Can Redistribution Change Policy Views? Aid and Attitudes Toward Refugees*

Travis Baseler   Thomas Ginn   Robert Hakiza

Helidah Ogude-Chambert   Olivia Woldemikael

November 2024

Abstract

Many public policies create (perceived) winners and losers, but there is little evidence on whether redistribution can support new political economy equilibria that raise aggregate welfare. We study a Ugandan policy that redistributes 30% of foreign aid for refugees to Ugandans while allowing refugees to work and move freely. To test whether compensation influences support for refugee integration, we randomly distribute cash grants to natives which are explicitly labeled as aid shared from the refugee response. We find substantial impacts on policy preferences that persist for at least two years and work through changing beliefs about the economic effects of refugees on Ugandans. Sharing information about public goods funded by the refugee response but not providing a grant has smaller, though still significant, effects. In contrast, we find no persistent impacts of inter-group contact, implemented as business mentorship by an experienced refugee. We find consistent impacts of compensation in Kenya, where support for refugees is lower. Our results indicate that economic interventions can shape policy views even on issues greatly influenced by cultural concerns, such as immigration.

Keywords: Refugees, Immigration, Political Economy of Aid, Post-Conflict, Welfare
JEL Codes: D74, D83, I38, O12

---

*Baseler: University of Rochester, travis.baseler@rochester.edu. Ginn: Center for Global Development, tginn@cgdev.org. Hakiza: Young African Refugees for Integral Development, robert.hakiza@yarid.net. Ogude-Chambert: University of Oxford, helidah.ogude@qeh.ox.ac.uk. Woldemikael: The New School, woldemikael@newschool.edu. We appreciate comments from David Atkin, Samuel Bazzi, Simone Bertoli, Christopher Blair, Arun Chandrasekhar, Michael Clemens, Kevin Donovan, Pascaline Dupas, Dave Evans, Marcel Fafchamps, Fred Finan, Andre Groeger, Jens Hainmueller, Rebecca Hamlin, Horacio Larreguy, Francesco Loiacono, Mashail Malik, Anna Maria Mayda, Melanie Morten, Pia Raffler, Justin Sandefur, Julia Seither, Walter Steingress, Marco Tabellini, Jeremy Weinstein, Marc Witte, and seminar participants at NBER/BREAD, Stanford, Harvard, CU Denver, The World Bank, Georgetown, WGAPE, the Joint Data Center, MIEDC, NOVAfrica, APSA, ESOC, and the International Conference on Migration and Development. We are grateful to staff at YARID; International Research Consortium, especially Daniel Kibuuka Musoke, Aidah Nakitende, and Daniel Senjovu; Lukendo Mbokani Jerry at OneYouth OneHeart Initiative; and JeanPaul Bahikye at RELON Kenya. We thank Lipeng Chen, Hyejin Lim, Ande Shen, and Christopher Weibel for outstanding research and field assistance. We are grateful for funding that was provided by the Conrad N. Hilton Foundation, the IKEA Foundation, Stanford University, the UK Foreign, Commonwealth & Development Office (FCDO), awarded through Innovation for Poverty Action's Peace & Recovery Program, and the UK Government, awarded through the "Building the Evidence on Protracted Forced Displacement" program managed by the World Bank Group (WBG) and established in partnership with the United Nations High Commissioner for Refugees (UNHCR). This work does not necessarily reflect the views of the UK Government, FCDO, WBG, UNHCR, or any of the authors' affiliations or funders. This study was approved by the Institutional Review Boards at Stanford (#44743), Harvard (IRB19-2041), Rochester (#4098), the Uganda National Council for Science and Technology (SS 5014), and Mildmay Uganda (0504-2019). The AEA RCT registration numbers are 5229 and 13127.

1 Introduction

Policy changes that raise aggregate welfare—and in which winners could hypothetically compensate losers to make everyone better off—may be politically infeasible. Politicians may recognize the aggregate gains from immigration or trade, for example, but block additional visas or trade agreements over fears of job losses among their constituents. Redistribution from winners to losers could in theory generate the necessary political support.[1] However, this bargaining can break down in multiple ways: non-economic considerations such as group identity often shape voters' views, the costs of a policy may be more salient than the benefits, and compensation could crowd out other sources of support such as altruism.[2]

Allowing refugees—people who have fled their home country due to persecution or conflict—to work is another example of a policy likely to have aggregate benefits. As of 2023, more than 42 million people are refugees or asylum-seekers (UNHCR, 2024*a*), and over half live in countries with significant, government-imposed barriers to the labor market such as work bans or encampment (Ginn et al., 2022). These restrictions, motivated in part by concerns of crowding out natives (whom we also refer to as *hosts*, following humanitarian terminology), lead to lost income, worse mental health (Hussam et al., 2022), and skill atrophy (Brell, Dustmann and Preston, 2020). They also constrain aid: without labor market access, returns to development programs are limited, and funding is allocated to humanitarian programs designed for short-term support like food aid. Displacement, however, is often long-term, and development aid likely yields higher returns for both refugees and hosts in the long run.[3]

Citizens of countries that host refugees might prefer a different political economy bargain: allow refugees to access the labor market and redistribute some of the resulting foreign aid or public finance surplus to natives. The expected gains to refugees would be significant, while the effects on many hosts would likely be small (Bahar, Ibáñez and Rozo, 2021, Verme and Schuettler, 2021, Ginn, 2023). This framework is outlined in the UN's 2018 Global Compact on Refugees, but the scope for aid to generate support for integration is unknown.

We designed two programs to investigate whether redistributing aid increases support for policies that facilitate integration. We offered these programs to native micro-entrepreneurs in the capital city of Uganda, a country that hosts over one million refugees. Uganda

[1] Examples of redistributing policy gains include H-1B Skills Training Grants, which use visa fees to fund training for citizens, and Trade Adjustment Assistance, which retrains workers displaced by trade.

[2] Additional barriers include difficulty identifying winners, losers, and the potential surplus to bargain over (Fernandez and Rodrik, 1991), distortions in politicians' allocation decisions (Finan and Mazzocco, 2020), and time inconsistency due to the potential for transfers to be reduced after the policy is approved.

[3] Sixty-six percent of refugees live in protracted situations of at least five years (UNHCR, 2024*a*), while 34% of aid for refugee situations went to development programs in 2020–21 (OECD, 2023). Restrictive work policies limit the effects of aid: the return to skills, for instance, is higher when refugees can accept formal jobs (Schuettler and Caron, 2020).

is a leading example of an aid-sharing bargain: government policy stipulates that 30% of international refugee aid be shared with natives (we refer to this as an *aid-sharing policy*) and allows refugees to work and move freely. Awareness of the aid-sharing policy, however, was low at baseline. Our first program delivered information about Uganda's aid-sharing policy and its connection to integration policies along with a listening exercise in which respondents were invited to share their views of refugees. We refer to this arm as *Information Only*. Our second program augmented the information with a business grant of 135 USD—about 3.5 months of average profit—and explained that the grant is an example of compensation for Ugandans under the policy bargain. We refer to this arm as *Information + Labeled Grant*.

There is substantial evidence, however, that attitudes about immigration are largely driven by cultural—as opposed to economic—opposition (Hainmueller and Hopkins, 2014, Alesina and Tabellini, 2024).[4] We therefore designed a third program facilitating contact between Ugandans and refugees in the form of business mentorship pairings. This treatment tests a variant of the contact hypothesis, which is often applied in displacement settings (Loiacono and Silva-Vargas, 2023). It also serves as a benchmark, allowing us to compare our two economically motivated interventions with one thought to act on cultural concerns.

Our experiment included three additional comparison arms. First, we offered a business grant that was not bundled with information on Uganda's policy bargain to study the impacts of the transfer itself. Second, we provided mentorship by an experienced Ugandan to separate the impacts of contact with a refugee mentor from other aspects of mentorship. Finally, we included a pure control group which did not receive any treatment.

Ugandans, however, exhibit relatively high support for refugees, and the rollout of our experiment during COVID-19 may have affected how the grants were perceived. We conducted a second experiment in rural Kenya—where opposition to refugee integration is greater—in which we distributed smaller grants of about 7.50 USD during more typical economic conditions in 2024. Kenya does not implement a similar aid-sharing bargain, and refugees' labor market access is restricted. Labeled grant recipients in Kenya watched a video explaining that they received the grant because Kenya hosts refugees, and that future aid-sharing between refugees and Kenyans could be increased with more labor market access for refugees. We also implemented a grant arm with no reference to refugees and a control arm.

We find that labeled grants substantially increase Ugandans' support for admitting refugees and for policies that facilitate integration compared to the control group, with effects persisting for at least two years. Receiving information about Uganda's aid-sharing

[4] Hainmueller and Hopkins (2014) distinguish between individual economic concerns, like labor market competition, and "sociotropic" concerns, which include cultural concerns and group-level economic effects like industry-level impacts. They find the strongest evidence for cultural concerns, some evidence for sociotropic economic concerns, and little evidence for personal economic conditions in shaping immigration attitudes.

policy, but no grant, creates similar but smaller impacts. We also find large effects of labeled grants on support for refugee integration in Kenya. In contrast, we find minimal average impacts of mentorship, either by a refugee or a Ugandan, despite high uptake of both programs. Mirroring impacts on policy views, we find that the Information + Labeled Grant and Information Only arms—but not mentorship—have persistent impacts on Ugandans' beliefs about the economic benefits of hosting refugees and cultural attitudes toward them.

Do impacts on self-reported views translate into changes in political behavior? We designed a proxy for voting using a phone campaign in Uganda, conducted by a non-profit distinct from the implementer and survey firm to reduce any experimenter demand effects. We find that labeled grant recipients were more likely to support a letter to local officials expressing approval of refugee hosting. We conducted several additional tests of demand effects, including a demand-elicitation exercise following De Quidt, Haushofer and Roth (2018) in Kenya, and a placebo information campaign, an incentivized dictator game, and a priming experiment in Uganda. Each test points to a limited role for demand effects.

Receiving an unlabeled grant increases support for integration policies in both Uganda and Kenya, but by less than a labeled grant. We find evidence for two distinct explanations of these findings. First, aid may be implicitly associated with refugees when it is distributed by an organization known to work with refugees, as was the case in our Uganda study. Second, receiving aid may reduce "resource resentment" against groups perceived to be major beneficiaries of aid, such as refugees, even when there is no explicit or implicit link between the aid and the refugee presence. These channels imply that our design cannot separately identify wealth effects. To disentangle wealth effects from other channels, we build a simple model of policy support under which wealth effects are identified from treatment impacts on intermediate outcomes. We estimate the model on Ugandan data and find a small role for wealth effects. We also find that unlabeled grants have greater impacts on policy views in richer areas of Kenya—the opposite pattern we would expect from wealth effects—little heterogeneity by income in Uganda, and no significant changes in economic well-being in Uganda, supporting our interpretation that wealth effects of grants were small.

Further analysis suggests that labeled grants augment the impacts of information through a credibility channel: recipients viewed the grant as a convincing example of aid-sharing. Recipients of labeled grants in Uganda were more likely to report that aid for refugees is shared with Ugandans and that aid organizations are trustworthy. Our findings do not appear to be driven by contact with refugees during or outside of the experiment, reciprocity to the implementing organization, or spillovers.

Overall, our findings indicate that redistributing potential surplus can be an effective tool to build political support for policies that create perceived winners and losers, especially when the connection between the policies and the transfers is clear. In the context of refugee inflows, countries that restrict refugees' work due to concerns about crowd-out can consider combining integration policies with aid redistribution, and countries that already share foreign aid with citizens could increase support for integration by making existing policies more widely known. Given its low marginal cost, our Information Only program is likely to be especially cost effective in these settings.

Related Literature. We contribute to the vast literature studying policy preferences under economic shocks, most of which focuses on high-income countries. Policies that reduce barriers to trade or immigration, for example, are likely to create uneven costs and benefits (Autor, Dorn and Hanson, 2013) and can incite political backlash (Dustmann, Vasiljeva and Damm, 2019, Autor et al., 2020, Di Tella and Rodrik, 2020, Mayda, Peri and Steingress, 2022).[5] Immigration can also diminish natives' preferences for redistribution (Alesina, Murard and Rapoport, 2021). In a survey experiment, Ehrlich and Hearn (2014) find that information about programs to support workers displaced by trade changes support for free trade among low-income respondents, but little is known about the mechanisms, impacts on issues where non-economic concerns are substantial such as immigration, and whether impacts persist.[6] Our paper investigates the role of redistribution and the underlying mechanisms in the context of refugee hosting, which is contentious across much of the world.

This paper also contributes to the literature on attitudes toward immigrants, refugees, and displaced people more broadly, which has also focused on the US and Europe (Alrababa'h et al., 2021). These studies often find that group-based rather than individual concerns determine attitudes (Hainmueller and Hopkins, 2014), and that cultural rather than economic drivers are the strongest predictors (Tabellini, 2020, Alesina and Tabellini, 2024). In low- and middle-income countries, where refugee arrivals are often accompanied by additional foreign aid, refugee immigration has not affected attitudes toward immigrants on average (Aksoy, Ginn and Malpassi, 2022). We show that aid reduces measures of social distance between natives and refugees even without inter-group contact, possibly because cultural attitudes change as a rationalization of new economic and policy views. Our finding that economic interventions can change cultural views is consistent with Jha (2012), Jha (2013), and Jha and Shayo (2019), which show that financial innovations that generate economic complementarities can support new political economy equilibria and reduce inter-group conflict by

[5]See Bonomi, Gennaioli and Tabellini (2021) and Grossman and Helpman (2021) for models in which voters weigh both economic and cultural concerns of groups they identify with when evaluating policies. Ruggie (1982) argues that after 1945, states built political support for openness to international markets by expanding social welfare in the "compromise of embedded liberalism."

[6]Ehrlich and Hearn (2014) find no impact on average support for free trade, driven by an increase (decrease) in support among low- (high-)income respondents. Kim and Pelc (2021) find that, after controlling for trade shocks, counties with more Trade Adjustment Assistance petitions see fewer calls for trade protection.

aligning competing groups' incentives.

Within the literature on attitudes toward immigrants is a set of papers studying the impacts of aid. Inflows of resources to refugees can create "resource resentment" among hosts (Pavanello et al., 2016, Kreibaum, 2016, Zhou, 2019), though Lehmann and Masterson (2020) find that aid to refugees in Lebanon reduced violence toward refugees, possibly through indirect benefits to natives. Four papers study the effects of transfers to both refugees and natives on social cohesion. In Uganda, Baseler et al. (2024) reproduce the results of labeled grants while delivering the information through a video. In Mozambique, Beltramo et al. (2024) find that transfers from the UN refugee agency led to higher levels of social cohesion but do not study the role of beliefs about the source of the transfers. In Ecuador and the Democratic Republic of Congo, respectively, Valli, Peterman and Hidrobo (2019) and Quattrochi et al. (2021) find no impacts from transfers on broad measures of social cohesion but do not analyze attitudes toward refugees.[7] In both settings to our knowledge, recipients were not told whether the aid was part of the response for the displaced. Zhou, Grossman and Ge (2023) find no effect of public goods improvements—together with the refugee presence—on attitudes toward migrants in Uganda, but do not identify the impact of improvements conditional on refugee presence. A potential explanation of these null impacts on attitudes, in light of our results, is that the connection between the transfers and the refugee presence was not clear to natives, who may perceive that refugees are taking assistance or public resources that would otherwise be allocated to them. Our study builds on this literature by identifying both the impact of compensation programs for natives and the effect of explicitly linking the transfers with the broader policy bargain.[8]

Our work also contributes to a large literature on the effects of inter-group contact on attitude formation. Expanding on Allport's (1954) seminal work, Mousa (2020), Lowe (2021), and Bursztyn et al. (2024) find that contact can reduce prejudice. In contrast, contact had few impacts on Israeli Jews' views of Palestinians (Enos and Gidron, 2018) or Afghans' views of internally displaced people (Zhou and Lyall, 2024). Contact is more frequently found to change attitudes toward individuals within the experiment than toward the broader groups those individuals belong to (Scacco and Warren, 2018, Mousa, 2020), and interventions targeting ethnic or racial prejudice typically generate weaker impacts (Paluck, Green and Green, 2019). Our study builds on this literature by showing that a collaborative contact program has less persistent impacts on policy views than direct aid programs explicitly

[7]Both papers analyze a general measure of social cohesion like trust, participation in community groups, and theft. Valli, Peterman and Hidrobo (2019) include "Xenophobia is not an issue."

[8]Our paper also relates to work on politicians receiving credit for development projects (Blattman, Emeriau and Fiala, 2018, Evans, Holtemeyer and Kosec, 2019, Guiteras and Mobarak, 2019, Lyall, Zhou and Imai, 2020, Zhou and Grossman, 2022).

connected to the refugee presence. Our results do not imply that contact of a different nature, such as friendship, would not change views, but are relevant for the many programs that attempt to improve inter-group relations through contact-based interventions.

Finally, we contribute to the literature on small businesses in lower-income countries, including on capital and business networks. Brooks, Donovan and Johnson (2018) find that a mentorship program in Kenya increased profits of inexperienced business owners more than a formal skills training program. Cai and Szeidl (2018) and Fafchamps and Quinn (2018) similarly find positive effects on businesses from expanding the owners' networks. We find substantial interest in mentorship, but no measurable impacts on business outcomes.

2 Overview of Refugee Policies and Attitudes

This section describes the setting of our study, focusing on policies and natives' attitudes toward refugees.

2.1 Refugee Policies in Uganda

With over 1.6 million refugees, Uganda hosts the largest population of refugees in Africa, and the sixth largest globally (UNHCR, 2024*c*). The majority of refugees live in rural settlements, where they receive assistance from humanitarian actors. Kampala, the capital city and the site of our study, hosts about 84,000 registered refugees which is 5% of the Kampala population (UBOS, 2024). The majority of refugees in Kampala is Congolese, with smaller numbers coming from Somalia, South Sudan, Rwanda, Burundi, and Ethiopia (AGORA, 2018). Refugees are well-known in Uganda for their fabrics, tailoring, and cosmetics (Monteith and Lwasa, 2017), which informs the selection of industries in our sample.

Aid-Sharing Policy Bargain. Under Ugandan policy, 30% of international aid budgets for refugees is shared with Ugandan host communities (UNHCR, 2018). This policy is in line with the global Comprehensive Refugee Response Framework—a component of the Global Compact on Refugees, adopted by the United Nations General Assembly in 2018—under which a portion of aid for the refugee response is directed to the hosts, and refugees are granted the right to access labor, housing, and education markets.[9] In Uganda, the aid-sharing policy predates these global agreements and since 2006, refugees can move freely within the country, start businesses, accept jobs, and access primary education and other public services under the Refugees Act 2006. However, there are far fewer aid organizations in Kampala than in the settlement areas, and Ugandans in Kampala see little evidence of aid-sharing. This makes it possible to study the impact of aid-sharing on policy preferences

[9] See Ash and Huang (2018) for a discussion of the *compact model*, in which host-country governments and donors agree on levels of aid and hosting policies jointly.

in a context where a national aid-sharing rule exists but awareness of it is low.

There is no centralized framework governing how aid organizations must spend redirected aid. Aid-sharing thus takes the form of both direct assistance, such as cash grants to Ugandans, and public goods investment, such as funding schools and hospitals in areas where both refugees and Ugandans live.

2.2 Attitudes Toward Refugees in Uganda

Ugandans' views toward hosting refugees are mixed. While a majority generally supports current policies, a significant minority expresses concerns about the economic burden, labor market competition effects, or security threat of hosting refugees (IRC, 2018*b*). Many Ugandans support continued humanitarian assistance to refugees; however, opinions are divided on allowing refugees to work or move freely within the country. Uganda ranks close to the median—72nd out of 139 countries—on Gallup's 2016 Migrant Acceptance Index (Esipova, Fleming and Ray, 2017). As we discuss in Section 3.6, this division in Ugandan public opinion mirrors attitudes documented within our sample, in which we observe high support for hosting refugees in general but mixed opinions on allowing refugees to work or move freely.

2.3 Refugee Policies and Attitudes in Kenya

Kenya is also a major host country for refugees, with over 775,000 refugees and asylum-seekers living throughout the country (UNHCR, 2024*b*). Kenya does not permit the same degree of integration as Uganda, imposing restrictions on work and movement. However, the government has recently adopted some pro-integration policies and is considering adopting more (Miller and Kitenge, 2023). In nationally representative surveys, attitudes toward refugees are less positive in Kenya than Uganda along several integration policy measures (IRC, 2018*a*). The same is true in our sample: for example, 46% of control-group respondents strongly disagreed that Kenya should accept more refugees, compared to 15% in Uganda (see Appendix Figure A1 for additional comparisons).

3 Experimental Design

This section provides an overview of our sample, data collection, and experimental arms. Additional details on study design, including program scripts, are available in Appendix B. We describe the design in Uganda in Sections 3.1 to 3.6 and in Kenya in Section 3.7.

3.1 Sample Selection

We drew our experimental sample from the population of owner-operators of tailor or salon businesses within 10 kilometers of the Kampala city center, which we listed in a censusing exercise described in Appendix B.1. We chose Ugandan micro-entrepreneurs who were no older than 40, had no more than five years of experience in their sector, and who spoke Luganda, English, or Swahili conversationally for inclusion in the experimental sample. We excluded businesses with five or more employees or very high profits or capital. This produced a set of 1,406 micro-entrepreneurs who form our experimental sample. Our sample is more educated than the national adult average (26% completing secondary school vs. 17% nationally), younger (77% aged 18–30 vs. 42%) and somewhat more opposed to allowing refugees to access labor markets at baseline (60% support for access vs. 72%) (IRC, 2018*b*, UBOS, 2021).

We selected tailor and salon owners for several reasons. Both refugees and Ugandans commonly own businesses in these sectors, making the potential competition effects from refugee integration salient for this population, while also making cross-nationality mentorship feasible. Both sectors require skills that can be taught and developed by a mentor without requiring significant new capital investment. Congolese styles in both sectors are popular among Ugandan consumers, suggesting potential benefits to Ugandan producers from collaborating with refugees.

3.2 Data Collection Timeline

Appendix Table A1 presents a timeline of our data collection and intervention activities. We conducted a micro-enterprise census in October 2019 and collected basic data on 3,414 owner-operators. We conducted a baseline survey from November–December 2019 with the experimental sample of 1,406 Ugandan micro-entrepreneurs, plus a set of more experienced entrepreneurs whom we recruited as mentors but who were not included in the experimental sample. We launched the interventions in January 2020 and paused operations in mid-March 2020, with the interventions only partially complete, due to the COVID-19 pandemic. We conducted a midline survey over the phone in October 2020. We resumed and completed (modified) intervention delivery between March and May 2021. We conducted three additional follow-up surveys after interventions were completed: a phone survey in August 2021 and two in-person surveys in May 2021 and March 2022.

Across our four follow-up surveys, we successfully surveyed 91% of the sample at least once. An indicator for being surveyed in at least one follow-up round is not significantly different across treatment groups, as shown in Appendix Table B4 (joint p-value = 0.46). Our round-by-round follow-up retention rates are 80% in the first survey (by phone), 74% in the second survey (in-person), 76% in the third survey (by phone), and 64% in the fourth survey (in-person). In an ANCOVA regression, retention rates were 8 percentage points (pp.) higher in Grant Only (p-val < 0.01) and 6 pp. higher in Ugandan Mentorship (p-val $= 0.07$) compared to Control, but rates in Information + Labeled Grant, Information Only, and Refugee Mentorship are similar to that in Control. We reproduce all of our main results weighting observations by the inverse probability of retention, estimated by lasso logistic regression. Results in Appendix Tables B6, B7, B8, and B9 show that our main results hold after adjusting for respondents' propensity to attrit. We also present Lee Bounds for each of our pre-specified outcome domains (see Section 3.5.2 for details) in Tables B10 and B11, although these bounds are wide for many outcomes.

3.3 Interventions

We implemented three main interventions to test the impact of aid redistribution on policy preferences and beliefs. Our interventions were carried out by Young African Refugees for Integral Development (YARID), a refugee-led non-profit in Kampala that employs and implements livelihoods and education projects for both refugees and Ugandans. In addition to cash grants, YARID offers services like job training and English and computer literacy classes. Before this project, YARID did not explicitly link its assistance programs for Ugandans to the government's aid-sharing policy or conduct information campaigns about refugees targeted to the general public, but did so randomly for the purpose of this research. Figure 1 summarizes our sample selection and treatment assignment process. Appendix B.4 provides details on uptake, which was at least 79% in each treatment arm.

Information Only. The first intervention provided information about Uganda's existing aid-sharing policy, which stipulates that 30% of foreign aid to refugees be shared with the host community through direct transfers or public good provision such as hospitals and schools that Ugandans can access. The script included a specific example of a hospital in Kampala funded partly by international aid for refugees. Participants were visited by a refugee or Ugandan staff member. The script outlined the policy bargain, linking aid-sharing—and the potential benefits to the respondent—with policies that allow refugees to integrate, as the following excerpt shows (full scripts are available in Appendix B.6):

> *Since refugees [in Uganda] can work, some of the aid money coming from international donors like Great Britain can be shared with Ugandans... In countries like Kenya where refugees cannot work, more aid money needs to be spent on food and basic needs for refugees, and so it cannot be shared with the host country. In Uganda, since refugees can get jobs and live outside of camps, aid money and programs can be shared with Ugandans like you.*

Because awareness of the aid-sharing policy was low at baseline (19% of respondents reported that any international aid for refugees is shared with Ugandans), we expected this treatment arm to change beliefs about the economic impact of hosting refugees. We complemented this information delivery with a listening exercise modeled after Kalla and Broockman (2020), in which the staff member invited the respondent to share their views of refugees and then shared a personal story related to refugees living in Kampala. This exercise was incorporated into the beginning of the information script to "break the ice" by building rapport between the respondent and the staff member and giving context for the purpose of the visit. We refer to this as the Information Only treatment arm.

Information + Labeled Grant. The second intervention provided a grant of 135 USD, or about 3.5 months of average business profit, delivered with the same information and listening exercise contained in the Information Only arm.[10] Staff explained that the grant was an example of aid-sharing: we therefore refer to this treatment as the Information + Labeled Grant arm, or sometimes as simply a *labeled grant*. A YARID staff member first visited the business owner to inform them about the grant and deliver the information. During a second meeting, the staff member paid directly for business expenses at a shop of the business owner's choosing. In the first wave of disbursements before COVID-19, we required that at least 60% of the grant be used for business purposes, motivated by the findings of Fafchamps et al. (2014) on similar in-kind transfers. The remaining balance was disbursed through mobile money.

Mentorship by a Refugee. The third intervention was a mentorship program that matched business owners with experienced refugee business owners within sector and gender.[11] The program included up to six in-person meetings between the mentor and mentee, roughly once per week, each facilitated by a YARID staff member who provided guidance and translation if necessary. This design is motivated by the contact hypothesis, in which cooperative relationships are theorized to reduce prejudice between majority and minority group members, and by the results of a similar mentorship program which demonstrated large impacts on profits (Brooks, Donovan and Johnson, 2018).[12] Many business owners in our sample report little contact with refugees at baseline: when asked to name four people they talk to most about business, 85% of owners named only other Ugandans, and 74% reported contacting zero refugees for social reasons in the past month, suggesting that there

[10]The grant size of 135 USD approximates a targeted compensation policy that would give large transfers to those most likely to be negatively affected by refugee integration. Our experiment in Kenya, which offered grants of around 7.50 USD, approximates a more distributed compensation policy.

[11]Mentors were recruited from the population of eligible refugee business owners in Kampala with at least 3 years of experience, and mentees were drawn from our sample of inexperienced Ugandan business owners with less than 5 years of experience. Overall, 86% of mentors were Congolese, 9% were Rwandan, and 4% were Burundian.

[12]The most common topics of discussion during meetings were customers, skills, equipment and tools, location choices, and suppliers. According to YARID facilitator reports, in 24% of meetings with refugee mentors, most of the conversation was translated. In 46% of meetings, the facilitator reported that the mentor and mentee had roughly equal control over the conversation.

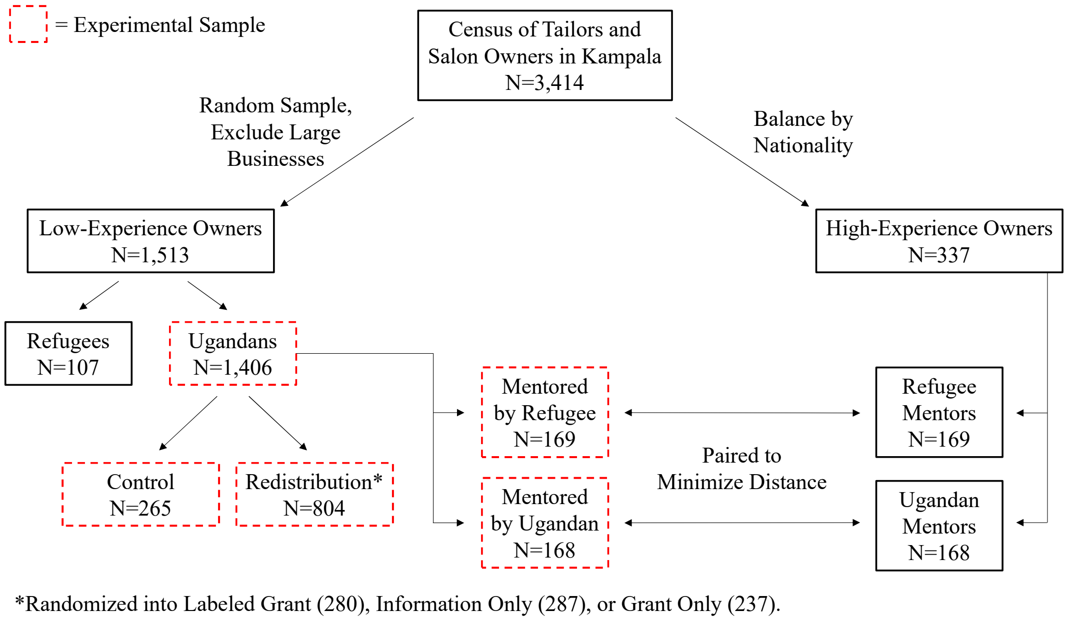
is considerable scope for additional contact to change views toward refugees.

Comparison Arms. In addition to our three main interventions, we included two additional treatment arms and a control group. The first provided a business grant identical to the labeled grant, but delivered by a Ugandan staff member without any information about refugees or Uganda's aid-sharing policy. We refer to this arm as Grant Only or the *unlabeled grant*. This arm allows us to separate impacts of labeling the grant as aid-sharing from impacts generated by the receipt of aid in itself. The second was a mentorship program that matched business owners with an experienced Ugandan business owner in their sector. Mentors were chosen to balance characteristics across Ugandan and refugee mentors (see Appendix Table B3). This arm allows us to isolate the impact of cooperative contact with more experienced refugees from other impacts of the mentorship program. We assigned only Ugandan staff members to facilitate the Grant Only and Mentorship by Ugandan treatment arms; other treatment arms were facilitated by both Ugandan and refugee staff members. Finally, a pure control group did not receive any treatment and was not contacted by YARID.

COVID-19 Disruptions. Interventions were implemented in-person beginning in January 2020. Due to COVID-19, we paused interventions in March 2020 and restarted all treatments remotely in March 2021. At the time of the pause, most business owners had been visited once to inform them of their treatment assignment, but only one-third of grants had been disbursed. All respondents treated before the pause were re-contacted with a refresher script by YARID when activities resumed. We converted mentorship meetings from in-person to remote when they resumed. YARID provided up to four facilitated mentorship meetings using three-way calling, regardless of the number of meetings that were held prior to our pause.

Given that our first follow-up survey was completed before interventions were finished, estimates from our first survey round represent intent-to-treat effects comprising both program effects and anticipation effects. Our remaining three surveys were completed after programs had finished. The switch from in-person to remote mentorship implies that our mentorship arms estimate an average intent-to-treat effect of in-person and remote meetings. In Section 5.2, we consider and reject that the transition to remote mentorship explains the low impacts of refugee mentorship. Finally, the disbursement of grants around COVID-19 complicates attempts to generalize our effect sizes to more typical economic conditions, a point we return to in Section 6.

Figure 1: Summary of Study Design



Notes: See Appendix B for additional details on study design. Businesses with high capital or profit were excluded from the experimental sample. Mentors were chosen to balance several characteristics across refugee and Ugandan mentors. Mentees and mentors were paired within gender-sector cells to minimize within-pair travel distance using a greedy matching algorithm.

3.4 Randomization

Within our experimental sample of 1,406 inexperienced Ugandan business owners, we assigned participants randomly to one of six treatment conditions: Control, Grant Only, Information Only, Information + Labeled Grant, Mentored by Refugee, or Mentored by Ugandan. We implemented a stratified randomization assignment using the Stata command *randtreat*. Mentees were paired to mentors within gender-sector cells to minimize within-pair travel distance using a greedy matching algorithm. For further details, see Appendix B.2.

Appendix Table B1 presents balance tests. We reject joint orthogonality of our treatments at the 10% level for 3 out of 31 baseline variables, suggesting that randomization was effective at creating balanced treatment groups. Among individuals surveyed at least once after baseline, 2 out of 31 baseline variables are significantly different at the 10% level, as shown in Appendix Table B2, suggesting that attrition did not generate imbalance.

3.5 Empirical Strategy

This section summarizes our strategy for measuring outcomes and identifying treatment effects. Additional details are available in our pre-analysis plan (Baseler et al., 2022).

3.5.1 Estimating Equations

We estimate intent-to-treat (ITT) effects using the following ANCOVA specification (McKenzie, 2012), stacking survey waves:

$$y_{it} = \sum_{j=1}^{5} \beta_j T_{ji} + \gamma y_{i0} + \delta M_{i0} + \eta X_i + \theta_t + \alpha_i + \text{Phone}_{it} + \text{Date}_{it} + \epsilon_{it} \tag{1}$$

where y_{it} is an outcome for individual i measured at time t, with $t = 0$ corresponding to baseline (pre-treatment) values; M_{i0} is an indicator for a missing value of y_{i0}; T_{ji} are treatment assignment indicators for treatment groups $j = \{1, 2, 3, 4, 5\}$; X_i is a vector of baseline controls chosen through double lasso (Chernozhukov et al., 2018); θ_t is a survey-round fixed effect; α_i is a randomization-stratum fixed effect; Phone_{it} is an indicator for whether the survey was completed over the phone (as we attempted to survey any respondents who relocated outside Kampala over the phone); Date_{it} is a linear date-of-survey control; and ϵ_{it} is an error term. Standard errors are clustered at the individual level. We run separate lassos for each dependent variable using the Stata package *pdslasso* (Ahrens, Hansen and Schaffer, 2019) and include all possible controls from the baseline in each. Our treatment effects of interest are given by the coefficient vector β_j and represent the average difference in outcome y_{it} between each treatment group and the control group, across individuals and post-treatment survey rounds, conditional on included controls. Throughout the paper, we focus primarily on treatment impacts of individual arms relative to Control or on pairwise comparisons between them. Treatment impacts relative to Control are directly informative of aid-sharing programs that could operate at scale.

3.5.2 Measurement and Multiple Hypothesis Testing

Because many of our outcomes of interest represent broad conceptual categories, such as "support for refugee integration policies," we organize our outcomes into a series of pre-specified domains representing classes of related hypotheses. In addition to analyzing outcomes individually, we compute a summary index following Anderson (2008). Each summary index represents a weighted average of standardized components within a domain.[13]

We transform survey questions that use Likert scales (Likert, 1932) and other categorical outcomes into binary measures, resolving neutrals towards the smaller group. Monetary values are winsorized at the 1st and 99th percentiles within each survey round, recorded as 0 for firms that are not operating, and expressed in 2019 US Dollars. To reduce survey length, not all outcomes were measured in all surveys; the number of observations may therefore

[13] Weights are the sum of row entries in the inverted covariance matrix of outcomes in a domain.

vary across outcomes.

