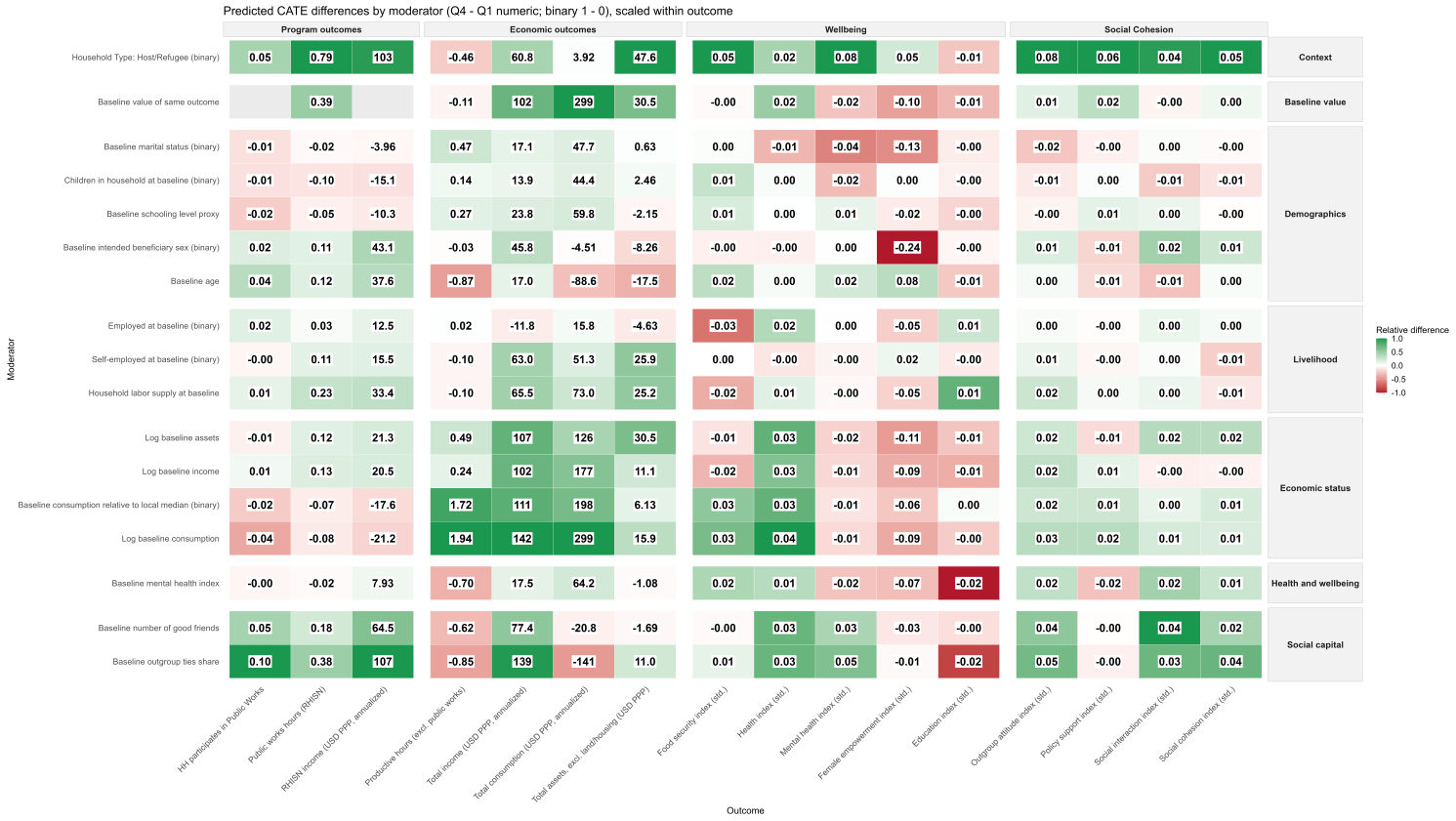


Figure D.3. Predicted CATE differences by moderator

Notes: This figure reports the differences in CATE predictions for each outcome, scaled within the outcome for comparability. Numeric moderators report the difference in predicted effects between the fourth and first quartiles; binary moderators compare the one and zero groups. Green cells indicate that the higher-valued group has a larger predicted CATE, while red cells indicate the opposite.

D.A Exploring the Role of Connectedness

In this appendix section we examine the association between connectedness and program effectiveness for refugees by comparing two dimensions of connectedness: i) camp vs.town residence, and ii) ethnic proximity.

1. *Camp vs. city residence.* We first assess whether refugees living in camps or towns show systematically different responses to the RHISN program. For example, it is possible that refugees in town are closer to the main markets and host communities and such may have more economic opportunities to leverage the program. Town refugees initially appear to receive more benefits of the program, with higher predicted participation, public works hours, and RHISN income (Figure D.4), but these differences disappear within regions, indicating that the they reflect regional composition rather than settlement type.[21] Consistent with these evidence, Table D.1 and Table D.2 show similar treatment effects for camp and town refugees, with the exception of food security, for which town refugees experience significantly larger gain. Overall, these results provide little evidence that settlement type systematically shapes RHISN's effectiveness.

2. *Ethic proximity.* We next explore whether pre-existing ethnic proximity with host communities shapes program effectiveness. Pugnido hosts both Nuer and Anywaa refugees. Anywaa refugees share language and kinship ties with the surrounding host population, while Nuer refugees largely do not. Anywaa households experience larger gains in mental health, female empowerment, and economic interaction with the outgroup, but there are no significant differential effects on income, assets, consumption, or productive labor (Table D.3 and Table D.4). As such, we cannot distinguish a real pattern suggesting more effectiveness of the program for either group.

[21] Figure D.5 shows that restricting the comparison to Afar and Amhara, which contain 282 of the 284 town refugees and hold both settlement types, reproduces the result.

