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Can Redistribution Change Policy Views?

Aid and Attitudes toward Refugees in Uganda

Travis Baseler, Thomas Ginn, Robert Hakiza, Helidah Ogude-Chambert, Olivia Woldemikael

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 conduct a randomized controlled trial in Kampala, Uganda studying foreign aid programs for Ugandans which are explicitly connected to the refugee presence. Cash grants labeled as part of the refugee aid response substantially increase support for admitting more refugees and allowing them to work and integrate. Sharing information about public goods funded by the refugee response has smaller, though still significant, effects. Impacts persist for at least two years and are associated with changing beliefs about the economic effects of refugees. We find minimal impacts of intergroup contact, implemented as business mentorship by an experienced refugee. Overall, our results suggest that economic interventions can meaningfully shape policy views when the connection between the policy and the assistance is salient.

KEYWORDS
Refugees, Political Economy of Aid, Firms & Productivity, Post-Conflict, Welfare

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1 Introduction

Policy changes that raise aggregate welfare—and in which the winners could hypothetically compensate the losers to make everyone better off—may be politically infeasible. Politicians may recognize the aggregate gains from immigration or international trade, for example, but block additional visas or trade agreements due to fears about 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: the costs of a policy may be more salient or visible to perceived losers than the benefits, voters may form their policy views based largely on non-economic considerations such as group identity, and compensation could crowd out other sources of policy support such as altruism.[2]

Allowing refugees—people who have fled their home country due to persecution, conflict, or generalized violence—to work is another example of a policy likely to have aggregate benefits which are unevenly distributed. As of 2022, more than 37 million refugees and asylum seekers were residing outside their country of origin (UNHCR, 2022*b*). Over half of them face significant, government-imposed barriers to the labor market such as work bans, dispersal policies, and requirements to live in camps (Ginn et al., 2022), partly due to concerns of crowd-out effects on natives. Movement restrictions prevent refugees from choosing locations that maximize long-run economic returns (Arendt, Dustmann and Ku, 2022), and prolonged detachment from employment leads to lost income, worse mental health (Hussam et al., 2022), and skill loss (Brell, Dustmann and Preston, 2020). These restrictions also constrain aid: without labor market access, the potential returns to development interventions are limited (Schuettler and Caron, 2020), and aid budgets are allocated to humanitarian programs like food aid or state welfare which are designed for short-term support. Displacement, however, is often long-term, and humanitarian assistance is likely to be more expensive and have lower returns for both refugees and citizens than development assistance—or refugee

[1] Examples of adopted or proposed redistributions of policy gains include the Trade Adjustment Assistance program in the United States and the European Globalisation Adjustment Fund, which are intended to support and retrain workers displaced by trade; compensation for residents living near power stations, waste disposal sites, wind farms, or other major industrial facilities; and sharing part of the international aid response for refugees with the communities that host refugees, the subject of this paper.

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

integration—in the long run.[3]

Host communities might prefer a different political economy equilibrium: allow refugees to access the labor market and redistribute some of the resulting foreign aid or public finance surplus to hosts.[4] The gains to refugees from labor market access are likely significant (Bahar, Cowgill and Guzman, 2022, Ibáñez et al., 2022), while the effects on many in the host community would likely be small—or positive (Clemens et al., 2018, Verme and Schuettler, 2021, Dhingra, Kilborn and Woldemikael, 2021, Bahar, Ibáñez and Rozo, 2021, Clemens, 2022, Ginn, 2023). When refugees can work, aid can be reallocated from humanitarian programs for refugees to development programs for both refugees and hosts, especially those who are close substitutes with refugees in the labor market. This framework is outlined in the Global Compact on Refugees adopted by the UN General Assembly in 2018, but the scope for reallocating aid to generate domestic political support for integration is unknown.[5]

We designed three programs that directly link foreign assistance to the host community with the presence of refugees and policies supporting their integration. Ugandan policy stipulates that a portion of international refugee aid be shared with host communities (we refer to this as Uganda's "aid-sharing policy"), but we show that awareness of this policy is low at baseline. We offer our three programs to Ugandan microentrepreneurs in the capital city of Uganda, a country that hosts over one million refugees. Microentrepreneurship is a common source of livelihood for both Ugandans and refugees in the capital, and thus these groups may come into direct competition. We delivered the programs through a non-profit founded and led by refugees to increase the perceived connection between the assistance and the refugee presence.