Within each domain, we compute sharpened q-values to control the false discovery rate. This procedure estimates the share of rejected null hypotheses that are false rejections. We indicate outcomes that were not pre-specified with a plus sign ($^+$) and report naive p-values from Equation 1 for these and for the domain summary indices. For hypotheses that we pre-specified as primary, we report Westfall-Young stepdown-adjusted p-values (Westfall and Young, 1993) to control the family-wise error rate in Appendix Table A16. This procedure estimates the probability of making one or more type I errors and adjusts for correlation across outcomes. The body of this paper presents only a subset of our pre-specified analysis; we report the full set of pre-specified outcomes and sharpened q-values in Appendix E.

3.6 Summary Statistics

Appendix Table A2 displays summary statistics for our experimental sample of 1,406 Ugandan micro-enterprise owners. The average owner was 28 years old with 11 years of education and 2.4 years of experience running a business in their sector. About two-thirds of owners are women, and tailors and salons are roughly equally represented. Their businesses earned an average of 37 USD per month, and about one-fifth of businesses had any employees.

At baseline, few owners were aware of Uganda's aid-sharing policy: 19% reported that any international aid for refugees is shared with Ugandans. Consistent with national averages, there was high general support for refugee hosting (72% of owners said they support Uganda's hosting of refugees) but more mixed views toward extending labor market access or freedom of movement (58–60% of owners said they support these policies). About half of owners said they would support allowing more refuges into Uganda.

Many business owners in our sample mentioned concerns related to the crowd-out effects of hosting refugees: 78% believed that refugees increase business or housing rents. About half of our sample believed that the net economic effect of refugee hosting is positive for Uganda. Many respondents (57%) said that refugees have a neutral impact on culture in Uganda, while 30% said the effect is negative. About 20% said they would be very comfortable marrying a refugee; about 40% said they would be very uncomfortable doing so.

3.7 Study Design in Kenya

In Kenya, we selected 7,078 households across 235 villages sampled as part of Barnett-Howell, Baseler and Ginn (2023), a separate project unrelated to refugees. Our settings in Kenya and Uganda differ along several dimensions. The *status quo* policies on refugees' work and movement are more restrictive in Kenya than in Uganda. Support for policies like these to integrate refugees is lower in the Kenyan than the Ugandan sample. The Kenyan sample lives in predominantly rural areas while the Ugandan sample lives in the capital city. Finally, while approximately 5% of residents in Kampala are refugees, few refugees live in the sampled Kenyan counties. Because our Kenya experiment was attached to a separate project, we were unable to collect data with the same richness as in Uganda, so we are restricted to analyzing policy-support outcomes.

We assigned 50 Kenyan villages to receive grants and 185 villages to control. Within villages assigned to receive grants, households were assigned to either a Grant Only or an Information + Labeled Grant arm. In Grant Only, households received 1,000 KSh (7.50 USD) labeled as generic support. In Information + Labeled Grant, households received the same grant and watched a short video made by a refugee-led non-profit, RELON Kenya, explaining that they are receiving the grant because Kenya hosts refugees and that future aid-sharing between refugees and Kenyans could be part of a national policy bargain for increased freedom of movement and labor market access for refugees (see Appendix B.10 for scripts).

We pre-specified the following design and analysis (Baseler and Ginn, 2024). Data on support for refugee integration were collected shortly after the grants were announced. To minimize spillovers between the Grant Only and Information + Labeled Grant groups, we include in our estimation sample only households surveyed on the first visit day in each village. This produces a final sample of 5,264 households. We estimate treatment impacts on policy views using Equation 1, excluding y_{i0}, M_{i0}, X_i, θ_t, Date_{it}, and Phone_{it}. For hypothesis tests involving comparisons between either Information + Labeled Grant or Grant Only and Control, we use randomization inference. For tests comparing Information + Labeled Grant to Grant Only, we compute Huber-White standard errors, as treatment assignment between these two arms was done at the household level.

About one month later, we re-surveyed Grant Only and Information + Labeled Grant households by phone to assess the persistence of treatment impacts and the scope for experimenter demand effects.[14] To do so, we implemented the weak demand treatment of De Quidt, Haushofer and Roth (2018), which attempts to induce a demand effect at least as strong as those implicit in the study design. Specifically, respondents in the Grant Only arm received the following script prior to questions about their policy views:

> *For the remaining questions, we think that participants who are shown these instructions will express more support than they normally would for admitting refugees and integrating them in Kenya—including letting them move and work freely.*

[14]This follow-up survey was not pre-specified. We analyze the same outcomes and use the estimating equations described in our pre-analysis plan.

Respondents in the Information + Labeled Grant arm received the same script with "less support" instead of "more support." Comparing responses across these two groups identifies the lower bound of demand-free beliefs, assuming that demand effects created by the explicit script are stronger than other implicit demand effects.[15] Given that our survey instructions encouraged respondents to be honest and assured them that no opinions would be judged, we expect this assumption to hold.

Appendix Table B5 shows that randomization appears to have successfully created balance across treatment arms, both in the full sample and in the set of households surveyed at follow-up. We successfully contacted 95% of those sampled for survey at follow-up, with no significant difference across treatment groups, as shown in Appendix Table B4.

3.8 Conceptual Framework and Hypotheses

Our primary hypothesis is that learning about and experiencing aid-sharing—compensation given as part of a policy bargain that includes refugee integration policies—increases support for those integration policies. We test this by comparing support for refugee integration in Information + Labeled Grant to Control. This result is most directly informative of the impact that aid-sharing would have on beneficiaries who recognize it as such. Second, we test whether learning about aid-sharing through an information campaign increases support for the same policies, and test this hypothesis by comparing support in Information Only to Control. This result is most informative of the impact that aid-sharing would have on indirect beneficiaries (such as users of public goods funded by aid-sharing) or non-beneficiaries who learn about it, possibly through a scaled-up information campaign. Third, we test whether a form of aid based on inter-nationality contact—free business mentorship by an experienced refugee—affects support for the same policies. We test this hypothesis by comparing support among those offered mentorship by a refugee to Control. This result is most informative of the impact that other contact-based programs, which could be funded through aid-sharing, would have on policy support. We present the results of these tests in Section 4.1.

Given positive impacts of the Information + Labeled Grant and Information Only arms on policy support, we next assess impacts on beliefs about the economic impacts of hosting refugees and on cultural attitudes toward refugees, as these are two key intermediate outcomes that can influence immigration policy views in theory (Tabellini, 2020, Bonomi, Gennaioli and Tabellini, 2021). As before, we test these hypotheses by comparing treated groups to Control. We present these results in Sections 4.3 and 4.4. As complementary evidence, we test whether economic beliefs and cultural attitudes are mediating impacts on

[15] This design maximizes our statistical power to detect a non-zero lower bound. Since our goal is to test whether the labeled grant changed true policy views, the upper bound is unnecessary.

policy views by interacting treatment assignment with indicators for high baseline economic and cultural views in Section 5.3.

We proceed to unpack the mechanisms behind changing policy views, economic beliefs, and cultural attitudes, focusing on two mechanisms identified by the literature on policy preference formation and immigration attitudes—resource resentment against refugees (Zhou, 2019) and wealth effects (Hainmueller and Hopkins, 2014)—and two that we contribute to the literature—knowledge of the redistribution policy and the credibility of the policy's implementation, which the labeled grant signals.[16] The Information Only arm tests the joint impact of knowledge of aid-sharing and the listening exercise. To test the role of credibility, we assess impacts of each treatment on beliefs that aid is in fact shared with Ugandans and on reported trust in implementing organizations. To test the role of resource resentment, we analyze beliefs that refugees receive too much aid compared to Ugandans.

We find that beneficiaries associated the Grant Only arm with refugees, implying that it does not isolate wealth effects. However, it is informative of the impacts of direct transfers to natives without explicit labeling by a humanitarian organization, which in places that host refugees are often closely associated with refugees themselves or with supporting refugee integration. To assess the role of wealth effects, we estimate a structural model, test for heterogeneity in treatment impacts by initial wealth, and evaluate impacts on economic well-being which could potentially mediate wealth effects.

4 Results

We find that redistributing refugee aid toward Ugandans in the form of a labeled grant substantially and persistently increases support for refugee hosting and integration policies. We find similar results in Kenya with labeled grants tied to the refugee presence and the potential for new aid-sharing. Sharing information about existing redistribution in Uganda—without a grant—has similar, but smaller, impacts. Facilitating cooperative inter-group contact has only transient impacts on policy preferences.

4.1 Support for Refugee Integration Policies in Uganda

We find that receiving a labeled grant significantly increases support for refugee hosting and integration, as shown in Table 1. Recipients of labeled grants were 13 pp. more likely to say that they support Uganda's hosting of refugees generally, on a base of 75% (p-val < 0.01). Labeled grants also increase support for admitting more refugees into Uganda (15 pp. on a

[16] To investigate mechanisms, we assess treatment impacts on secondary outcomes and heterogeneity in treatment impacts, as our joint treatment—Information + Labeled Grant—does not identify interaction effects between grants and information at the individual level if the complier sets in Grant Only and Information Only overlap.

base of 61%, p-val < 0.01), extending the right to work (13 pp. on a base of 72%, p-val $<$ 0.01), and extending freedom of movement to refugees (6 pp. on a base of 54%, p-val $= 0.04$). The impact on our pre-specified domain summary index is 0.36 standard deviations (sd.; p-val < 0.01; family-wise error rate < 0.01). Adjustments for multiple hypothesis testing do not affect these conclusions, as shown in Appendix Table E1.

Labeled grants have greater impacts on the integration policy support index among those with less support for integration at baseline—or greater economic or cultural concerns about refugee hosting—as shown in Appendix Table A3, and labeled grant recipients were significantly less likely to indicate strong opposition for integration policies, as shown in Appendix Table A4.[17] These results suggest that aid-sharing may influence policy views even in environments where opposition to integration is stronger, a hypothesis we later test directly by examining impacts in Kenya.

Our Information Only treatment also significantly impacts policy preferences, though by less than receiving a labeled grant (coeff. = 0.22 sd.; p-val on comparison to labeled grants = 0.02). Across specific policy outcomes, effect sizes are generally half to two-thirds the size of impacts of the labeled grant, though differences are not always statistically significant. Our Grant Only treatment also impacts policy preferences in the same direction, though by a smaller magnitude than labeled grants (coeff. = 0.25 sd.; p-val on comparison to labeled grants = 0.05).[18] As we discuss further in Section 5.1, this result is likely due not to wealth effects but to an implicit labeling of the grants operating through contact with the refugee-led implementing NGO, together with a reduction in resource resentment against refugees.

Mentorship by an experienced refugee has much smaller impacts on policy preferences compared to labeled grants (p-value on index comparison < 0.01). We observe modest increases in support for extending labor market access (8 pp. on a base of 72%, p-val $=$ 0.01), but smaller and statistically insignificant (at the 5% level) impacts on general support for hosting, support for admitting more refugees, and support for freedom of movement. The impact on the domain summary index is 0.12 sd. (p-val $= 0.10$). In Section 5.2, we test and reject that additional mentorship meetings would have generated persistent impacts on policy views. This smaller effect is also not due to low perceived value of the program: 71% of mentees reported that they were satisfied with the program in both mentorship arms and 78% said that they learned something from the program that was helpful for their business.

[17] Impacts on policy views are similar when we re-weight our sample to match average education, age, and integration support as measured in nationally representative surveys, as shown in Appendix Table A5.

[18] A comparison of impacts in Information + Labeled Grant and Grant Only is significant at $p = 0.05$ on our summary index and at $p = 0.10$ for two out of the five policies shown in Table 1. In Kenya, where there was little scope for implicit labeling of grants, we reject equality of Information + Labeled Grant and Grant Only on every measured outcome with $p < 0.01$ (see Table 2).

Table 1: Support for Refugee Integration Policies—Uganda

	Integration Policies Index	Supports Refugee Hosting	Supports More Refugees	Supports Right to Work	Supports Freedom of Movement	Supported Phone Campaign^{+}
Info. + Labeled Grant	0.36***	0.13***	0.15***	0.13***	0.06**	0.10***
	(0.06)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.04]	[0.01]
Information Only	0.22***	0.06**	0.10***	0.08***	0.03	0.02
	(0.07)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
	[0.00]	[0.02]	[0.00]	[0.00]	[0.37]	[0.55]
Grant Only	0.25***	0.09***	0.12***	0.10***	0.00	0.04
	(0.07)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.89]	[0.26]
Mentored by Refugee	0.12*	0.04	0.06*	0.08**	-0.03	-0.01
	(0.07)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
	[0.10]	[0.25]	[0.10]	[0.01]	[0.44]	[0.77]
Mentored by Ugandan	0.10	0.07**	0.04	0.02	-0.06*	-0.03
	(0.08)	(0.03)	(0.04)	(0.03)	(0.04)	(0.04)
	[0.18]	[0.03]	[0.24]	[0.46]	[0.09]	[0.54]
Observations	3,051	3,040	3,038	3,039	3,031	1,406
Control Mean: Baseline	0.00	0.73	0.51	0.60	0.60	.
Control Mean: Follow-Ups	-0.00	0.75	0.60	0.72	0.54	0.23
Lab. Grant = Info Only	0.02	0.00	0.08	0.04	0.26	0.04
Lab. Grant = Grant Only	0.05	0.06	0.39	0.12	0.05	0.16
Lab. Grant = R-Mentee	0.00	0.00	0.01	0.04	0.01	0.01
R-Mentee = Info Only	0.13	0.38	0.24	0.78	0.13	0.42
R-Mentee = U-Mentee	0.80	0.35	0.66	0.11	0.40	0.77

An observation is a surveyed respondent per post-baseline survey round in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double lasso. Standard errors clustered at the enterprise level in parentheses. Brackets and the last five rows display two-sided p-values. Outcomes that were not pre-specified are denoted with $^{+}$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table B14 shows the program cost per unit change in support for refugee hosting overall. The Information Only arm has the lowest cost per unit change in support (6.50 USD per 1 pp. treatment effect), roughly half that of Information + Labeled Grant (about 12 USD per 1 pp. effect). Unsurprisingly, Grant Only is the most expensive at 17 USD per 1 pp. change.

Do Impacts on Self-Reported Views Affect Political Behavior? To test for changes in preferences that might affect political behavior, we sought to induce a naturalistic situation outside of our surveys that required individuals in our sample to make a decision either in favor or not in favor of refugee hosting, similar to voting in a referendum. To do so, we partnered with an organization that was independent of both the survey firm and implementing non-profit. One year after the interventions were completed, that organization conducted a phone-call campaign asking each member of our sample whether they wanted to support a letter to local officials expressing their approval of refugee hosting. The campaign was intended to allow respondents to express their policy views without any risks of opposing the government, as only the number of supporters—not names—were included in the final letter. We recorded a one-minute message explaining the campaign, and respondents could press 1 to support or 2 to oppose. Over 80% of the sample answered the call or replied to a follow-up SMS. See Appendix B.9 for the script and Appendix Table A6 for detailed results.

As shown in Table 1, labeled grant recipients were 10 pp. more likely to support the letter (on a base of 23%, p-val < 0.01), with no significant impacts for other treatment arms.[19] In addition to suggesting a connection between reported views and political behavior, these results help us rule out impacts driven entirely by experimenter demand effects, a point we return to in Section 5.2.

Persistence of Treatment Impacts. Treatment impacts on policy preferences persist for at least two years after the interventions began, as shown in Figure 2, which displays treatment impacts estimated separately by survey round. We see no evidence of attenuation of the treatment effects of labeled grants, unlabeled grants, or information as of the final survey in March 2022. Given that interventions began in early 2020 (and resumed in early 2021), this suggests that redistribution can impact policy views in the long run and persist through a large economic shock like COVID-19.

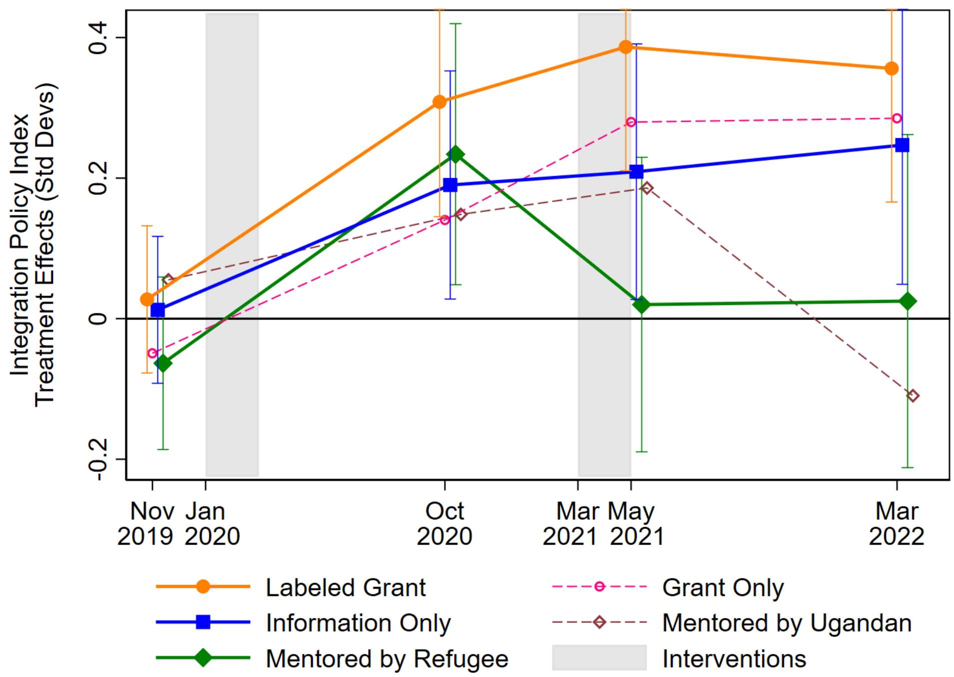
4.2 Support for Refugee Integration Policies in Kenya

In Kenya, labeled grants substantially increase support for refugee integration. Table 2 presents impacts on the same policy support outcomes analyzed in Uganda. Labeled grants increase a summary index of integration policy support by 0.59 sd. ($p < 0.01$) compared to Control, an effect even larger than that in Uganda (0.36 sd.). Given that support for refugee integration is lower in Kenya compared to Uganda (see Appendix Figure A1), this finding is consistent with the greater impacts observed among those with lower baseline support in Uganda.[20] It also points to a limited role of the size of the grant—as the grant size in Kenya was about 6% of the grant size in Uganda—compared to an "extensive margin" effect of receiving a grant at all. Impacts are large and statistically significant for each component of our summary measure: support for specific integration policies such as freedom of movement or right to work rises by 18–24 pp (p-vals < 0.01). Impacts are large for outcomes with high

[19] While the results for other treatment arms are statistically insignificant, they are proportionate to impacts on self-reported policy views, and—as for self-reported measures—there are stronger and statistically significant results among those who were most opposed to refugee integration at baseline.

[20] The timing of the surveys also differs between Kenya and Uganda. In Kenya, respondents were surveyed in the same sitting as the intervention. While we do not measure same-day impacts in Uganda, the effects were relatively stable across follow-up rounds.

Figure 2: Timing of Treatment Impacts on Support for Refugee Integration Policies



Notes: Each line shows the estimated treatment impact on a summary index of preferences for policies supporting refugee integration within a given survey wave. We did not collect these measures in the third follow-up survey. Shaded gray areas show the timing of our interventions, which began in January 2020 and resumed in March 2021 after our pause due to COVID-19. Vertical bars show 95% confidence intervals for the Information + Labeled Grant, Information Only, and Mentored by Refugee arms.

support in the control group (75% support hosting refugees overall) as well as those with lower support (43% support allowing additional refugees into Kenya).

As in Uganda, we observe positive, but smaller, impacts of Grant Only relative to Control. Our summary index measure is 0.18 sd. higher in Grant Only ($p < 0.01$). Across outcomes, the impact of Grant Only ranges from about one-quarter to one-third that of Information + Labeled Grant. These positive impacts are potentially surprising given that the grant was small (a one-time transfer of about 5% of monthly household consumption) and there was no direct contact between respondents and the implementing organization, minimizing the possibility that the grant was implicitly labeled as related to refugees. In Section 5.1, we argue that a reduction in resource resentment toward refugees can potentially explain this finding.

The program cost per unit change in support for refugee hosting overall is lower in Kenya than in Uganda, as shown in Appendix Table B14. Labeled grants cost about 3 USD per 1 pp. treatment effect, compared to about 12 USD in Uganda. The Grant Only arm cost

Table 2: Support for Refugee Integration Policies—Kenya

	Integration Policies Index	Supports Refugee Hosting	Supports More Refugees	Supports Right to Work	Supports Free Movement	Supports Providing Land	Supports Citizen -ship
Immediate Impacts							
Info. + Labeled Grant	0.59***	0.16***	0.24***	0.24***	0.23***	0.23***	0.18***
	(0.05)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Grant Only	0.18***	0.06***	0.06**	0.06**	0.08***	0.07**	0.06**
	(0.06)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
	[0.00]	[0.00]	[0.01]	[0.01]	[0.00]	[0.01]	[0.02]
Observations	5,264	5,264	5,264	5,264	5,264	5,264	5,264
Control Mean	0.00	0.75	0.43	0.56	0.52	0.47	0.56
Lab. Grant = Grant	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Demand-Free Bound							
Info. + Labeled Grant	0.19***	0.05***	0.06*	0.05**	0.05*	0.04	0.09***
	(0.06)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)
	[0.00]	[0.00]	[0.06]	[0.04]	[0.05]	[0.11]	[0.00]
Observations	1,046	1,046	1,046	1,046	1,046	1,046	1,046
Grant Only Mean	-0.00	0.89	0.45	0.80	0.74	0.71	0.68

Each observation is a household in Kenya. *Immediate Impacts* are measured the same day after grant and information distribution. *Demand-Free Bound* computed using the method of De Quidt, Haushofer and Roth (2018) to identify the lower bound of demand-free treatment effects—labeled grant recipients receive a script attempting to induce negative demand effects, while grant only recipients receive a positive script. These results are measured using follow-up surveys conducted only in Labeled Grant and Grant Only about one month after the first survey (the omitted category is Grant Only). For *Immediate Impacts* comparisons between Labeled Grant or Grant Only and Pure Control, standard errors are clustered at the village level and p-values are computed through randomization inference, permuting treatment assignment 2,000 times using the Stata command *ritest* (Heß, 2017). For comparisons between Labeled Grant and Grant Only, standard errors and p-values are heteroskedasticity-robust. *Lab. Grant = Grant* shows p-values from a regression of the outcome on an *Information + Labeled Grant* indicator estimated on Information + Labeled Grant and Grant Only households only. Standard errors in parentheses; p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

about 8 USD per 1 pp. effect, compared to 17 USD in Uganda.

Our estimated demand-free lower bound of the impact of Information + Labeled Grant relative to Grant Only is positive and statistically significant on our summary index (0.19 sd., $p < 0.01$), with consistent results across outcomes (component-level impacts vary from 4–9 pp. with p-values ranging from 0.00 to 0.11). These impacts are generally lower than the immediate differences between Information + Labeled Grant and Grant Only. This is consistent with partially demand-driven impacts, but may also be due to spillovers from Information + Labeled Grant to Grant Only—as assignment between these two groups was conducted at the household level and about one month had passed since the information was given—or to treatment impact decay over time.

As in Uganda, we find that treatment impacts are greater where initial support is lower. Since we lack a pre-treatment measure of policy preferences in Kenya, we divide our sample into low, medium, and high integration support based on terciles of the average integration policies summary index in the Control group within respondents' sub-counties. Appendix Table A7 presents results. Immediately after treatment, labeled grants have the largest impacts relative to Control in low-support areas, followed by medium- and finally high-support areas, though impacts are large and statistically significant in all three sub-samples. At follow-up, the estimated demand-free lower-bound impacts of labeled grants relative to Grant Only remain large and similar in low- and medium-support areas, but is close to zero in high-support areas.

4.3 Economic Beliefs in Uganda

Our interventions may affect policy views by changing beliefs about the economic impacts of refugee hosting. Ugandans who received a labeled grant were significantly more likely than Control business owners to report receiving support linked to the refugee presence, as shown in Table 3.[21] Business owners who received a labeled grant were 15 pp. more likely to report that international aid for refugees is shared with Ugandans (on a base of 37%, p-val $<$ 0.01),[22] and 16 pp. more likely to say refugees have a positive effect on the economy overall (on a base of 42%, p-val < 0.01). They were also more likely to say that refugees benefit them personally, and that refugees have skills. The impact on our pre-specified domain summary index is 0.3 sd. (p-val < 0.01). Adjustments for multiple hypothesis testing do not affect these conclusions, as shown in Appendix Table E5.

Our Information Only and Grant Only treatments also changed beliefs about the economic impacts of refugee hosting. Business owners in the Grant Only treatment arm were 8 pp. more likely than Control business owners to report receiving support linked to the refugee presence, an impact 4 pp. smaller than that among labeled grant recipients (p-value on comparison = 0.04). As discussed in Section 5.1, we believe this is due to an implicit labeling of the grant given that the implementing organization is well-known in our study

[21] To minimize the association between the data firm and the implementer, we did not measure this association explicitly. Instead, we asked respondents about "the purpose" of aid received recently and enumerators coded whether they spontaneously mentioned refugees in their response. Respondents in Grant Only were also more likely to associate support with refugees, though by less than in Information + Labeled Grant (p-val = 0.04), for reasons we discuss in Section 5.1.

[22] Awareness of aid-sharing is higher in Control in follow-up surveys than at baseline (37% versus 17%), suggesting that Ugandans are learning about aid-sharing independently of our experiment. We believe this is happening through aid distributed during the COVID-19 pandemic; 2% of the control group had received any assistance in the year before the baseline survey, while 46% received assistance during COVID-19 lockdowns.

Table 3: Beliefs About Economic Impacts of Hosting Refugees—Uganda

	Economic Beliefs Index	Associated Support w Refugees^{+}	Knows About Aid-Sharing	Pos Effect on Economy Overall	Pos Effect on You Personally	Refugees Have Skills
Info + Labeled Grant	0.30***	0.12***	0.15***	0.16***	0.09***	0.10**
	(0.07)	(0.02)	(0.03)	(0.03)	(0.04)	(0.04)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.01]	[0.02]
Information Only	0.22***	0.02	0.05	0.12***	0.06*	0.02
	(0.07)	(0.01)	(0.03)	(0.03)	(0.03)	(0.04)
	[0.00]	[0.13]	[0.11]	[0.00]	[0.08]	[0.69]
Grant Only	0.21***	0.08***	0.09***	0.10***	0.11***	0.03
	(0.07)	(0.01)	(0.03)	(0.04)	(0.04)	(0.04)
	[0.00]	[0.00]	[0.01]	[0.01]	[0.00]	[0.47]
Mentored by Refugee	0.07	0.03**	-0.03	0.04	-0.04	0.01
	(0.08)	(0.02)	(0.04)	(0.04)	(0.04)	(0.05)
	[0.34]	[0.03]	[0.42]	[0.37]	[0.31]	[0.81]
Mentored by Ugandan	0.07	0.05***	0.02	0.04	0.06	0.00
	(0.08)	(0.02)	(0.04)	(0.04)	(0.04)	(0.05)
	[0.35]	[0.00]	[0.54]	[0.34]	[0.15]	[0.92]
Observations	3,003	3,061	3,061	2,787	2,906	1,671
Control Mean: Baseline	0.00	.	0.17	0.50	0.41	0.51
Control Mean: Follow-Ups	-0.00	0.02	0.37	0.42	0.44	0.42
Lab. Grant = Info Only	0.25	0.00	0.00	0.19	0.32	0.04
Lab. Grant = Grant Only	0.23	0.04	0.09	0.07	0.69	0.11
Lab. Grant = R-Mentee	0.00	0.00	0.00	0.00	0.00	0.06
R-Mentee = Info Only	0.05	0.31	0.02	0.03	0.01	0.91
R-Mentee = U-Mentee	1.00	0.34	0.19	0.96	0.02	0.89

Associated Support w Refugees is measured implicitly with the question "Have you received assistance from any government or NGO program in the last 5 years, such as cash, mentorship services, food, etc.?" and if so, "What organization provided the assistance?" and "What do you know about (that organization)?" Enumerators were asked to code if the respondent mentioned refugees in their answer. *Knows About Aid-Sharing* measured with the question "Are any of the international donations to refugees in Uganda shared with Ugandans?" An observation is a surveyed respondent per post-baseline survey round in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double lasso. Standard errors clustered at the enterprise level in parentheses. Brackets and the last five rows display two-sided p-values. Outcomes that were not pre-specified are denoted with $^{+}$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

area. Overall, effect sizes are roughly half to two-thirds of the size of impacts of the labeled grant. Mentorship had no discernible impacts on economic beliefs.

4.4 Cultural Attitudes in Uganda

Policy attitudes could also change due to updated cultural attitudes toward refugees. We find that labeled grant recipients in Uganda changed some of their cultural attitudes toward refugees, as shown in Table 4. Our cultural measures focus on "generalized" attitudes toward refugees as a group rather than toward the specific refugees respondents may have met as part of our experimental protocol. We observe a decrease in perceived social distance between respondents and refugees: the labeled grant increased the share who reported that they would be comfortable being close friends with a refugee by 7 pp., and marrying a refugee by 13 pp. (p-vals < 0.01). We do not observe significant changes in beliefs about the impact of refugees on Ugandan culture, or in whether refugees deserve sympathy. The impact on our pre-specified domain summary index is 0.16 sd. (p-val $= 0.01$). Adjustments for multiple hypothesis testing do not affect these conclusions, as shown in Appendix Table E10. As we discuss using additional analysis in Section 5.3, impacts on cultural attitudes toward refugees appear to be driven not by contact with refugees, but indirectly through effects on economic beliefs and policy views.

Our Information Only treatment modestly changed cultural attitudes toward refugees, though the impacts are generally small and inconsistent across outcomes. Our Grant Only treatment had modest impacts on cultural attitudes, generally of slightly smaller magnitude than—but not statistically significantly different from—the impacts of labeled grants.

Mentorship had no discernible impacts on cultural attitudes. Subjective assessments of the mentorship program were positive (see Section 4.1), and many mentees reported continuing to meet with their mentor after the program ended (see Appendix C.2). However, we see no impacts on an index of contact with refugees outside the program (Appendix Table E14). These findings are consistent with results from Baseler et al. (2024), which finds positive impacts of labeled grants on cultural attitudes and no impacts of a cross-nationality mentorship program involving weekly meetings over six months. They are also consistent with the common finding in the contact literature that attitudes toward individuals within the experiment change more easily than attitudes toward broader groups (Scacco and Warren, 2018, Mousa, 2020).

During our surveys, we conducted a simple dictator game in which the respondent distributed 3,000 UGX (about 0.80 USD) between themselves, a program that helps refugees in Kampala, and a program that helps poor Ugandans in Kampala. This offers a financially incentivized measure of positive attitudes toward refugees. Labeled grants increase the proportion donated to refugees by 5 pp. (on a base of 28%, p-val < 0.01). The Grant Only arm also increased the proportion donated, by 4 pp. (p-val $= 0.01$). Other treatment arms had no significant effects on the proportion donated.

4.5 Business Outcomes and Household Welfare in Uganda

Our treatment arms had small and insignificant impacts on business profit, business capital, business practices, and a summary index of household welfare, as shown in Appendix Table A9. Business profit earned over the month preceding the survey was slightly lower among grant recipients and owners mentored by Ugandans, by 2–3 USD on a base of 21

Table 4: Cultural Attitudes Toward Refugees—Uganda

	Cultural Attitudes Index	Comfortable Refugee Friends	Comfortable Refugee Spouse	Prop. Donated Refugees	Positive Effect on Culture	Refugees Deserve Sympathy
Info + Labeled Grant	0.16**	0.07***	0.13***	0.05***	-0.00	0.03
	(0.07)	(0.03)	(0.04)	(0.02)	(0.03)	(0.04)
	[0.01]	[0.01]	[0.00]	[0.00]	[1.00]	[0.44]
Information Only	0.06	0.07**	0.07*	-0.00	0.05*	0.04
	(0.06)	(0.03)	(0.04)	(0.02)	(0.03)	(0.04)
	[0.32]	[0.02]	[0.10]	[0.93]	[0.09]	[0.38]
Grant Only	0.13*	0.06**	0.07*	0.04***	-0.02	0.08**
	(0.07)	(0.03)	(0.04)	(0.02)	(0.03)	(0.04)
	[0.06]	[0.04]	[0.09]	[0.01]	[0.45]	[0.04]
Mentored by Refugee	-0.03	0.01	0.05	-0.02	0.02	-0.02
	(0.07)	(0.03)	(0.05)	(0.02)	(0.04)	(0.05)
	[0.69]	[0.85]	[0.27]	[0.29]	[0.51]	[0.69]
Mentored by Ugandan	0.03	0.04	0.02	-0.00	0.05	-0.02
	(0.07)	(0.03)	(0.05)	(0.02)	(0.03)	(0.04)
	[0.71]	[0.24]	[0.67]	[0.92]	[0.11]	[0.64]
Observations	3,061	1,942	1,942	3,061	2,612	1,814
Control Mean: Baseline	0.00	0.78	0.49	0.21	0.71	0.46
Control Mean: Follow-Ups	0.00	0.82	0.49	0.28	0.69	0.54
Lab. Grant = Info Only	0.10	0.82	0.12	0.00	0.09	0.91
Lab. Grant = Grant Only	0.55	0.49	0.16	0.77	0.45	0.18
Lab. Grant = R-Mentee	0.01	0.04	0.10	0.00	0.51	0.27
R-Mentee = Info Only	0.18	0.06	0.75	0.30	0.45	0.23
R-Mentee = U-Mentee	0.45	0.37	0.53	0.38	0.43	0.96

An observation is a surveyed respondent per post-baseline survey round in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double lasso. Standard errors clustered at the enterprise level in parentheses. Brackets and the last five rows display two-sided p-values. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

USD. While somewhat surprising, the impacts are not statistically significant, and may reflect the impact of COVID-19 lockdowns, which reduced the scope for making profit while also reducing the incentive to invest (rather than consume) the grant. Impacts on business capital are similarly noisy. We find modest impacts of grants and mentorship on our index of business practices—which we modify from McKenzie and Woodruff (2017)—though only the impact of grants alone is statistically significant at the 10% level. We estimate a positive but insignificant impact of grants on a summary index measure of household well-being.[23]

[23] Changes in labor supply can affect welfare impacts through the value of owners' time (Agness et al., 2022). We do not find significant differences in time use across treatment groups (see Appendix Table E16) and so do not make any adjustments.

5 Mechanisms Behind Changing Policy Views

Why does learning about aid-sharing—either through new information or by receiving a grant—increase support for refugee integration? In this section, we present additional evidence for or against specific mechanisms behind our results and consider whether grants acted on economic or cultural concerns about refugees. We rely primarily on secondary outcomes or treatment effect heterogeneity to adjudicate potential mechanisms and view this evidence as suggestive rather than definitive.

5.1 Information and Wealth Effects

We find evidence for three mechanisms behind impacts of labeled grants on policy views: knowledge of the redistribution policy, the credibility of that information, and resource resentment against refugees. We argue that our findings are difficult to reconcile with a large role for wealth effects.

Knowledge of Aid-Sharing. Learning about Uganda's existing aid-sharing policy through the Information Only arm, without any associated grant, led to significant and persistent impacts on support for refugee integration policies. Additionally, labeled grants had larger impacts on a summary index of support for refugee integration policies compared to unlabeled grants in both Uganda ($p = 0.05$) and Kenya ($p < 0.01$). This indicates that at least part of the impact of labeled grants operates purely through the information provided.[24] Our design cannot disentangle impacts of the economic content from the listening exercise of the Information Only arm, but as we discuss in Section 5.2, our data are most consistent with responses to the economic content.

Recipients of unlabeled grants were also more likely to report that international donations to refugees are shared with Ugandans (by 9 pp., $p = 0.01$). Our findings suggest that some of these business owners learned that the grant came from a refugee-led organization, lending an implicit labeling of the grant as associated with the refugee presence.[25] Although we intended to minimize associations with refugees in Grant Only, our implementing partner is a well-known refugee-led organization in Kampala, and some grant recipients either already knew about the organization or learned about it after the intervention. We see that owners in

[24]It may also be that labeled grants led participants to seek out additional information about refugees or about the policy—indeed, we observe an increase in reports that business owners are talking to others about refugees by 6 pp. in Grant Only and Information Only (p-vals = 0.10 and 0.12 respectively) and 8 pp. in Information + Labeled Grant ($p = 0.03$), as shown in Appendix Table A10.