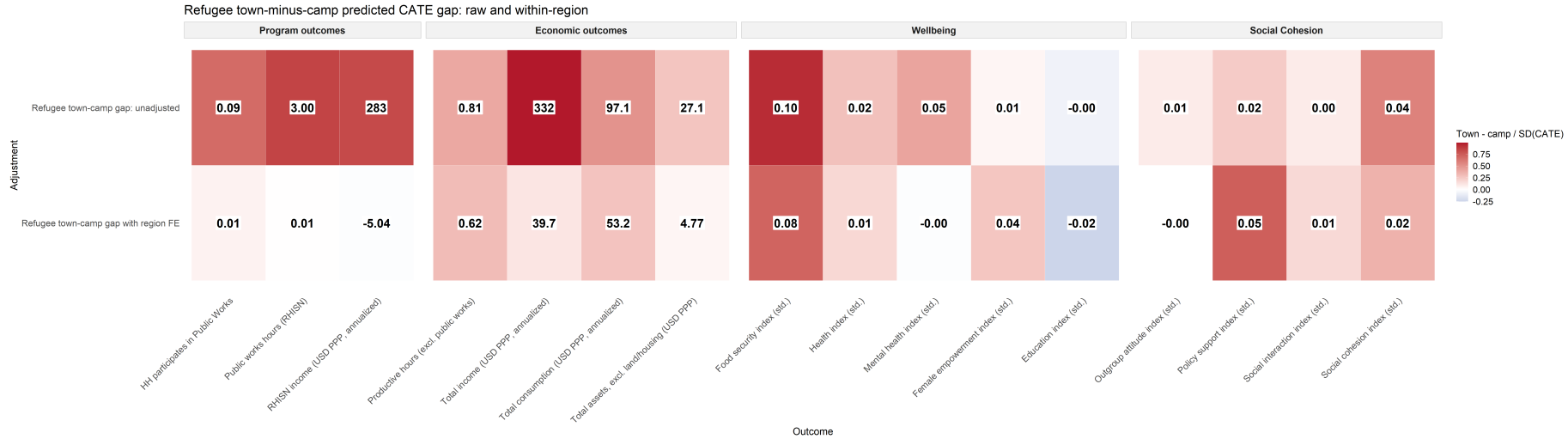


Figure D.4. Predicted CATE gaps between town and camp refugees, before and after region fixed effects. Each estimate comes from a regression of household CATE predictions on an indicator for refugee town residence, with standard errors clustered at the program-location level. The standard errors describe dispersion in generated predictions and do not incorporate uncertainty from the forest estimation step.

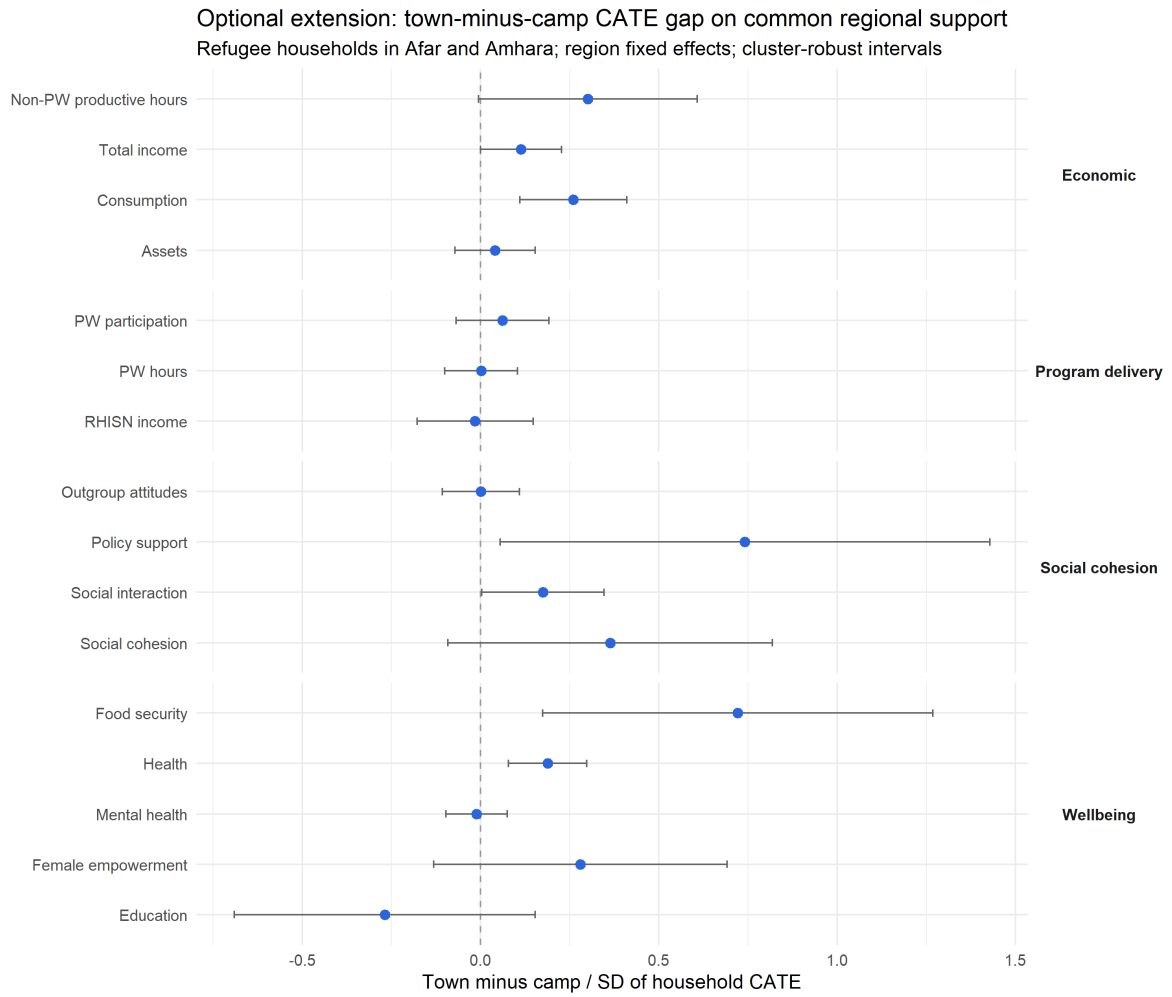


Figure D.5. Town-camp predicted CATE gaps restricted to Afar and Amhara. These two regions contain 282 of the 284 refugees living in towns and contain both settlement types, which imposes geographic common support on the comparison.

Table D.1. In-town versus in-camp refugees: well-being and social outcomes

Panel A. Well-being outcomes

			Treatment Effects	
Outcome	N	Mean	In-camp	Town diff.
(1) Food security index (std.)	5094	-0.042	0.06*	0.35***
(2) Health index (std.)	5094	-0.024	0.05*	-0.05
(3) Mental health index (std.)	5094	0.071	0.07*	0.19
(4) Female empowerment index (std.)	5093	-0.184	0.44***	-0.01
(5) Child health index (std.)	4141	-0.004	0.07*	0.10
(6) Child nutrition index (std.)	4141	-0.111	0.06	0.00
(7) SDQ scale (0-40, std.)	4134	-0.124	0.05	0.02
(8) Amenities index (std.)	5094	0.15	0.03	-0.15**

Panel B. Social outcomes

			Treatment Effects	
Outcome	N	Mean	In-camp	Town diff.
(1) Outgroup attitude index (std.)	5094	0.173	-0.02	-0.01
(2) Policy support index (std.)	5094	0.311	-0.04*	0.11
(3) Economic interaction index (std.)	5094	0.338	-0.01	0.13
(4) Social interaction index (std.)	5094	0.319	-0.05	0.06
(5) Social cohesion index (std.)	5094	-0.025	-0.01	0.11
(6) Political integration index (std.)	5094	0.013	0.08**	0.02
(7) Combined social index (std.)	5094	0.188	-0.01	0.07

Notes: The sample is refugee households. The "In-camp" column reports the program effect for refugees living in camps. The "Town diff." column reports the additional effect for refugees living in towns. Settlement type is not randomized, so these estimates describe effect modification rather than a causal effect of town residence. All indices have been redefined so that higher values reflect better outcomes and are standardized using the overall control group mean. The estimation includes camp fixed effects, controls for local saturation of treatment, and controls for baseline values where available. Standard errors are clustered at the neighborhood level. *** p<0.01, ** p<0.05, * p<0.1.