[25]The same is true for the Mentored by Ugandan arm, which experienced positive impacts on policy support that did not persist. As shown in Appendix Table A10, business owners offered mentorship by a Ugandan were significantly more likely to report receiving support associated with refugees compared to Control, and may thus have viewed the free program as an example of aid-sharing.

the Grant Only treatment arm were more likely to report receiving support, and to associate that support with refugees, than the control group (though less than the Information + Labeled Grant group with $p = 0.04$, as shown in Table 3).[26] Taken together, these results support our interpretation that knowledge of aid-sharing affects policy views, and imply that simple differences in impacts between Grant Only and Information + Labeled Grant understate the role of knowledge of aid-sharing. They also imply that our Grant Only intervention combines any wealth effect of the grant with some of the effect of receiving aid from an organization associated with the refugee presence. In Section 5.1.1, we estimate a model to recover the wealth effects of grants.

Credibility. The effects of labeled grants on policy views were generally 50–100% greater than the effects of information about aid-sharing alone. Our results suggest that the direct receipt of aid makes the accompanying information more credible.[27] Recipients of labeled grants were more likely than those in Information Only to say that some of the aid from the international refugee response is shared with Ugandans (p-val on comparison < 0.01), as shown in Table 3, which is consistent either with a credibility or a salience effect. They were also more likely to say that international organizations are trustworthy compared to Information Only (diff. = 20 pp. on a base of 44%, p-val < 0.01), as shown in Appendix Table A11, which is most consistent with a credibility channel.[28]

Resource Resentment. Receiving aid appears to reduce what Zhou (2019) terms *resource resentment*, or negative views toward a group perceived to be receiving unfair levels of aid. As shown in Appendix Table A11, recipients of unlabeled grants were significantly less likely to report that refugees receive too much aid relative to Ugandans (15 pp. on a base of 77%, p-val < 0.01). This may be driven in part by changing attitudes toward aid organizations, as unlabeled grant recipients were more likely to say that local and international aid organizations care about them (by 10–11 pp., p-vals = 0.09 and 0.04 respectively) and are trustworthy (by 23 pp., p-val < 0.01). It may also be partly related to changing beliefs about the distribution of aid, as unlabeled grant recipients were 8 pp. less likely to say that refugees receive more aid than Ugandans (on a base of 71%, p-val = 0.14).[29]

[26] Surveyors and YARID staff reported that some grant recipients may have chosen not to report receiving aid because of concerns that the organization would ask for it back. This concern was specifically addressed when the program was introduced, but respondents noted that scams are widespread.

[27] This is related to Bauhoff and Kandpal (2024), who find that incentives in pay-for-performance contracts work by signaling which information is important, making information delivery more effective.

[28] While our implementing partner, YARID, is not an international non-profit, many Ugandans in Kampala associate the refugee presence with international organizations like UNHCR.

[29] Resource resentment cannot explain the larger impacts of labeled compared to unlabeled grants on policy views: if anything, labeled grant recipients were more likely to say that refugees receive too much aid compared to unlabeled grant recipients. This difference is possibly due to the information treatment increasing awareness or salience of aid toward refugees compared to receiving a grant alone.

In Kenya, where unlabeled grants were distributed without any link to refugees and where impacts were measured immediately (minimizing the potential for spillovers), we also observe positive impacts of grants on support for refugee integration. This implies that implicit labeling cannot be the sole factor driving impacts in Grant Only. While we do not have data to test the resource resentment channel directly in Kenya, it can explain the greater impacts of unlabeled grants in areas where average support for refugee integration is low (Appendix Table A7): if opposition to integration in these areas is partly due to resentment toward groups perceived to be major beneficiaries of aid, such as refugees, then we would expect receiving aid to reduce that resentment in areas where it is strongest. In contrast, a standard wealth-effects model is inconsistent with our findings in both countries, as we describe below.

Wealth Effects. In theory, changes in beliefs could be driven by wealth effects of the grant, for example by reducing feelings of scarcity and thus the salience of resource competition with refugees. Because unlabeled grants appear to change beliefs through implicit labeling in Uganda and resource resentment in Uganda and Kenya, we cannot identify wealth effects using our experimental design alone. Instead, we analyze heterogeneity in treatment impacts in both countries and build a simple model to estimate counterfactual treatment impacts net of wealth effects.

If wealth effects played a significant role in driving our results, we would expect to see poorer respondents at baseline—for whom changes in feelings of scarcity arising from the grant should be larger—exhibit greater treatment impacts of grants. We find, overall, the opposite result. In Uganda, baseline economic well-being does not predict treatment impacts of labeled or unlabeled grants (Appendix Tables A3 and A14). In Kenya, treatment impacts of unlabeled grants are greater in areas with *higher* average household consumption (Appendix Table A8).[30]

Two additional pieces of evidence suggest a small role for wealth effects. First, we find null effects of the grants on economic outcomes in Uganda, suggesting that the scope for wealth effects was limited there. Second, the size of the transfer in Kenya was small (one-time transfer of about 5% of monthly household consumption), again consistent with little scope for wealth effects. These results are consistent with the findings of our model, as described below.

[30]This finding may be due to the negative correlation between average sub-county support and household consumption in Kenya (correlation coefficient = -0.63), as treatment impacts of grants are greater in areas with less support (Appendix Table A7).

5.1.1 A Model to Separate Wealth Effects

To recover treatment impacts of labeled grants net of wealth effects, we build a simple structural model which separates wealth effects from other channels using treatment impacts on intermediate outcomes in Uganda. In the model, voters decide whether to support a policy as a function of their wealth, a joint measure of knowledge of aid-sharing and resource resentment, and a labeled-grant fixed effect capturing other channels such as beliefs about personal benefits. Intuitively, treatment impacts in the Information Only arm imply an expected relationship between changes in aid-sharing knowledge or resentment and policy views. The deviation from this relationship in Grant Only identifies the average wealth effect of the grant, while the deviation in Information + Labeled Grant identifies wealth effects plus any interaction effects between grants and information operating net of the wealth and awareness/resentment channels. Details on the model are in Appendix D.

Our model identifies a wealth effect that is small compared to impacts driven by knowledge of aid-sharing and resource resentment. Across all three policy support measures shown in Table 1 for which Grant Only impacts are significant—support for refugee hosting overall, for admitting more refugees, and for providing labor market access—estimated treatment impacts of labeled grants net of the wealth channel are 11–12 pp. (p-values < 0.01), while wealth effects are small and statistically insignificant (Appendix Table D1).

5.2 Other Potential Mechanisms

In this section, we summarize our tests of other potential explanations behind our results. Details are available in Appendix C.

Experimenter Demand Effects. A potential concern is that the observed changes in policy views are driven entirely by experimenter demand effects. For example, grant beneficiaries may be more likely to expect future assistance, which they may believe is tied to their survey responses. We designed our study to minimize potential demand effects: in both countries, respondents were reminded throughout each survey that their answers would remain anonymous and would not affect their eligibility for aid, and all grant recipients were told that the grant was a one-time transfer. In Uganda, data collection occurred during separate visits by a distinct organization from YARID.

Three pieces of evidence from our main results point to a limited role of demand effects in driving our results. First, we find significant impacts of labeled grants on support of a phone-call campaign which was not explicitly connected to the implementing or survey firms in any way (see Section 4.1). Second, we estimate a positive and statistically significant demand-free lower bound using the method of De Quidt, Haushofer and Roth (2018). Third, we find significant impacts on the share of an endowment donated to a program supporting refugees in an incentivized dictator game (Table 4).

In addition to these results, we conducted three further tests of demand effects: a placebo treatment informing grant recipients of YARID's position on an unrelated issue in Uganda, a survey experiment priming respondents about the grants in Uganda, and a test of whether labeled grants led respondents to expect future assistance in Kenya. We find no impacts across these three tests, as discussed in Appendix C.1.

Contact With Refugees. We find no evidence that treatment impacts are driven by contact with refugees as program facilitators or through increased contact with refugees outside of our programs: we see no differences in treatment impacts by facilitator nationality or treatment impacts on contact with refugees outside the programs.

Reciprocity to YARID. Our findings are not consistent with an intrinsic reciprocity effect to the implementing organization in Uganda (Finan and Schechter, 2012). Information Only increased support for refugee integration policies despite involving no material support, and the placebo campaign did not affect business owners' attitudes toward the placebo issue (child labor), even among grant recipients.

Personal vs. Group Benefits. Compared to information alone, receiving a labeled grant could change beliefs about whether the benefits of aid-sharing accrue primarily to the respondent personally as opposed to Ugandan society more broadly. Our results do not support this alternative: treatment impacts on beliefs about personal economic benefits and broader economic benefits are roughly proportionate across Information + Labeled Grant and Information Only (see Table 3). Additionally, we do not observe significantly different treatment impacts of Information Only in Uganda based on whether the respondent uses the public goods mentioned in our script—hospital or schools—which represents potential future personal benefits. This is consistent with the results of Hainmueller and Hopkins (2014), who find a dominant role for perceived group-level impacts in driving immigration views.

Differential Attrition. While we experienced moderate differential round-by-round attrition in our Uganda experiment, respondents who could not be surveyed after the baseline (9% of the sample) are balanced across treatment groups, randomization balance appears to hold among those surveyed after baseline, and our findings are robust to reweighting by the inverse probability of being surveyed in each round. The 95% confidence interval on the lower Lee bound does not cross zero for the impacts of Information + Labeled Grant and Information Only on support for integration policies, although the bounds are wide for many outcomes. In Kenya, our attrition rate is low and balanced across treatment groups. These findings point to a limited role of differential attrition in influencing our main results.

Spillovers. To test for spillovers in the Uganda experiment, we asked treated respondents whether they had talked with other business owners about the program. We also measured whether Control and Grant Only respondents had heard from program participants that YARID was associated with refugees. Both of these rates were low, consistent with limited spillovers in our experiment.

Altruism Crowd-Out. We do not find evidence that redistribution crowds out other sources of policy support such as altruism. The positive impact of labeled grants on the share donated to refugees in an incentivized dictator game suggests that aid-sharing facilitates, rather than crowds out, altruism.

Degree and Nature of Inter-Group Contact. Would a more intensive contact intervention, or a program facilitating peer-to-peer rather than mentor-to-mentee interactions, have produced more persistent impacts on policy views? In Appendix C.8, we exploit random variation in the start date of the mentorship program to test whether business owners who starting their program earlier—and who therefore had more in-person meetings and more meetings overall—experienced persistent changes in policy views. We find that they did not: while initial impacts on views in this group were large, these impacts faded nearly to zero over time (see Appendix Table A15). Consistent with this interpretation, Ugandans assigned to the more intensive cross-nationality mentorship groups of Baseler et al. (2024)—which involved weekly, peer-to-peer in-person meetings with both refugee mentors and refugee peers over six months—did not change their views on refugee integration policies or cultural views.

Content of Information Scripts. The scripts we used in both experiments combined information about economic benefits of refugee hosting (or hypothetical benefits, in Kenya) with introductory content explaining who refugees are, why they migrate, and—in the Ugandan experiment—a listening exercise inviting the respondent to share their views of refugees. Our design identifies only the joint impact of these components. However, the large impacts of Information Only and Information + Labeled Grant on beliefs about refugees' economic impacts on Uganda, combined with the null effects of these arms on beliefs about whether refugees deserve sympathy, suggest that Ugandans responded primarily to the economic content of the script. Additionally, the design of Baseler et al. (2024) included grants with information about aid-sharing but not a listening exercise and found similar impacts on policy preferences.

5.3 Economic Versus Cultural Beliefs

A large literature examines whether attitudes toward immigrants are driven more by economic or cultural beliefs. We find that cultural concerns are a stronger predictor of policy views at baseline compared to economic concerns in Uganda (see Appendix Table A13), consistent with other settings (Tabellini, 2020).[31] Nevertheless, we find that policy views respond more to our economic interventions—grants and information—than to our more cultural intervention—contact with a refugee—and that once policy and economic views shift, cultural attitudes follow.

Heterogeneous Impacts By Baseline Economic or Cultural Concerns. We find that all of our interventions had greater impacts on the policy views of Ugandans with *either* above-median economic or cultural concerns about refugee hosting at baseline, as shown in Appendix Table A3. To further assess the relative importance of economic and cultural views in mediating treatment impacts, we examine specifications that interact treatment indicators separately with baseline measures of economic and cultural views and baseline economic well-being, presented in Appendix Table A14.

We find that greater economic concerns about refugees at baseline consistently predict stronger treatment effects of both grants and information, even when controlling for cultural concerns, economic-well being, and their interactions with treatment indicators. In the specification that includes all three dimensions of heterogeneity and their interactions, we find that economic concerns predict treatment impacts to the greatest degree across both grant arms and Information Only. Baseline cultural concerns also consistently predict stronger treatment effects across specifications. There is some evidence of stronger treatment effects among those with better baseline economic well-being, which is consistent with our finding that wealth effects are small.

Interpretation. The concentrated treatment impacts among those with economic concerns about refugee integration are unsurprising given that the information was focused on economic policy and the grant is itself an economic intervention. The concentrated impacts among those with cultural concerns are hard to reconcile with a heuristic in which only cultural interventions affect culturally rooted policy opposition. However, they are consistent with prior research on the role of financial instruments that create incentives for peaceful coexistence in reducing inter-group conflict (Jha, 2012, 2013, Jha and Shayo, 2019). In our setting, aid-sharing appears to act as such an instrument by supporting an equilibrium

[31] We pre-specify as cultural those determinants of immigration views that are not about economic impacts. For example, we group perceived social distance, perceived impacts on host country culture, and altruism as cultural mechanisms potentially influencing immigration policy preferences.

that dominates the *status quo* for both groups. Less formally, the tendency to divide into tribes—proxied by baseline cultural concerns about refugees—is muted by the introduction of financial incentives for integration.

Why Did Grants Affect Cultural Views? As discussed in Section 5.2, we do not find that grants increased contact with refugees. Rather, our findings suggest that impacts on cultural attitudes appear as a rationalization of changing economic and policy beliefs: once our interventions had changed policy views, cultural concerns were vestigial and could be dropped. As shown in Appendix Table A15, we find that impacts on cultural attitudes lag other impacts: while there were large and significant impacts of labeled grants on preferences for integration and economic beliefs about refugees in the first follow-up survey (0.33 and 0.22 sd., p-vals < 0.01 and $= 0.03$ respectively), we find no impact on cultural attitudes at that time (coeff. $= 0.04$ sd.). In subsequent surveys, we observe significant impacts of labeled grants on all three of these domains, and can reject that impacts on cultural attitudes were equivalent to estimated first-round impacts in the 16-month survey (p-val $= 0.05$) and the pooled 16- and 26-month surveys (p-val $= 0.03$). Effects in the Information Only arm display a similar pattern. While other explanations including experimenter demand effects are possible, the delayed timing of these impacts is suggestive of cultural attitudes that are partly rationalized from changing economic and policy views, possibly to reduce the cognitive dissonance involved with holding positive economic but negative cultural views toward refugees.

6 Discussion

We provide experimental evidence from two countries that compensation—redistributing the aggregate gains from a policy—can influence political views and build support for a policy bargain. We find substantial impacts of both a large grant in Uganda and a small grant in Kenya, indicating that both large, targeted and small, distributed compensation policies could in principle form part of a policy bargain incorporating aid-sharing and refugee integration. While policy bargains are common, we provide evidence that when the connection between an economic intervention and the policy is clear, compensation can influence views regardless of whether opposition is cultural or economic.

The apparently long-term change in views caused by our programs is difficult to reconcile with a basic *quid pro quo* model in which support for hosting is granted in exchange for direct cash compensation, since our grant interventions involved only one-time transfers. Rather, we believe that policy views are likely to be closely related to beliefs about fairness. Sharing aid from the refugee response may alleviate some natives' concerns that the costs of hosting have been placed upon them unfairly. Such an explanation could help rationalize the surprising finding that small, unlabeled transfers from an unknown organization increased support for refugee hosting in Kenya. We see further exploration into how beliefs about fairness influence natives' attitudes and policy views as a promising avenue for future research.

The programs we study have the potential to scale in low- and middle-income host countries. Many organizations that assist refugees already include host community members in their programs, but few that we are aware of directly connect assistance to the refugee presence. The marginal cost of delivering this information on top of an existing intervention would likely be minimal, as it was in our context. Beyond countries where aid-sharing exists alongside labor market access for refugees, such as Uganda and Jordan, our results are informative for countries on the margin of adopting major aid-sharing policy bargains, such as Kenya and Ethiopia (Miller and Kitenge, 2023). Additional funding is available for host countries that are willing to facilitate refugees' integration, as reflected in the UN's Global Compact on Refugees. For instance, the World Bank's Window for Host Communities and Refugees has allocated 6.6 billion USD for long-term programming in host countries with "adequate" policy frameworks for refugees (World Bank, 2022). Our work suggests that an information campaign to connect public goods investments to the refugee presence and integration policies could increase support for these policies and positive attitudes toward refugees. Our results in Kenya also imply that even a small amount of money can have large impacts on attitudes. Future research could further explore the role of transfer size.

Future research in high-income contexts, where asylum-seekers' access to labor markets is also often limited, could test compensation programs that redistribute public finances. Marbach, Hainmueller and Hangartner (2018) find, for instance, that an employment ban on asylum-seekers in Germany cost 40 million Euros annually in services and foregone taxes. The apparent role of cultural or high-level economic concerns in shaping policy views in our setting mirrors findings from the US and Europe (Hainmueller and Hopkins, 2014), suggesting that our findings may extend beyond the contexts we study.

Finally, our findings could also apply to other public policy issues like economic immigration and international trade. Clemens (2011) estimates that reducing legals barriers to immigration could yield trillions of dollars in global aggregate gains, but this policy often faces strong political opposition. Freeman (2006), Edelberg and Watson (2022) and Lokshin and Ravallion (2022) among others propose strategies to reallocate some of these gains to potential losers and political opponents, including taxation of immigrants and proportional funding of local public goods. Our results suggest that redistributing these gains is a promising avenue to build support for a new political economy equilibrium that both improves aggregate welfare and mitigates, or reverses, the policy's harms.

References

Agness, Daniel J, Travis Baseler, Sylvain Chassang, Pascaline Dupas, and Erik Snowberg. 2022. "Valuing the time of the self-employed." National Bureau of Economic Research.

AGORA. 2018. "Understanding the Needs of Urban Refugees and Host Communities Residing in Vulnerable Neighborhoods of Kampala."

Ahrens, Achim, Christian B Hansen, and Mark Schaffer. 2019. "PDSLASSO: Stata module for post-selection and post-regularization OLS or IV estimation and inference."

Aksoy, Cevat Giray, Thomas Ginn, and Franco Malpassi. 2022. "Attitudes and Policies toward Refugees."

Alesina, Alberto, and Marco Tabellini. 2024. "The Political Effects of Immigration: Culture or Economics?" *Journal of Economic Literature*, 62(1): 5–46.

Alesina, Alberto, Elie Murard, and Hillel Rapoport. 2021. "Immigration and preferences for redistribution in Europe." *Journal of Economic Geography*, 21(6): 925–954.

Allport, Gordon Willard. 1954. *The Nature of Prejudice.* Addison-Wesley: Reading, MA.

Alrababa'h, Ala', Andrea Dillon, Scott Williamson, Jens Hainmueller, Dominik Hangartner, and Jeremy Weinstein. 2021. "Attitudes toward migrants in a highly impacted economy: Evidence from the Syrian refugee crisis in Jordan." *Comparative Political Studies*, 54(1): 33–76.

Anderson, Michael L. 2008. "Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects." *Journal of the American Statistical Association*, 103(484): 1481–1495.

Ash, Nazanin, and Cindy Huang. 2018. "Using the Compact Model to Support Host States and Refugee Self-reliance."

Autor, David, David Dorn, Gordon Hanson, and Kaveh Majlesi. 2020. "Importing political polarization? The electoral consequences of rising trade exposure." *American Economic Review*, 110(10): 3139–3183.

Autor, David H., David Dorn, and Gordon H. Hanson. 2013. "The China Syndrome: Local Labor Market Effects of Import Competition in the United States." *American Economic Review*, 103(6): 2121–68.

Bahar, Dany, Ana María Ibáñez, and Sandra V Rozo. 2021. "Give me your tired and your poor: Impact of a large-scale amnesty program for undocumented refugees." *Journal of Development Economics*, 151: 102652.

Barnett-Howell, Zachary, Travis Baseler, and Thomas Ginn. 2023. "The Role of Information and Networks in Migration." AEA RCT Registry.

Baseler, Travis, and Thomas Ginn. 2024. "Can Redistribution Change Policy Views? Evidence from Kenya." AEA RCT Registry.

Baseler, Travis, Thomas Ginn, Ibrahim Kasirye, Belinda Muya, and Andrew Zeitlin. 2024. "re:Build: Cash grants and mentorship to strengthen refugee economic and social integration in Uganda."

Baseler, Travis, Thomas Ginn, Robert Hakiza, Helidah Ogude-Chambert, and Olivia Woldemikael. 2022. "Can Aid Change Attitudes Toward Refugees? Experimental Evidence from Uganda." AEA RCT Registry.

Bauhoff, Sebastian, and Eeshani Kandpal. 2024. "Pay-for-Performance Contracts in the Lab and the Real World: Evidence from Nigeria."

Beltramo, Theresa, Nimoh Florence, Matthew O'Brien, and Sandra Sequeira. 2024. "Financial Security, Climate Shocks and Social Cohesion."

Blattman, Christopher, Mathilde Emeriau, and Nathan Fiala. 2018. "Do anti-poverty programs sway voters? Experimental evidence from Uganda." *Review of Economics and Statistics*, 100(5): 891–905.

Bonomi, Giampaolo, Nicola Gennaioli, and Guido Tabellini. 2021. "Identity, Beliefs, and Political Conflict*." *The Quarterly Journal of Economics*, 136(4): 2371–2411.

Brell, Courtney, Christian Dustmann, and Ian Preston. 2020. "The Labor Market Integration of Refugee Migrants in High-Income Countries." *Journal of Economic Perspectives*, 34(1): 94–121.

Brooks, Wyatt, Kevin Donovan, and Terence R Johnson. 2018. "Mentors or teachers? Microenterprise training in Kenya." *American Economic Journal: Applied Economics*, 10(4): 196–221.

Bursztyn, Leonardo, Thomas Chaney, Tarek A. Hassan, and Aakaash Rao. 2024. "The Immigrant Next Door." *American Economic Review*, 114(2): 348–84.

Cai, Jing, and Adam Szeidl. 2018. "Interfirm relationships and business performance." *The Quarterly Journal of Economics*, 133(3): 1229–1282.

Chernozhukov, Victor, Denis Chetverikov, Mert Demirer, Esther Duflo, Christian Hansen, Whitney Newey, and James Robins. 2018. "Double/debiased machine learning for treatment and structural parameters." *The Econometrics Journal*, 21: C1–C68.

Clemens, Michael A. 2011. "Economics and emigration: Trillion-dollar bills on the sidewalk?" *Journal of Economic Perspectives*, 25(3): 83–106.

Corno, Lucia, Eliana La Ferrara, and Justine Burns. 2022. "Interaction, stereotypes, and performance: Evidence from South Africa." *American Economic Review*, 112(12): 3848–75.

De Quidt, Jonathan, Johannes Haushofer, and Christopher Roth. 2018. "Measuring and bounding experimenter demand." *American Economic Review*, 108(11): 3266–3302.

Di Tella, Rafael, and Dani Rodrik. 2020. "Labour market shocks and the demand for trade protection: Evidence from online surveys." *The Economic Journal*, 130(628): 1008–1030.

Dustmann, Christian, Kristine Vasiljeva, and Anna Piil Damm. 2019. "Refugee migration and electoral outcomes." *The Review of Economic Studies*, 86(5): 2035–2091.

Edelberg, Wendy, and Tara Watson. 2022. "A More Equitable Distribution of the Positive Fiscal Benefits of Immigration." *The Hamilton Project, Brookings Institute*.

Ehrlich, Sean D., and Eddie Hearn. 2014. "Does Compensating the Losers Increase Support for Trade? An Experimental Test of the Embedded Liberalism Thesis." *Foreign Policy Analysis*, 10(2): 149–164.

Enos, Ryan D., and Noam Gidron. 2018. "Exclusion and Cooperation in Diverse Societies: Experimental Evidence from Israel." *American Political Science Review*, 112(4): 742–757.

Esipova, Neli, John Fleming, and Julie Ray. 2017. "New Index Shows Least-, Most-Accepting Countries for Migrants."

Evans, David K, Brian Holtemeyer, and Katrina Kosec. 2019. "Cash transfers increase trust in local government." *World Development*, 114: 138–155.

Fafchamps, Marcel, and Simon Quinn. 2018. "Networks and manufacturing firms in Africa: Results from a randomized field experiment." *The World Bank Economic Review*, 32(3): 656–675.

Fafchamps, Marcel, David McKenzie, Simon Quinn, and Christopher Woodruff. 2014. "Microenterprise growth and the flypaper effect: Evidence from a randomized experiment in Ghana." *Journal of Development Economics*, 106: 211–226.

Fernandez, Raquel, and Dani Rodrik. 1991. "Resistance to Reform: Status Quo Bias in the Presence of Individual- Specific Uncertainty." *The American Economic Review*, 81(5): 1146–1155.

Finan, Frederico, and Laura Schechter. 2012. "Vote-Buying and Reciprocity." *Econometrica*, 80(2): 863–881.

Finan, Frederico, and Maurizio Mazzocco. 2020. "Electoral Incentives and the Allocation of Public Funds." *Journal of the European Economic Association*, 19(5): 2467–2512.

Freeman, Richard B. 2006. "People flows in globalization." *Journal of Economic Perspectives*, 20(2): 145–170.

Ginn, Thomas. 2023. "Labor Market Access and Outcomes for Refugees." *World Bank - UNHCR Joint Data Center.*

Ginn, Thomas, Reva Resstack, Helen Dempster, Emily Arnold-Fernández, Sarah Miller, Martha Guerrero Ble, and Bahati Kanyamanza. 2022. "2022 Global Refugee Work Rights Report." *Center for Global Development, Refugees International, and Asylum Access.*

Grossman, Gene M, and Elhanan Helpman. 2021. "Identity politics and trade policy." *The Review of Economic Studies*, 88(3): 1101–1126.

Guiteras, Raymond P, and Ahmed Mushfiq Mobarak. 2019. "Does development aid undermine political accountability? Leader and constituent responses to a large-scale intervention."

Hainmueller, Jens. 2012. "Entropy Balancing for Causal Effects: A Multivariate Reweighting Method to Produce Balanced Samples in Observational Studies." *Political Analysis*, 20(1): 25–46.

Hainmueller, Jens, and Daniel J Hopkins. 2014. "Public attitudes toward immigration." *Annual Review of Political Science*, 17: 225–249.

Heß, Simon. 2017. "Randomization inference with Stata: A guide and software." *Stata Journal*, 17(3): 630–651.

Hussam, Reshmaan N, Erin M Kelley, Gregory V Lane, and Fatima T Zahra. 2022. "The Psychosocial Value of Employment: Evidence from a Refugee Camp." *American Economic Review*, 112(11): 3694–3724.

IRC. 2018*a*. "Kenya: Citizens' Perceptions on Refugees."

IRC. 2018*b*. "Uganda: Citizens' Perceptions on Refugees."

Jha, Saumitra. 2012. "Sharing the Future: Financial Innovation and Innovators in Solving the Political Economy Challenges of Development." In *Institutions and Comparative Economic Development. International Economic Association Series*, , ed. Masahiko Aoki, Timur Kuran and Gérard Roland, Chapter 7, 131–151. Palgrave Macmillan.

Jha, Saumitra. 2013. "Trade, Institutions, and Ethnic Tolerance: Evidence from South Asia." *American Political Science Review*, 107(4): 806–832.

Jha, Saumitra, and Moses Shayo. 2019. "Valuing Peace: The Effects of Financial Market

Exposure on Votes and Political Attitudes." *Econometrica*, 87(5): 1561–1588.

Kalla, Joshua L, and David E Broockman. 2020. "Reducing Exclusionary Attitudes through Interpersonal Conversation: Evidence from Three Field Experiments." *American Political Science Review*, 114(2): 410–425.

Kim, Sung Eun, and Krzysztof J. Pelc. 2021. "The Politics of Trade Adjustment Versus Trade Protection." *Comparative Political Studies*, 54(13): 2354–2381.

Kreibaum, Merle. 2016. "Their suffering, our burden? How Congolese refugees affect the Ugandan population." *World Development*, 78: 262–287.

Lehmann, M Christian, and Daniel TR Masterson. 2020. "Does aid reduce anti-refugee violence? Evidence from Syrian refugees in Lebanon." *American Political Science Review*, 114(4): 1335–1342.

Likert, Rensis. 1932. "A technique for the measurement of attitudes." *Archives of Psychology.*

Loiacono, Francesco, and Mariajose Silva-Vargas. 2023. "Matching with the right attitude: the effect of matching firms with refugee workers."

Lokshin, Michael, and Martin Ravallion. 2022. "A market for work permits." *Economic Policy*, 37(111): 471–499.

Lowe, Matt. 2021. "Types of contact: A field experiment on collaborative and adversarial caste integration." *American Economic Review*, 111(6): 1807–44.

Lyall, Jason, Yang-Yang Zhou, and Kosuke Imai. 2020. "Can economic assistance shape combatant support in wartime? Experimental evidence from Afghanistan." *American Political Science Review*, 114(1): 126–143.

Marbach, Moritz, Jens Hainmueller, and Dominik Hangartner. 2018. "The long-term impact of employment bans on the economic integration of refugees." *Science Advances*, 4(9): eaap9519.

Mayda, Anna Maria, Giovanni Peri, and Walter Steingress. 2022. "The Political Impact of Immigration: Evidence from the United States." *American Economic Journal: Applied Economics*, 14(1): 358–89.

McKenzie, David. 2012. "Beyond baseline and follow-up: The case for more T in experiments." *Journal of Development Economics*, 99(2): 210–221.

McKenzie, David, and Christopher Woodruff. 2017. "Business practices in small firms in developing countries." *Management Science*, 63(9): 2967–2981.

Miller, Sarah, and David Kitenge. 2023. "Kenya's Bold New Shirika Refugee Plan is Model for Future."

Monteith, William, and Shuaib Lwasa. 2017. "The participation of urban displaced populations in (in) formal markets: contrasting experiences in Kampala, Uganda." *Environment and Urbanization*, 29(2): 383–402.

Mousa, Salma. 2020. "Building social cohesion between Christians and Muslims through soccer in post-ISIS Iraq." *Science*, 369(6505): 866–870.

OECD. 2023. "Development Finance for Refugee Situations: Volume and Trends, 2020–2021."

Paluck, Elizabeth Levy, Seth A Green, and Donald P Green. 2019. "The contact hypothesis re-evaluated." *Behavioural Public Policy*, 3(2): 129–158.

Pavanello, Sara, Carol Watson, W Onyango-Ouma, and Paul Bukuluki. 2016. "Effects of cash transfers on community interactions: emerging evidence." *The Journal of*

Development Studies, 52(8): 1147–1161.

Pettigrew, Thomas F, and Linda R Tropp. 2006. "A meta-analytic test of intergroup contact theory." *Journal of Personality and Social Psychology*, 90(5): 751.

Quattrochi, John, Ghislain Bisimwa, Peter Van der Windt, and Maarten Voors. 2021. "Effect of an Economic Transfer Program on Mental Health of Displaced Persons and Host Populations in Democratic Republic of Congo: A Randomised Controlled Trial."

Ruggie, John Gerard. 1982. "International regimes, transactions, and change: embedded liberalism in the postwar economic order." *International Organization*, 36(2): 379–415.

Scacco, Alexandra, and Shana 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.

Schuettler, Kirsten, and Laura Caron. 2020. "Jobs interventions for refugees and internally displaced persons."

Tabellini, Marco. 2020. "Gifts of the immigrants, woes of the natives: Lessons from the age of mass migration." *The Review of Economic Studies*, 87(1): 454–486.

UBOS. 2021. "Uganda National Household Survey 2019/2020."

UBOS. 2024. "National Population and Housing Census: Preliminary Results."

UNHCR. 2018. "Uganda Country Refugee Response Plan."

UNHCR. 2024*a*. "Global Trends: Forced Displacement in 2023."

UNHCR. 2024*b*. "Operational Data Portal: Kenya." Accessed August 2, 2024.

UNHCR. 2024*c*. "Uganda Comprehensive Refugee Response Portal." Accessed August 2, 2024.

Valli, Elsa, Amber Peterman, and Melissa Hidrobo. 2019. "Economic transfers and social cohesion in a refugee-hosting setting." *The Journal of Development Studies*, 55(1): 128–146.

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

Westfall, Peter H., and S. Stanley Young. 1993. *Resampling-Based Multiple Testing: Examples and Methods for p-Value Adjustment. Wiley Series in Probability and Statistics*, New York:Wiley.

World Bank. 2022. "10 Things to Know About the Window for Host Communities and Refugees."

Zhou, Yang-Yang. 2019. "How Refugee Resentment Shapes National Identity and Citizen Participation in Africa."

Zhou, Yang-Yang, and Guy Grossman. 2022. "When Refugee Presence Increases Incumbent Support through Development."

Zhou, Yang-Yang, and Jason Lyall. 2024. "Prolonged Contact Does Not Reshape Locals' Attitudes toward Migrants in Wartime Settings: Experimental Evidence from Afghanistan." *American Journal of Political Science*.

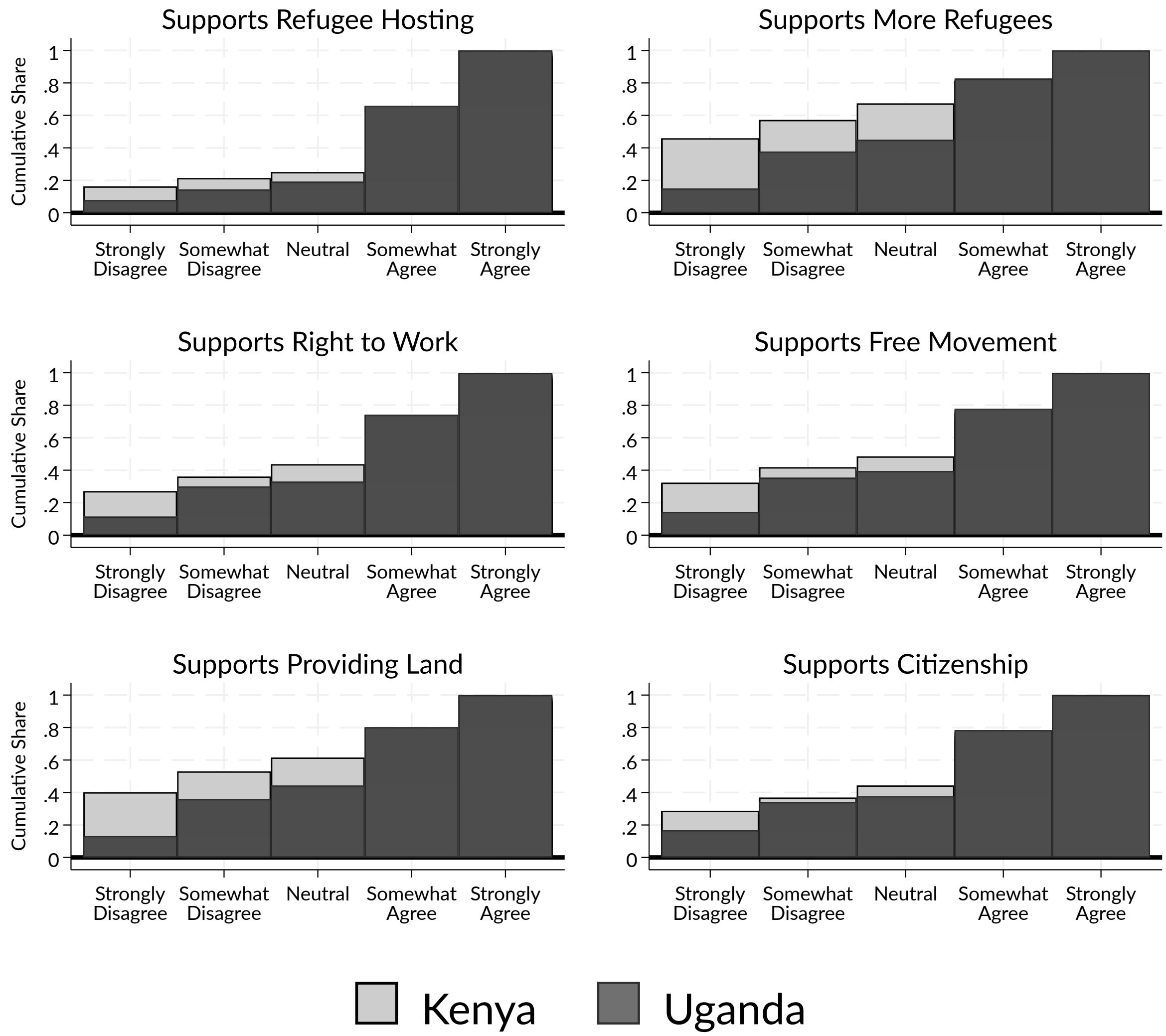
Zhou, Yang-Yang, Guy Grossman, and Shuning Ge. 2023. "Inclusive Refugee-Hosting can Improve Local Development and Prevent Public Backlash." *World Development*, 166(106203).

Appendix for "Can Redistribution Change Policy Views? Aid and Attitudes Toward Refugees"

Contents

A Additional Figures and Tables

Figure A1: Comparison of Control-Group Policy Views: Uganda and Kenya



Notes: Each plot shows the cumulative distribution function of a 5-point Likert measure of support for refugee hosting (with support increasing along the x-axis) separately in Uganda and Kenya. Support in Uganda first-order stochastically dominates that in Kenya for each measure. Uganda sample includes the control group in our final survey in March 2022; Kenya sample includes the control group in our first survey in April 2024.

Table A1: Study Timeline

	2019			2020				2021				2022	2024	
	Oct	Nov	Dec	Jan	Feb	Mar	Oct	Mar	Apr	May	Aug	Mar	Apr	May
Intervention Activities:														
Grant & Info Visits				■	■	■		■	■	■				
Mentorship Meetings (In-Person)				■	■	■								
Mentorship Meetings (Remote)								■	■	■				
Grants & Info Visits (Kenya)													■	
Data Collection Activities:														
Census	■													
Baseline Survey		■	■											
Follow-Up 1: 9 Months, Phone							■							
Follow-Up 2: 16 Months, In-Person										■				
Follow-Up 3: 19 Months, Phone											■			
Follow-Up 4: 26 Months, In-Person												■		
Immediate Impacts: In-Person (Kenya)													■	
Follow-Up: 1 Month, Phone (Kenya)														■

Months involving intervention or study activities are shaded in gray. Months with no activities are not shown.

Table A2: Baseline Summary Statistics

	Mean	Standard Deviation	N
Owner and Business Characteristics			
Age (Years)	27.5	5.34	1,405
Education (Years)	10.7	3.24	1,406
Female	0.68	0.47	1,406
Tailor	0.45	0.50	1,406
Experience in Sector (Years)	2.38	1.32	1,406
Profit (USD/Month)	37.0	35.7	1,406
Has Any Employees	0.22	0.42	1,406
Refugee Integration Policy Views			
Aware of Aid-Sharing	0.19	0.39	1,406
Supports Refugee Hosting	0.72	0.45	1,406
Supports More Refugees	0.52	0.50	1,406
Supports Freedom of Movement	0.58	0.49	1,406
Supports Right to Work	0.60	0.49	1,406
Economic Beliefs			
Refugees Increase Rents	0.78	0.41	1,312
Refugees Increase Goods Prices	0.62	0.48	1,313
Refugees Worsen Public Goods	0.27	0.45	1,300
Refugees' Economic Effect is Positive	0.53	0.50	1,334

Source: Baseline surveys of experimental sample in Uganda. Questions on refugees' impacts on prices and public goods are asked about Congolese and Somalis, and are coded as 1 if either answer is "Yes." "Don't Know" responses are coded as missing.

Table A3: Heterogeneity in Impacts on Support for Refugee Integration Policies

	Female Owner	Refugee Facilitator	Business Profit	Supports Hosting Index	Economic Beliefs Index	Cultural Attitudes Index	Household Well-Being Index	Contact Refugees (Choice)	Contact Refs. (Circumstance)	Knows About Aid-Sharing	Mentor Profit
Info. + Labeled Grant × X	0.06	-0.01	-0.15	-0.30**	-0.31**	-0.28**	0.04	0.11	0.18	-0.06	
	(0.14)	(0.09)	(0.12)	(0.13)	(0.13)	(0.13)	(0.13)	(0.15)	(0.13)	(0.16)	
	[0.68]	[0.88]	[0.22]	[0.02]	[0.02]	[0.03]	[0.74]	[0.47]	[0.17]	[0.71]	
Info. + Labeled Grant	0.32***	0.37***	0.43***	0.54***	0.53***	0.51***	0.34***	0.28**	0.25**	0.38***	0.36***
	(0.12)	(0.09)	(0.09)	(0.11)	(0.10)	(0.10)	(0.09)	(0.13)	(0.10)	(0.07)	(0.06)
	[0.01]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.03]	[0.01]	[0.00]	[0.00]
Information Only × X	0.22	0.06	-0.20	-0.21	-0.29**	-0.29**	0.05	0.13	0.11	-0.02	
	(0.15)	(0.09)	(0.13)	(0.13)	(0.13)	(0.13)	(0.13)	(0.16)	(0.14)	(0.16)	
	[0.13]	[0.49]	[0.12]	[0.11]	[0.03]	[0.03]	[0.70]	[0.42]	[0.41]	[0.92]	
Information Only	0.08	0.18**	0.32***	0.35***	0.39***	0.37***	0.20**	0.12	0.15	0.23***	0.22***
	(0.13)	(0.09)	(0.09)	(0.11)	(0.10)	(0.11)	(0.09)	(0.14)	(0.11)	(0.07)	(0.07)
	[0.55]	[0.04]	[0.00]	[0.00]	[0.00]	[0.00]	[0.03]	[0.38]	[0.15]	[0.00]	[0.00]
Grant Only × X	0.01		-0.16	-0.21	-0.35***	-0.31**	-0.12	-0.15	-0.07	-0.12	
	(0.14)		(0.13)	(0.13)	(0.13)	(0.13)	(0.13)	(0.15)	(0.13)	(0.15)	
	[0.93]		[0.23]	[0.12]	[0.01]	[0.02]	[0.35]	[0.31]	[0.63]	[0.42]	
Grant Only	0.24**	0.24***	0.32***	0.37***	0.43***	0.41***	0.29***	0.35***	0.29***	0.27***	0.25***
	(0.12)	(0.07)	(0.09)	(0.11)	(0.11)	(0.10)	(0.08)	(0.13)	(0.11)	(0.08)	(0.07)
	[0.04]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.01]	[0.01]	[0.00]	[0.00]
Mentored by Refugee × X	-0.00		-0.21	-0.18	-0.30**	-0.20	0.00	0.00	0.04	-0.04	-0.04
	(0.16)		(0.15)	(0.15)	(0.15)	(0.14)	(0.14)	(0.16)	(0.15)	(0.17)	(0.10)
	[0.99]		[0.15]	[0.21]	[0.04]	[0.16]	[0.98]	[0.99]	[0.78]	[0.81]	[0.70]
Mentored by Refugee	0.12	0.12*	0.22**	0.23**	0.29**	0.23**	0.11	0.12	0.08	0.13	0.13
	(0.13)	(0.07)	(0.10)	(0.11)	(0.12)	(0.11)	(0.10)	(0.14)	(0.12)	(0.08)	(0.09)
	[0.35]	[0.09]	[0.02]	[0.05]	[0.02]	[0.04]	[0.25]	[0.37]	[0.49]	[0.11]	[0.13]
Mentored by Ugandan × X	0.07		-0.40***	-0.16	-0.31**	-0.30*	-0.08	-0.02	0.10	-0.03	-0.05
	(0.16)		(0.15)	(0.15)	(0.15)	(0.15)	(0.15)	(0.17)	(0.16)	(0.17)	(0.11)
	[0.67]		[0.01]	[0.30]	[0.04]	[0.05]	[0.61]	[0.89]	[0.53]	[0.84]	[0.66]
Mentored by Ugandan	0.06	0.10	0.27***	0.18	0.27**	0.26**	0.15	0.12	0.04	0.11	0.13
	(0.13)	(0.07)	(0.10)	(0.12)	(0.12)	(0.13)	(0.10)	(0.15)	(0.13)	(0.09)	(0.09)
	[0.66]	[0.18]	[0.01]	[0.13]	[0.02]	[0.04]	[0.14]	[0.43]	[0.76]	[0.21]	[0.16]
X	-0.18		0.25**	0.25**	0.31***	0.19*	0.03	0.11	-0.08	0.09	
	(0.15)		(0.12)	(0.12)	(0.11)	(0.11)	(0.11)	(0.13)	(0.16)	(0.11)	
	[0.23]		[0.04]	[0.04]	[0.01]	[0.09]	[0.79]	[0.40]	[0.63]	[0.44]	
Observations	3,051	3,051	3,051	3,051	3,051	3,051	3,051	3,051	3,051	3,051	3,051

The dependent variable for each column is the integration policies summary index. Each column title lists the dimension of baseline heterogeneity (X) that is analyzed in the regression. X denotes above median values for continuous variables. An observation is a surveyed respondent per post-baseline survey round in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double lasso. Standard errors clustered at the enterprise level in parentheses; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Strongly Support and Strongly Oppose Inclusive Policies

	Strongly Supports Refugee Hosting+	Strongly Opposes Refugee Hosting+	Strongly Supports More Refugees+	Strongly Opposes More Refugees+	Strongly Supports Freedom of Movement+	Strongly Opposes Freedom of Movement+	Strongly Supports Right to Work+	Strongly Opposes Right to Work+
Info. + Labeled Grant	0.10***	-0.06***	0.06**	-0.05**	0.08***	-0.04*	0.08***	-0.07***
	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)
	[0.00]	[0.00]	[0.03]	[0.01]	[0.00]	[0.08]	[0.01]	[0.00]
Information Only	0.03	-0.03*	0.03	-0.05**	0.03	-0.00	0.04	-0.05***
	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)
	[0.38]	[0.09]	[0.32]	[0.04]	[0.28]	[0.93]	[0.13]	[0.00]
Grant Only	0.04	-0.03*	0.04	-0.05**	0.01	-0.02	0.01	-0.05***
	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)
	[0.20]	[0.10]	[0.21]	[0.02]	[0.84]	[0.46]	[0.64]	[0.00]
Mentored by Refugee	-0.01	-0.01	0.01	-0.03	0.03	0.00	0.03	-0.04*
	(0.04)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)
	[0.67]	[0.53]	[0.84]	[0.21]	[0.28]	[0.94]	[0.36]	[0.09]
Mentored by Ugandan	-0.00	-0.05**	0.01	-0.00	-0.02	0.03	0.01	-0.02
	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)
	[0.89]	[0.01]	[0.82]	[0.89]	[0.53]	[0.35]	[0.76]	[0.49]
Observations	3,040	3,040	3,038	3,038	3,031	3,031	3,039	3,039
Control Mean: Baseline	0.44	0.14	0.24	0.19	0.17	0.16	0.26	0.09
Control Mean: Follow-Ups	0.43	0.11	0.33	0.16	0.23	0.19	0.37	0.11
Labeled Grant = Info Only	0.02	0.06	0.25	0.70	0.05	0.08	0.23	0.30
Labeled Grant = Grant Only	0.08	0.09	0.43	0.92	0.00	0.34	0.03	0.36
Labeled Grant = R-Mentee	0.00	0.02	0.08	0.35	0.14	0.11	0.15	0.06
R-Mentee = Info Only	0.23	0.40	0.49	0.54	0.86	0.88	0.68	0.31
R-Mentee = U-Mentee	0.78	0.10	0.99	0.29	0.11	0.42	0.56	0.34

An observation is a surveyed respondent per post-baseline survey round in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double lasso. Standard errors clustered at the enterprise level in parentheses. Brackets and the last five rows display two-sided p-values. Outcomes that were not pre-specified are denoted with $^+$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Support for Refugee Integration (Weighted to Match Population Average Age, Education, and Policy Support)

	Integration Policies Index	Supports Refugee Hosting	Supports More Refugees	Supports Right to Work	Supports Freedom of Movement	Supported Phone Campaign^{+}
Info. + Labeled Grant	0.36***	0.12***	0.17***	0.10***	0.09**	0.06
	(0.09)	(0.03)	(0.04)	(0.04)	(0.04)	(0.05)
	[0.00]	[0.00]	[0.00]	[0.01]	[0.04]	[0.25]
Information Only	0.22**	0.07*	0.10**	0.08**	0.03	0.03
	(0.09)	(0.03)	(0.04)	(0.04)	(0.04)	(0.05)
	[0.01]	[0.06]	[0.02]	[0.03]	[0.46]	[0.60]
Grant Only	0.21**	0.08**	0.11***	0.07*	-0.00	0.00
	(0.09)	(0.03)	(0.04)	(0.04)	(0.04)	(0.05)
	[0.02]	[0.03]	[0.01]	[0.05]	[0.97]	[0.93]
Mentored by Refugee	0.00	-0.01	0.03	0.03	-0.05	-0.07
	(0.10)	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)
	[0.97]	[0.80]	[0.52]	[0.50]	[0.31]	[0.18]
Mentored by Ugandan	0.04	0.02	0.01	0.00	-0.05	-0.03
	(0.11)	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)
	[0.70]	[0.56]	[0.89]	[0.93]	[0.33]	[0.52]
Observations	3,049	3,038	3,037	3,037	3,029	1,405
Control Mean: Baseline	0.03	0.73	0.51	0.60	0.60	.
Control Mean: Follow-Ups	-0.00	0.75	0.60	0.72	0.54	0.23
Labeled Grant = Info Only	0.06	0.07	0.06	0.44	0.13	0.52
Labeled Grant = Grant Only	0.03	0.13	0.10	0.30	0.03	0.31
Labeled Grant = R-Mentee	0.00	0.00	0.00	0.05	0.01	0.01
R-Mentee = Info Only	0.02	0.05	0.12	0.21	0.10	0.05
R-Mentee = U-Mentee	0.73	0.43	0.61	0.57	0.98	0.56

An observation is a surveyed respondent per post-baseline survey round in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double lasso. All regressions use entropy balancing (Hainmueller, 2012) to match three moments to means in the Ugandan adult population: the share aged 18–30, the share completing secondary school, and the share that supports allowing refugees to work (IRC, 2018*b*, UBOS, 2021). Standard errors clustered at the enterprise level in parentheses; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Full Set of Phone Campaign Outcomes

	Supported Phone Campaign^{+}	Opposed Phone Campaign^{+}	Answered Call^{+}
Info. + Labeled Grant	0.10***	-0.02	-0.01
	(0.04)	(0.02)	(0.03)
	[0.01]	[0.28]	[0.69]
Information Only	0.02	0.02	-0.01
	(0.04)	(0.02)	(0.03)
	[0.55]	[0.35]	[0.88]
Grant Only	0.04	0.01	0.01
	(0.04)	(0.02)	(0.04)
	[0.26]	[0.54]	[0.75]
Mentored by Refugee	-0.01	0.00	0.03
	(0.04)	(0.02)	(0.04)
	[0.77]	[0.90]	[0.50]
Mentored by Ugandan	-0.03	0.03	0.02
	(0.04)	(0.03)	(0.04)
	[0.54]	[0.20]	[0.58]
Observations	1,406	1,406	1,406
Control Mean: Follow-Ups	0.23	0.06	0.80
Labeled Grant = Info Only	0.04	0.04	0.81
Labeled Grant = Grant Only	0.16	0.11	0.49
Labeled Grant = R-Mentee	0.01	0.27	0.30
R-Mentee = Info Only	0.42	0.45	0.41
R-Mentee = U-Mentee	0.77	0.26	0.90

The sample is the experimental sample in Uganda. Results estimated through OLS regression with baseline controls selected through double lasso. Robust standard errors in parentheses. Brackets and the last five rows display two-sided p-values. Outcomes that were not pre-specified are denoted with $^{+}$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Support for Refugee Integration in Kenya, by Area Control-Group Support

	Average Area Support:		
	Low	Medium	High
Immediate Impacts (Integration Support Index)			
Info. + Labeled Grant	0.81***	0.58***	0.36***
	(0.10)	(0.06)	(0.08)
	[0.00]	[0.00]	[0.00]
Grant Only	0.45***	-0.02	0.09
	(0.09)	(0.11)	(0.08)
	[0.00]	[0.84]	[0.30]
Observations	1,831	1,687	1,701
Control Mean	-0.22	0.01	0.25
Labeled Grant = Grant	0.00	0.00	0.00
Demand-Free Bound (Integration Support Index)			
Info. + Labeled Grant	0.37***	0.31***	-0.04
	(0.12)	(0.10)	(0.08)
	[0.00]	[0.00]	[0.65]
Observations	264	371	368
Grant Only Mean	-0.06	-0.13	0.18

See Table 2 for notes on estimation and sampling. Each observation is a household in Kenya. Sample is split into terciles based on the average value of the integration support summary index in the Control group within the household's sub-county (there are 35 sub-counties). Two sub-counties without any Control observations are excluded from the sample. Standard errors in parentheses; p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Support for Refugee Integration in Kenya, by Area Control-Group Consumption

	Average Area Consumption:		
	Low	Medium	High
Immediate Impacts (Integration Support Index)			
Info. + Labeled Grant	0.42***	0.53***	0.83***
	(0.09)	(0.06)	(0.10)
	[0.00]	[0.00]	[0.00]
Grant Only	0.01	0.17	0.41***
	(0.09)	(0.11)	(0.11)
	[0.88]	[0.06]	[0.00]
Observations	1,840	1,740	1,639
Control Mean	0.14	-0.02	-0.13
Labeled Grant = Grant	0.00	0.00	0.00
Demand-Free Bound (Integration Support Index)			
Info. + Labeled Grant	0.06	0.18*	0.34***
	(0.10)	(0.10)	(0.10)
	[0.51]	[0.08]	[0.00]
Observations	356	282	365
Grant Only Mean	0.11	0.12	-0.20

See Table 2 for notes on estimation and sampling. Each observation is a household in Kenya. Sample is split into terciles based on the average household consumption in the control group within the household's sub-county (there are 35 sub-counties). Two sub-counties without any Control observations are excluded from the sample. Standard errors in parentheses; p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Business Outcomes and Household Welfare

	Household Well-Being Index	Business Profits (USD/Month)	Business Capital (USD)	Business Practices Index
Info. + Labeled Grant	0.05	-2.81	-56.34	0.04
	(0.06)	(2.35)	(44.49)	(0.08)
	[0.38]	[0.23]	[0.21]	[0.58]
Information Only	-0.05	-0.87	19.34	-0.02
	(0.07)	(2.52)	(48.05)	(0.08)
	[0.46]	[0.73]	[0.69]	[0.84]
Grant Only	0.04	-1.77	7.82	0.12*
	(0.06)	(2.52)	(46.85)	(0.07)
	[0.52]	[0.48]	[0.87]	[0.09]
Mentored by Refugee	-0.02	1.14	-35.17	0.06
	(0.08)	(2.83)	(50.66)	(0.09)
	[0.75]	[0.69]	[0.49]	[0.47]
Mentored by Ugandan	0.11	-2.35	15.15	0.11
	(0.07)	(2.74)	(53.67)	(0.08)
	[0.11]	[0.39]	[0.78]	[0.19]
Observations	4,132	4,029	2,819	1,942
Control Mean: Baseline	0.00	39.61	495.56	0.00
Control Mean: Follow-Ups	0.00	20.69	632.54	0.00
Labeled Grant = Info Only	0.06	0.39	0.09	0.44
Labeled Grant = Grant Only	0.82	0.65	0.14	0.26
Labeled Grant = R-Mentee	0.26	0.14	0.66	0.81
R-Mentee = Info Only	0.74	0.48	0.29	0.37
R-Mentee = U-Mentee	0.07	0.26	0.37	0.64

An observation is a surveyed respondent per post-baseline survey round in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double lasso. Standard errors clustered at the enterprise level in parentheses. Brackets and the last five rows display two-sided p-values. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Program Associations, Recall, and Discussions of Refugees

	Reported Any Support^{+}	Associated Support w YARID^{+}	Associated Support w Data Firm^{+}	Discussed Refugees^{+}
Info. + Labeled Grant	0.24***	0.20***	0.09***	0.08**
	(0.03)	(0.02)	(0.02)	(0.04)
	[0.00]	[0.00]	[0.00]	[0.03]
Information Only	-0.00	0.01	0.02*	0.06
	(0.03)	(0.01)	(0.01)	(0.04)
	[0.93]	[0.29]	[0.08]	[0.12]
Grant Only	0.26***	0.18***	0.10***	0.06
	(0.03)	(0.02)	(0.02)	(0.04)
	[0.00]	[0.00]	[0.00]	[0.10]
Mentored by Refugee	0.02	0.03***	0.03	-0.01
	(0.03)	(0.01)	(0.02)	(0.04)
	[0.53]	[0.01]	[0.11]	[0.83]
Mentored by Ugandan	0.04	0.03***	0.02	0.06
	(0.03)	(0.01)	(0.01)	(0.04)
	[0.14]	[0.00]	[0.15]	[0.16]
Observations	3,061	3,061	3,061	1,648
Control Mean: Baseline	.	.	.	0.22
Control Mean: Follow-Ups	0.32	0.00	0.04	0.22
Labeled Grant = Info Only	0.00	0.00	0.00	0.51
Labeled Grant = Grant Only	0.58	0.31	0.55	0.60
Labeled Grant = R-Mentee	0.00	0.00	0.00	0.03
R-Mentee = Info Only	0.47	0.04	0.88	0.11
R-Mentee = U-Mentee	0.45	0.59	0.79	0.14

Reports of support—and associations with YARID and data firm—are measured without prompting in a question about aid received from NGOs. An observation is a surveyed respondent per post-baseline survey round in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double lasso. Standard errors clustered at the enterprise level in parentheses. Brackets and the last five rows display two-sided p-values. Outcomes that were not pre-specified are denoted with $^{+}$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: Perceived Fairness of Aid Distribution

	Int'l Aid Is Distributed Fairly^{+}	Refugees Get Too Much Aid^{+}	Refugees Get More Aid^{+}	Local Aid Orgs Care About Me^{+}	Int'l Aid Orgs Care About Me^{+}	Int'l Aid Orgs Are Trustworthy^{+}
Info. + Labeled Grant	0.06	-0.05	0.00	0.12**	0.09*	0.16***
	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)	(0.06)
	[0.30]	[0.36]	[0.99]	[0.03]	[0.09]	[0.01]
Information Only	-0.03	-0.09*	-0.08	-0.05	-0.07	-0.03
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)
	[0.53]	[0.09]	[0.14]	[0.39]	[0.18]	[0.60]
Grant Only	-0.01	-0.15***	-0.08	0.10*	0.11**	0.23***
	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)	(0.06)
	[0.82]	[0.00]	[0.14]	[0.09]	[0.04]	[0.00]
Mentored by Refugee	-0.03	-0.07	-0.10*	0.01	0.01	0.14*
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)
	[0.69]	[0.25]	[0.10]	[0.92]	[0.91]	[0.05]
Mentored by Ugandan	-0.04	-0.02	-0.00	0.05	0.04	-0.00
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)
	[0.45]	[0.73]	[0.98]	[0.48]	[0.51]	[0.98]
Observations	780	821	821	699	871	653
Control Mean: Baseline	.	.	.	.	.	.
Control Mean: Follow-Ups	0.31	0.77	0.71	0.30	0.33	0.44
Labeled Grant = Info Only	0.09	0.42	0.13	0.00	0.00	0.00
Labeled Grant = Grant Only	0.21	0.05	0.12	0.75	0.69	0.26
Labeled Grant = R-Mentee	0.19	0.70	0.09	0.08	0.18	0.73
R-Mentee = Info Only	0.89	0.77	0.73	0.40	0.21	0.02
R-Mentee = U-Mentee	0.78	0.45	0.13	0.59	0.62	0.07

An observation is a surveyed respondent in the 26-month survey round in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double lasso. Standard errors clustered at the enterprise level in parentheses. Brackets and the last five rows display two-sided p-values. Outcomes that were not pre-specified are denoted with $^{+}$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A12: Heterogeneity in Impacts on Integration Policies Index (Public Good Usage)

	Uses Hospitals	Children Go to School With Foreigners	Uses Hospitals Or Schools
Info. + Labeled Grant × X	0.14	-0.01	0.09
	(0.14)	(0.14)	(0.16)
	[0.33]	[0.93]	[0.56]
Info. + Labeled Grant	0.22**	0.33***	0.24*
	(0.10)	(0.09)	(0.13)
	[0.03]	[0.00]	[0.07]
Information Only × X	0.06	0.05	0.04
	(0.15)	(0.15)	(0.17)
	[0.69]	[0.74]	[0.84]
Information Only	0.14	0.17*	0.15
	(0.11)	(0.10)	(0.15)
	[0.20]	[0.08]	[0.30]
Grant Only × X	0.01	-0.13	-0.04
	(0.15)	(0.14)	(0.17)
	[0.95]	[0.35]	[0.79]
Grant Only	0.20*	0.25***	0.22
	(0.11)	(0.10)	(0.15)
	[0.07]	[0.01]	[0.13]
Mentored by Refugee × X	0.05	-0.03	0.06
	(0.17)	(0.16)	(0.18)
	[0.77]	[0.86]	[0.75]
Mentored by Refugee	-0.00	0.06	-0.01
	(0.13)	(0.11)	(0.15)
	[0.98]	[0.59]	[0.94]
Mentored by Ugandan × X	0.12	-0.17	-0.06
	(0.17)	(0.17)	(0.18)
	[0.47]	[0.33]	[0.73]
Mentored by Ugandan	-0.06	0.07	0.04
	(0.13)	(0.11)	(0.16)
	[0.65]	[0.50]	[0.77]
X	-0.04	0.11	0.02
	(0.11)	(0.11)	(0.13)
	[0.71]	[0.29]	[0.88]
Observations	2,499	2,503	2,503

The dependent variable for each column is the integration policies summary index. Each column title lists the dimension of heterogeneity (X)—which in this table is measured after treatment—that is analyzed in the regression. An observation is a surveyed respondent per post-baseline survey round in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double lasso. Standard errors clustered at the enterprise level in parentheses; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A13: Baseline Correlates of Support for Refugee Integration

	Integration Policies Index	Supports Refugee Hosting	Supports More Refugees	Supports Right to Work	Supports Freedom of Movement
Economic Beliefs About Refugees	0.10	0.04			0.05
Cultural Views About Refugees	0.32	0.14	0.15	0.09	0.06
Knowledge of Hosting Policy	0.07	0.04	0.05	0.07	
Business Profit					0.04
Household Well-Being	0.08	0.04		0.05	
Observations	1,406	1,406	1,406	1,406	1,406
Outcome Mean	0.00	0.72	0.52	0.60	0.58

Each column shows post-estimation OLS coefficients from a regression of a baseline policy outcome on the set of other primary and attitudinal domain summary indices among the experimental sample in Uganda. All domain summary indices normalized to mean 0, standard deviation 1.

Table A14: Expanded Treatment Effect Heterogeneity

	Integration Policies Index	Integration Policies Index	Integration Policies Index	Integration Policies Index
Info. + Labeled Grant × Pos. Economic	-0.28**	-0.33**		-0.31**
	(0.13)	(0.13)		(0.13)
	[0.03]	[0.01]		[0.02]
Info. + Labeled Grant × Pos. Cultural	-0.20		-0.26**	-0.18
	(0.13)		(0.13)	(0.13)
	[0.11]		[0.04]	[0.17]
Info. + Labeled Grant × High Well-Being		0.10	0.08	0.11
		(0.13)	(0.13)	(0.13)
		[0.45]	[0.55]	[0.40]
Info. + Labeled Grant	0.62***	0.50***	0.46***	0.57***
	(0.12)	(0.12)	(0.12)	(0.13)
	[0.00]	[0.00]	[0.00]	[0.00]
Information Only × Pos. Economic	-0.25*	-0.34***		-0.30**
	(0.13)	(0.13)		(0.13)
	[0.06]	[0.01]		[0.03]
Information Only × Pos. Cultural	-0.21		-0.29**	-0.20
	(0.14)		(0.14)	(0.14)
	[0.12]		[0.03]	[0.14]
Information Only × High Well-Being		0.12	0.11	0.12
		(0.13)	(0.13)	(0.13)
		[0.37]	[0.38]	[0.35]
Information Only	0.47***	0.35***	0.30**	0.42***
	(0.12)	(0.13)	(0.13)	(0.14)
	[0.00]	[0.00]	[0.02]	[0.00]
Grant Only × Pos. Economic	-0.31**	-0.36***		-0.32**
	(0.13)	(0.13)		(0.13)
	[0.02]	[0.01]		[0.02]
Grant Only × Pos. Cultural	-0.24*		-0.30**	-0.22*
	(0.13)		(0.13)	(0.13)
	[0.08]		[0.02]	[0.09]
Grant Only × High Well-Being		0.01	-0.02	0.00
		(0.13)	(0.13)	(0.13)
		[0.96]	[0.86]	[0.98]
Grant Only	0.53***	0.44***	0.41***	0.53***
	(0.12)	(0.12)	(0.12)	(0.13)
	[0.00]	[0.00]	[0.00]	[0.00]
Observations	3,051	3,051	3,051	3,051

The dependent variable for each column is the integration policies summary index. *Pos. Economic* indicates respondents with above-median beliefs about the economic impact of refugees at baseline. *Pos. Cultural* indicates respondents with above-median cultural attitudes toward refugees at baseline. *High Well-Being* indicates respondents with an above-median household well-being measure at baseline. All heterogeneity variables measured using domain summary indices. An observation is a surveyed respondent per post-baseline survey round in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double lasso and include controls and interactions for both mentorship treatment groups (not shown). Standard errors clustered at the enterprise level in parentheses; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A15: More intensive refugee mentorship does not produce persistent impacts on policy views.

	9-Month Survey			16-Month Survey			26-Month Survey		
	Integration Policies Index	Economic Beliefs Index	Cultural Attitudes Index	Integration Policies Index	Economic Beliefs Index	Cultural Attitudes Index	Integration Policies Index	Economic Beliefs Index	Cultural Attitudes Index
Info. + Labeled Grant	0.33***	0.22**	0.04	0.38***	0.38***	0.23**	0.36***	0.23**	0.15
	(0.09)	(0.10)	(0.09)	(0.09)	(0.11)	(0.10)	(0.10)	(0.11)	(0.11)
	[0.00]	[0.03]	[0.69]	[0.00]	[0.00]	[0.02]	[0.00]	[0.03]	[0.16]
Information Only	0.15*	0.25**	-0.04	0.19**	0.25**	0.05	0.25**	0.09	0.18*
	(0.09)	(0.10)	(0.09)	(0.09)	(0.11)	(0.10)	(0.10)	(0.11)	(0.11)
	[0.09]	[0.01]	[0.67]	[0.04]	[0.02]	[0.63]	[0.02]	[0.38]	[0.09]
Grant Only	0.14	0.15	0.05	0.23***	0.26**	0.16*	0.28***	0.33***	0.22**
	(0.09)	(0.10)	(0.09)	(0.09)	(0.11)	(0.10)	(0.10)	(0.11)	(0.11)
	[0.13]	[0.16]	[0.60]	[0.01]	[0.01]	[0.09]	[0.00]	[0.00]	[0.05]
Standard Refugee Mentorship	0.19*	0.04	-0.06	-0.05	-0.10	-0.02	-0.03	-0.03	-0.05
	(0.11)	(0.13)	(0.12)	(0.11)	(0.14)	(0.12)	(0.13)	(0.13)	(0.15)
	[0.07]	[0.75]	[0.62]	[0.64]	[0.47]	[0.86]	[0.84]	[0.83]	[0.72]
Standard Ugandan Mentorship	0.13	0.10	0.06	0.09	0.03	-0.11	-0.07	0.13	0.09
	(0.11)	(0.14)	(0.11)	(0.12)	(0.13)	(0.12)	(0.13)	(0.14)	(0.14)
	[0.23]	[0.44]	[0.54]	[0.47]	[0.82]	[0.36]	[0.58]	[0.32]	[0.50]
Intensive Refugee Mentorship	0.55***	0.52***	0.10	0.22	-0.10	0.08	0.09	0.00	-0.11
	(0.16)	(0.19)	(0.15)	(0.18)	(0.19)	(0.19)	(0.21)	(0.20)	(0.25)
	[0.00]	[0.00]	[0.50]	[0.21]	[0.59]	[0.67]	[0.65]	[1.00]	[0.64]
Intensive Ugandan Mentorship	0.02	0.11	0.12	0.32**	0.22	0.23	-0.27	0.04	-0.04
	(0.16)	(0.17)	(0.15)	(0.13)	(0.18)	(0.15)	(0.22)	(0.20)	(0.20)
	[0.92]	[0.51]	[0.45]	[0.02]	[0.21]	[0.13]	[0.23]	[0.85]	[0.83]
Observations	1,109	1,070	1,119	1,041	1,000	1,041	901	892	901
Refugee = Ugandan (Standard)	0.61	0.67	0.31	0.27	0.41	0.51	0.76	0.29	0.38
Refugee = Ugandan (Intense)	0.01	0.08	0.93	0.62	0.17	0.50	0.21	0.89	0.81

An observation is a surveyed respondent per post-baseline survey round in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double lasso. Each domain summary index is re-computed with a fixed set of components for comparability across survey rounds. Each set of 3 columns estimates impacts within a single post-intervention survey round. *Intensive Mentorship* was offered to 100 business owners: these owners started their mentorship meetings earlier and so had more in-person and total meetings. *Standard Mentorship* refers to those assigned to mentorship but not in the intensive group. Robust standard errors in parentheses; two-sided p-values in brackets.

Westfall-Young Stepdown-Adjusted p-Values

The table below shows the Westfall-Young stepdown-adjusted p-values for our four primary hypotheses in Uganda, which are

- Labeled grants will increase support for refugee integration policies.
- Refugee mentorship will increase support for refugee integration policies.
- Labeled grants will increase business profits.
- Refugee mentorship will increase business profits.

Domain 1 contains information on support for refugee integration policies, and domain 2 contains information on business profits. Anderson summary indices are used here as dependent variables for each domain. Bootstrap estimation is performed 10,000 times.

Table A16: Westfall-Young Stepdown-Adjusted p-Values for Primary Hypotheses

	Integration Policies Index	Business Profits
Information + Labeled Grant	0.360***	-0.065
	(0.064)	(0.060)
	[0.000]	[0.500]
Mentored by Refugee	0.120	0.021
	(0.072)	(0.069)
	[0.306]	[0.767]
Observations	3,051	4,029

Standard errors in parentheses. Westfall-Young p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B Additional Details on Research Design

This appendix provides additional details on our research design, including sampling, details of intervention design (including scripts) and treatment roll-out, and descriptive tables on randomization balance and attrition from the sample.

B.1 Additional Sampling Details—Uganda

During the listing survey in October of 2019, we surveyed all tailors and hair salons within 10 kilometers of the Kampala city center.[32] We surveyed either the owner of the business or a manger who retains most of the profits.

For the baseline survey in November 2019 through January 2020, we selected a subset of the businesses contacted at listing. For the experimental sample, we chose "inexperienced" Ugandan business owners with no more than 5 years of sector experience, who were 40 years of age or younger, and who spoke Luganda, English, or Swahili conversationally. We also required that their business have fewer than five employees, profits under 271 USD (one million Ugandan Shillings), and capital under 2,710 USD (approximately ten million Ugandan Shillings). We also surveyed experienced Ugandans and refugees—who form our sample of potential mentors—and inexperienced refugees. Given their relatively low numbers, all non-Ugandans, excluding a few male tailors explained in the next section, were included.