E Mechanism: Additional Supporting Evidence

Table D.2. In-town versus in-camp refugees: economic outcomes

Panel A. Labor supply

			Treatment Effects	
Outcome	N	Mean	In-camp	Town diff.
(1) Total productive hours	5094	19.5	4.27***	4.79
(2) Public works hours (RHISN)	5094	0.324	4.47***	2.41
(3) Productive hours (excl. public works)	5094	19.2	-0.207	2.38
(4) Wage employment hours	5094	4.89	-0.608	-0.279
(5) Hours in HH agriculture	5094	7.39	0.871**	0.979
(6) Hours in non-ag self-employment	5094	6.88	-0.412	0.401
(7) HH hourly wage	2135	64.8	15.6**	16.6
(8) HH hourly wage (excl. public works)	720	60.2	6.72	-20.4
(9) Job search hours	5094	2.65	-0.488***	-0.91*
(10) **1**(Unemployment)	5094	0.543	-0.0411**	-0.0583

Panel B. Income

			Treatment Effects	
Outcome	N	Mean	In-camp	Town diff.
(1) Total income (USD PPP, annualized)	5094	2,517	563***	261
(2) RHISN public works income	5094	35.8	468***	226
(3) RHISN other benefits	5094	14.3	139***	-7.05
(4) Total income (excl. RHISN)	5094	2,467	-47.6	46.1
(5) Wage employment income (excl. RHISN)	5094	299	-36.8	-18
(6) Agriculture & livestock income	5094	90	-3.11	-36
(7) Non-ag enterprise profits	5094	201	-12.1	77.5
(8) Aid and transfers (excl. RHISN)	5094	1,696	35.7	-20.2
(9) Net remittances	5094	181	-29.9**	35.2
(10) Rental income	5094	1.71	1.25	-3.84

Panel C. Assets

			Treatment Effects	
Outcome	N	Mean	In-camp	Town diff.
(1) Total assets, excl. land/housing (USD PPP)	5094	818	174***	115
(2) Livestock and agricultural assets	5094	214	34.9**	-83**
(3) Non-ag enterprise assets	5094	13.7	2.87	15.4
(4) Household durables	5094	613	-5.79	130
(5) Net financial savings	5094	-22.8	141***	62.8**
(6) Housing value	5094	6.22	0.428	0.00933
(7) Land value	3412	17	1.12	0.121

Panel D. Consumption

			Treatment Effects	
Outcome	N	Mean	In-camp	Town diff.
(1) Total consumption (USD PPP, annualized)	5094	5,617	234***	-33.3
(2) Food consumption	5094	4,733	197***	-98.4
(3) Consumption of NGO-provided	5094	1,816	73.3**	-268
(4) Consumption of own-production	5094	181	30.9	-15.8
(5) Food cash expenditure	5094	2,714	98.7*	186
(6) Other food consumption	5094	21.6	-5.25**	-1.98
(7) Non-food consumption	5094	884	41.9	65.7
(8) Durables	5094	367	16.1	40.4
(9) Education	5073	10.3	-0.521	0.654
(10) Health	5067	47.3	-0.734	0.804
(11) Non-food non-durables	5094	496	23.6*	19.1
(12) Other	5094	20.9	1.38	16.1**

Notes: See Table D.1. Monetary outcomes are reported in real USD PPP and winsorized at 98%.

Table D.3. Nuer versus Anywaa refugees in Pugnido: well-being and social outcomes

Panel A. Well-being outcomes

			Treatment Effects	
Outcome	N	Mean	Nuer	Anywaa diff.
(1) Food security index (std.)	1140	0.026	0.01	0.20
(2) Health index (std.)	1140	-0.213	0.06	0.08
(3) Mental health index (std.)	1140	0.034	0.07	0.30***
(4) Female empowerment index (std.)	1140	-0.444	0.41***	0.22*
(5) Child health index (std.)	1019	0.361	-0.03	0.10
(6) Child nutrition index (std.)	1019	-0.154	0.09	-0.03
(7) SDQ scale (0-40, std.)	1019	-0.033	0.05	-0.03
(8) Amenities index (std.)	1140	0.46	0.10	-0.31**

Panel B. Social outcomes

			Treatment Effects	
Outcome	N	Mean	Nuer	Anywaa diff.
(1) Outgroup attitude index (std.)	1140	-0.003	0.01	0.01
(2) Policy support index (std.)	1140	0.021	0.07	-0.17**
(3) Economic interaction index (std.)	1140	0.372	-0.03	0.25**
(4) Social interaction index (std.)	1140	-0.129	-0.03	-0.03
(5) Social cohesion index (std.)	1140	0.102	-0.04	-0.07
(6) Political integration index (std.)	1140	0.183	0.18**	-0.04
(7) Combined social index (std.)	1140	0.091	0.03	-0.01

Notes: The sample is refugee households in Pugnido. The "Nuer" column reports the program effect for Nuer refugees. The "Anywaa diff." column reports the additional effect for Anywaa refugees, who share language and kinship ties with the surrounding host population. Ethnicity is not randomized, so these estimates describe effect modification. Other conventions follow Table D.1.

Table D.4. Nuer versus Anywaa refugees in Pugnido: economic outcomes

Panel A. Labor supply

			Treatment Effects	
Outcome	N	Mean	Nuer	Anywaa diff.
(1) Total productive hours	1140	12.4	2.42*	0.0135
(2) Public works hours (RHISN)	1140	0.342	1.76***	1.17*
(3) Productive hours (excl. public works)	1140	12.1	0.646	-1.15
(4) Wage employment hours	1140	1.36	0.575	1.17***
(5) Hours in HH agriculture	1140	4.73	-0.333	0.903
(6) Hours in non-ag self-employment	1140	6	0.375	-3.58
(7) HH hourly wage	303	73.9	49.5*	-30.3
(8) HH hourly wage (excl. public works)	137	43	74.2***	-40.8
(9) Job search hours	1140	3.58	-0.515	0.564
(10) **1**(Unemployment)	1140	0.694	-0.0403	0.0845

Panel B. Income

			Treatment Effects	
Outcome	N	Mean	Nuer	Anywaa diff.
(1) Total income (USD PPP, annualized)	1140	2,143	376***	275
(2) RHISN public works income	1140	46.4	215***	4.91
(3) RHISN other benefits	1140	23.1	74.6***	-74.6***
(4) Total income (excl. RHISN)	1140	2,073	85	330
(5) Wage employment income (excl. RHISN)	1140	68.5	107**	79.5
(6) Agriculture & livestock income	1140	76.4	17	71.9***
(7) Non-ag enterprise profits	1140	316	-41.1	94.5
(8) Aid and transfers (excl. RHISN)	1140	1,524	29.9	-34.9
(9) Net remittances	1140	81.4	-36.1*	83***
(10) Rental income	1140	6.52	5.7	-5.7

Panel C. Assets

			Treatment Effects	
Outcome	N	Mean	Nuer	Anywaa diff.
(1) Total assets, excl. land/housing (USD PPP)	1140	657	127**	94.4
(2) Livestock and agricultural assets	1140	99.3	22.1	-6.96
(3) Non-ag enterprise assets	1140	15	-1.44	27.7*
(4) Household durables	1140	531	11.7	49.3
(5) Net financial savings	1140	10.9	96.8***	20.6
(6) Housing value	1140	18	1.89	0.0449
(7) Land value	839	31.4	8.35***	16.3

Panel D. Consumption

			Treatment Effects	
Outcome	N	Mean	Nuer	Anywaa diff.
(1) Total consumption (USD PPP, annualized)	1140	5,631	400*	-146
(2) Food consumption	1140	4,868	367**	-168
(3) Consumption of NGO-provided	1140	2,378	97.4	-35.2
(4) Consumption of own-production	1140	339	106*	-46.7
(5) Food cash expenditure	1140	2,140	172	-121
(6) Other food consumption	1140	10.5	-6.26	9.76*
(7) Non-food consumption	1140	763	37.1	2.72
(8) Durables	1140	411	-19.1	52.8
(9) Education	1133	7.68	0.369	3.47
(10) Health	1131	29.7	-3.76	-14.4
(11) Non-food non-durables	1140	328	44.5**	-30.9
(12) Other	1140	23.3	11.9*	2.91

Notes: See Table D.3. Monetary outcomes are reported in real USD PPP and winsorized at 98%.