To be a mentor, the business owner needed at least 3 years of experience. Ideally, mentors would have at least six years of experience so as not to overlap with the experimental sample. However, the supply of experienced refugees in three out of four gender-sector cells was too low for a sufficiently powered experiment. We thus reduced the experience requirement for mentors to three years for male and female salon owners and female tailors, and kept the six year requirement for male tailors. After forming our sample of potential mentors, we observed that the sample was already largely balanced across nationality groups. However, there was a greater number of highly experienced Ugandan potential mentors. We therefore dropped 15 Ugandan potential mentors with 6–10 years of experience, choosing these 15 who had the greatest Mahalanobis distance (defined along business profit, business capital, age, and years of education) compared to refugee mentors with the same level of experience. This produced an equal number of eligible refugee and Ugandan mentors who are largely balanced on these characteristics (see Appendix Table B3).

We chose to primarily recruit mentors of Congolese origin as Congolese sellers have an especially strong reputation in salons and tailor shops. The Congolese "bitenge" fabric, clothing styles, and hair styles are highly-regarded by Kampala consumers.[33] We hypothesized that the high concentration and reputational advantage of refugees was desirable for this study to increase the chances for skill transfer and collaboration to emerge from refugee-Ugandan pairs in mentorship. Overall, 86% of mentors were Congolese, 9% were Rwandan, and 4% were Burundian.

[32] We began with a systematic sampling strategy that selected respondents randomly based on their location, but after finding fewer tailor and salon businesses than expected we changed our sampling strategy to include the full population of tailors and salons in these areas. Our estimates are therefore unweighted.

[33] Bitenge is assumed by many customers to be imported from the DRC, though others noted it is increasingly imported from China and marketed as DRC-origin.

B.2 Randomization

In Uganda, randomization was conducted within strata defined by gender, sector, and mentor eligibility. Respondents in our sample were designated as "mentor eligible" if they had 3–5 years of experience in their sector. Half of these mentor-eligible respondents were randomly assigned to be a mentor; the other half were assigned to one of five treatment groups according to the same process used for mentor-ineligible respondents. Within each of these cells, we computed median profits and median attitudes towards hosting as the first principal component of support for seven integration policies. We chose treatment probabilities within stratum based on the number of available refugee mentors in that gender-sector cell, and set the probability of assignment to the Ugandan mentorship arm to be equal to that of the refugee-mentorship arm. The remaining sample was divided roughly equally between Information + Labeled Grant, Information Only, Grant Only, and Control.

In Kenya, village assignment to receive either type of grant was stratified on county. Within villages assigned to receive grants, households were evenly randomized to a Grant Only arm or a Labeled Grant arm, stratifying on county and age of the respondent. For the purpose of randomization inference, we permute household assignment within randomization strata at the village level.

B.3 Tests of Balance and Selective Attrition

Tables B1, B2, and B3 present tests of randomization balance within the experimental sample, randomization balance within the non-attriting experimental sample, and balance of mentor characteristics across refugee and Ugandan mentors respectively. Table B4 presents tests of differential attrition for ever being surveyed and for round-by-round survey status respectively. Table B5 presents tests of randomization balance in the Kenya study. Tables B6, B7, B8, and B9 present results from the main text applying inverse probability weights to account for differential attrition. Tables B10 and B11 present Lee Bounds on treatment impacts for each pre-specified domain (across two tables).

Table B1: Randomization Balance

	Mean: Info+Lab. Grant	Mean: Grant Only	Mean: Info Only	Mean: Mentored by Ref.	Mean: Mentored by Ug.	Mean: Control	Joint p-Value
Age (Years)	27.22	28.02	27.37	27.43	27.37	27.34	0.44
Education (Years)	10.89	10.51	10.72	10.57	10.92	10.73	0.37
Experience in Sector (Years)	2.49	2.45	2.47	2.28	2.32	2.21	0.25
Profit (USD/Month)	37.40	36.29	35.32	38.28	36.72	38.21	0.44
Has Any Employees	0.22	0.22	0.25	0.20	0.17	0.25	0.63
Aware of Aid-Sharing	0.21	0.18	0.16	0.21	0.20	0.17	0.55
Supports Refugee Hosting	0.71	0.71	0.69	0.69	0.80	0.74	0.04
Supports More Refugees	0.54	0.54	0.49	0.50	0.56	0.49	0.07
Supports Freedom of Movement	0.57	0.59	0.62	0.53	0.55	0.59	0.60
Supports Right to Work	0.62	0.59	0.57	0.61	0.61	0.58	0.54
Refugees Increase Rents	0.78	0.79	0.75	0.78	0.79	0.80	0.85
Refugees Increase Goods Prices	0.63	0.65	0.63	0.62	0.58	0.62	0.94
Refugees Worsen Public Goods	0.23	0.29	0.29	0.32	0.25	0.27	0.46
Refs' Economic Effect is Positive	0.52	0.54	0.58	0.54	0.50	0.51	0.49
Integration Policies Ix.	0.02	0.02	-0.02	-0.08	0.05	0.00	0.57
Knowledge Ix.	0.20	0.11	0.04	0.16	0.05	0.00	0.15
Economic Beliefs Ix.	-0.05	-0.09	0.00	0.01	-0.02	0.00	0.82
Economic Perceptions Ix.	-0.07	0.01	0.00	0.09	0.16	0.00	0.39
Economic Perceptions Ix.	0.08	0.02	0.14	0.26	0.04	0.00	0.11
Cultural Attitudes Ix.	0.01	0.14	0.00	-0.07	0.06	0.00	0.20
Contact Refs. by Choice Ix.	-0.02	0.01	0.00	0.02	0.12	0.00	0.98
Contact Refs. by Circumst. Ix.	-0.13	0.09	0.04	0.02	0.04	-0.00	0.05
Business Practices Ix.	-0.04	-0.05	0.06	-0.07	-0.07	0.00	0.85
Household Well-Being Ix.	-0.01	-0.06	-0.07	-0.08	-0.04	-0.00	0.90
General Policy Ix.	0.19	0.07	0.16	0.13	-0.02	-0.00	0.15
Foreigners: Econ Beliefs Ix.	0.03	0.08	0.10	0.10	-0.03	0.00	0.74
Foreigners: Cultural Attitudes Ix.	-0.03	0.05	0.16	-0.07	0.14	-0.00	0.11
Other Tribes: Contact Ix.	-0.08	0.01	0.09	-0.01	-0.09	0.00	0.42
Other Tribes: Economic Beliefs Ix.	0.02	-0.10	0.01	0.00	0.15	0.00	0.34
Other Tribes: Social Proximity Ix.	0.02	0.15	0.03	-0.04	-0.02	-0.00	0.15
Gender Role Ix.	0.01	0.21	-0.07	0.15	0.10	0.00	0.12

Baseline surveys of experimental sample in Uganda. First six columns show baseline variable means within treatment groups. Column 7 shows p-values from joint F-tests that means are equal in all treatment groups. *Ix.* indicates an index measure (mean zero, std. dev. 1).

Table B2: Randomization Balance (Among Non-Attriters)

	Mean: Info+Lab. Grant	Mean: Grant Only	Mean: Info Only	Mean: Mentored by Ref.	Mean: Mentored by Ug.	Mean: Control	Joint p-Value
Age (Years)	27.27	28.09	27.52	27.19	27.53	27.67	0.53
Education (Years)	10.97	10.42	10.70	10.63	11.15	10.84	0.28
Experience in Sector (Years)	2.52	2.51	2.46	2.26	2.32	2.28	0.20
Profit (USD/Month)	37.20	37.21	34.46	37.04	36.00	38.29	0.78
Has Any Employees	0.22	0.22	0.27	0.19	0.17	0.26	0.74
Aware of Aid-Sharing	0.23	0.18	0.17	0.22	0.22	0.19	0.56
Supports Refugee Hosting	0.73	0.72	0.69	0.71	0.82	0.73	0.04
Supports More Refugees	0.54	0.55	0.49	0.50	0.57	0.48	0.28
Supports Freedom of Movement	0.56	0.60	0.62	0.54	0.53	0.56	0.64
Supports Right to Work	0.65	0.61	0.58	0.63	0.61	0.60	0.56
Refugees Increase Rents	0.79	0.79	0.78	0.76	0.79	0.81	0.96
Refugees Increase Goods Prices	0.61	0.65	0.64	0.62	0.57	0.62	0.90
Refugees Worsen Public Goods	0.25	0.31	0.29	0.33	0.26	0.27	0.67
Refs' Economic Effect is Positive	0.52	0.55	0.57	0.52	0.47	0.51	0.50
Integration Policies Ix.	0.04	0.06	-0.00	-0.04	0.05	-0.04	0.61
Knowledge Ix.	0.25	0.04	0.04	0.17	0.07	-0.02	0.07
Economic Beliefs Ix.	-0.04	-0.11	-0.00	0.02	-0.10	0.01	0.60
Economic Perceptions Ix.	-0.05	-0.01	-0.04	0.08	0.13	0.05	0.70
Economic Perceptions Ix.	0.10	0.00	0.10	0.28	0.05	0.04	0.19
Cultural Attitudes Ix.	-0.01	0.15	-0.02	-0.08	0.07	-0.03	0.18
Contact Refs. by Choice Ix.	-0.02	0.02	0.01	0.10	0.13	-0.05	0.93
Contact Refs. by Circumst. Ix.	-0.08	0.14	0.05	0.09	0.00	0.06	0.18
Business Practices Ix.	-0.02	-0.06	0.04	-0.01	-0.07	0.07	0.83
Household Well-Being Ix.	0.06	-0.06	-0.07	-0.07	-0.04	0.02	0.64
General Policy Ix.	0.23	0.05	0.10	0.16	-0.02	0.04	0.14
Foreigners: Economic Beliefs Ix.	0.05	0.06	0.07	0.14	-0.06	-0.01	0.62
Foreigners: Cultural Attitudes Ix.	-0.01	0.06	0.15	-0.01	0.14	0.02	0.48
Other Tribes: Contact Ix.	-0.07	0.03	0.09	0.04	-0.11	-0.03	0.47
Other Tribes: Economic Beliefs Ix.	-0.04	-0.11	0.00	-0.02	0.13	-0.03	0.44
Other Tribes: Social Proximity Ix.	0.05	0.17	0.05	0.05	0.03	0.04	0.47
Gender Role Ix.	0.04	0.23	-0.05	0.18	0.06	0.04	0.32

Sample includes all baseline individuals of experimental sample in Uganda who were surveyed in at least one follow-up round. First six columns show baseline variable means within treatment groups. Column 7 shows p-values from joint F-tests that means are equal in all treatment groups. *Ix.* indicates an index measure (mean zero, std. dev. 1).

Table B3: Balance of Ugandan and Refugee Mentor Characteristics

	Ugandan Mentors	Refugee Mentors	Difference (U–R)	p-Value
Age (Years)	34.4	35.0	-0.5	0.59
	(9.99)	(8.63)	(1.0)	
Education (Years)	9.87	10.8	-0.9	0.02
	(3.29)	(4.03)	(0.4)	
Experience in Sector (Years)	9.26	9.62	-0.4	0.64
	(7.60)	(6.73)	(0.8)	
Profit (USD/Month)	42.8	47.7	-4.9	0.35
	(42.8)	(53.4)	(5.3)	
Has Any Employees	0.22	0.20	0.0	0.62
	(0.42)	(0.40)	(0.04)	
Number of Observations	170	169	339	

First two columns show means (standard deviations) within Ugandan and refugee mentors, respectively. Third column shows differences in means (standard errors) and the fourth column shows the p-value from a two-sided t-test of equivalence of means. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B4: Tests of Differential Attrition

	Ever Surveyed+ (Uganda)	Surveyed (Uganda)	Surveyed (Uganda)	Surveyed (Uganda)	Surveyed (Uganda)	Surveyed (Uganda)	Surveyed (Kenya)
Info. + Labeled Grant	0.00	0.04	0.04	0.06*	0.04	0.03	-0.01
	(0.02)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.01)
	[0.93]	[0.12]	[0.26]	[0.09]	[0.27]	[0.49]	[0.39]
Information Only	-0.04	0.01	0.05	0.03	0.01	-0.07	
	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	
	[0.13]	[0.81]	[0.15]	[0.43]	[0.72]	[0.11]	
Grant Only	0.01	0.08***	0.08**	0.09**	0.10**	0.07	
	(0.02)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	
	[0.58]	[0.00]	[0.02]	[0.03]	[0.01]	[0.10]	
Mentored by Refugee	-0.01	0.03	0.04	0.02	0.03	0.02	
	(0.03)	(0.03)	(0.04)	(0.05)	(0.04)	(0.05)	
	[0.71]	[0.39]	[0.31]	[0.59]	[0.54]	[0.70]	
Mentored by Ugandan	0.00	0.06*	0.11***	0.07*	0.03	0.01	
	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.05)	
	[0.97]	[0.07]	[0.01]	[0.09]	[0.50]	[0.76]	
Waves	All	Pooled	Follow-Up 1	Follow-Up 2	Follow-Up 3	Follow-Up 4	Follow-Up 1
Observations	1,406	5,624	1,406	1,406	1,406	1,406	1,098
Mean	0.91	0.73	0.80	0.74	0.76	0.64	0.95
Joint Orthogonality p-value	0.46	0.04	0.08	0.24	0.16	0.05	0.39

Ever Surveyed denotes whether the individual was surveyed in any follow-up survey round. *Surveyed* is defined at the survey-round level. Column 2 shows pooled ANCOVA estimates controlling for randomization-stratum and survey-wave fixed effects; Columns 3–7 show survey-round-specific estimates controlling for randomization-stratum fixed effects. Standard errors clustered at the respondent level in parentheses. Brackets and the last five rows display two-sided p-values. Outcomes not pre-specified are denoted with $^+$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B5: Randomization Balance, Kenya Extension

	Mean: Pure Control	Mean: Grant Only	Mean: Labeled Grant	Joint p-Value	N
Full Sample					
Age	46.6	47.0	46.7	0.73	5,262
Female	0.68	0.72	0.70	0.23	5,264
Head of Household	0.88	0.88	0.89	0.36	5,264
Education (Years)	7.79	7.70	7.79	0.84	5,236
Married	0.69	0.70	0.69	1.00	5,206
Employed	0.31	0.31	0.31	0.98	5,214
Income	23.8	21.6	24.6	0.33	5,220
Hours Worked, Past Week	22.8	22.5	24.9	0.15	5,264
Commute Time, Minutes	11.8	11.5	12.1	0.83	5,264
Life on Right Track	0.66	0.63	0.65	0.44	5,214
Mostly Happy, Past Month	0.67	0.62	0.60	0.05	5,211
Household Size	4.89	4.84	4.92	0.83	5,264
Household Expenditure	137.3	140.2	137.8	0.86	5,221
Household Savings	6.06	5.92	5.62	0.90	5,253
Household Durable Investment	6.45	5.60	6.41	0.57	5,258
Follow-Up Sample					
Age	46.6	47.0	46.7	0.88	1,045
Female	0.68	0.72	0.70	0.77	1,046
Head of Household	0.88	0.88	0.89	0.33	1,046
Education (Years)	7.79	7.70	7.79	0.87	1,039
Married	0.69	0.70	0.69	0.93	1,035
Employed	0.31	0.31	0.31	0.86	1,036
Income	23.8	21.6	24.6	0.25	1,037
Hours Worked, Past Week	22.8	22.5	24.9	0.19	1,046
Commute Time, Minutes	11.8	11.5	12.1	0.64	1,046
Life on Right Track	0.66	0.63	0.65	0.82	1,038
Mostly Happy, Past Month	0.67	0.62	0.60	0.51	1,035
Household Size	4.89	4.84	4.92	0.44	1,046
Household Expenditure	137.3	140.2	137.8	0.89	1,040
Household Savings	6.06	5.92	5.62	1.00	1,042
Household Durable Investment	6.45	5.60	6.41	0.50	1,042

Follow-Up Sample includes households surveyed at the one-month follow-up in Kenya; the follow-up was not conducted with the pure control group. First three columns show means within treatment groups. Fourth column shows p-values from joint F-tests that means are equal in all treatment groups, recovered from a regression of each variable on treatment and randomization-stratum dummies with standard errors that are clustered at the village level in the full sample and heteroskedasticity-robust in the follow-up sample. Monetary units are USD/month.

Table B6: Support for Refugee Integration (Weighted to Account for Attrition)

	Integration Policies Index	Supports Refugee Hosting	Supports More Refugees	Supports Right to Work	Supports Freedom of Movement	Supported Phone Campaign
Info. + Labeled Grant	0.36***	0.13***	0.15***	0.13***	0.07**	0.10***
	(0.06)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.03]	[0.01]
Information Only	0.22***	0.06**	0.10***	0.08***	0.04	0.02
	(0.07)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
	[0.00]	[0.04]	[0.00]	[0.00]	[0.26]	[0.62]
Grant Only	0.25***	0.09***	0.13***	0.09***	0.01	0.04
	(0.07)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.69]	[0.25]
Mentored by Refugee	0.11	0.03	0.05	0.07**	-0.03	-0.00
	(0.07)	(0.03)	(0.04)	(0.03)	(0.04)	(0.04)
	[0.13]	[0.37]	[0.14]	[0.02]	[0.50]	[0.92]
Mentored by Ugandan	0.08	0.06*	0.04	0.02	-0.07*	-0.02
	(0.08)	(0.03)	(0.04)	(0.03)	(0.04)	(0.04)
	[0.28]	[0.06]	[0.33]	[0.65]	[0.05]	[0.56]
Observations	3,051	3,040	3,038	3,039	3,031	1,406
Control Mean: Baseline	0.03	0.73	0.51	0.60	0.60	.
Control Mean: Follow-Ups	-0.00	0.75	0.60	0.72	0.54	0.23
Labeled Grant = Info Only	0.03	0.00	0.08	0.06	0.32	0.03
Labeled Grant = Grant Only	0.07	0.07	0.49	0.16	0.08	0.17
Labeled Grant = R-Mentee	0.00	0.00	0.00	0.05	0.01	0.02
R-Mentee = Info Only	0.12	0.37	0.19	0.75	0.11	0.60
R-Mentee = U-Mentee	0.73	0.37	0.67	0.08	0.26	0.67

An observation is a surveyed respondent per post-baseline survey round in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double lasso. All regressions weight observations by the probability of survey retention, estimated using lasso logit regression. Standard errors clustered at the enterprise level in parentheses; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B7: Beliefs About Economic Impacts of Hosting Refugees (Weighted to Account for Attrition)

	Economic Beliefs Index	Associated Support w Refugees	Knows About Aid-Sharing	Pos Effect on Economy Overall	Pos Effect on You Personally	Refugees Have Skills
Info. + Labeled Grant	0.30***	0.13***	0.15***	0.16***	0.09**	0.10**
	(0.07)	(0.02)	(0.03)	(0.04)	(0.04)	(0.04)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.01]	[0.01]
Information Only	0.21***	0.02	0.05	0.12***	0.06	0.01
	(0.07)	(0.01)	(0.03)	(0.04)	(0.03)	(0.04)
	[0.00]	[0.10]	[0.13]	[0.00]	[0.10]	[0.73]
Grant Only	0.22***	0.09***	0.09***	0.10***	0.11***	0.04
	(0.07)	(0.01)	(0.03)	(0.04)	(0.04)	(0.04)
	[0.00]	[0.00]	[0.01]	[0.01]	[0.00]	[0.40]
Mentored by Refugee	0.08	0.04**	-0.03	0.04	-0.04	0.02
	(0.08)	(0.02)	(0.04)	(0.04)	(0.04)	(0.05)
	[0.31]	[0.01]	[0.44]	[0.34]	[0.31]	[0.70]
Mentored by Ugandan	0.08	0.05***	0.02	0.04	0.06	0.01
	(0.08)	(0.02)	(0.04)	(0.04)	(0.04)	(0.05)
	[0.32]	[0.00]	[0.59]	[0.33]	[0.15]	[0.81]
Observations	3,003	3,061	3,061	2,787	2,906	1,671
Control Mean: Baseline	0.03	0.00	0.17	0.50	0.41	0.51
Control Mean: Follow-Ups	-0.00	0.02	0.37	0.42	0.44	0.42
Labeled Grant = Info Only	0.20	0.00	0.00	0.18	0.28	0.03
Labeled Grant = Grant Only	0.24	0.05	0.11	0.08	0.66	0.13
Labeled Grant = R-Mentee	0.00	0.00	0.00	0.00	0.00	0.08
R-Mentee = Info Only	0.07	0.17	0.03	0.04	0.01	0.93
R-Mentee = U-Mentee	0.99	0.65	0.23	0.98	0.02	0.88

An observation is a surveyed respondent per post-baseline survey round in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double lasso. All regressions weight observations by the probability of survey retention, estimated using lasso logit regression. Standard errors clustered at the enterprise level in parentheses; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B8: Cultural Attitudes Toward Refugees (Weighted to Account for Attrition)

	Cultural Attitudes Index	Comfortable Refugee Friends	Comfortable Refugee Spouse	Prop. Donated Refugees	Positive Effect on Culture	Refugees Deserve Sympathy
Info. + Labeled Grant	0.17**	0.07***	0.13***	0.05***	-0.00	0.03
	(0.07)	(0.03)	(0.04)	(0.02)	(0.03)	(0.04)
	[0.01]	[0.01]	[0.00]	[0.00]	[0.95]	[0.47]
Information Only	0.07	0.06**	0.07*	-0.00	0.05*	0.03
	(0.07)	(0.03)	(0.04)	(0.02)	(0.03)	(0.04)
	[0.31]	[0.02]	[0.08]	[0.94]	[0.10]	[0.47]
Grant Only	0.13*	0.05*	0.07*	0.04***	-0.03	0.08*
	(0.07)	(0.03)	(0.04)	(0.02)	(0.03)	(0.04)
	[0.06]	[0.05]	[0.07]	[0.01]	[0.41]	[0.05]
Mentored by Refugee	-0.03	0.00	0.06	-0.02	0.02	-0.03
	(0.07)	(0.03)	(0.05)	(0.02)	(0.04)	(0.05)
	[0.67]	[0.91]	[0.23]	[0.28]	[0.60]	[0.52]
Mentored by Ugandan	0.02	0.03	0.01	-0.00	0.05	-0.03
	(0.07)	(0.03)	(0.05)	(0.02)	(0.03)	(0.04)
	[0.74]	[0.31]	[0.76]	[0.97]	[0.16]	[0.56]
Observations	3,061	1,942	1,942	3,061	2,612	1,814
Control Mean: Baseline	0.04	0.78	0.49	0.21	0.71	0.46
Control Mean: Follow-Ups	0.00	0.82	0.49	0.28	0.69	0.54
Labeled Grant = Info Only	0.11	0.78	0.16	0.00	0.08	1.00
Labeled Grant = Grant Only	0.55	0.50	0.21	0.81	0.45	0.21
Labeled Grant = R-Mentee	0.01	0.04	0.14	0.00	0.57	0.19
R-Mentee = Info Only	0.16	0.07	0.77	0.29	0.38	0.19
R-Mentee = U-Mentee	0.46	0.42	0.41	0.35	0.45	0.94

An observation is a surveyed respondent per post-baseline survey round in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double lasso. All regressions weight observations by the probability of survey retention, estimated using lasso logit regression. Standard errors clustered at the enterprise level in parentheses; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B9: Business Outcomes and Household Welfare (Weighted to Account for Attrition)

	Household Well-Being Index	Business Profits (USD/Month)	Business Capital (USD)	Business Practices Index
Info. + Labeled Grant	0.04	-3.22	-57.22	0.03
	(0.06)	(2.44)	(44.96)	(0.08)
	[0.50]	[0.19]	[0.20]	[0.74]
Information Only	-0.05	-0.60	16.74	-0.02
	(0.07)	(2.67)	(49.23)	(0.08)
	[0.43]	[0.82]	[0.73]	[0.78]
Grant Only	0.03	-2.15	7.91	0.11
	(0.07)	(2.65)	(47.76)	(0.07)
	[0.63]	[0.42]	[0.87]	[0.12]
Mentored by Refugee	-0.04	0.98	-37.11	0.05
	(0.08)	(2.89)	(51.02)	(0.09)
	[0.65]	[0.73]	[0.47]	[0.55]
Mentored by Ugandan	0.10	-2.46	12.51	0.10
	(0.07)	(2.81)	(53.74)	(0.08)
	[0.13]	[0.38]	[0.82]	[0.22]
Observations	4,132	4,029	2,819	1,942
Control Mean: Baseline	-0.03	39.61	495.56	0.05
Control Mean: Follow-Ups	0.00	20.69	632.54	0.00
Labeled Grant = Info Only	0.09	0.27	0.10	0.54
Labeled Grant = Grant Only	0.85	0.64	0.14	0.23
Labeled Grant = R-Mentee	0.27	0.11	0.68	0.75
R-Mentee = Info Only	0.82	0.59	0.30	0.40
R-Mentee = U-Mentee	0.06	0.26	0.37	0.61

An observation is a surveyed respondent per post-baseline survey round in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double lasso. All regressions weight observations by the probability of survey retention, estimated using lasso logit regression. Standard errors clustered at the enterprise level in parentheses; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B10: Lee Bounds on Treatment Impacts, Domains 1–9

	Integration Policies Index	Profit (Standardized)	Refugee Knowledge Index	Economic Beliefs Index	Cultural Attitudes Index	Contact Refugees by Choice Index	Contact Refugees by Circumst. Index	Business Practices Index
Info. + Labeled Grant								
lower	[0.17,0.41]	[-0.27,-0.03]	[0.03,0.34]	[0.02,0.33]	[-0.10,0.19]	[-0.33,0.05]	[-0.38,0.05]	[-0.30,0.07]
upper	[0.36,0.65]	[-0.05,0.25]	[0.27,0.58]	[0.27,0.60]	[0.15,0.46]	[0.04,0.35]	[-0.01,0.39]	[0.06,0.45]
Observations	1,772	2,139	1,774	1,746	1,774	1,357	1,355	1,357
Information Only								
lower	[0.07,0.33]	[-0.18,0.07]	[-0.08,0.25]	[0.01,0.34]	[-0.13,0.20]	[-0.63,0.58]	[-0.14,0.32]	[-0.26,0.23]
upper	[0.05,0.43]	[-0.18,0.19]	[-0.04,0.28]	[0.09,0.43]	[-0.08,0.28]	[-0.72,0.71]	[-0.25,0.48]	[-0.42,0.40]
Observations	1,804	2,162	1,804	1,780	1,804	1,378	1,374	1,378
Grant Only								
lower	[-0.03,0.23]	[-0.30,-0.05]	[-0.21,0.10]	[-0.17,0.14]	[-0.21,0.06]	[-0.42,-0.10]	[-0.48,-0.16]	[-0.26,0.07]
upper	[0.34,0.60]	[0.09,0.33]	[0.22,0.51]	[0.28,0.60]	[0.21,0.49]	[0.07,0.38]	[0.03,0.41]	[0.23,0.54]
Observations	1,620	1,965	1,623	1,596	1,623	1,229	1,228	1,229
Mentored by Refugee								
lower	[-0.11,0.19]	[-0.19,0.08]	[-0.35,0.01]	[-0.22,0.14]	[-0.30,0.03]	[-0.37,0.04]	[-0.36,0.19]	[-0.28,0.16]
upper	[0.08,0.43]	[0.01,0.33]	[-0.12,0.22]	[0.02,0.39]	[-0.07,0.28]	[-0.07,0.28]	[0.02,0.48]	[0.08,0.47]
Observations	1,411	1,694	1,414	1,387	1,414	1,082	1,081	1,082
Mentored by Ugandan								
lower	[-0.17,0.12]	[-0.38,-0.10]	[-0.26,0.09]	[-0.29,0.06]	[-0.28,0.02]	[-0.35,-0.03]	[-0.39,0.00]	[-0.28,0.11]
upper	[0.20,0.50]	[-0.01,0.31]	[0.13,0.45]	[0.11,0.45]	[0.09,0.39]	[0.01,0.39]	[0.07,0.50]	[0.19,0.54]
Observations	1,408	1,697	1,410	1,382	1,410	1,068	1,067	1,068

Each cell shows a 95% confidence interval for an upper or lower Lee bound. Lee bounds estimated using only the control group and one treatment group in Uganda. Each outcome is the residual from an ANCOVA regression of the domain summary index on a randomization-stratum and survey-wave fixed effect, a dummy for whether the survey was conducted over the phone, a linear survey date control, and the baseline value of the summary index.

Table B11: Lee Bounds on Treatment Impacts, Domains 10–17

	Household Well-Being Index	General Policy Index	Foreigners: Economic Beliefs Index	Foreigners: Cultural Attitudes Index	Other Tribes: Contact Index	Other Tribes: Economic Beliefs Index	Other Tribes: Cultural Attitudes Index	Gender Role Index
Info. + Labeled Grant								
lower	[-0.13,0.09]	[-0.14,0.12]	[-0.13,0.27]	[-0.45,0.11]	[-0.28,0.02]	[-0.16,0.18]	[-0.06,0.27]	[-0.65,0.41]
upper	[0.07,0.34]	[0.07,0.37]	[0.14,0.65]	[-0.02,0.50]	[-0.01,0.31]	[0.02,0.59]	[0.23,0.60]	[-0.26,0.56]
Observations	2,180	2,038	1,226	1,171	1,290	1,215	1,240	844
Information Only								
lower	[-0.18,0.06]	[-0.16,0.12]	[-0.17,0.37]	[-0.43,0.11]	[-0.46,0.28]	[-0.23,0.38]	[-0.51,0.66]	[-0.44,0.55]
upper	[-0.22,0.21]	[-0.13,0.20]	[-0.09,0.34]	[-0.35,0.22]	[-1.30,1.20]	[-0.09,0.27]	[-0.11,0.27]	[-0.17,0.59]
Observations	2,208	2,073	1,246	1,180	1,309	1,242	1,264	913
Grant Only								
lower	[-0.20,0.03]	[-0.22,0.04]	[-0.26,0.15]	[-0.42,0.02]	[-0.39,-0.11]	[-0.14,0.21]	[-0.30,0.09]	[-0.61,0.10]
upper	[0.17,0.41]	[0.16,0.42]	[0.33,0.72]	[0.29,0.76]	[-0.04,0.26]	[0.39,0.63]	[0.29,0.59]	[0.23,0.83]
Observations	2,008	1,885	1,112	1,059	1,163	1,106	1,127	786
Mentored by Refugee								
lower	[-0.22,0.04]	[-0.25,0.06]	[-0.53,0.01]	[-0.41,0.22]	[-0.40,-0.10]	[-0.06,0.32]	[-0.22,0.19]	[-0.76,0.16]
upper	[-0.02,0.30]	[-0.07,0.27]	[-0.32,0.24]	[-0.09,0.51]	[-0.22,0.10]	[0.00,0.70]	[0.02,0.52]	[-0.55,0.55]
Observations	1,736	1,618	970	929	1,024	966	987	705
Mentored by Ugandan								
lower	[-0.09,0.14]	[-0.10,0.19]	[-0.34,0.13]	[-0.54,-0.03]	[-0.34,-0.05]	[-0.36,0.07]	[-0.35,0.11]	[-0.74,0.11]
upper	[0.20,0.45]	[0.24,0.53]	[0.15,0.71]	[0.10,0.67]	[-0.02,0.30]	[0.07,0.73]	[0.24,0.65]	[0.03,0.75]
Observations	1,732	1,625	974	928	1,016	966	982	690

Each cell shows a 95% confidence interval for an upper or lower Lee bound. Lee bounds estimated using only the control group and one treatment group in Uganda. Each outcome is the residual from an ANCOVA regression of the domain summary index on a randomization-stratum and survey-wave fixed effect, a dummy for whether the survey was conducted over the phone, a linear survey date control, and the baseline value of the summary index.

B.4 Treatment Roll-Out—Uganda

The interventions were launched in late January of 2020 and paused on March 20, 2020 due to COVID-19. At the time of the suspension, YARID had visited: 82% of Information Only, 75% of Grant Only and Information + Labeled Grant for the first meeting to explain the program and 33% of those groups for the second meeting to disburse the grant, and 83% of the mentorship treatment arms. Seventy-two percent of the mentorship pairs met at least once, with 23% of those having met all six times. Table B12 presents tabulations of actual treatment status (defined as receiving the grant in Grant Only and Information + Labeled Grant, receiving the information in Information Only, and having at least one mentorship meeting in Refugee and Ugandan Mentorship). Table B13 shows the number of mentorship meetings held by year across Refugee and Ugandan Mentorship arms.[34]

Table B12: Assignment and Actual Treatment Status

	Labeled Grant	Information Only	Grant Only	Mentored by Refugee	Mentored by Ugandan	Control
Assigned	280	287	237	169	168	265
Treated	233	257	194	133	135	.
Percentage	83	90	82	79	80	.

Source: YARID Administrative data. Each cell shows the number of respondents who were assigned to, and actually treated with, a given treatment arm in Uganda.

Table B13: Facilitated Mentorship Meetings

	In-Person (2020)			Phone (2021)			
	Mean Num.	At Least One (%)	Max Num.	Mean Num.	At Least One (%)	Max Num.	N
Mentored by Refugee (All)	2.1	71	6	2.5	67	4	169
Standard	1.5	74	3	2.6	71	4	119
Intensive	3.5	64	6	2.1	58	4	50
Mentored by Ugandan (All)	2.1	73	6	2.6	69	4	168
Standard	1.5	76	3	2.8	75	4	118
Intensive	3.3	64	6	2	54	4	50

Source: YARID Administrative data

[34] Before the pause, the in-person conversations lasted an average of 44 minutes. After interventions restarted, the phone conversations lasted an average of 24 minutes.

B.5 Cost Effectiveness

Table B14: Cost Effectiveness

	Treatment Effect on Supports Refugee Hosting (pp)	Cost per Person (USD)	Cost per 1 pp Treatment Effect
Info. + Labeled Grant (Uganda)	13	153	11.77
Information Only (Uganda)	6	39	6.50
Grant Only (Uganda)	9	153	17.00
Mentored by Refugee (Uganda)	4	132	33.00
Labeled Grant (Kenya)	16	47	2.94
Grant Only (Kenya)	6	47	7.83

Each row is a treatment arm. Treatment effects are shown for the outcome "Supports Refugee Hosting"(Tables 1 and 2, Column 2), expressed in percentage points. Costs shown in USD. Cost estimates in Uganda are calculated by dividing the realized costs for three categories—grants (combining Labeled Grant and Grant Only), Information Only, and mentorship (combining Mentored by a Refugee and Mentored by a Ugandan)—by the respective treatment arm sizes to obtain cost per person (intent-to-treat) estimates. YARID overhead costs are divided equally per targeted person—totaling $27 each—and added to the per person treatment costs. Cost estimates in Kenya are calculated by adding the cost of the grant ($7.50) to the Information Only and overhead cost estimates from Uganda. The marginal cost of labeling the grants—the additional time for the enumerator—is less than $0.50 and omitted.

Table B14 shows cost estimates per person changing their policy view on support for hosting generally. The cost per person is lower in Kenya given the smaller grant size and similar impacts. In Uganda, Information Only is the least expensive per person, followed by Information + Labeled Grant. Two caveats are in order. First: cost comparisons in Uganda take as given the existence of aid-sharing programs. Without these programs in place, the Information Only arm is not replicable. Second: we base these estimates on our design which included in-person visits in Information Only. An alternative program that used radio, television, or other media to distribute information would likely be cheaper, but our design does not speak directly to such a program.

B.6 Information Only Script—Uganda

Introduction: I'd like to tell you a little bit about our organization's mission. If you have any questions, please stop me, and I am happy to discuss. Our program works in areas that host refugees. Refugees are people who do not feel safe in their home countries. They or their families have often been targeted by violent groups, and they are looking for a place where they can feel safe. Refugees come to Uganda from the Congo, South Sudan, Somalia, Rwanda, Burundi, and other countries, and the reason is that they believe they are safer in Uganda than the country where they were born. Many have had family members killed by violent groups, and they were often forced to abandon their belongings, their land, and sometimes their family.

Empathetic Listening (Based on Kalla-Broockman Model):
Step 1: Uncover Honest Opinion. What do you think of refugees in Kampala? What is on either side of the issue for you? What are some reasons that you would think of them

favorably? How about unfavorably?

Step 2: Connect Around Experiences with Refugees. Have you had any experiences with refugees? How did that feel? Do you know any refugees?

If No	**If Yes**
- What kind of role do you see refugees playing in your community?	- Who are you closest to? - How are they doing? - What is their story? - What do you think that was like for them? - Tell me more?

****Share personal refugee story *****
I am here working with YARID today because I...

Step 3: Connect Around Compassion Experiences. I think having these conversations is important because it gives us a chance to think about how we want to treat everyone in our community, including refugees, because we've all faced tough times and needed others...

Your Compassion Story	**Business Owners' Compassion Story**
I remember when ...	Was there a time when someone showed you compassion and you really needed it? Maybe a friend or parent? What as the situation How old were you? How did that feel? Why?

Step 4: Address Concerns. Thank you so much for having this conversation with me... Earlier you mentioned (concern) as a concern? What are your fears? What is on your mind now? What are you picturing might happen? Do you have a personal connection to that concern?

Step 5: Make Your Case. I think it's important to support refugees and host refugees because I want everyone in our community, including refugees, our families, as well as our friends and neighbours to be treated with compassion and not feel excluded or suffer discrimination.

Information About Hosting and Aid-Sharing: When refugees come to Uganda, Uganda is a very generous host. Uganda lets refugees work, for example. They can apply for jobs and support themselves if they are hired by a business, and their work contributes to the Ugandan economy. Uganda also gives refugees freedom to move. There are many settlements and camps in Uganda where refugees can live, but if they have other opportunities outside of the settlement, they are free to live where they want to in Uganda. Some countries, even ones close to Uganda like Kenya and Ethiopia, are not as welcoming to refugees. In these countries, refugees cannot work legally. They must support themselves in the black market and hope they are not caught by authorities. In Kenya and Ethiopia, refugees also cannot live outside of the camps. They are not free to move to places where they might find a job or have family. Uganda is much more generous by allowing refugees to work and the freedom of movement to live outside of camps.

Because of this generous policy, many refugees in Uganda can support themselves. Since refugees can work, some of the aid money coming from international donors like Great Britain can be shared with Ugandans. This aid money shared between refugees and Ugandans can help with health, education, small businesses, and poverty. In countries like Kenya where refugees cannot work, more aid money needs to be spent on food and basic needs for refugees, and so it cannot be shared with the host country. In Uganda, since refugees can get jobs and live outside of camps, aid money and programs can be shared with Ugandans like you. Does that make sense? In Uganda, 30% of international aid money for refugees goes to supporting Ugandans.

This aid has been used to support schools and hospitals in areas where there are many refugees, including Kampala. The schools and hospitals are built for both Ugandans and refugees to use. International donors pay for these buildings and services because Uganda is a generous host to many refugees. For instance, Kisenyi Hospital was supported by donors to appreciate Ugandans' generous hosting of refugees. The World Bank also gave Uganda 500 million USD recently to support the Ministry of Education. In other countries, this money only goes to refugees who need the money since they can't work.

My organization, YARID, is another example where aid money is shared between refugees and Ugandans. YARID was founded by refugees from the Congo with the goal of helping people in Kampala — refugees from any country and Ugandans alike. YARID runs training programs on English, computer literacy, and small business practices for people in need. It is based in Kampala and has thousands of people since its founding.

B.7 Information + Labeled Grant Script—Uganda

Introduction: I'm here to offer an opportunity to participate in a pilot program that offers grants to small businesses in Kampala. As part of our program I'd like to tell you a little bit about our organization's mission and why we are starting this small business grant program in areas of Kampala that host refugees. If you have any questions, please stop me, and I am happy to discuss. Our program works in areas that host refugees. Refugees are people who do not feel safe in their home countries. They or their families have often been targeted by violent groups, and they are looking for a place where they can feel safe. Refugees come to Uganda from the Congo, South Sudan, Somalia, Rwanda, Burundi, and other countries, and the reason is that they believe they are safer in Uganda than the country where they were born. Many have had family members killed by violent groups, and they were often forced to abandon their belongings, their land, and sometimes their family.

[**IDENTICAL EMPATHETIC LISTENING ACTIVITY HERE**]

Information About Hosting and Aid-Sharing: When refugees come to Uganda, Uganda is a very generous host. Uganda lets refugees work, for example. They can apply for jobs and support themselves if they are hired by a business, and their work contributes to the Ugandan economy. Uganda also gives refugees freedom to move. There are many settlements and camps in Uganda where refugees can live, but if they have other opportunities outside of the settlement, they are free to live where they want to in Uganda. Some countries, even ones close to Uganda like Kenya and Ethiopia, are not as welcoming to refugees. In these countries, refugees cannot work legally. They must support themselves in the black market and hope they are not caught by authorities. In Kenya and Ethiopia, refugees also cannot live outside of the camps. They are not free to move to places where they might find a job or have family. Uganda is much more generous by allowing refugees to work and the freedom of movement to live outside of camps.

Because of this generous policy, many refugees in Uganda can support themselves. Since refugees can work, some of the aid money coming from international donors like Great Britain can be shared with Ugandans. This aid money shared between refugees and Ugandans can help with health, education, small businesses, and poverty. In countries like Kenya where refugees cannot work, more aid money needs to be spent on food and basic needs for refugees, and so it cannot be shared with the host country. In Uganda, since refugees can get jobs and live outside of camps, aid money and programs can be shared with Ugandans like you. Does that make sense? In Uganda, 30% of international aid money for refugees goes to supporting Ugandans.

This aid has been used to support schools and hospitals in areas where there are many refugees, including Kampala. The schools and hospitals are built for both Ugandans and refugees to use. International donors pay for these buildings and services because Uganda is a generous host to many refugees. For instance, Kisenyi Hospital was supported by donors to appreciate Ugandans' generous hosting of refugees. The World Bank also gave Uganda 500 million USD recently to support the Ministry of Education. In other countries, this money only goes to refugees who need the money since they can't work.

My organization, YARID, is another example where aid money is shared between refugees and Ugandans. YARID was founded by refugees from the Congo with the goal of helping people in Kampala — refugees from any country and Ugandans alike. YARID runs training programs on English, computer literacy, and small business practices for people in need. It is based in Kampala and has thousands of people since its founding.

The program I'm visiting you about today is run by YARID and is part of the aid-sharing between refugees and Ugandans.

Description of the Grant: As part of this project you will be placed in a program that gives cash grants to micro-entrepreneurs. The grant is worth 500,000 UGX total. At least 300,000 UGX must be used for purchasing equipment for your business. This money can be used to purchase anything related to your business, such as machinery or inventory. The 300,000 UGX cannot be used for personal expenses such as rent, medical fees, or school fees. Whatever money remains from the 500,000 UGX will be given to you as cash. This grant is intended for business use, but we understand if there is an urgent need in your household. Therefore there are no rules for this remaining cash – you can spend it on anything you want.

You will have some time to think about what you want to buy, and we will set up an appointment for a later date. I will return to visit your business on that date and accompany you to make the purchase. Remember, at least 300,000 out of the 500,000 UGX must be spent on purchases for your business, which we will make together at a supplier. This is to ensure that enough money is used on capital or inventory. After you've made your purchases of at least 300,000, we will give you whatever money remains from the 500,000 as cash. So, for example, if you spend 300,000 on inventory for your business, we will give you 200,000 in cash. If you spend 200,000 on inventory and 200,000 on tools, we will give you 100,000 in cash. The total will always be 500,000 and you must spend at least 300,000 on your business. Do you have any questions right now about the program?

You will not need to do anything for us. We have already determined that you are eligible for the grant. You will never have to pay back the grant to us or to anyone else. Your participation is voluntary, and you can withdraw from the program at any time. Do you agree to participate?

The grant program is completely separate from your opinion about refugees. Today, we will exchange contact information, but we will not be doing any transactions today. You will have up to 1-2 weeks to decide what you want to buy and set up an appointment. Make sure to take enough time to consider what you want, shop around, and compare prices. You can also use your some of your own money if you'd like to buy something that costs more than 500,000 UGX.

B.8 Grant Only Script—Uganda

I'm here to offer an opportunity to participate in a pilot program that offers grants to small businesses in Kampala.

Description of the Grant: As part of this project you will be placed in a program that gives cash grants to micro-entrepreneurs. The grant is worth 500,000 UGX total. At least 300,000 UGX must be used for purchasing equipment for your business. This money can be used to purchase anything related to your business, such as machinery or inventory. The 300,000 UGX cannot be used for personal expenses such as rent, medical fees, or school fees. Whatever money remains from the 500,000 UGX will be given to you as cash. This grant is intended for business use, but we understand if there is an urgent need in your household. Therefore there are no rules for this remaining cash – you can spend it on anything you want.

You will have some time to think about what you want to buy, and we will set up an appointment for a later date. I will return to visit your business on that date and accompany you to make the purchase. Remember, at least 300,000 out of the 500,000 UGX must be spent on purchases for your business, which we will make together at a supplier. This is to ensure that enough money is used on capital or inventory. After you've made your purchases of at least 300,000, we will give you whatever money remains from the 500,000 as cash. So, for example, if you spend 300,000 on inventory for your business, we will give you 200,000 in cash. If you spend 200,000 on inventory and 200,000 on tools, we will give you 100,000 in cash. The total will always be 500,000 and you must spend at least 300,000 on your business. Do you have any questions right now about the program?

You will not need to do anything for us. We have already determined that you are eligible for the grant. You will never have to pay back the grant to us or to anyone else. Your participation is voluntary, and you can withdraw from the program at any time. Do you agree to participate?

Today, we will exchange contact information, but we will not be doing any transactions today. You will have up to 1-2 weeks to decide what you want to buy and set up an appointment. Make sure to take enough time to consider what you want, shop around, and compare prices. You can also use your some of your own money if you'd like to buy something that costs more than 500,000 UGX.

B.9 Phone Campaign Script—Uganda

Hello, this is Florence from OneYouth OneHeart Initiative. Our organization supports refugees who live in Kampala. We are sending MPs and LC1s a note of appreciation for allowing refugees to live and work in Kampala, and we want to tell them how many Ugandans support these policies for refugees too. Do you support this note in favor of refugees' right to work in Kampala? We will not ask for money, and it is free to reply. Please press 1 for YES to support the note. Press 2 for NO to decline. To answer this question, please use the keypad on your phone. Again, please press 1 now to endorse this note that appreciates the MPs and LC1s who support refugees, or press 2 now to decline. Press 9 to repeat this message. Thank you!

B.10 Information + Labeled Grant Script—Kenya

Hi, my name is JeanPaul. I work for RELON Kenya, and today we're testing a pilot program. Our organization works in areas that host refugees. Refugees are people who do not feel safe in their home countries. Many have had family members killed by violent groups, and they were often forced to abandon their belongings, their land, and sometimes their family.

Kenya hosts many refugees. These refugees receive aid programs from other countries like the United States and Great Britain. This aid is important for refugees, but we also want Kenyans to benefit from this assistance and from hosting refugees in Kenya. Therefore, you have been selected to receive a one-time grant of 1,000 KSh as part of our pilot program today. Again, this money is coming to you because Kenya hosts many refugees, and we want Kenyans like you to benefit too.

Right now, most of the aid money is given to refugees because it is hard for them to find work. In Kenya, most refugees cannot move freely and must stay in camps in border counties like Turkana and Garissa. This means it is difficult for them to find jobs, as there are few economic opportunities in the camps.

Refugees could better support themselves in Kenya if they could find work and move to places where there are more jobs available. Then they would need less assistance from other countries like the United States, so even more aid money could be shared with Kenyans like you. If refugees could find good jobs and have the freedom to live where they want to in Kenya, more international donations could support Kenyan schools, hospitals, small businesses, and farmers. In Uganda, for example, refugees can work and live where they want to, and this means that international donors can support schools, hospitals, and businesses that benefit Ugandans.

My organization, RELON Kenya, is another example where aid money is shared between refugees and Kenyans. RELON Kenya is a network of organizations that are founded by refugees. Our goal is to help people in Kenya – refugees and Kenyans alike. Our organizations run programs like legal assistance, education, and business support and have helped thousands of people, both refugees and Kenyans.

Thank you for your time today and for hosting refugees in Kenya.

C Details on Tests of Alternative Mechanisms

This appendix presents details on the tests of alternative mechanisms summarized in Section 5.2.

C.1 Experimenter Demand Effects

The implementing organization in Uganda, YARID, is refugee-led and in part refugee-staffed. Business owners may therefore believe that their chances of receiving future assistance are increased by expressing pro-refugee views.[35] Alternatively, demand effects may be generated by feelings of gift exchange, if respondents who received assistance from YARID viewed the assistance as a *quid pro quo*, and so gave responses they think YARID wanted to hear but do not believe themselves.

In this section, we discuss evidence beyond our three main results pointing against substantial demand effects: an independent phone campaign, the demand elicitation activity of De Quidt, Haushofer and Roth (2018), and an incentivized dictator game. We conducted three additional experimental tests of demand effects—a placebo treatment, a priming experiment, and an elicitation of expectations of future assistance.

Our tests of demand effects were designed to test whether true policy preferences changed, as opposed to simply self reports. It is possible that demand effects are stronger for questions related to cultural attitudes, and we cannot fully rule this out. However, the lack of treatment effects of our mentorship programs on cultural views—in contrast to positive effects of Information Only, which provided no material support—suggests that the scope for demand effects to influence cultural attitudes is also limited. Finally, note that our demand elicitation test based on De Quidt, Haushofer and Roth (2018) is especially helpful at ruling out the possibility that participants learned over time about the study's expected results and changed their answers accordingly—perhaps because the same questions were asked multiple times—under the assumption that explicit expectations are at least as impactful as implicit expectations.

C.1.1 Placebo Information Treatment

To further test whether respondents' answers were influenced by their perceptions of YARID's position—as opposed to the new information provided through our interventions—and whether receiving cash amplifies such an effect, we ran a placebo information campaign on an unrelated political issue, child labor, which shared YARID's position but did not provide any new information. Similar to refugee hosting, child labor policies are somewhat, but not extremely, sensitive issues in Uganda. We chose our outcomes for these tests to have a similar level of support as refugee hosting.[36] YARID conducted a short campaign opposing child labor within the Grant Only and Information Only arms of our sample. The script

[35] Or, respondents in the control group could exhibit a negative demand effect if they resented not receiving a grant. This is inconsistent with the general stability of control group policy views over time (see Table 1). Demand effects could also lead us to underestimate impacts on true beliefs if the control group believes that it is likely to receive aid in the future.

[36] Baseline support for YARID's position on hiring children under the age of 15 (that is, opposition to hiring them) is 65%. Under age 17, it is 51% (Table C1). These means are similar to baseline support for the refugee integration policies analyzed in Table 1 (51–73%), implying that ceiling effects should not be a concern.

was short, and facilitators were instructed to avoid conversations about the issue. Our goal was to inform respondents of YARID's position only in order to test whether knowledge of YARID's position influenced answers. We intentionally excluded information on the issue of child labor, which could have influenced attitudes through other channels besides knowledge of YARID's position. Our method makes YARID's stance explicit for a placebo issue but excludes any other information on the issue that could affect respondents' views. Our logic is thus analogous to the demand-elicitation instructions in De Quidt, Haushofer and Roth (2018), in that we assume the effect of the explicit stance in the placebo campaign is at least as strong as the implicit stance in the refugee campaign. The script read by YARID facilitators was:

> *Hello, I am [NAME] from YARID. We are an organization that supports people living in Kampala in the areas of small business support, adult education, and women's empowerment. You've been participating in a study and pilot program with us. This call will take about 2 minutes today. Is that ok?*
>
> *[FOR GRANT ONLY GROUP:] You received 500,000 UGX as part of the project.*
>
> *We wanted to follow-up with a separate campaign we are running to stop child labor. We believe that children under the age of 15 should not be working, even for their family's business, and should instead be in school. We are calling to deliver the message that YARID takes a strong position against child labor. Thank you for your time today.*

By comparing the expressed views on child labor of the Information Only arm to the control group (pooled with Information + Labeled Grant, Mentored by Refugee, and Mentored by Ugandan for this specification, which also did not receive the placebo campaign), we test whether knowledge of YARID's view alone affected respondents' expressed preferences, perhaps due to hope for future assistance conditional on "acceptable" answers. In addition, by comparing the impact of the campaign in the Grant Only to the Information Only arms, we can identify whether receiving assistance from YARID amplifies any demand effects, which would complicate our comparison of the Information + Labeled Grant and Information Only arms.

In follow-up surveys taken after the child labor campaign, we find no impacts on attitudes toward child labor in either the Grant Only or the Information Only arm, as shown in Table C1. This indicates that experimenter demand effects within this sample are likely to be low in general, with or without the receipt of assistance.

C.1.2 Priming Experiment

We conducted a within-survey priming experiment in Uganda by randomly asking some respondents about the assistance they had received before eliciting their views toward refugees. We find no significant impact of priming on expressed views (see Appendix Table C2), consistent with limited demand effects in this setting.[37]

[37] The priming experiment was conducted only around the questions on refugees presented in Table C2 and not around our main outcomes on political views to avoid distorting those main outcomes. We believe

Table C1: Impact of Child Labor Information Campaign

	Child Labor Attitudes Index$^+$	No Child Labor Under 15$^+$	No Child Labor Under 17$^+$
Grant Only	-0.08	-0.00	-0.06
	(0.10)	(0.05)	(0.05)
	[0.42]	[0.99]	[0.23]
Information Only	-0.01	-0.04	0.03
	(0.09)	(0.05)	(0.05)
	[0.93]	[0.42]	[0.48]
Observations	732	731	731
Control Mean	0.00	0.65	0.51
Grant = Info	0.56	0.52	0.12

An observation is a surveyed respondent in the 26-month survey round in Uganda. Results estimated through OLS regression with baseline controls chosen through double lasso. Robust standard errors in parentheses; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C2: Within-Survey Priming Experiment

	Primed Outcomes Index	Have Money	Receive More Aid Than Needed	Can Support Themselves	Deserve Sympathy	Refugees Have Skills
Primed on Aid Received$^+$	-0.00	0.02	-0.03	0.01	0.02	0.01
	(0.06)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
	[0.97]	[0.60]	[0.37]	[0.82]	[0.56]	[0.78]
Observations	1,004	884	857	917	953	890
Control Mean	-0.02	0.55	0.52	0.38	0.56	0.46

An observation is a surveyed respondent in the 16-month survey in Uganda. Results estimated through OLS regression with baseline controls chosen through double lasso. Robust standard errors in parentheses; two-sided p-values in brackets. Outcomes not pre-specified denoted with $^+$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C.1.3 Expectations of Future Assistance

Labeled grants may lead respondents to believe that expressing support for refugees will increase their chance of receiving future assistance. We test whether this is the case in our Kenya follow-up survey by asking respondents whether they expect to receive cash from anyone outside their village in the next three months. To provide a benchmark and to partly mask the purpose of the question, we first asked whether they expect to receive cash from anyone inside their village in the next three months. As shown in Table C3, we observe no differences in future aid expectations between Information + Labeled Grant and Grant Only on either measure (coefficients = 0.00–0.01).

The consistent results of Baseler et al. (2024) also help rule out that expectations of

any demand effects would be equally likely for the selected questions, since respondents were not aware of our primary outcomes of interest.

future assistance are driving treatment effects. In that study, one group was given a lump-sum grant bundled with information about aid-sharing, while the control group was informed that they would receive a grant in 18 months. The control group did not receive the same information script but is aware that the implementing partner supports refugees in Uganda through program messaging at registration and sign-up events which included both refugees and Ugandans. Therefore, the control group should by design have the highest expectation of future aid and the strongest incentive to overstate their preferences for refugee integration.

Table C3: Expectations of Future Aid—Kenya

	Expects Aid From Within Village	Expects Aid From Outside Village
Labeled Grant	0.00	0.01
	(0.03)	(0.03)
	[0.93]	[0.76]
Observations	1,046	1,046
Control Mean	0.30	0.38

Each observation is a household. These results are measured using follow-up surveys conducted only in Labeled Grant and Grant Only about one month after the first survey in Kenya. Outcomes are measured using survey questions asking whether the respondent expects to receive cash gifts from anyone inside (outside) their village in the next 3 months. Heteroskedasticity-robust standard errors in parentheses; p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C.1.4 Other Evidence

We do not observe treatment impacts on every outcome related to refugee hosting policy or economic and cultural attitudes about refugee hosting. This is inconsistent with the most extreme demand effects but does not rule out demand effects that appear in some outcomes but not others. To the extent that people with neutral views are the most sensitive to demand effects, the significant treatment impacts on policy views among those who strongly opposed refugee integration (see Appendix Table A4) also indicate a change in true beliefs.

C.2 Contact With Refugees

We find no evidence that treatment impacts are driven by contact with refugees as program facilitators or through increased contact with refugees outside of our programs in Uganda, and that impacts driven through contact with refugees as mentors do not persist. Despite COVID-19 interruptions, our mentorship program involved moderate collaborative inter-group contact relative to other experiments that facilitate contact between different ethnic, national, or religious groups (Pettigrew and Tropp, 2006, Mousa, 2020, Corno, La Ferrara and Burns, 2022). High uptake rates suggest that business owners found the mentorship meetings valuable: 80% of owners assigned to mentorship by a Ugandan and 79% of owners assigned to mentorship by a refugee participated in the program by having at least one meeting.[38] Nevertheless, we find few persistent impacts of mentorship on policy preferences, economic beliefs, or cultural attitudes. We also do not find that contact with a refugee YARID facilitator, relative to a Ugandan YARID facilitator, affects the treatment impacts in Information + Labeled Grant or Information Only arms, as shown in Appendix Table A3, Column 2.

We find no impacts of any treatment arm on contact with refugees by choice, as shown in Appendix Table E14. This indicates that treatment impacts were not mediated by contact with refugees outside the experiment.

C.3 Reciprocity to YARID

In principle, the impacts we observe could reflect intrinsic reciprocity, as in Finan and Schechter (2012), to the implementing non-profit, YARID. Under a reciprocity norm, people feel a desire to increase the payoffs of those who have helped them. If business owners wished to assist YARID—as a result of the grants they received—they may have done so by adopting beliefs they perceive as aligned with YARID, such as beliefs favoring refugee integration. Note that such a channel could exist independently of the experimenter demand effects we consider above. Experimenter demand effects drive gaps between true and reported beliefs; reciprocity could in theory lead owners to update their true beliefs.

Two pieces of evidence suggest that reciprocity norms are not driving our results. First, our Information Only arm increased support for refugee integration policies despite involving no material support from YARID. Second, the placebo campaign described above—delivered by YARID opposing child labor—did not affect business owners' attitudes toward child labor, even among grant recipients. Even if grant recipients did feel a desire to reciprocate, that desire does not appear to manifest in their policy views.

C.4 Personal vs. Group Benefits

To test whether the greater impact of labeled grants compared to information alone is driven by the greater personal benefit conferred by the grant, we exploit the fact that our information script in Uganda focused on hospitals and schools near where our respondents live as examples of public goods funded by aid coming from the refugee response. If variation in personal economic benefits is explaining the differences in impacts across treatment groups, we would expect it to explain variation within the Information Only group as well. Table A12 shows estimates of heterogeneous treatment effects on our index summarizing support for refugee integration based on an indicator for hospital use, an indicator for whether the respondent has children who attend school with foreigners (a proxy for whether the school receives funding from the refugee presence), and an indicator for the union of these two measures, with the caveat that these measures were taken after treatment. We do not find significant differences along these measures, although the estimate for hospital use is positive.

C.5 Differential Attrition

In Uganda, respondents who could not be surveyed after the baseline (9% of the sample) are balanced across treatment groups, as shown in Appendix Table B4. Moreover, the

[38] In the 26-month survey, 35% of those mentored by a refugee report meeting their mentor after the program ended and 18% report meeting within the 30 days preceding the survey.

attrition rates pooled across survey rounds and estimated through ANCOVA regression were not significantly different at the 5% level for any treatment arm compared to Control except for Grant Only, where retention was 8 pp. higher. Retention rates in the pooled specification were modestly higher in Information + Labeled Grant (4 pp., p-val = 0.12) and Mentored by Ugandan (6 pp., p-val = 0.07) compared to Control. Reassuringly, all of our main comparisons of interest are between groups with similar round-by-round attrition rates: Information + Labeled Grant vs. Information Only (p-val = 0.20), Information + Labeled Grant vs. Grant Only (p-val = 0.16), Information + Labeled Grant vs. Mentored by Refugee (p-val = 0.62), Information + Labeled Grant vs. Control (p-val = 0.12), Mentored by Refugee vs. Mentored by Ugandan (p-val = 0.41), and Mentored by Refugee vs. Control (p-val = 0.39). Finally, attrition does not appear to have significantly changed the baseline balance created by randomization (see Appendix Table B1 and Table B2).

Nevertheless, to further assess whether differential attrition is influencing our results in Uganda, we reproduce all of our main results weighting observations by the inverse probability of round-specific retention, estimated by lasso logistic regression.[39] Results, shown in Appendix Tables B6, B7, B8, and B9 are extremely similar to unweighted results. As shown in Appendix Tables B10 and B11, the 95% confidence interval on the lower Lee bound does not cross zero for the impacts of Information + Labeled Grant and Information Only on support for integration policies, although the Lee bounds are wide in some cases. We conclude that differential attrition is not a significant factor in explaining our main results.

C.6 Spillovers

To test for spillovers, we asked treated respondents in the final survey whether they had talked with others about the program and if so, with whom. Talking with others about the program was common—64% said they had—but almost all of this occurs with friends and family. Eleven percent of treated respondents said they talked with other business owners in Kampala about the program. We view this as an upper bound of potential spillovers, since most business owners in Kampala are not in our sample. A stricter test comes from the other side of the information exchange. Only 1% of the Grant Only and 1% of the Control group said they heard that YARID was associated with refugees from a program participant. This statistic is also likely an upper bound for experimental spillovers because YARID operates many programs in the area beyond the ones we evaluated.

C.7 Altruism Crowd-Out

We do not find that redistribution crowds out other sources of policy support such as altruism. We can confidently reject full crowding-out: such an effect would lead us to find null or negative treatment impacts on support for refugee hosting, but in fact these impacts are large, positive, and persistent. We also find evidence against even partial crowding-out. We observe a positive impact of labeled grants on the share donated to refugees in an incentivized dictator game in Uganda, consistent with an increase in altruistic feelings toward refugees. We also observe no negative treatment impacts on the share of respondents reporting that

[39] Specifically, we use the Stata command *lasso logit* with survey retention as the outcome variable and the full set of baseline controls used in Equation 1, partialling out randomization-stratum, survey-wave, and treatment-group fixed effects, and clustering standard errors at the individual level.

most refugees deserve sympathy and positive treatment impacts on measures of perceived social proximity, such as willingness to socialize with or marry refugees.

C.8 Degree and Nature of Inter-Group Contact

We exploit a feature of our randomization design in which a random subset of Ugandan business owners assigned to mentorship started their mentorship meetings earlier. Because of the earlier start date, these business owners had more contact—specifically more in-person contact—with their mentors before the programs were paused due to COVID-19.[40] Within the group assigned to refugee mentors, business owners in the "intensive mentorship" sample met with their mentors in person 3.5 times on average, compared to 1.5 for mentees not in the intensive sample. Including remote meetings, the intensive sample had 5.6 meetings compared to 4.1 in the later sample. See Table B13 for additional summary statistics on mentorship implementation.

We find substantial early impacts on the policy views of business owners who were mentored more intensively by refugees, but these impacts fade out over time, as shown in Appendix Table A15. About 9 months after the meetings began, intensive refugee mentorship had increased our index measure of support for refugee integration by 0.55 sd. ($p < 0.01$). This effect falls to 0.22 sd. ($p = 0.21$) after 16 months and 0.09 sd. ($p = 0.65$) after 26 months. Impacts on beliefs about the economic effects of refugees on Uganda follow a similar pattern, with large initial impacts that fade to insignificance over time. At no point do we observe significant impacts of intensive refugee mentorship on cultural views. Impacts of less intensive refugee mentorship on support for refugee integration are small and positive after 9 months (coeff. = 0.19, $p = 0.07$) but are also smaller and insignificant over time.

[40]Specifically, we randomized 100 business owners within both mentorship arms to start their meetings before the remaining sample so that we could initially assess take-up and viability of the program features. We opted not change the program design after we observed high take-up and positive feedback from this sample.

D Disentangling Wealth and Information Effects With a Model

In this section, we build a simple structural model to estimate the wealth effect of grants in Uganda in the presence of implicit labeling in the Grants Only arm.[41] We allow our treatments to affect views by changing knowledge of aid-sharing or by reducing resource resentment against refugees, and identify wealth effects net of these impacts. Our experimental results suggest that recipients of unlabeled grants perceived the grant to be an example of aid-sharing—likely because our implementing partner was a well-known refugee-led organization—and that grants reduced views that refugees get too much aid.[42] Estimating the wealth effect of grants allows us to recover the marginal impact of the label by computing counterfactual treatment impacts of labeled grants absent any wealth effects.

Consider a set of voters indexed by i deciding whether to support a policy favoring refugee integration. Each voter is exposed through the randomized program $X \in \{LG, G, I\}$—with LG, G, and I denoting Information + Labeled Grant, Grant Only, and Information Only respectively—to a wealth shock Δ_W^X and an awareness shock Δ_A^X relative to voters in a control arm C. We additionally allow for a labeled-grant fixed effect α_{LG}, capturing potential impacts of labeled grants operating independently of the awareness and wealth channels.[43] We choose to model the joint effect of knowledge of aid-sharing and resource resentment—specifically, we estimate treatment impacts on an indicator variable for whether the respondent knows that aid is shared between refugees and Ugandans, or says that refugees do not receive too much aid compared with Ugandans—which we refer to as "awareness."[44] Preferences over supporting the integration policy are represented by the indirect utility function:

$$U_i(\text{Support}_i = 1) = \gamma_W \text{Wealth}_i + \gamma_A \text{Aware}_i + \alpha_{LG} + \epsilon_i,$$

where ϵ_i is an idiosyncratic preference shock distributed independently of wealth and awareness according to a type-I extreme value distribution with shape parameters (μ, θ). Using the cumulative distribution function of ϵ_i and its independence, average policy support in a

[41]By wealth effect, we mean the change in views that could result directly from the capital infusion, perhaps due to feelings of economic scarcity. While we were unable to measure any effects on wealth from the treatments, it's possible the effects were temporary or present in other outcomes we did not measure.

[42]The Grant Only arm increased knowledge of aid-sharing by 9 pp. ($p < 0.01$), as shown in Table 3 Column 3. It also reduced the share of respondents reporting that refugees receive too much aid relative to Ugandans by 15 pp. ($p < 0.01$), as shown in Appendix Table A11 Column 2.

[43]For example, voters who receive a labeled grant may conclude that aid-sharing is more likely to benefit them personally compared to those receiving information alone, even conditional on Δ_A^X. On the other hand, if labeled grants make information about aid-sharing more credible, this effect will operate through Δ_A^X.

[44]An alternate model that separates knowledge of aid-sharing from resource resentment is also identified and yields similar results: the estimated impact of the labeled grant on support for refugee hosting net of wealth effects is 0.144 ($p < 0.01$) in the alternative model. However, these estimates are noisier because our estimate of γ_A is unbounded as $\Delta_A^I \to 0$. We therefore focus on modeling the joint impact of knowledge and resentment.

treatment arm X is approximated by:

$$E[\text{Support}_i^X] = 1 - \exp\{-\exp\{(\mu - \bar{c}_0 + \gamma_A \Delta_A^X + \gamma_W \Delta_W^X + \alpha_{LG})/\theta\}\}. \tag{2}$$

where Δ_A^X and Δ_W^X are treatment impacts on average awareness and wealth respectively and $\bar{c}_0 \equiv E[U_i(\text{Support}_i = 0) - \gamma_W \text{Wealth}_{i0} - \gamma_A \text{Aware}_{i0}]$, with the 0 subscripts denoting baseline levels. Note that random assignment of X implies that $\bar{c}_0$ is equal in expectation across treatment conditions.[45] Without loss of generality, we normalize $\theta = 1$ and let $\tilde{S}^X \equiv \log\left(-\log(1 - E[\text{Support}_i^X])\right)$.[46] This gives:

$$\begin{aligned} \tilde{S}^{LG} - \tilde{S}^C &= \gamma_A \Delta_A^{LG} + \gamma_W \Delta_W^{LG} + \alpha_{LG} \\ \tilde{S}^G - \tilde{S}^C &= \gamma_A \Delta_A^G + \gamma_W \Delta_W^G \\ \tilde{S}^I - \tilde{S}^C &= \gamma_A \Delta_A^I. \end{aligned} \tag{3}$$

These three differences express three treatment impacts in terms of known quantities—observed policy support and treatment impacts on awareness—and three unknowns: γ_A, $\gamma_W \Delta_W^X$, and α_{LG}. Note that the size of the grant Δ_W^X is the same in G and LG by design. We solve for counterfactual average support by treatment group using Equation 2 and setting the relevant mechanism to zero: for example, setting $\gamma_W = 0$ recovers mean support net of wealth effects.[47] We estimate confidence intervals using 2,000 bootstrap samples, re-estimating treatment impacts on support and awareness and solving Equation 3 in each under the constraint $\gamma_A \geq 0$.[48] We repeat this for the three binary support measures shown in Table 1 for which Grant Only impacts are statistically significant: support for refugee hosting overall, support for admitting more refugees, and support for labor market access.

Results. Across all three policy support outcomes, we identify large awareness effects and small wealth effects. Treatment effects in Grant Only when the awareness channel is shut down—that is, isolating the wealth effect of grants—are estimated to be positive but statistically insignificant. Treatment effects in Information + Labeled Grant when the wealth channel is shut down are similar in magnitude to estimates in Table 1 and statistically significant ($p < 0.01$ for all three outcomes).

Interpretation. These results are consistent with our reduced-form analysis in both countries, as discussed in Section 5.1. We also find a small labeled-grant fixed effect, consistent with little scope for interactions between information and grants except through impacts on awareness. Instead, labeled grants appear to increase trust in donor institutions, which can affect awareness of aid-sharing by making the information given more credible.

[45] The substitution of the means of wealth and awareness for their individual values in Equation 2 is justified by the independence of ϵ_i. Simulations show that the approximation error is small. We draw ϵ_i from a type-I extreme value distribution using our estimated parameters and compute mean support using baseline data on wealth and awareness of aid-sharing, or their mean levels in that data as an approximation. Across 2,000 simulations, the mean absolute approximation error is less than 1% of support for refugee hosting.

[46] Because θ enters each difference in Equation 3 multiplicitavely, it acts only as a scaling factor and does

Table D1: Parameter Estimates

	Supports Refugee Hosting	Supports More Refugees	Supports Right to Work
Parameter Estimates			
Awareness Coefficient (γ_A)	1.14**	1.71***	1.58***
	(0.36,2.80)	(0.85,3.93)	(0.76,3.65)
	[0.02]	[0.01]	[0.01]
Wealth Term ($\gamma_W \Delta_W$)	0.10	0.07	0.04
	(-0.09,0.24)	(-0.19,0.23)	(-0.20,0.20)
	[0.34]	[0.52]	[0.71]
Labeled Grant Fixed Effect (α_{LG})	0.11	0.00	0.06
	(-0.08,0.26)	(-0.23,0.18)	(-0.23,0.18)
	[0.30]	[0.99]	[0.58]
Counterfactual Treatment Effects			
Info. + Labeled Grant: No Wealth Effect	0.11***	0.12***	0.12***
	(0.05,0.17)	(0.06,0.23)	(0.07,0.20)
	[0.00]	[0.00]	[0.00]
Grant: Wealth Effect	0.04	0.03	0.01
	(-0.03,0.08)	(-0.07,0.09)	(-0.07,0.07)
	[0.34]	[0.52]	[0.71]

See Appendix D for estimation details. γ_A, $\gamma_W \Delta_W$, and α_{LG} are obtained by solving Equation 3 under the constraint $\gamma_A > 0$. *Info. + Labeled Grant: No Wealth Effect* shows the counterfactual treatment effect of Labeled Grant when $\gamma_W = 0$ estimated using Equation 2. *Grant: Wealth Effect* shows the counterfactual treatment effect of Grant Only when $\gamma_A = 0$ estimated using Equation 2. Bootstrap 90% confidence intervals in parentheses show the 5th and 95th percentiles from 2,000 simulations. p-values in brackets test the two-sided hypothesis that the coefficient or treatment effect equals zero, and are estimated by doubling the largest α for which the bootstrap $1 - \alpha$ confidence interval includes zero. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

not influence counterfactual estimates.