Table E.1. Regional TOT Effects: RHISN Program Compliance

		Overall		Region-Wise Effects			
Outcome	N	No PW Mean	PW Part.	Asayita	Dabat	Bambasi	Pugnido
(1) **1**(HH participates in livelihood training)	11426	0.045	0.823***	0.739***	0.927***	0.837***	0.757***
			(0.0177)	(0.0443)	(0.0133)	(0.028)	(0.0432)
(2) Weekly PW Eligible Hours (HH)	11418	3.2	17.5***	17.2***	13.5***	18.6***	24.7***
			(0.559)	(0.931)	(0.258)	(0.734)	(1.47)
(3) Weekly public work hours (intended beneficiary)	10097	0.083	6.56***	4.52***	11***	4.09***	3.87***
			(0.439)	(0.595)	(0.595)	(0.403)	(0.439)
(4) Weekly public work hours (other HH members)	11426	0.081	0.866***	0.526**	1.31***	0.753***	0.737**
			(0.132)	(0.204)	(0.267)	(0.199)	(0.3)
(5) Eligible RHISN income (USD PPP, annualized)	11418	320	1,755***	1,722***	1,348***	1,864***	2,475***
			(56)	(93.1)	(25.8)	(73.5)	(147)
(6) RHISN income (USD PPP, annualized)	11426	9	709***	595***	1,138***	487***	323***
			(40.8)	(81.3)	(25.2)	(53.8)	(35.5)
(7) **1**(Aware of RHISN)	1106	0.475	0.468***	0.523***	0.592***	0.371***	0.266**
			(0.0411)	(0.0616)	(0.0534)	(0.0802)	(0.129)

Notes: The table reports the treatment effect on beneficiaries enrolled in the RHISN program. Rows (1), (2), (3) and (9) are dummy variables indicating realized treatment (rather than assignment), participation in public works in the past year, participation in the RHISN livelihood training in the past year, and having heard of the RHISN program, respectively. Rows (4-6) are defined as the amount of weekly public work hours the household is eligible for, and the actual intended beneficiary and household weekly labor supply in public works. Rows (7-8) are defined as the amount of public work income the household is eligible for and the annual household employment income from public works. They are reported in real USD PPP and winsorized at 98%. The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets, and square brackets report q-values adjusted for False Discovery Rate. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table E.2. Summary Statistics in Low Saturation Areas in the Control Group: Social Outcomes

			HH Type		Asayita		Dabat		Bambasi		Pugnido	
Outcome	N	Overall	Refugee	Host	Refugee	Host	Refugee	Host	Refugee	Host	Refugee	Host
(1) Outgroup attitude index (std.)	3244	0.0291	0.185	-0.119	0.433	-0.228	0.267	0.421	-0.11	-0.893	-0.00309	-0.0468
		(0.987)	(0.82)	(1.1)	(0.77)	(0.938)	(0.793)	(0.943)	(0.749)	(1.24)	(0.847)	(1)
(2) Policy support index (std.)	3244	0.00337	0.299	-0.277	0.448	-0.0901	0.419	0.361	0.256	-0.872	0.0205	-0.631
		(1.01)	(0.712)	(1.16)	(0.611)	(0.922)	(0.626)	(0.77)	(0.661)	(1.38)	(0.856)	(1.19)
(3) Economic interaction index (std.)	3244	0.0594	0.369	-0.234	0.769	-0.48	-0.181	-0.263	0.143	-0.266	0.372	0.0109
		(1.06)	(1.16)	(0.862)	(1.18)	(0.358)	(0.615)	(0.631)	(0.95)	(0.799)	(1.36)	(1.22)
(4) Social interaction index (std.)	3244	0.0744	0.398	-0.233	1.21	-0.475	0.0669	0.0448	-0.126	-0.492	-0.129	-0.117
		(1.07)	(1.32)	(0.612)	(1.56)	(0.467)	(0.789)	(0.656)	(0.595)	(0.454)	(1.08)	(0.625)
(5) Social cohesion index (std.)	3244	-0.00547	-0.0159	0.00442	0.0369	0.129	-0.273	0.41	-0.0179	-0.58	0.102	-0.11
		(0.993)	(0.902)	(1.07)	(0.9)	(0.973)	(1.09)	(1.18)	(0.75)	(1.13)	(0.824)	(0.842)
(6) Combined social index (std.)	3244	0.0278	0.207	-0.143	0.478	-0.216	0.0444	0.186	0.00717	-0.585	0.0908	-0.109
		(0.602)	(0.534)	(0.614)	(0.538)	(0.469)	(0.428)	(0.527)	(0.404)	(0.637)	(0.531)	(0.608)

Notes: The table reports summary statistics for beneficiaries (T) enrolled in the RHISN program and the control group (C). Rows (1)-(6) are midline values. The outcomes in rows (7)-(12) were only measured at endline. Row (1) indicates whether the respondent has heard of the RHISN program. Row (2) shows the extent to which the respondent agrees that RHISN public work activities benefit local communities (scale 1-4). Row (3) captures satisfaction with RHISN (scale 1-5). Row (4) captures respondents' opinion on whether RHISN should be a mixed or a separate program, i.e. whether refugees and hosts should work in the same or different groups. Rows (5)-(6) indicate whether the respondent thinks that refugees should receive more humanitarian/government spending. Rows (7)-(9) indicate whether the respondent thinks they know who manages and pays for RHISN (Ministry of Urban and Infrastructure, Refugees and Returnees Service, UNHCR, The World Bank or other) and how RHISN selects beneficiaries. Rows (10)-(12) capture the respondent's agreement with these statements (scale 1-5).