[47]To solve Equation 2 given coefficient estimates, note that $\tilde{S}^C = \mu - \bar{c}_0$.

[48]This constraint binds for 28 out of 6,000 bootstrap estimates, or 0.5%.

E All Pre-Specified Results

This appendix presents the results specified in our Uganda pre-analysis plan, and additional analysis that is not pre-specified, denoted with a $^+$ in the domain heading or outcome.

Domain 1: Support for refugee integration policies

- Overall, during coronavirus, I am in favor of Uganda hosting and assisting refugees. (*Questions not exactly the same: no COVID-19 info in baseline and after first follow-up.*)
- After coronavirus, I am in favor of Uganda hosting and assisting refugees. (*Questions not exactly the same: no COVID-19 info in baseline.*)
- In July refugees from Congo were allowed to come to Uganda. They were tested for coronavirus, quarantined, and settled into camps. I am in favor of allowing refugees who test negative to move to Uganda right now.
- After coronavirus ends, Uganda should accept more refugees. (*Questions not exactly the same: no COVID-19 info in baseline.*)
- During coronavirus, Uganda should relocate all refugees to live in the settlements, including those currently living in Kampala. (*Questions not exactly the same: no COVID-19 info in baseline and after first follow-up.*)
- For those who answered "agree" or "strongly agree": Should the relocation be permanent or only during coronavirus?
- Uganda should continue allowing refugees who already live in Uganda to work outside the settlements, according to any lockdown rules, during coronavirus. (*Questions not exactly the same: no COVID-19 info in baseline and after first follow-up.*)
- After coronavirus ends, Uganda should continue allowing refugees to work outside the settlements. (*Questions not exactly the same: no COVID-19 info in baseline.*)

Table E1: Full Set of Outcomes in Domain 1

	Integration Policies Index	Supports Hosting Current	Supports Hosting Post-COVID	More Refugees Current	More Refugees Post-COVID	Freedom of Movement Current	Freedom of Movement Post-COVID	Right to Work Current	Right to Work Post-COVID
Info. + Labeled Grant	0.36***	0.13***	0.08***	0.15***	0.11***	0.06**	0.06	0.13***	0.08***
	(0.06)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)
	[0.00]	[0.00]	[0.02]	[0.00]	[0.00]	[0.05]	[0.10]	[0.00]	[0.01]
Information Only	0.22***	0.06**	0.08***	0.10***	0.04	0.03	0.05	0.08***	0.03
	(0.07)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)
	[0.00]	[0.04]	[0.02]	[0.01]	[0.19]	[0.23]	[0.16]	[0.01]	[0.21]
Grant Only	0.25***	0.09***	0.11***	0.12***	0.08**	0.00	0.01	0.10***	0.06**
	(0.07)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)
	[0.00]	[0.01]	[0.00]	[0.00]	[0.03]	[0.45]	[0.41]	[0.00]	[0.05]
Mentored by Refugee	0.12*	0.04	0.04	0.06*	0.02	-0.03	0.03	0.08**	0.04
	(0.07)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)
	[0.10]	[0.19]	[0.21]	[0.09]	[0.38]	[0.27]	[0.31]	[0.03]	[0.14]
Mentored by Ugandan	0.10	0.07**	0.08**	0.04	0.07*	-0.06*	0.02	0.02	0.02
	(0.08)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)
	[0.18]	[0.04]	[0.03]	[0.19]	[0.07]	[0.09]	[0.39]	[0.27]	[0.27]
Observations	3,051	3,040	2,142	3,038	2,138	3,031	1,089	3,039	2,139
Control Mean: Baseline	0.00	0.73	0.73	0.51	0.51	0.60	0.60	0.60	0.60
Control Mean: Follow-Ups	-0.00	0.75	0.75	0.60	0.68	0.54	0.78	0.72	0.79
Labeled Grant = Info Only	0.02	0.00	0.90	0.08	0.02	0.26	0.74	0.04	0.05
Labeled Grant = Grant Only	0.05	0.06	0.38	0.39	0.38	0.05	0.18	0.12	0.44
Labeled Grant = R-Mentee	0.00	0.00	0.14	0.01	0.01	0.01	0.40	0.04	0.26
R-Mentee = Info Only	0.13	0.38	0.16	0.24	0.53	0.13	0.57	0.78	0.60
R-Mentee = U-Mentee	0.80	0.35	0.15	0.66	0.16	0.40	0.82	0.11	0.54

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 1.1: Support for refugee integration policies (Additional Measures)+

Table E2: Full Set of Outcomes in Domain 111

	Complete Index	Provide Land in Settlements	Provide Indef Stay	Provide Citizen-Ship	More Afghanistan Refugees	Freedom of Movement Friends	More Refugees Friends
Info. + Labeled Grant	0.38***	0.15***	0.06**	0.14***	0.10**	0.01	0.05
	(0.06)	(0.04)	(0.02)	(0.04)	(0.05)	(0.06)	(0.06)
	[0.00]	[0.00]	[0.12]	[0.00]	[0.14]	[1.00]	[0.84]
Information Only	0.20***	0.08**	0.02	0.07*	0.05	-0.05	0.01
	(0.07)	(0.04)	(0.03)	(0.04)	(0.05)	(0.06)	(0.06)
	[0.00]	[0.12]	[0.87]	[0.17]	[0.71]	[0.84]	[1.00]
Grant Only	0.23***	0.11***	0.03	0.08**	0.12**	0.04	0.14**
	(0.07)	(0.04)	(0.03)	(0.04)	(0.05)	(0.06)	(0.06)
	[0.00]	[0.04]	[0.63]	[0.12]	[0.12]	[0.84]	[0.12]
Mentored by Refugee	0.11	0.03	-0.02	0.03	-0.00	-0.04	-0.07
	(0.07)	(0.04)	(0.03)	(0.05)	(0.06)	(0.07)	(0.07)
	[0.13]	[0.87]	[0.92]	[0.87]	[1.00]	[0.87]	[0.71]
Mentored by Ugandan	0.14*	0.08*	-0.01	0.06	0.00	0.01	-0.02
	(0.08)	(0.04)	(0.03)	(0.05)	(0.06)	(0.07)	(0.07)
	[0.08]	[0.17]	[1.00]	[0.38]	[1.00]	[1.00]	[1.00]
Observations	3,051	1,942	1,773	1,942	888	673	674
Control Mean: Baseline	0.00	0.55	0.84	0.54	.	.	.
Control Mean: Follow-Ups	-0.00	0.53	0.86	0.61	0.47	0.60	0.50
Labeled Grant = Info Only	0.00	0.08	0.07	0.06	0.35	0.37	0.59
Labeled Grant = Grant Only	0.02	0.31	0.20	0.12	0.75	0.55	0.15
Labeled Grant = R-Mentee	0.00	0.00	0.01	0.01	0.08	0.45	0.09
R-Mentee = Info Only	0.20	0.18	0.30	0.32	0.36	0.96	0.26
R-Mentee = U-Mentee	0.74	0.26	0.84	0.46	0.95	0.49	0.56

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 2: Business profits

- What were the profits of your business during the last 30 days?

Table E3: Full Set of Outcomes in Domain 2

	Profit (Standardized)	Profit
Info. + Labeled Grant	-0.06	-0.19
	(0.06)	(0.15)
	[0.28]	[1.00]
Information Only	-0.04	-0.09
	(0.06)	(0.16)
	[0.55]	[1.00]
Grant Only	-0.04	-0.11
	(0.06)	(0.16)
	[0.52]	[1.00]
Mentored by Refugee	0.02	0.06
	(0.07)	(0.18)
	[0.76]	[1.00]
Mentored by Ugandan	-0.12	-0.28
	(0.07)	(0.19)
	[0.11]	[1.00]
Observations	4,029	4,029
Control Mean: Baseline	0.00	39.61
Control Mean: Follow-Ups	-0.00	20.69
Labeled Grant = Info Only	0.65	0.52
Labeled Grant = Grant Only	0.69	0.62
Labeled Grant = R-Mentee	0.19	0.14
R-Mentee = Info Only	0.38	0.39
R-Mentee = U-Mentee	0.07	0.08

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 3: Knowledge of refugees and hosting policy

- How many refugees in Uganda live outside of camps or settlements: all, most, some, few, or none? ("Some" or "few" will be considered correct answers)
- Are refugees allowed to live outside of the camps or settlements? ("yes" is correct)
- Are any of the international donations to refugees in Uganda shared with Ugandans? ("yes" is correct)

Table E4: Full Set of Outcomes in Domain 3

	Knowledge Index	Live Outside Settlements	Allowed Outside Settlements	Knows About Aid-Sharing
Info. + Labeled Grant	0.30***	0.07*	0.11***	0.15***
	(0.07)	(0.04)	(0.04)	(0.03)
	[0.00]	[0.15]	[0.01]	[0.00]
Information Only	0.11*	0.00	0.11***	0.05
	(0.06)	(0.04)	(0.04)	(0.03)
	[0.09]	[0.82]	[0.01]	[0.18]
Grant Only	0.17**	0.03	0.07*	0.09***
	(0.07)	(0.04)	(0.04)	(0.03)
	[0.01]	[0.50]	[0.18]	[0.02]
Mentored by Refugee	-0.06	0.01	-0.02	-0.03
	(0.07)	(0.04)	(0.04)	(0.04)
	[0.43]	[0.68]	[0.52]	[0.49]
Mentored by Ugandan	0.10	0.01	0.07	0.02
	(0.07)	(0.04)	(0.04)	(0.04)
	[0.17]	[0.69]	[0.18]	[0.52]
Observations	3,061	1,942	1,942	3,061
Control Mean: Baseline	0.00	0.36	0.45	0.17
Control Mean: Follow-Ups	0.00	0.47	0.47	0.37
Labeled Grant = Info Only	0.00	0.06	0.96	0.00
Labeled Grant = Grant Only	0.04	0.26	0.21	0.09
Labeled Grant = R-Mentee	0.00	0.17	0.00	0.00
R-Mentee = Info Only	0.02	0.78	0.00	0.02
R-Mentee = U-Mentee	0.04	0.93	0.04	0.19

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 4: Beliefs about economic effects of refugees

- How do the [sector] businesses managed by people from other countries affect your business overall? Do they help you a lot, help you a little, hurt you a little, hurt you a lot, or have no effect on you? (*Compared to the similar question on Ugandans from your tribe.*)
- Taking everything into consideration, would you say the overall economic effect of refugees on Uganda has been positive, negative, or neutral?
- How about the overall economic effect of refugees on you personally?
- How many refugees have skills and contribute to the economy?

Table E5: Full Set of Outcomes in Domain 4

	Economic Beliefs Index	Pos Effect on Your Business	Pos Effect on Economy Overall	Pos Effect on You Personally	Refugees Have Skills
Info. + Labeled Grant	0.30***	0.03	0.16***	0.09***	0.10**
	(0.07)	(0.05)	(0.03)	(0.04)	(0.04)
	[0.00]	[0.62]	[0.00]	[0.03]	[0.04]
Information Only	0.22***	0.06	0.12***	0.06*	0.02
	(0.07)	(0.05)	(0.03)	(0.03)	(0.04)
	[0.00]	[0.38]	[0.01]	[0.19]	[0.73]
Grant Only	0.21***	0.03	0.10***	0.11***	0.03
	(0.07)	(0.05)	(0.04)	(0.04)	(0.04)
	[0.00]	[0.62]	[0.03]	[0.02]	[0.54]
Mentored by Refugee	0.07	0.08	0.04	-0.04	0.01
	(0.08)	(0.05)	(0.04)	(0.04)	(0.05)
	[0.34]	[0.22]	[0.49]	[0.49]	[0.83]
Mentored by Ugandan	0.07	-0.05	0.04	0.06	0.00
	(0.08)	(0.05)	(0.04)	(0.04)	(0.05)
	[0.35]	[0.49]	[0.49]	[0.29]	[0.85]
Observations	3,003	887	2,787	2,906	1,671
Control Mean: Baseline	0.00	0.63	0.50	0.41	0.51
Control Mean: Follow-Ups	-0.00	0.73	0.42	0.44	0.42
Labeled Grant = Info Only	0.25	0.50	0.19	0.32	0.04
Labeled Grant = Grant Only	0.23	0.99	0.07	0.69	0.11
Labeled Grant = R-Mentee	0.00	0.26	0.00	0.00	0.06
R-Mentee = Info Only	0.05	0.58	0.03	0.01	0.91
R-Mentee = U-Mentee	1.00	0.02	0.96	0.02	0.89

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 4a: Beliefs about economic effects of Congolese refugees

- Taking everything into consideration, would you say the overall economic effect of Congolese on Uganda has been positive, negative, or neutral?
- How have access and quality of schools and health facilities been affected by Congolese in Kampala?
- How have rents been affected by Congolese in Kampala?
- How have prices of goods you buy, other than rents, been affected by Congolese in Kampala?

Table E6: Full Set of Outcomes in Domain 41

	Economic Beliefs Index	Overall Economy	Schools & Healthcare	Prices Rent	Prices Other Goods
Info. + Labeled Grant	0.23***	0.12***	0.05	0.05	0.02
	(0.08)	(0.04)	(0.04)	(0.03)	(0.04)
	[0.01]	[0.05]	[1.00]	[1.00]	[1.00]
Information Only	0.09	0.08*	-0.02	0.01	0.03
	(0.08)	(0.05)	(0.04)	(0.03)	(0.04)
	[0.31]	[0.86]	[1.00]	[1.00]	[1.00]
Grant Only	0.11	0.13***	-0.01	0.02	-0.01
	(0.08)	(0.04)	(0.04)	(0.03)	(0.04)
	[0.20]	[0.05]	[1.00]	[1.00]	[1.00]
Mentored by Refugee	-0.03	-0.00	-0.03	0.01	0.02
	(0.10)	(0.05)	(0.04)	(0.04)	(0.05)
	[0.75]	[1.00]	[1.00]	[1.00]	[1.00]
Mentored by Ugandan	0.02	0.06	-0.03	0.01	0.00
	(0.09)	(0.05)	(0.04)	(0.03)	(0.04)
	[0.86]	[1.00]	[1.00]	[1.00]	[1.00]
Observations	1,626	1,482	1,517	1,584	1,590
Control Mean: Baseline	0.00	0.47	0.20	0.30	0.49
Control Mean: Follow-Ups	-0.00	0.55	0.23	0.15	0.32
Labeled Grant = Info Only	0.09	0.30	0.06	0.27	0.80
Labeled Grant = Grant Only	0.15	0.81	0.11	0.37	0.38
Labeled Grant = R-Mentee	0.01	0.01	0.04	0.33	0.98
R-Mentee = Info Only	0.24	0.09	0.66	0.94	0.82
R-Mentee = U-Mentee	0.64	0.20	0.83	0.91	0.76

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 4b: Beliefs about economic effects of Somali refugees

- Taking everything into consideration, would you say the overall economic effect of Somalis on Uganda has been positive, negative, or neutral?
- How have access and quality of schools and health facilities been affected by Somalis in Kampala?
- How have rents been affected by Somalis in Kampala?
- How have prices of goods you buy, other than rents, been affected by Somalis in Kampala?

Table E7: Full Set of Outcomes in Domain 42

	Economic Beliefs Index	Overall Economy	Schools & Healthcare	Prices Rent	Prices Other Goods
Info. + Labeled Grant	0.03	0.15***	-0.14**	-0.04	-0.01
	(0.08)	(0.05)	(0.06)	(0.03)	(0.04)
	[0.73]	[0.04]	[0.12]	[1.00]	[1.00]
Information Only	0.04	0.06	-0.00	-0.02	-0.03
	(0.08)	(0.05)	(0.06)	(0.03)	(0.04)
	[0.66]	[1.00]	[1.00]	[1.00]	[1.00]
Grant Only	0.08	0.12**	-0.03	-0.03	0.04
	(0.09)	(0.05)	(0.06)	(0.03)	(0.04)
	[0.35]	[0.12]	[1.00]	[1.00]	[1.00]
Mentored by Refugee	-0.03	-0.03	-0.03	0.01	-0.01
	(0.10)	(0.05)	(0.06)	(0.04)	(0.05)
	[0.79]	[1.00]	[1.00]	[1.00]	[1.00]
Mentored by Ugandan	-0.01	0.02	-0.04	-0.01	0.01
	(0.09)	(0.05)	(0.06)	(0.03)	(0.05)
	[0.91]	[1.00]	[1.00]	[1.00]	[1.00]
Observations	1,609	1,389	1,384	1,524	1,537
Control Mean: Baseline	0.00	0.41	0.19	0.20	0.40
Control Mean: Follow-Ups	-0.00	0.46	1.95	2.86	2.68
Labeled Grant = Info Only	0.91	0.06	0.02	0.55	0.64
Labeled Grant = Grant Only	0.53	0.53	0.06	0.71	0.21
Labeled Grant = R-Mentee	0.57	0.00	0.09	0.21	0.92
R-Mentee = Info Only	0.51	0.06	0.61	0.42	0.64
R-Mentee = U-Mentee	0.88	0.35	0.85	0.65	0.82

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 5a: Beliefs that refugees receive too much aid

- How many refugees have a lot of money? All, most, some, few, or none?
- How many refugees get more assistance than they need?

Table E8: Full Set of Outcomes in Domain 51

	Economic Perceptions Index	Have Money	Receive More Aid Than Needed
Info. + Labeled Grant	0.06	0.02	0.01
	(0.08)	(0.04)	(0.04)
	[0.44]	[1.00]	[1.00]
Information Only	0.00	-0.01	0.00
	(0.08)	(0.04)	(0.04)
	[0.99]	[1.00]	[1.00]
Grant Only	-0.00	-0.02	0.02
	(0.08)	(0.04)	(0.04)
	[0.99]	[1.00]	[1.00]
Mentored by Refugee	-0.09	-0.01	-0.03
	(0.09)	(0.05)	(0.04)
	[0.31]	[1.00]	[1.00]
Mentored by Ugandan	-0.06	-0.05	-0.01
	(0.09)	(0.05)	(0.04)
	[0.51]	[1.00]	[1.00]
Observations	1,828	1,709	1,717
Control Mean: Baseline	0.00	0.58	0.51
Control Mean: Follow-Ups	0.00	0.55	0.55
Labeled Grant = Info Only	0.46	0.46	0.83
Labeled Grant = Grant Only	0.45	0.37	0.83
Labeled Grant = R-Mentee	0.09	0.59	0.33
R-Mentee = Info Only	0.31	0.89	0.43
R-Mentee = U-Mentee	0.78	0.42	0.66

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 5b: Beliefs that refugees can support themselves

- How many refugees are able to support themselves financially without assistance?

Table E9: Full Set of Outcomes in Domain 52

	Economic Perceptions Index	Can Support Themselves
Info. + Labeled Grant	-0.07	-0.04
	(0.08)	(0.04)
	[0.33]	[1.00]
Information Only	0.02	0.01
	(0.08)	(0.04)
	[0.80]	[1.00]
Grant Only	-0.15**	-0.08**
	(0.08)	(0.04)
	[0.04]	[0.21]
Mentored by Refugee	0.01	0.00
	(0.09)	(0.05)
	[0.92]	[1.00]
Mentored by Ugandan	-0.01	-0.01
	(0.09)	(0.04)
	[0.87]	[1.00]
Observations	1,757	1,757
Control Mean: Baseline	0.00	0.47
Control Mean: Follow-Ups	0.00	0.38
Labeled Grant = Info Only	0.24	0.23
Labeled Grant = Grant Only	0.28	0.28
Labeled Grant = R-Mentee	0.38	0.37
R-Mentee = Info Only	0.92	0.91
R-Mentee = U-Mentee	0.81	0.83

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 6: Cultural attitudes toward refugees

- What effect have refugees had on culture in Uganda?
- I would be comfortable marrying a refugee. (*Social distance index constructed based on these four questions.*)
- I would be comfortable having a refugee marry a member of my family.
- I would be comfortable having a refugee as a close, personal friend.
- I would be comfortable having a refugee as a neighbor.
- How many refugees deserve sympathy and support?
- Our research team has an extra UGX available. We can give it to you or share it between you and two charity organizations in Uganda. The first charity helps poor Ugandans living in Kampala. The second charity helps refugees living in Kampala. We are going to let you decide how to split the money. How much of the UGX should we give to the charity supporting poor Ugandans in Kampala? (*Questions not exactly the same: 3000 total in baseline, 1500 total in follow-up 1, and 3000 total in follow-ups two and four. Proportion calculated. Not included in index calculation.*)
- How much of the remaining UGX should we give to the charity supporting refugees in Kampala? (*Questions not exactly the same: 3000 total in baseline, 1500 total in follow-up 1, and 3000 total in follow-ups two and four. Proportion calculated.*)
- How safe do you feel walking around areas in Kampala where people from other countries live? You can say very safe, somewhat safe, neutral, somewhat unsafe, very unsafe, or that it depends on the nationality. (*Compared to the similar question on walking around most areas in Kampala.*)
- Is there tension between Ugandans and people from other nationalities?

Table E10: Full Set of Outcomes in Domain 6

	Cultural Attitudes Index	Positive Effect on Culture	Social Proximity Index	Refugees Deserve Sympathy	Prop. Donated Refugees	Prop. Donated Ugandans	Feel Safe in Areas w Foreigners	No Tension with Foreigners
Info. + Labeled Grant	0.16**	-0.00	0.28***	0.03	0.05***	0.02	-0.03	0.07
	(0.07)	(0.03)	(0.08)	(0.04)	(0.02)	(0.02)	(0.03)	(0.05)
	[0.01]	[1.00]	[0.01]	[0.69]	[0.06]	[0.59]	[0.69]	[0.45]
Information Only	0.06	0.05*	0.20**	0.04	-0.00	0.01	-0.05	0.00
	(0.06)	(0.03)	(0.08)	(0.04)	(0.02)	(0.02)	(0.03)	(0.05)
	[0.32]	[0.41]	[0.10]	[0.69]	[1.00]	[0.87]	[0.45]	[1.00]
Grant Only	0.13*	-0.02	0.19**	0.08**	0.04***	0.03*	-0.06*	0.04
	(0.07)	(0.03)	(0.08)	(0.04)	(0.02)	(0.02)	(0.04)	(0.05)
	[0.06]	[0.69]	[0.10]	[0.25]	[0.10]	[0.41]	[0.41]	[0.69]
Mentored by Refugee	-0.03	0.02	0.07	-0.02	-0.02	-0.02	-0.07	0.07
	(0.07)	(0.04)	(0.09)	(0.05)	(0.02)	(0.02)	(0.04)	(0.05)
	[0.69]	[0.69]	[0.69]	[0.87]	[0.61]	[0.61]	[0.41]	[0.45]
Mentored by Ugandan	0.03	0.05	0.08	-0.02	-0.00	-0.01	-0.02	-0.03
	(0.07)	(0.03)	(0.09)	(0.04)	(0.02)	(0.02)	(0.04)	(0.05)
	[0.71]	[0.41]	[0.69]	[0.87]	[1.00]	[0.69]	[0.87]	[0.69]
Observations	3,061	2,612	1,942	1,814	3,061	3,061	1,648	1,312
Control Mean: Baseline	0.00	0.71	0.02	0.46	0.21	0.32	0.69	0.86
Control Mean: Follow-Ups	0.00	0.69	0.00	0.54	0.28	0.35	0.78	0.65
Labeled Grant = Info Only	0.10	0.09	0.20	0.91	0.00	0.40	0.56	0.15
Labeled Grant = Grant Only	0.55	0.45	0.18	0.18	0.77	0.56	0.38	0.50
Labeled Grant = R-Mentee	0.01	0.51	0.01	0.27	0.00	0.04	0.38	0.90
R-Mentee = Info Only	0.18	0.45	0.15	0.23	0.30	0.15	0.69	0.16
R-Mentee = U-Mentee	0.45	0.43	0.92	0.96	0.38	0.69	0.25	0.05

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 6alt: Alternative Measures of Donations+

Table E11: Full Set of Outcomes in Domain 63

	Donation Index	Donation Refugees > Ugandans	Donation Refugees ≥ Ugandans
Info. + Labeled Grant	0.07	0.00	0.04
	(0.06)	(0.02)	(0.03)
	[0.27]	[1.00]	[1.00]
Information Only	-0.04	-0.01	-0.01
	(0.06)	(0.02)	(0.03)
	[0.55]	[1.00]	[1.00]
Grant Only	0.02	0.00	0.02
	(0.07)	(0.02)	(0.03)
	[0.80]	[1.00]	[1.00]
Mentored by Refugee	0.03	-0.00	0.02
	(0.08)	(0.02)	(0.03)
	[0.74]	[1.00]	[1.00]
Mentored by Ugandan	0.02	0.00	0.01
	(0.07)	(0.02)	(0.03)
	[0.83]	[1.00]	[1.00]
Observations	3,061	3,061	3,061
Control Mean: Baseline	0.00	0.04	0.77
Control Mean: Follow-Ups	-0.00	0.08	0.80
Labeled Grant = Info Only	0.07	0.65	0.04
Labeled Grant = Grant Only	0.37	0.99	0.37
Labeled Grant = R-Mentee	0.53	0.83	0.51
R-Mentee = Info Only	0.39	0.88	0.23
R-Mentee = U-Mentee	0.90	0.79	0.71

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 6a: Cultural attitudes toward Congolese refugees

- I would be comfortable marrying a Congolese. (*Social distance index constructed based on these four questions.*)
- I would be comfortable having a Congolese marry a member of my family.
- I would be comfortable having a Congolese as a close, personal friend.
- I would be comfortable having a Congolese as a neighbor.
- What effect have Congolese had on culture in Uganda?
- Please tell us how the dress code has been affected by Congolese in Kampala. You can answer positive, negative, or no effect.
- How have acceptable behaviors (such as how people talk to each other) been affected by Congolese in Kampala?

Table E12: Full Set of Outcomes in Domain 61

	Cultural Attitudes Index	Social Proximity Index	Pos Effect Culture	Pos Effect Dress Code	Pos Effect Behaviors
Info. + Labeled Grant	0.32***	0.28***	0.08*	0.06	0.06
	(0.09)	(0.08)	(0.04)	(0.04)	(0.04)
	[0.00]	[0.01]	[0.26]	[0.49]	[0.41]
Information Only	0.11	0.15*	0.05	-0.01	-0.01
	(0.08)	(0.08)	(0.04)	(0.04)	(0.04)
	[0.19]	[0.26]	[0.63]	[1.00]	[1.00]
Grant Only	0.27***	0.26***	0.11**	0.03	0.04
	(0.08)	(0.08)	(0.04)	(0.04)	(0.03)
	[0.00]	[0.01]	[0.08]	[0.95]	[0.63]
Mentored by Refugee	-0.02	0.04	0.01	-0.05	-0.00
	(0.11)	(0.10)	(0.05)	(0.05)	(0.04)
	[0.85]	[1.00]	[1.00]	[0.79]	[1.00]
Mentored by Ugandan	0.09	0.05	0.06	-0.01	0.01
	(0.09)	(0.09)	(0.05)	(0.05)	(0.04)
	[0.30]	[1.00]	[0.58]	[1.00]	[1.00]
Observations	1,647	1,646	1,430	1,545	1,503
Control Mean: Baseline	0.00	0.04	0.80	0.51	0.77
Control Mean: Follow-Ups	-0.00	-0.00	0.24	0.44	0.20
Labeled Grant = Info Only	0.01	0.08	0.43	0.10	0.08
Labeled Grant = Grant Only	0.62	0.81	0.50	0.49	0.57
Labeled Grant = R-Mentee	0.00	0.01	0.17	0.03	0.15
R-Mentee = Info Only	0.21	0.26	0.48	0.42	0.90
R-Mentee = U-Mentee	0.28	0.92	0.35	0.54	0.81

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 6b: Cultural attitudes toward Somali refugees

- *Social distance index constructed based on these four questions.*
  - I would be comfortable marrying a Somalis.
  - I would be comfortable having a Somalis marry a member of my family.
  - I would be comfortable having a Somalis as a close, personal friend.
  - I would be comfortable having a Somalis as a neighbor.
- What effect have Somalis had on culture in Uganda?
- Please tell us how the dress code has been affected by Somalis in Kampala. You can answer positive, negative, or no effect.
- How have acceptable behaviors (such as how people talk to each other) been affected by Somalis in Kampala?

Table E13: Full Set of Outcomes in Domain 62

	Cultural Attitudes Index	Social Proximity Index	Pos Effect Culture	Pos Effect Dress Code	Pos Effect Behaviors
Info. + Labeled Grant	0.27***	0.23***	0.06	0.01	0.06*
	(0.08)	(0.08)	(0.04)	(0.04)	(0.03)
	[0.00]	[0.12]	[0.52]	[1.00]	[0.52]
Information Only	0.09	0.07	0.02	-0.06	0.03
	(0.08)	(0.09)	(0.04)	(0.04)	(0.03)
	[0.27]	[1.00]	[1.00]	[0.52]	[1.00]
Grant Only	0.10	0.14	0.07*	-0.02	-0.00
	(0.09)	(0.09)	(0.04)	(0.04)	(0.03)
	[0.25]	[0.52]	[0.52]	[1.00]	[1.00]
Mentored by Refugee	-0.00	0.09	0.02	-0.09*	0.02
	(0.09)	(0.10)	(0.04)	(0.05)	(0.04)
	[0.98]	[1.00]	[1.00]	[0.52]	[1.00]
Mentored by Ugandan	0.04	0.02	0.02	0.02	0.00
	(0.08)	(0.09)	(0.04)	(0.05)	(0.04)
	[0.60]	[1.00]	[1.00]	[1.00]	[1.00]
Observations	1,647	1,647	1,353	1,519	1,447
Control Mean: Baseline	0.00	0.06	0.74	0.40	0.71
Control Mean: Follow-Ups	-0.00	0.00	0.46	0.40	0.49
Labeled Grant = Info Only	0.03	0.05	0.19	0.09	0.28
Labeled Grant = Grant Only	0.05	0.27	0.79	0.59	0.05
Labeled Grant = R-Mentee	0.00	0.14	0.32	0.05	0.23
R-Mentee = Info Only	0.32	0.85	0.89	0.62	0.84
R-Mentee = U-Mentee	0.61	0.51	0.95	0.04	0.68

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 7: Contact with refugees by choice

- How many of your business collaborators are from another country?
- Would you be open to collaborating with business owners from another country?
- In the last 30 days, have you bought supplies (such as materials for your business), tools, or machines from someone from another country?
- Have you ever had an apprentice or person from outside your household at your business who was learning skills but not paid who was from another country?
- Are any of your employees from a different country than you?
- In the past 30 days, how many people from another country have you contacted for any social reason, such as having a long conversation?
- Number of people from another country listed in the networks module.

Table E14: Full Set of Outcomes in Domain 7

	Contact Refugees by Choice Index	Foreign Business Collaborators	Open to Collab w Foreigners	Foreign Suppliers	Foreign Apprentices	Foreign Employees	Foreign Contacts	Foreign Networks
Info. + Labeled Grant	0.06	0.16	0.01	0.02	0.00	-0.01	0.47**	-0.01
	(0.08)	(0.13)	(0.01)	(0.03)	(0.03)	(0.02)	(0.21)	(0.03)
	[0.41]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Information Only	-0.02	0.24	0.00	-0.02	-0.01	-0.00	0.23	-0.03
	(0.09)	(0.15)	(0.01)	(0.03)	(0.03)	(0.02)	(0.25)	(0.04)
	[0.84]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Grant Only	0.02	0.04	0.00	0.05	-0.00	-0.01	0.13	-0.04
	(0.08)	(0.13)	(0.01)	(0.03)	(0.03)	(0.02)	(0.19)	(0.03)
	[0.80]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Mentored by Refugee	0.00	0.14	0.01	0.02	-0.05	-0.03**	-0.12	0.00
	(0.08)	(0.27)	(0.01)	(0.04)	(0.04)	(0.02)	(0.22)	(0.04)
	[0.98]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Mentored by Ugandan	0.05	-0.03	0.01	0.02	-0.01	-0.03*	0.44	-0.02
	(0.09)	(0.21)	(0.01)	(0.04)	(0.03)	(0.02)	(0.36)	(0.04)
	[0.59]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Observations	1,942	2,749	1,701	1,942	1,637	1,636	1,934	1,648
Control Mean: Baseline	0.00	0.18	0.96	0.12	0.08	0.01	0.61	0.03
Control Mean: Follow-Ups	-0.00	0.64	0.98	0.20	0.18	0.05	1.26	0.14
Labeled Grant = Info Only	0.31	0.62	0.67	0.12	0.83	0.75	0.36	0.64
Labeled Grant = Grant Only	0.58	0.46	0.70	0.46	0.87	0.98	0.14	0.45
Labeled Grant = R-Mentee	0.43	0.94	0.69	0.98	0.13	0.08	0.02	0.72
R-Mentee = Info Only	0.81	0.74	0.45	0.21	0.18	0.06	0.23	0.48
R-Mentee = U-Mentee	0.60	0.61	0.84	0.92	0.30	0.66	0.20	0.53

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 8: Contact with refugees by circumstance

- How many people from other countries live in your neighborhood? Many, some, few, or none?
- How many businesses in your sector in this area are managed by people from another country?
- How many of your customers are from another country?

Table E15: Full Set of Outcomes in Domain 8

	Contact Refugees by Circumst. Index	Foreigners in Neighborhood	Foreign Businesses in Area	Foreign Customers
Info. + Labeled Grant	0.05	0.06	-0.02	0.02
	(0.08)	(0.04)	(0.02)	(0.03)
	[0.55]	[1.00]	[1.00]	[1.00]
Information Only	0.10	0.08**	-0.00	0.01
	(0.08)	(0.04)	(0.02)	(0.03)
	[0.20]	[0.55]	[1.00]	[1.00]
Grant Only	-0.01	0.03	-0.02	-0.01
	(0.08)	(0.04)	(0.02)	(0.03)
	[0.90]	[1.00]	[1.00]	[1.00]
Mentored by Refugee	0.13	0.03	0.02	0.01
	(0.09)	(0.05)	(0.03)	(0.04)
	[0.19]	[1.00]	[1.00]	[1.00]
Mentored by Ugandan	0.06	0.09**	-0.01	-0.02
	(0.08)	(0.04)	(0.03)	(0.03)
	[0.47]	[0.55]	[1.00]	[1.00]
Observations	1,933	1,844	1,408	1,902
Control Mean: Baseline	0.00	0.56	0.07	0.16
Control Mean: Follow-Ups	0.00	0.53	0.07	0.19
Labeled Grant = Info Only	0.52	0.57	0.44	0.65
Labeled Grant = Grant Only	0.46	0.60	0.81	0.27
Labeled Grant = R-Mentee	0.43	0.55	0.13	0.74
R-Mentee = Info Only	0.78	0.27	0.40	0.94
R-Mentee = U-Mentee	0.52	0.21	0.26	0.44

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 9: Business practices

- If you were to sell all the business-related equipment you own right now (such as chairs, machines and tools), how much do you think you could make? (*Business capital is the sum of the value of the equipment and the value of the inventory.*)
- If you were to sell all the inventory you own right now (e.g. fabric, thread, soap), how much do you think you could make?
- Over the past 7 days, how many hours did you work at this business?
- In the past year, how many times did you take out a loan for your business? (*Omitted from index calculation due to ambiguous interpretation*)
- How much total business-related debt do you currently have? (*Omitted from index calculation due to ambiguous interpretation*)
- Number of contacts listed in the networks module.
- Over the past year, how often did you spend money advertising your business? Every day, every week, every month, a couple times, or never?
- How often did you keep written books/accounting records? Always, frequently, sometimes, occasionally, or never?
- How often did you sell goods or provide services to customers on credit? For all sales, most sales, some sales, a few sales, or never?
- How often did you buy materials, tools, or machines for your business on credit? For all sales, most sales, some sales, a few sales, or never?

Table E16: Full Set of Outcomes in Domain 9

	Business Practices Index	Business Capital	Working Hours (Inv Hyp Sin)	Business Loans	Business Debt	Business Networks	Marketing	Record Keeping	Sell on Credit	Buy on Credit
Info. + Labeled Grant	0.04	-0.09	-0.04	0.06	-0.09	0.02	0.01	0.02	0.07*	0.01
	(0.08)	(0.08)	(0.10)	(0.10)	(0.20)	(0.11)	(0.03)	(0.04)	(0.04)	(0.02)
	[0.58]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Information Only	-0.02	-0.02	-0.10	0.01	-0.02	-0.01	0.01	-0.04	0.03	0.04
	(0.08)	(0.08)	(0.10)	(0.09)	(0.20)	(0.11)	(0.03)	(0.04)	(0.04)	(0.03)
	[0.84]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Grant Only	0.12*	0.09	-0.07	0.06	0.06	0.09	0.03	-0.01	0.03	0.06**
	(0.07)	(0.08)	(0.10)	(0.11)	(0.21)	(0.11)	(0.03)	(0.04)	(0.04)	(0.03)
	[0.09]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Mentored by Refugee	0.06	-0.13	0.06	-0.01	-0.09	-0.02	0.02	0.03	0.03	0.04
	(0.09)	(0.09)	(0.11)	(0.11)	(0.23)	(0.13)	(0.04)	(0.05)	(0.05)	(0.03)
	[0.47]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Mentored by Ugandan	0.11	-0.02	0.14	0.14	0.19	-0.03	0.03	0.01	0.01	0.04
	(0.08)	(0.09)	(0.10)	(0.12)	(0.23)	(0.13)	(0.03)	(0.05)	(0.05)	(0.03)
	[0.19]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Observations	1,942	2,819	4,127	1,901	2,750	1,648	1,648	1,648	1,648	1,648
Control Mean: Baseline	0.00	495.56	81.52	0.38	26.80	1.77	0.06	0.41	0.27	0.05
Control Mean: Follow-Ups	0.00	632.54	62.55	0.68	66.75	1.91	0.17	0.51	0.43	0.09
Labeled Grant = Info Only	0.44	0.33	0.50	0.67	0.75	0.77	0.92	0.11	0.25	0.29
Labeled Grant = Grant Only	0.26	0.01	0.79	0.96	0.47	0.52	0.71	0.47	0.25	0.09
Labeled Grant = R-Mentee	0.81	0.61	0.34	0.56	1.00	0.77	0.96	0.93	0.37	0.38
R-Mentee = Info Only	0.37	0.18	0.12	0.85	0.78	0.97	0.90	0.16	0.92	0.96
R-Mentee = U-Mentee	0.64	0.24	0.46	0.29	0.28	0.95	0.82	0.71	0.62	0.84

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 9a: Marketing+

Table E17: Full Set of Outcomes in Domain 91

	Marketing Index+	Check Competitor Prices	Check Competitor Products	Consult Customers on Products	Ask Customer Who Left	Ask Supplers abt Products	Give Special Offers	Spend Money Advertising
Info. + Labeled Grant	0.12	-0.01	-0.01	0.04	0.06	0.01	0.05	0.01
	(0.08)	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)
	[0.15]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Information Only	-0.03	-0.02	-0.04	-0.02	-0.01	-0.00	0.01	0.01
	(0.09)	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)
	[0.76]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Grant Only	0.13	0.03	0.00	0.04	0.03	0.01	0.04	0.02
	(0.09)	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)
	[0.14]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Mentored by Refugee	0.07	0.01	0.04	0.04	0.04	-0.05	0.01	0.01
	(0.10)	(0.05)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)
	[0.50]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Mentored by Ugandan	0.11	0.04	0.02	0.01	0.00	-0.00	0.05	0.02
	(0.10)	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)
	[0.27]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Observations	1,648	1,648	1,648	1,648	1,648	1,648	1,648	1,648
Control Mean: Baseline	.	.	.	.	.	.	.	.
Control Mean: Follow-Ups	-0.00	0.68	0.73	0.80	0.62	0.80	0.77	0.17
Labeled Grant = Info Only	0.10	0.91	0.42	0.08	0.11	0.70	0.25	0.98
Labeled Grant = Grant Only	0.89	0.31	0.74	0.86	0.51	0.89	0.87	0.70
Labeled Grant = R-Mentee	0.60	0.63	0.27	0.87	0.59	0.15	0.35	0.98
R-Mentee = Info Only	0.37	0.58	0.06	0.10	0.37	0.25	1.00	0.97
R-Mentee = U-Mentee	0.72	0.44	0.71	0.46	0.49	0.28	0.37	0.84

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 9b: Stock Practices^{+}

Table E18: Full Set of Outcomes in Domain 92

	Stock Index^{+}	Negotiate Price w Supplier	Compare btw Suppliers	Use Up Stock
Info. + Labeled Grant	-0.15*	-0.02	-0.02	-0.06
	(0.09)	(0.04)	(0.03)	(0.04)
	[0.08]	[1.00]	[1.00]	[1.00]
Information Only	-0.14	-0.02	-0.03	-0.04
	(0.09)	(0.04)	(0.03)	(0.04)
	[0.12]	[1.00]	[1.00]	[1.00]
Grant Only	-0.06	0.03	0.01	-0.06
	(0.08)	(0.04)	(0.03)	(0.04)
	[0.50]	[1.00]	[1.00]	[1.00]
Mentored by Refugee	-0.16	-0.02	-0.01	-0.08*
	(0.10)	(0.04)	(0.04)	(0.04)
	[0.12]	[1.00]	[1.00]	[1.00]
Mentored by Ugandan	-0.05	0.02	-0.00	-0.04
	(0.09)	(0.04)	(0.04)	(0.05)
	[0.57]	[1.00]	[1.00]	[1.00]
Observations	1,648	1,648	1,648	1,648
Control Mean: Baseline	.	.	.	.
Control Mean: Follow-Ups	-0.00	0.78	0.81	0.36
Labeled Grant = Info Only	0.92	0.98	0.83	0.60
Labeled Grant = Grant Only	0.26	0.18	0.38	0.94
Labeled Grant = R-Mentee	0.92	0.98	0.77	0.60
R-Mentee = Info Only	0.85	0.97	0.64	0.33
R-Mentee = U-Mentee	0.33	0.43	0.85	0.33

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 9c: Record-Keeping⁺

Table E19: Full Set of Outcomes in Domain 93

	Record-Keeping Index$^+$	Record Purchase & Sale	Have Written Budget	Keep Accounting Records
Info. + Labeled Grant	0.10	0.06	0.01	0.02
	(0.09)	(0.04)	(0.04)	(0.04)
	[0.26]	[1.00]	[1.00]	[1.00]
Information Only	-0.05	-0.01	-0.03	-0.04
	(0.09)	(0.04)	(0.04)	(0.04)
	[0.53]	[1.00]	[1.00]	[1.00]
Grant Only	0.04	0.05	-0.01	-0.01
	(0.09)	(0.04)	(0.05)	(0.04)
	[0.70]	[1.00]	[1.00]	[1.00]
Mentored by Refugee	0.10	0.07	-0.01	0.02
	(0.10)	(0.05)	(0.05)	(0.05)
	[0.34]	[1.00]	[1.00]	[1.00]
Mentored by Ugandan	0.06	0.06	-0.02	-0.01
	(0.10)	(0.05)	(0.05)	(0.05)
	[0.55]	[1.00]	[1.00]	[1.00]
Observations	1,648	1,648	1,648	1,648
Control Mean: Baseline	.	.	.	.
Control Mean: Follow-Ups	0.00	0.57	0.51	0.51
Labeled Grant = Info Only	0.06	0.06	0.30	0.12
Labeled Grant = Grant Only	0.47	0.71	0.65	0.48
Labeled Grant = R-Mentee	1.00	0.86	0.70	0.98
R-Mentee = Info Only	0.12	0.10	0.61	0.17
R-Mentee = U-Mentee	0.70	0.82	0.77	0.56

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 9d: Changes in Business Practices+

Table E20: Full Set of Outcomes in Domain 95

	Change Index+	Change Suppliers	Change Services	Change Ads	Change Business Management	Change Business Size
Info. + Labeled Grant	-0.03	0.03	0.10*	0.03	-0.12**	-0.05
	(0.12)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
	[0.82]	[0.90]	[0.24]	[0.90]	[0.24]	[0.82]
Information Only	-0.12	-0.02	0.03	0.06	-0.12**	-0.10**
	(0.11)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
	[0.30]	[0.94]	[0.90]	[0.49]	[0.24]	[0.24]
Grant Only	0.15	0.07	0.11**	0.03	-0.02	0.04
	(0.11)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
	[0.17]	[0.48]	[0.24]	[0.90]	[0.94]	[0.90]
Mentored by Refugee	-0.20	-0.03	0.02	-0.05	-0.12*	-0.08
	(0.13)	(0.06)	(0.06)	(0.05)	(0.06)	(0.06)
	[0.12]	[0.90]	[0.94]	[0.78]	[0.24]	[0.44]
Mentored by Ugandan	0.00	0.01	0.03	0.12**	-0.03	-0.10*
	(0.13)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
	[0.97]	[0.95]	[0.90]	[0.24]	[0.90]	[0.24]
Observations	916	916	916	916	916	916
Control Mean: Baseline	.	.	.	.	.	.
Control Mean: Follow-Ups	-0.00	0.46	0.45	0.23	0.62	0.44
Labeled Grant = Info Only	0.44	0.38	0.17	0.58	0.95	0.25
Labeled Grant = Grant Only	0.13	0.40	0.89	0.96	0.04	0.09
Labeled Grant = R-Mentee	0.19	0.35	0.16	0.10	0.91	0.54
R-Mentee = Info Only	0.52	0.82	0.79	0.03	0.94	0.70
R-Mentee = U-Mentee	0.15	0.53	0.79	0.00	0.21	0.76

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 10: Household well-being

- What were the profits of your business during the last 30 days? (*Total household income is the sum of the following four questions.*)
- What were the profits of [any other household-owned] businesses (excluding this one) during the last 30 days?
- How much wage income did you earn in the last 30 days?
- How much wage income did [other members of your household] earn in the last 30 days?
- Business survival, measured using an indicator for whether the main business is operating at the time of the survey
- How much money was your household able to save in the past 30 days?
- Compared to the average Ugandan in your neighborhood, how would you describe the economic situation of your household? Much better, somewhat better, about the same, somewhat worse, or much worse?
- Over the past 30 days, how often have you or anyone in your household gone without enough food to eat? (*(Questions not exactly the same: over the past 30 days in baseline and follow-ups 2–4 and over the past week in follow-up 1.)*)
- Over the past 30 days, how often have you or anyone in your household struggled to afford basic household expenses (such as medicine, rent, school fees)? (*(Questions not exactly the same: over the past 30 days in baseline and follow-ups 2–4 and over the past week in follow-up 1.)*)
- In the past 30 days, have you or anyone in your household had to sell assets (jewelry, furniture, clothing, tools, machines, land) in order to afford basic household expenses?
- In the past 30 days, has your household had to stop education for a child due to lack of finances?

Table E21: Full Set of Outcomes in Domain 10

	Household Well-Being Index	Total Household Income (IHS)	Business Survival	Saving	Relative Economic Situation	Have Food	Fine w Household Expenses	No Need to Sell Assets	Can Afford Child Education
Info. + Labeled Grant	0.05	-0.07	0.01	0.12	-0.02	0.03*	0.00	-0.01	-0.01
	(0.06)	(0.20)	(0.02)	(0.15)	(0.04)	(0.02)	(0.02)	(0.03)	(0.04)
	[0.38]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Information Only	-0.05	-0.10	-0.01	-0.17	-0.01	0.01	-0.03	0.03	0.01
	(0.07)	(0.22)	(0.02)	(0.15)	(0.04)	(0.02)	(0.02)	(0.03)	(0.04)
	[0.46]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Grant Only	0.04	-0.21	-0.00	0.17	0.05	0.02	0.02	0.02	-0.00
	(0.06)	(0.21)	(0.02)	(0.15)	(0.04)	(0.02)	(0.02)	(0.03)	(0.04)
	[0.52]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Mentored by Refugee	-0.02	0.13	-0.00	-0.10	-0.04	-0.02	-0.01	0.02	0.03
	(0.08)	(0.23)	(0.02)	(0.17)	(0.05)	(0.02)	(0.03)	(0.03)	(0.05)
	[0.75]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Mentored by Ugandan	0.11	-0.28	0.02	0.03	0.06	0.03	0.03	0.05*	0.05
	(0.07)	(0.25)	(0.02)	(0.16)	(0.05)	(0.02)	(0.02)	(0.03)	(0.05)
	[0.11]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Observations	4,132	2,162	4,132	2,910	1,648	4,119	4,121	3,013	1,780
Control Mean: Baseline	0.00	81.32	.	22.52	0.43	0.95	0.91	0.90	0.58
Control Mean: Follow-Ups	0.00	59.43	0.93	26.04	0.43	0.91	0.79	0.78	0.67
Labeled Grant = Info Only	0.06	0.90	0.11	0.04	0.89	0.28	0.22	0.20	0.57
Labeled Grant = Grant Only	0.82	0.49	0.34	0.74	0.10	0.47	0.29	0.34	0.88
Labeled Grant = R-Mentee	0.26	0.36	0.37	0.18	0.61	0.01	0.78	0.35	0.36
R-Mentee = Info Only	0.74	0.33	0.66	0.65	0.53	0.09	0.46	0.78	0.68
R-Mentee = U-Mentee	0.07	0.11	0.15	0.44	0.03	0.01	0.22	0.23	0.65

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 11: Policy preferences and representation

- Do you agree or disagree with the following statement: Uganda should accept more foreigners besides refugees.
- For foreigners, besides refugees, which option do you think Uganda should follow? (analyzed as 4 binary variables)
- How satisfied are you with the LC1 for this area?
- How satisfied are you with the MP for this area?

Table E22: Full Set of Outcomes in Domain 11

	General Policy Index	Immigrants: Accept More	Immigrants: Allow To Stay	Satisfied w Local Politician	Satisfied w MP
Info. + Labeled Grant	0.08	0.08**	0.11***	-0.01	0.02
	(0.06)	(0.04)	(0.04)	(0.03)	(0.03)
	[0.15]	[0.16]	[0.09]	[1.00]	[1.00]
Information Only	0.00	0.03	0.04	0.01	-0.03
	(0.06)	(0.04)	(0.04)	(0.03)	(0.03)
	[0.94]	[0.92]	[0.92]	[1.00]	[0.92]
Grant Only	0.07	0.11***	0.09**	0.01	-0.01
	(0.06)	(0.04)	(0.04)	(0.03)	(0.03)
	[0.23]	[0.09]	[0.14]	[1.00]	[1.00]
Mentored by Refugee	-0.01	0.01	-0.00	-0.01	-0.01
	(0.07)	(0.05)	(0.04)	(0.04)	(0.04)
	[0.88]	[1.00]	[1.00]	[1.00]	[1.00]
Mentored by Ugandan	0.19***	0.05	0.09*	0.04	0.08**
	(0.06)	(0.04)	(0.05)	(0.03)	(0.04)
	[0.00]	[0.86]	[0.18]	[0.86]	[0.14]
Observations	3,779	1,648	1,648	2,555	3,363
Control Mean: Baseline	0.00	0.73	0.40	0.79	0.48
Control Mean: Follow-Ups	-0.00	0.68	0.29	0.76	0.51
Labeled Grant = Info Only	0.18	0.23	0.08	0.67	0.10
Labeled Grant = Grant Only	0.87	0.38	0.70	0.60	0.32
Labeled Grant = R-Mentee	0.18	0.15	0.02	0.85	0.33
R-Mentee = Info Only	0.84	0.65	0.36	0.61	0.62
R-Mentee = U-Mentee	0.01	0.47	0.07	0.20	0.02

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 12: Beliefs about economic effects of non-refugee immigrants

- Taking everything into consideration, would you say the overall economic effect of foreigners other than refugees on Uganda has been positive, negative, or neutral?
- How about the overall economic effect of foreigners other than refugees on you personally?

Table E23: Full Set of Outcomes in Domain 12

	Foreigners: Economic Beliefs Index	Immigrants: Effect on Economy	Immigrants: Effect on You
Info. + Labeled Grant	0.20**	0.06*	0.10**
	(0.08)	(0.04)	(0.04)
	[0.01]	[0.13]	[0.08]
Information Only	0.12	0.09**	0.01
	(0.08)	(0.04)	(0.04)
	[0.14]	[0.08]	[0.44]
Grant Only	0.18**	0.09**	0.06
	(0.08)	(0.04)	(0.05)
	[0.03]	[0.08]	[0.22]
Mentored by Refugee	-0.18*	-0.04	-0.10**
	(0.10)	(0.04)	(0.05)
	[0.06]	[0.24]	[0.08]
Mentored by Ugandan	0.09	0.05	0.02
	(0.09)	(0.04)	(0.05)
	[0.30]	[0.22]	[0.44]
Observations	1,604	1,548	1,575
Control Mean: Baseline	0.00	0.66	0.39
Control Mean: Follow-Ups	-0.00	0.68	0.53
Labeled Grant = Info Only	0.34	0.38	0.03
Labeled Grant = Grant Only	0.82	0.47	0.30
Labeled Grant = R-Mentee	0.00	0.01	0.00
R-Mentee = Info Only	0.00	0.00	0.03
R-Mentee = U-Mentee	0.01	0.04	0.01

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 13: Cultural attitudes toward other non-refugee immigrants

- What effect have foreigners besides refugees had on culture in Uganda?

Table E24: Full Set of Outcomes in Domain 13

	Foreigners: Cultural Attitudes Index	Immigrants: Effect on Culture
Info. + Labeled Grant	0.05	0.02
	(0.09)	(0.04)
	[0.56]	[0.96]
Information Only	-0.12	-0.05
	(0.09)	(0.04)
	[0.20]	[0.90]
Grant Only	0.16*	0.07*
	(0.09)	(0.04)
	[0.09]	[0.90]
Mentored by Refugee	0.06	0.03
	(0.10)	(0.04)
	[0.55]	[0.96]
Mentored by Ugandan	0.05	0.02
	(0.10)	(0.04)
	[0.65]	[0.96]
Observations	1,451	1,451
Control Mean: Baseline	0.00	0.64
Control Mean: Follow-Ups	0.00	0.48
Labeled Grant = Info Only	0.06	0.08
Labeled Grant = Grant Only	0.26	0.22
Labeled Grant = R-Mentee	0.94	0.86
R-Mentee = Info Only	0.08	0.08
R-Mentee = U-Mentee	0.89	0.83

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 14: Contact with Ugandans from another tribe

- How many of your customers are Ugandans from a different tribe?
- How many businesses in your sector in this area are managed by Ugandans from another tribe?
- How many of your business collaborators are Ugandans from a different tribe?
- Would you be open to collaborating with Ugandans from other tribes?
- Have you ever had an apprentice or person from outside your household at your business who was learning skills but not paid who was from another tribe?
- Are any of your employees from a different tribe than you?
- In the past 30 days, how many people from a different tribe have you contacted for any social reason, such as having a long conversation?
- Number of people from another tribe listed in the networks module.

Table E25: Full Set of Outcomes in Domain 14

	Other Tribes: Contact Index	Customers	Business	Business Collab.	Open to Collab.	Apprentices	Employees	Contacts	Networks
Info. + Labeled Grant	0.04	-0.02	-0.00	0.82*	0.02**	-0.03	0.02	-1.10	0.05
	(0.07)	(0.04)	(0.04)	(0.46)	(0.01)	(0.04)	(0.03)	(2.42)	(0.08)
	[0.59]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Information Only	-0.08	-0.03	0.05	0.25	0.01	-0.00	0.00	-3.82*	-0.03
	(0.08)	(0.04)	(0.04)	(0.35)	(0.01)	(0.04)	(0.03)	(2.27)	(0.09)
	[0.31]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Grant Only	-0.04	0.01	0.00	0.01	0.00	0.05	-0.01	-3.43	0.01
	(0.07)	(0.04)	(0.04)	(0.29)	(0.01)	(0.04)	(0.04)	(2.25)	(0.08)
	[0.58]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Mentored by Refugee	-0.16**	-0.01	-0.02	-0.48	0.02	-0.06	0.00	-5.06**	-0.04
	(0.07)	(0.05)	(0.05)	(0.35)	(0.01)	(0.05)	(0.04)	(2.13)	(0.10)
	[0.01]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Mentored by Ugandan	-0.02	0.00	0.05	0.58	0.02	-0.05	-0.01	-2.96	0.02
	(0.07)	(0.05)	(0.04)	(0.57)	(0.01)	(0.05)	(0.04)	(2.08)	(0.09)
	[0.76]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Observations	1,766	1,593	1,521	1,726	1,016	1,637	1,636	1,616	1,648
Control Mean: Baseline	0.00	0.39	0.34	1.62	0.99	0.38	0.15	4.90	0.74
Control Mean: Follow-Ups	-0.00	0.38	0.36	2.68	0.98	0.53	0.22	9.52	0.86
Labeled Grant = Info Only	0.06	0.83	0.20	0.22	0.21	0.44	0.54	0.02	0.29
Labeled Grant = Grant Only	0.17	0.57	0.94	0.06	0.05	0.06	0.33	0.05	0.60
Labeled Grant = R-Mentee	0.00	0.83	0.61	0.01	0.44	0.58	0.63	0.00	0.31
R-Mentee = Info Only	0.16	0.68	0.11	0.05	0.68	0.22	0.95	0.10	0.96
R-Mentee = U-Mentee	0.01	0.89	0.14	0.09	1.00	0.88	0.73	0.02	0.57

Each column shows an outcome with respect to contact with other tribes. An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 15: Beliefs about economic effects of Ugandans from another tribe

- How do the businesses managed by Ugandans from a different tribe affect your business overall? Do they help you a lot, help you a little, hurt you a little, hurt you a lot, or have no effect on you? (*Compared to the similar question on Ugandans from your tribe.*)

Table E26: Full Set of Outcomes in Domain 15

	Other Tribes: Economic Beliefs Index	Other Tribes: Effect on Your Business
Info. + Labeled Grant	0.06	0.02
	(0.08)	(0.03)
	[0.47]	[0.55]
Information Only	0.08	0.03
	(0.08)	(0.03)
	[0.36]	[0.55]
Grant Only	0.14*	0.06*
	(0.08)	(0.03)
	[0.08]	[0.25]
Mentored by Refugee	0.17*	0.07*
	(0.09)	(0.04)
	[0.06]	[0.25]
Mentored by Ugandan	-0.03	-0.01
	(0.10)	(0.04)
	[0.78]	[0.86]
Observations	1,583	1,583
Control Mean: Baseline	0.00	0.79
Control Mean: Follow-Ups	-0.00	0.79
Labeled Grant = Info Only	0.81	0.81
Labeled Grant = Grant Only	0.23	0.23
Labeled Grant = R-Mentee	0.18	0.18
R-Mentee = Info Only	0.28	0.27
R-Mentee = U-Mentee	0.04	0.03

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 16: Cultural attitudes toward Ugandans from another tribe

Social distance index constructed based on these four questions:

- I would be comfortable marrying a Ugandan from another tribe.
- I would be comfortable having a Ugandan from another tribe marry a member of my family.
- I would be comfortable having a Ugandan from another tribe as a close, personal friend.
- I would be comfortable having a Ugandan from another tribe as a neighbor.

Table E27: Full Set of Outcomes in Domain 16

	Other Tribes: Social Proximity Index	Other Tribes: Social Proximity
Info. + Labeled Grant	0.20**	0.20**
	(0.08)	(0.08)
	[0.01]	[0.06]
Information Only	0.08	0.08
	(0.08)	(0.08)
	[0.31]	[1.00]
Grant Only	0.06	0.07
	(0.09)	(0.09)
	[0.53]	[1.00]
Mentored by Refugee	0.04	0.04
	(0.09)	(0.09)
	[0.65]	[1.00]
Mentored by Ugandan	0.01	0.03
	(0.11)	(0.10)
	[0.91]	[1.00]
Observations	1,648	1,648
Control Mean: Baseline	0.00	0.07
Control Mean: Follow-Ups	0.00	-0.00
Labeled Grant = Info Only	0.12	0.10
Labeled Grant = Grant Only	0.09	0.09
Labeled Grant = R-Mentee	0.07	0.06
R-Mentee = Info Only	0.64	0.67
R-Mentee = U-Mentee	0.78	0.88

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 17: Gender roles

- Do you share all of your profits from this business with your spouse?
- Who decides how the household's money is spent?

Table E28: Full Set of Outcomes in Domain 171

	Gender Role Index	Share Profits w Spouse	Women Decide Expenditure
Info. + Labeled Grant	0.03	0.01	-0.01
	(0.14)	(0.07)	(0.06)
	[0.84]	[1.00]	[1.00]
Information Only	0.16	0.05	0.05
	(0.14)	(0.07)	(0.05)
	[0.24]	[1.00]	[1.00]
Grant Only	0.11	0.08	-0.01
	(0.13)	(0.07)	(0.06)
	[0.42]	[1.00]	[1.00]
Mentored by Refugee	-0.16	-0.05	-0.05
	(0.17)	(0.07)	(0.06)
	[0.35]	[1.00]	[1.00]
Mentored by Ugandan	-0.08	-0.01	-0.04
	(0.17)	(0.07)	(0.07)
	[0.65]	[1.00]	[1.00]
Observations	654	654	654
Control Mean: Baseline	0.00	0.20	0.80
Control Mean: Follow-Ups	0.00	0.51	0.78
Labeled Grant = Info Only	0.39	0.60	0.24
Labeled Grant = Grant Only	0.59	0.38	0.95
Labeled Grant = R-Mentee	0.31	0.36	0.56
R-Mentee = Info Only	0.07	0.15	0.10
R-Mentee = U-Mentee	0.68	0.58	0.94

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 18: COVID-19 household shock

- In total, about how much income did your family earn during the 4 months of the lockdown (April - July)? Do not count money that you borrowed.
- During the lockdown, how often did you or anyone in your household go without enough food to eat? Always, often, sometimes, or never?
- During the lockdown, how often did you or anyone in your household struggle to afford basic household expenses other than food (such as medicine, rent, school fees)?
- During the lockdown, did you or anyone in your household have to sell assets (jewelry, furniture, clothing, tools, machines, land) in order to afford basic household expenses?
- How much did you borrow during the lockdown to pay for basic necessities like food, housing, and medicine?

Table E29: Full Set of Outcomes in Domain 18

	COVID Shock Index	COVID: Income	COVID: Have Food	COVID: Fine w Household Expenses	COVID: No Need to Sell Assets	COVID: Borrowing
Info. + Labeled Grant	-0.01	-0.14	0.01	-0.04	-0.00	-0.22
	(0.10)	(0.22)	(0.04)	(0.04)	(0.04)	(0.26)
	[0.95]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Information Only	-0.07	0.31	-0.08*	-0.14***	-0.06	-0.04
	(0.10)	(0.24)	(0.04)	(0.04)	(0.04)	(0.26)
	[0.51]	[1.00]	[1.00]	[0.04]	[1.00]	[1.00]
Grant Only	-0.11	-0.15	-0.04	-0.06	-0.00	0.23
	(0.10)	(0.22)	(0.05)	(0.05)	(0.05)	(0.28)
	[0.28]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Mentored by Refugee	0.05	-0.10	-0.01	0.00	0.05	-0.03
	(0.11)	(0.27)	(0.05)	(0.05)	(0.05)	(0.29)
	[0.68]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Mentored by Ugandan	-0.07	0.03	0.04	-0.06	-0.04	0.19
	(0.11)	(0.27)	(0.05)	(0.05)	(0.05)	(0.30)
	[0.50]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Observations	1,119	1,068	1,112	1,113	1,119	1,117
Control Mean: Baseline	.	.	.	.	.	.
Control Mean: Follow-Ups	0.00	160.46	0.74	0.73	0.72	576.32
Labeled Grant = Info Only	0.53	0.06	0.04	0.02	0.19	0.49
Labeled Grant = Grant Only	0.30	0.93	0.25	0.64	0.94	0.10
Labeled Grant = R-Mentee	0.63	0.91	0.69	0.43	0.21	0.50
R-Mentee = Info Only	0.31	0.14	0.17	0.00	0.02	0.95
R-Mentee = U-Mentee	0.31	0.67	0.37	0.22	0.06	0.50

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Domain 19: Government or NGO Support$^+$

- Over the past year, has your household received any assistance from an NGO or international organization? If so, what are the names of the organizations running those programs?
- Enumerator: Did they mention YARID in their answer?
- Enumerator: Did they mention this study, research, or survey firm in their answer?
- What was the purpose of those programs?
- Enumerator: Did they mention refugees in their answer?

Table E30: Full Set of Outcomes in Domain 19

	Attribution Index	Reported Any Support$^+$	Associated Support w YARID$^+$	Associated Support w Data Firm$^+$	Associated Support w Refugees$^+$
Info. + Labeled Grant	0.64***	0.24***	0.20***	0.09***	0.12***
	(0.07)	(0.03)	(0.02)	(0.02)	(0.02)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Information Only	0.05	-0.00	0.01	0.02*	0.02
	(0.06)	(0.03)	(0.01)	(0.01)	(0.01)
	[0.43]	[0.22]	[0.13]	[0.06]	[0.07]
Grant Only	0.64***	0.26***	0.18***	0.10***	0.08***
	(0.07)	(0.03)	(0.02)	(0.02)	(0.01)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Mentored by Refugee	0.11	0.02	0.03***	0.03	0.03**
	(0.07)	(0.03)	(0.01)	(0.02)	(0.02)
	[0.13]	[0.20]	[0.01]	[0.07]	[0.03]
Mentored by Ugandan	0.16**	0.04	0.03***	0.02	0.05***
	(0.07)	(0.03)	(0.01)	(0.01)	(0.02)
	[0.02]	[0.07]	[0.00]	[0.08]	[0.00]
Observations	3,061	3,061	3,061	3,061	3,061
Control Mean: Baseline	.	.	.	.	.
Control Mean: Follow-Ups	0.00	0.32	0.00	0.04	0.02
Labeled Grant = Info Only	0.00	0.00	0.00	0.00	0.00
Labeled Grant = Grant Only	0.94	0.58	0.31	0.55	0.04
Labeled Grant = R-Mentee	0.00	0.00	0.00	0.00	0.00
R-Mentee = Info Only	0.41	0.47	0.04	0.88	0.31
R-Mentee = U-Mentee	0.54	0.45	0.59	0.79	0.34

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E31: Heterogeneity in Treatment Impacts on Business Profit

	Female Owner	Business Practices Index	Business Network Size	Mentor Profit	Mentor Experience	Distance to Mentor
Info. + Lab. Grant × X	-0.17	-0.07	-0.15			
	(0.13)	(0.12)	(0.12)			
	[0.19]	[0.58]	[0.21]			
Info. + Lab. Grant	0.05	-0.02	0.02	-0.07	-0.06	-0.06
	(0.11)	(0.08)	(0.09)	(0.06)	(0.06)	(0.06)
	[0.67]	[0.77]	[0.84]	[0.28]	[0.28]	[0.28]
Information Only × X	-0.19	-0.01	0.01			
	(0.14)	(0.13)	(0.13)			
	[0.17]	[0.92]	[0.95]			
Information Only	0.09	-0.03	-0.04	-0.04	-0.04	-0.04
	(0.11)	(0.09)	(0.10)	(0.06)	(0.06)	(0.06)
	[0.44]	[0.77]	[0.68]	[0.55]	[0.56]	[0.54]
Grant Only × X	-0.16	-0.00	-0.13			
	(0.14)	(0.13)	(0.13)			
	[0.25]	[0.97]	[0.30]			
Grant Only	0.07	-0.03	0.05	-0.04	-0.04	-0.04
	(0.12)	(0.08)	(0.10)	(0.06)	(0.06)	(0.06)
	[0.54]	[0.68]	[0.62]	[0.52]	[0.52]	[0.52]
Mentored by Refugee × X	-0.05	-0.07	-0.24*	0.04	-0.01	0.05
	(0.15)	(0.14)	(0.14)	(0.10)	(0.11)	(0.11)
	[0.75]	[0.63]	[0.08]	[0.70]	[0.96]	[0.66]
Mentored by Refugee	0.05	0.04	0.17*	0.00	0.02	-0.01
	(0.13)	(0.08)	(0.10)	(0.08)	(0.09)	(0.10)
	[0.72]	[0.59]	[0.09]	[0.96]	[0.79]	[0.95]
Mentored by Ugandan × X	-0.31**	0.16	-0.09	0.01	0.05	0.01
	(0.16)	(0.14)	(0.15)	(0.11)	(0.12)	(0.12)
	[0.05]	[0.28]	[0.53]	[0.90]	[0.69]	[0.93]
Mentored by Ugandan	0.09	-0.17*	-0.06	-0.12	-0.14	-0.12
	(0.13)	(0.09)	(0.11)	(0.09)	(0.09)	(0.08)
	[0.48]	[0.06]	[0.59]	[0.16]	[0.14]	[0.14]
X	-0.84***	0.08	0.07			
	(0.15)	(0.10)	(0.09)			
	[0.00]	[0.41]	[0.48]			
Observations	4,029	4,029	4,029	4,029	4,029	4,029

The dependent variable for each column is business profits. Each column title lists the dimension of heterogeneity (X) that is analyzed in the regression. An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E32: Heterogeneity in Treatment Impacts by Treatment Timing

	Integration Policies Index	Profit (Standardized)
Info. + Labeled Grant × Treated	0.14	-0.12
	(0.11)	(0.13)
	[0.23]	[0.33]
Info. + Labeled Grant	0.20*	0.18*
	(0.12)	(0.10)
	[0.08]	[0.06]
Info. + Labeled Grant × Treated × Months Since Treatment	0.00	-0.01*
	(0.01)	(0.01)
	[0.56]	[0.05]
Information Only × Treated	0.08	0.09
	(0.19)	(0.15)
	[0.66]	[0.54]
Information Only × Treated × Months Since Treatment	0.01	-0.00
	(0.01)	(0.01)
	[0.22]	[0.98]
Information Only	0.06	-0.13
	(0.19)	(0.13)
	[0.76]	[0.32]
Grant Only × Treated	-0.00	0.15
	(0.15)	(0.15)
	[0.99]	[0.32]
Grant Only × Treated × Months Since Treatment	0.01*	-0.01
	(0.01)	(0.01)
	[0.07]	[0.44]
Grant Only	0.13	-0.10
	(0.14)	(0.12)
	[0.35]	[0.37]
Mentored by Refugee × Treated	0.02	-0.09
	(0.17)	(0.19)
	[0.91]	[0.62]
Mentored by Refugee × Treated × # Meetings	0.02	0.02
	(0.02)	(0.02)
	[0.45]	[0.39]
Mentored by Refugee	0.03	-0.01
	(0.14)	(0.14)
	[0.82]	[0.96]
Mentored by Ugandan × Treated	0.04	0.13
	(0.17)	(0.19)
	[0.80]	[0.49]
Mentored by Ugandan × Treated × # Meetings	0.02	0.02
	(0.02)	(0.02)
	[0.43]	[0.36]
Mentored by Ugandan	-0.00	-0.31**
	(0.13)	(0.14)
	[1.00]	[0.03]
Observations	3,051	4,029

An observation is a surveyed respondent, with one per post-baseline survey round, in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. *Treated* is an indicator for having the first visit by NGO staff (Labeled Grant, Information Only, and Grant Only), or for having any mentorship meetings (Mentored by Refugee, Mentored by Ugandan). *Months Since Treatment* is the months between the first visit and the survey. Standard errors clustered at the enterprise level in parentheses; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.