E.A Reciprocity Channel

Table E.3. Information experiment: first stage

		Control Mean		Overall TE:	Information: Overall		Information: Refugee		Information: Hosts	
Outcome	N	Refugee	Hosts	Benefic	Benefic	Control	Benefic	Control	Benefic	Control
(1) **1**(Aware of RHISN)	1106	0.492	0.518	0.348***	-0.0347	-0.00847	-0.0414	0.0191	-0.0293	-0.0326
				(0.0467)	(0.0275)	(0.0522)	(0.039)	(0.0818)	(0.0397)	(0.0679)
(2) RHISN benefits local community (1-4, std.)	798	3.54	3.53	0.0301	0.000	-0.13*	0.0479	-0.189	-0.0397	-0.0817
				(0.0559)	(0.0439)	(0.0773)	(0.0637)	(0.129)	(0.0601)	(0.0951)
(3) RHISN satisfaction	570	4.33	4.52	-0.028	0.0482	0.0714	0.016	-0.197	0.0719	0.143
				(0.162)	(0.067)	(0.27)	(0.0736)	(0.659)	(0.107)	(0.243)
(4) **1**(RHISN should be mixed)	798	0.778	0.763	0.0642	-0.00657	0.0456	-0.0138	-0.0542	0.000533	0.118
				(0.0479)	(0.0309)	(0.0574)	(0.0466)	(0.0803)	(0.0419)	(0.0759)
(5) Humanitarian spending (likert, 1-5)	11426	4.72	4.47	0.0109	-0.0037	0.0287	0.0197	0.0162	-0.0222	0.0413
				(0.0205)	(0.016)	(0.0222)	(0.0185)	(0.0296)	(0.0247)	(0.0326)
(6) Government spending (likert, 1-5)	11426	4.57	4.1	0.0544**	-0.0357*	0.0254	-0.00064	0.035	-0.0638*	0.0199
				(0.0268)	(0.0214)	(0.0247)	(0.022)	(0.0313)	(0.0341)	(0.0365)
(7) Know who manages RHISN (=1)	655	0.619	0.759	0.0535	-0.0453	-0.142***	-0.0543	-0.157***	0.00921	-0.0495
				(0.0365)	(0.0324)	(0.046)	(0.0365)	(0.0492)	(0.0837)	(0.161)
(8) Know who paid for RHISN (=1)	655	0.649	0.759	0.0561	-0.0257	-0.137**	-0.0243	-0.171***	-0.0399	0.0841
				(0.0578)	(0.0321)	(0.0611)	(0.0344)	(0.0543)	(0.078)	(0.16)
(9) Know how RHISN selects benef. (=1)	456	0.764	0.897	0.0176	0.00687	-0.02	-0.00468	-0.0449	0.0697*	0.118
				(0.0429)	(0.039)	(0.0647)	(0.0446)	(0.0732)	(0.0407)	(0.117)
(10) RHISN presence due to refugees (likert, 1-5)	456	3.29	3.69	0.15	0.027	0.133	0.0524	0.175	-0.0341	-0.0727
				(0.124)	(0.107)	(0.141)	(0.121)	(0.143)	(0.193)	(0.418)
(11) Refugee gains at locals' expense (likert, 1-5)	456	2.37	2.24	-0.156	0.0938	-0.0715	0.0681	-0.0948	0.228	0.0207
				(0.0969)	(0.0776)	(0.141)	(0.0827)	(0.158)	(0.197)	(0.25)
(12) Refugee aid reduces local resources (likert, 1-5)	456	2.31	2.1	0.0264	-0.00681	0.035	0.0181	-0.0247	-0.121	0.304*
				(0.0818)	(0.0986)	(0.0941)	(0.105)	(0.117)	(0.188)	(0.161)

Notes: The table reports the treatment effect on beneficiaries enrolled in the RHISN program. The outcomes in rows (7)-(12) were only measured at endline. Row (1) indicates whether the respondent has heard of the RHISN program. Row (2) shows the extent to which the respondent agrees that RHISN public work activities benefit local communities (scale 1-4). Row (3) captures satisfaction with RHISN (scale 1-5). Row (4) captures respondents' opinion on whether RHISN should be a mixed or a separate program, i.e. whether refugees and hosts should work in the same or different groups. Rows (5)-(6) indicate whether the respondent thinks that refugees should receive more humanitarian/government spending. Rows (7)-(9) indicate whether the respondent thinks they know who manages and pays for RHISN (Ministry of Urban and Infrastructure, Refugees and Returnees Service, UNHCR, The World Bank or other) and how RHISN selects beneficiaries. Rows (10)-(12) capture the respondent's agreement with these statements (scale 1-5).

Table E.4. Results from Information Intervention about RHISN

		Control Mean		Overall TE:	Information: Overall		Information: Refugee		Information: Hosts	
Outcome	N	Refugee	Hosts	Benefic	Benefic	Control	Benefic	Control	Benefic	Control
(1) RHISN benefits local community (1-4, std.)	798	3.54	3.53	0.0301	0.000	-0.13*	0.0479	-0.189	-0.0397	-0.0817
				(0.0559)	(0.0439)	(0.0773)	(0.0637)	(0.129)	(0.0601)	(0.0951)
(2) RHISN satisfaction	570	4.33	4.52	-0.028	0.0482	0.0714	0.016	-0.197	0.0719	0.143
				(0.162)	(0.067)	(0.27)	(0.0736)	(0.659)	(0.107)	(0.243)
(3) **1**(RHISN should be mixed)	798	0.778	0.763	0.0642	-0.00657	0.0456	-0.0138	-0.0542	0.000533	0.118
				(0.0479)	(0.0309)	(0.0574)	(0.0466)	(0.0803)	(0.0419)	(0.0759)
(4) Outgroup attitude index (std.)	11426	0.173	-0.14	0.04	0.01	-0.02	0.02	-0.03	0.01	-0.01
				(0.03)	(0.02)	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)
(5) Policy support index (std.)	11426	0.311	-0.252	0.06**	-0.03	0.03	-0.00	0.01	-0.05	0.05
				(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.04)
(6) Political integration index (std.)	11426	0.013	-0.011	0.09***	-0.00	-0.05*	0.02	-0.03	-0.02	-0.06
				(0.03)	(0.02)	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)

Notes: The table reports the treatment effect on beneficiaries enrolled in the RHISN program. Row (1) shows the extent to which the respondent agrees that RHISN public work activities benefit local communities (scale 1-5). Row (2) captures satisfaction with RHISN (scale 1-5). Row (3) captures respondents' opinion on whether RHISN should be a mixed or a separate program, i.e. whether refugees and hosts should work in the same or different groups. All index components have been redefined so that they move in the same direction, with higher values reflecting better outcomes. All indices have been standardized using the overall control group mean. The indices are constructed as follows. Outgroup attitude: comfort and trust in everyday interactions with the outgroup, including living proximity, working relations, children's contact, and perceived community contributions. Policy attitudes: support for refugee inclusion policies, including citizenship, freedom of movement, work permits, and public spending on refugees. Political integration: civic and political engagement, including identity document ownership, community leadership, civic participation, and trust in local government. The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table E.5. Results from Information Intervention about RHISN (cont'd)

		Control Mean		Overall TE:	Information: Overall		Information: Refugee		Information: Hosts	
Outcome	N	Refugee	Hosts	Benefic	Benefic	Control	Benefic	Control	Benefic	Control
(1) RHISN benefits local community (1-4, std.)	798	3.54	3.53	0.0301	0.000	-0.13*	0.0479	-0.189	-0.0397	-0.0817
				(0.0559)	(0.0439)	(0.0773)	(0.0637)	(0.129)	(0.0601)	(0.0951)
(2) RHISN satisfaction	570	4.33	4.52	-0.028	0.0482	0.0714	0.016	-0.197	0.0719	0.143
				(0.162)	(0.067)	(0.27)	(0.0736)	(0.659)	(0.107)	(0.243)
(3) **1**(RHISN should be mixed)	798	0.778	0.763	0.0642	-0.00657	0.0456	-0.0138	-0.0542	0.000533	0.118
				(0.0479)	(0.0309)	(0.0574)	(0.0466)	(0.0803)	(0.0419)	(0.0759)
(4) Outgroup attitude index (std.)	11426	0.173	-0.14	0.04	0.01	-0.02	0.02	-0.03	0.01	-0.01
				(0.03)	(0.02)	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)
(5) Policy support index (std.)	11426	0.311	-0.252	0.06**	-0.03	0.03	-0.00	0.01	-0.05	0.05
				(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.04)
(6) Political integration index (std.)	11426	0.013	-0.011	0.09***	-0.00	-0.05*	0.02	-0.03	-0.02	-0.06
				(0.03)	(0.02)	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)

Notes: The table reports the treatment effect on beneficiaries enrolled in the RHISN program. Row (1) shows the extent to which the respondent agrees that RHISN public work activities benefit local communities (scale 1-5). Row (2) captures satisfaction with RHISN (scale 1-5). Row (3) captures respondents' opinion on whether RHISN should be a mixed or a separate program, i.e. whether refugees and hosts should work in the same or different groups. All index components have been redefined so that they move in the same direction, with higher values reflecting better outcomes. All indices have been standardized using the overall control group mean. The indices are constructed as follows. **OA** (Outgroup Attitude): comfort and trust in everyday interactions with the outgroup, including living proximity, working relations, children's contact, and perceived community contributions. **PA** (Policy Attitudes): support for refugee inclusion policies, including citizenship, freedom of movement, work permits, and public spending on refugees. **PI** (Political Integration): civic and political engagement, including identity document ownership, community leadership, civic participation, and trust in local government. Combined social index is constructed as the average of the indices in rows (1)–(6). The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

E.B Income Effects

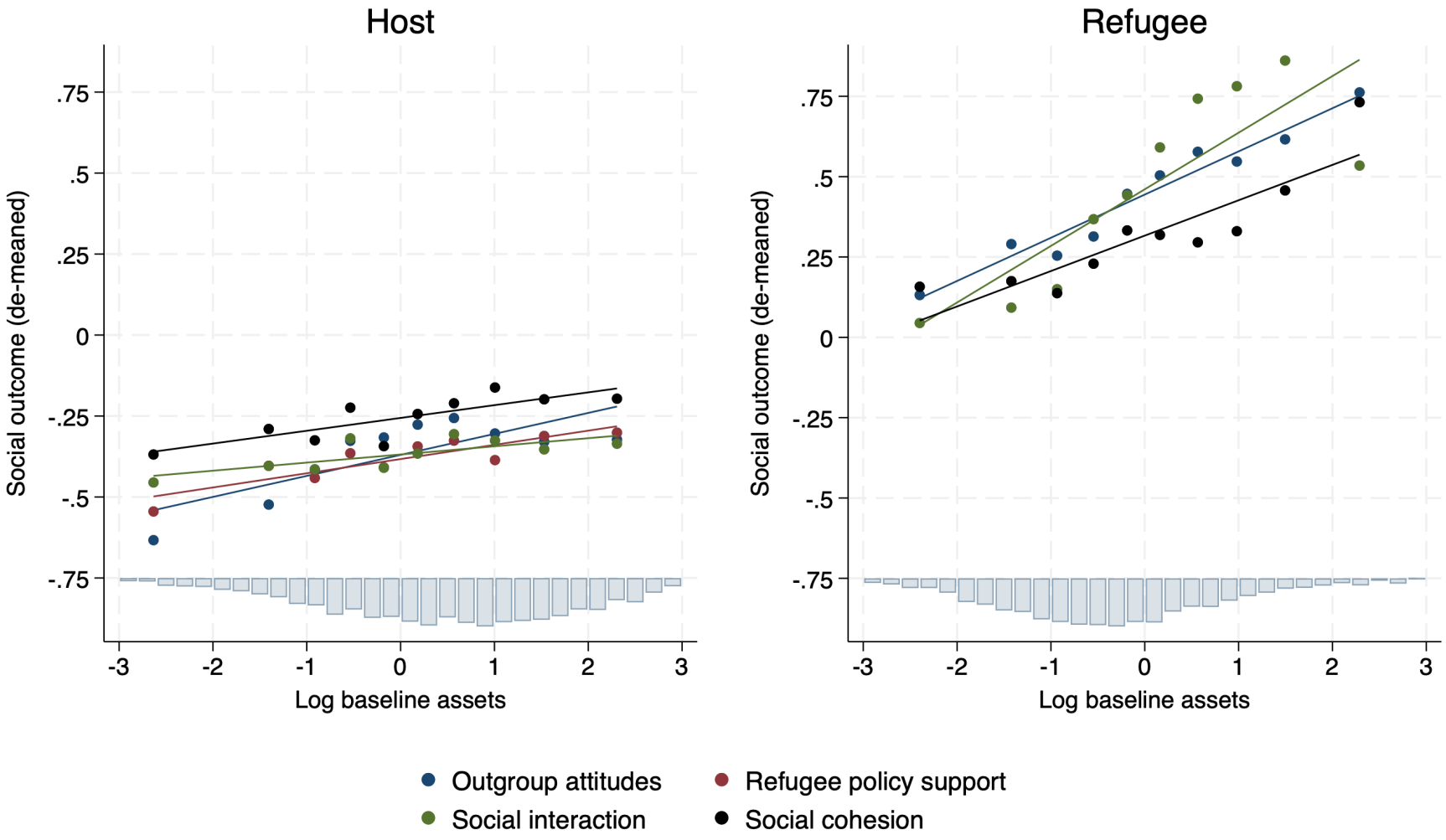


Figure E.1. Baseline economic status and social outcomes. The figure plots de-meaned social outcome indices against log baseline assets separately for host and refugee households, with fitted linear relationships and marginal histograms. The sample covers the three regions where baseline data were collected and excludes Dabat, where conflict prevented baseline collection.

E.C Network Effects

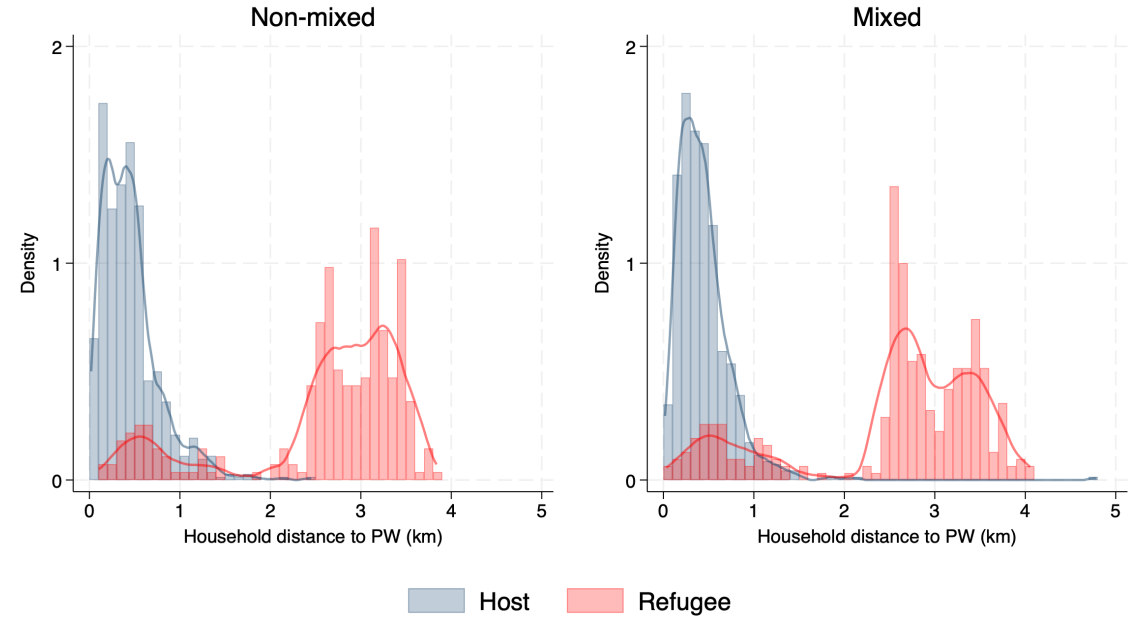
Table E.6. Network Characteristics

	Asayita		Bambasi		Pugnido		Dabat	
Network characteristic	Host	Refugee	Host	Refugee	Host	Refugee	Host	Refugee
Number of contacts per HH	3.57	3.23	3.01	2.82	3.14	3.06	4.08	3.47
Network type								
Close relative	48.96	60.59	49.68	50.37	42.87	39.82	45.74	46.45
Close friend	31.34	26.02	29.28	33.20	32.63	30.80	28.11	28.36
Good friend	15.01	10.84	12.86	9.13	17.89	17.59	17.40	19.30
Economic relationship	4.68	2.55	8.19	7.30	6.62	11.78	8.75	5.88
Contact characteristics								
From the outgroup	0.40	24.11	0.22	0.35	0.81	13.90	0.48	4.03
Same sex	78.04	74.86	77.26	73.21	82.25	86.10	79.17	76.21
Location of contact								
Same town/camp	72.82	48.91	89.39	97.73	94.57	89.26	.	.
Same neighborhood	55.20	37.32	67.73	86.53	75.98	61.21	.	.
Different neighborhood	17.62	11.59	21.65	11.20	18.59	28.04	.	.
Adjacent town/camp	0.18	36.36	0.26	0.59	1.21	5.52	.	.
Elswhere	27.00	14.89	10.35	1.68	4.22	5.22	.	.
Ethiopia	26.50	10.15	10.26	0.84	3.82	2.51	.	.
Other	0.51	4.74	0.09	0.84	0.40	2.71	.	.
Know contact from								
Family	49.95	62.59	54.14	53.82	51.75	45.15	49.76	47.27
Current/previous neighbour	16.33	17.40	22.17	24.96	14.09	26.56	18.71	22.73
Work	8.95	2.84	10.52	5.53	2.61	3.99	13.67	5.58
Community group	12.43	7.86	3.12	5.62	13.50	10.84	3.60	3.94
RHISN	.	.	.	.	.	.	1.52	1.33
Other	12.33	9.31	10.05	10.06	18.05	13.45	12.74	19.15

Notes: The table reports the baseline network characteristics for Asayita, Bambasi and Pugnido and the midline network characteristics for Dabat, since Dabat was not surveyed at baseline.

		Control Mean		Overall TE:	Spillover: Overall			Spillover: Refugee		
Outcome	N	Refugee	Hosts	Benefic	Overall	Benefic	Control	Overall	Benefic	Control
(1) Outgroup attitude index (std.)	4964	0.406	0.204	0.09**	0.15*	0.15*	0.13	0.01	0.00	0.02
				(0.04)	(0.08)	(0.09)	(0.09)	(0.08)	(0.10)	(0.08)
(2) Policy support index (std.)	4964	0.407	0.179	0.04	0.03	0.03	0.00	-0.05	-0.08	0.01
				(0.03)	(0.05)	(0.05)	(0.06)	(0.08)	(0.09)	(0.10)
(3) Economic interaction index (std.)	4964	0.457	-0.353	-0.05*	0.07	0.12*	-0.03	0.17**	0.32**	-0.12
				(0.03)	(0.04)	(0.06)	(0.08)	(0.07)	(0.14)	(0.11)
(4) Social interaction index (std.)	4964	0.981	-0.143	0.05*	0.06	0.07	0.05	-0.03	-0.02	-0.04
				(0.03)	(0.04)	(0.05)	(0.06)	(0.10)	(0.10)	(0.13)
(5) Social cohesion index (std.)	4964	-0.159	0.272	0.12***	0.13	0.12	0.14	0.09	0.04	0.18
				(0.04)	(0.13)	(0.14)	(0.13)	(0.13)	(0.12)	(0.22)
(6) Political integration index (std.)	4964	-0.071	0.035	0.10**	0.10	0.08	0.12	-0.01	0.00	-0.05
				(0.04)	(0.07)	(0.07)	(0.09)	(0.15)	(0.16)	(0.16)
(7) Combined social index (std.)	4964	0.337	0.032	0.06**	0.09**	0.10**	0.07	0.03	0.04	-0.00
				(0.03)	(0.04)	(0.04)	(0.05)	(0.06)	(0.05)	(0.08)

Notes: The table reports the effect of treatment and mixing on beneficiaries enrolled in the RHISN program in Amhara (Dabat) and parts of Afar (Asayita), where mixing was implemented properly. Estimates use double and debiased machine learning. All index components have been redefined so that they move in the same direction, with higher values reflecting better outcomes. All indices have been standardized using the overall control group mean. The indices are constructed as follows. **OA** (Outgroup Attitude): comfort and trust in everyday interactions with the outgroup, including living proximity, working relations, children's contact, and perceived community contributions. **PA** (Policy Attitudes): support for refugee inclusion policies, including citizenship, freedom of movement, work permits, and public spending on refugees. **EI** (Economic Interaction): market-based interactions with the outgroup, including sales and working together. **SI** (Social Interaction): social ties with the outgroup, including friendships, community group participation, shared activities, and children's play. **SC** (Social Cohesion): trust and pro-social behavior toward the outgroup, including interpersonal trust, charitable donations, perceived integration, and national identity. **PI** (Political Integration): civic and political engagement, including identity document ownership, community leadership, civic participation, and trust in local government. Combined social index is constructed as the average of the indices in rows (1)–(6). The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.



Mixed difference: Refugees = 0.034 km (p = 0.638); Hosts = 0.006 km (p = 0.747)

Figure V.2. Distance to Public Work Groups

Table V.7. Spillover Effects of RHISN by household type: Outgroup Attitudes

		Control	Overall	Mixing Effects			Test	HighMix Spillover		
Outcome	N	Mean	Benefic (β_1)	Overall (δ_1)	Refugee ($\delta_{1,R}$)	Host ($\delta_{1,H}$)	$\delta_{1,R} = \delta_{1,H}$	Overall (δ_2)	Benefic (ϕ_3)	Control (ϕ_2)
(1) Outgroup attitude index (std.)	4703	0.277	0.10**	0.07*	0.02	0.11**	0.272	0.14*	0.15*	0.12
			(0.04)	(0.04)	(0.06)	(0.05)		(0.08)	(0.09)	(0.09)
			[0.067]	[0.262]	[1.000]	[0.199]	[1.000]	[0.482]	[0.366]	[1.000]
(2) Outgroup attitude index alt. (std.)	4703	0.283	0.07*	0.05	0.00	0.09*	0.203	0.14	0.16*	0.10
			(0.04)	(0.04)	(0.05)	(0.05)		(0.09)	(0.09)	(0.10)
			[0.219]	[0.265]	[1.000]	[0.212]	[1.000]	[0.482]	[0.366]	[1.000]
(3) Job rank diff.	482	-0.139	0.0184	-0.108	0.0197	-0.14	0.469	0.104	0.101	0.108
			(0.133)	(0.116)	(0.172)	(0.141)		(0.146)	(0.174)	(0.229)
			[0.466]	[0.321]	[1.000]	[0.245]	[1.000]	[0.654]	[0.653]	[1.000]
(4) Neighbors comfort (likert, 1-5)	4703	4.28	0.0388	0.00836	-0.0427	0.0348	0.178	0.0787	0.0762	0.0836
			(0.0379)	(0.0351)	(0.0308)	(0.0482)		(0.0713)	(0.0731)	(0.0874)
			[0.297]	[0.436]	[1.000]	[0.297]	[1.000]	[0.539]	[0.434]	[1.000]
(5) Offspring marriage (likert, 1-5)	4703	3.74	0.0794*	0.0109	-0.106	0.082	0.074	0.149	0.189	0.0712
			(0.0469)	(0.0573)	(0.0841)	(0.0618)		(0.115)	(0.117)	(0.145)
			[0.219]	[0.436]	[1.000]	[0.234]	[1.000]	[0.539]	[0.366]	[1.000]
(6) Dinner together (freq., 1-5)	4703	2.39	0.185***	0.0229	-0.0651	0.0774	0.285	0.154	0.172	0.118
			(0.0659)	(0.0672)	(0.122)	(0.0566)		(0.113)	(0.127)	(0.143)
			[0.056]	[0.436]	[1.000]	[0.234]	[1.000]	[0.539]	[0.366]	[1.000]
(7) Returning valuables (likert, 1-4)	4703	2.59	0.0419	0.0387	0.0263	0.0464	0.676	0.0935	0.116*	0.0488
			(0.041)	(0.0255)	(0.0331)	(0.0354)		(0.0597)	(0.0646)	(0.0727)
			[0.297]	[0.265]	[1.000]	[0.234]	[1.000]	[0.482]	[0.366]	[1.000]
(8) Emotional support (likert, 1-4)	4703	2.88	0.042	0.06*	0.0381	0.0748*	0.516	0.111	0.147**	0.0398
			(0.0287)	(0.03)	(0.0389)	(0.0414)		(0.068)	(0.0727)	(0.0742)
			[0.262]	[0.242]	[1.000]	[0.212]	[1.000]	[0.482]	[0.366]	[1.000]
(9) Work with (likert, 1-5)	4703	4.24	0.0746*	0.117***	0.129**	0.123***	0.937	0.0548	0.036	0.0916
			(0.0399)	(0.034)	(0.0603)	(0.0407)		(0.0607)	(0.0617)	(0.0876)
			[0.219]	[0.018]	[1.000]	[0.079]	[1.000]	[0.539]	[0.653]	[1.000]
(10) Work under supervision (likert, 1-5)	4703	4.1	0.066	0.0815*	0.0892	0.0846*	0.962	0.0669	0.0322	0.135*
			(0.0412)	(0.0421)	(0.0867)	(0.0476)		(0.075)	(0.0807)	(0.0798)
			[0.225]	[0.242]	[1.000]	[0.212]	[1.000]	[0.539]	[0.736]	[1.000]
(11) Local police (likert, 1-5)	4703	3.98	0.0301	0.0645	-0.0106	0.114**	0.15	0.00482	-0.0145	0.0428
			(0.0474)	(0.0422)	(0.0663)	(0.0551)		(0.0889)	(0.1)	(0.0998)
			[0.359]	[0.265]	[1.000]	[0.212]	[1.000]	[0.918]	[0.911]	[1.000]
(12) Same school (likert, 1-5)	4703	4.55	0.00538	0.06**	0.0712*	0.058	0.804	-0.0067	-0.0157	0.0109
			(0.0292)	(0.0269)	(0.0391)	(0.0364)		(0.0451)	(0.0446)	(0.0585)
			[0.466]	[0.242]	[1.000]	[0.234]	[1.000]	[0.918]	[0.736]	[1.000]
(13) Refugee effect in Ethiopia (likert, 1-3)	4703	2.74	0.0496***	-0.0264	-0.0277	-0.0235	0.901	0.0875**	0.0833*	0.0956**
			(0.0173)	(0.018)	(0.0239)	(0.0241)		(0.037)	(0.042)	(0.0421)
			[0.056]	[0.265]	[1.000]	[0.245]	[1.000]	[0.472]	[0.366]	[0.663]
(14) Activities easy together (0-5)	1618	2.72	-0.0986	0.0587	0.0587	0.	0.	-0.0221	0.0171	-0.0944
			(0.0719)	(0.0804)	(0.0804)	(0.)		(0.333)	(0.354)	(0.324)
			[0.297]	[0.321]	[1.000]	[0.]	[0.]	[0.918]	[0.927]	[1.000]

Notes: The table reports the effect of treatment and mixing on beneficiaries enrolled in the RHISN program in Amhara (Dabat) and parts of Afar (Asayita), where mixing was implemented properly. The outcomes have been redefined so that they move in the same direction, with higher values reflecting better outcomes. Outgroup attitude index (1) is constructed as the average of rows (3-14) and is standardized using the overall control group mean. Outgroup attitude index alt. (2) is an alternative index that excludes rows (6), (9) and (10), i.e. possible mechanical RHISN-driven increases in positive attitudes towards the outgroup. Rows (4-5) and (9-12) are defined as the degree of comfort with i) being neighbors with, ii) offspring marry someone from, iii) working with, iv) working under supervision of, v) accepting as a local police officer, and vi) attendance at the same school of children from the ingroup with someone from the outgroup (Scale 1-5). Row (6) is a dummy variable indicating if the intended beneficiary had dinner/tea with someone from the outgroup in the past year. Rows (7-8) are defined as the extent to which the intended beneficiary agrees that someone from the outgroup would return something valuable that they left behind and that they could lean on someone from the outgroup during difficult times (Scale 1-4). Row (13) is defined as the opinion on the net effect of refugees in Ethiopia (Scale 1-3). Row (14) is defined as the sum of number of things that are easy to do between hosts & refugees out of market exchange, labor mobility, use of financial services, social participation and sharing local resources. The estimation includes camp fixed effects, control for local saturation of treatment, and control for baseline values where available. Standard errors are clustered at the neighborhood level. Standard errors are reported in round brackets. *** p<0.01, ** p<0.05, * p<0.1.