The first program delivers information about Uganda's aid-sharing policy. A staff member

[3]Seventy-four percent of refugees live in protracted situations that have lasted at least five years (UNHCR, 2022*a*), while 71 percent of the 24.2 billion USD spent on Official Development Assistance for refugee situations in 2018–19 went to short-term humanitarian programs (OECD, 2021). Marbach, Hainmueller and Hangartner (2018) provide another example, finding that employment bans on asylum seekers in Germany cost 40 million Euros annually in public services and foregone tax revenue. Schuettler and Caron (2020) note that policy barriers often limit the potential medium-term effects of aid to refugees: the return to skills, for instance, is higher when refugees are eligible for formal jobs.

[4]We use "hosting" and "host community" to describe native-born individuals living in the same country or area as refugees, consistent with humanitarian terminology. Refugees in this context do not typically live with a host family in the same dwelling.

[5]Aid can potentially shape support at both the country and individual levels. We study the individual level in this paper, but at the country level see Tsourapas (2019) for a discussion of how conditional offers of assistance from international donors shape policy for countries hosting Syrians and Ash and Huang (2018) for a discussion of the compact model in refugee-hosting contexts.

explains that part of foreign aid for refugees is shared with Ugandans, gives examples of public goods like schools and hospitals funded by international refugee aid, and conducts a listening exercise modeled after Kalla and Broockman (2020) which provides context for the visit by inviting the respondent to share their views toward refugees. The second program augments the information delivery with a business grant of USD 135, representing about 3.5 months of profit on average, which is framed as an example of compensation for Ugandans under the country's aid-sharing policy. We refer to this treatment as a "labeled grant." The third program matches each microentrepreneur with a more experienced refugee business owner in a one-on-one mentorship program. Peer mentorship can be effective at improving small-business profitability because mentors can share individual, time-varying information (Brooks, Donovan and Johnson, 2018), and therefore offers an opportunity to link assistance to Ugandan entrepreneurs with the refugee presence through intergroup contact. Meetings are facilitated by a staff member in part to overcome any language barriers. We implemented each program within the tailoring and hairdressing sectors, in part because refugee owners are widely perceived as successful in these sectors and thus may be attractive as potential mentors. We test whether these programs affect Ugandans' support for refugee hosting and integration, beliefs about the economic impact of refugee hosting, social attitudes toward refugees, and economic outcomes in the firm and household.

We include three additional treatment arms to isolate potential channels. First, we randomly assign a business grant that is not bundled with information on refugees to isolate any income or wealth effects. Second, we assign a group to mentorship by an experienced Ugandan—balancing refugee and Ugandan mentors across several dimensions to increase comparability—to isolate the impact of mentorship by a refugee from mentorship in general. Finally, we include a pure control group which did not receive any treatment.

The labeled grant substantially increases Ugandans' support for hosting refugees and providing the right to work and freedom of movement, compared to the control group. These effects appear immediately and persist for at least two years beyond the start of our intervention. Recipients of labeled grants are more likely to believe that refugees have a net positive impact on the Ugandan economy and on them personally, and to know that international aid for refugees is shared with Ugandans. Some social attitudes are also impacted: recipients of labeled grants are more likely to report a willingness to socialize with refugees, but no more likely to say that refugees have a positive impact on Ugandan culture. Receiving informa-

tion about Uganda's aid-sharing policy, but no business grant, creates similar, but smaller, impacts. We argue that this difference is driven in part by increased trust in the government and aid organizations among grant recipients. Finally, receiving an unlabeled business grant also increases support for refugee hosting. We hypothesize that this finding is driven in part by a reduction in resource resentment created by the receipt of aid generally, and in part by an association of the grant with the refugee-led non-governmental organization (NGO) that distributed them. In all treatment groups, changes in policy views are larger among those who expressed greater economic concerns about hosting refugees prior to treatment.

Do the impacts we observe on self-reported views translate into changes in real-world political behavior? The ideal outcome to test this hypothesis would be voting choices in a referendum related to admitting refugees or providing the right to work or freedom of movement. While measuring such an outcome was not possible in our design, we attempted to capture a proxy for voting behavior through a phone-call campaign that asked each member of our sample whether they wanted to support a letter to local officials expressing their approval of refugee hosting. The campaign was conducted by an organization distinct from both the implementing NGO and the data collection firm to reduce the potential influence of experimenter demand effects or social desirability bias stemming from expectations of future aid, gift exchange, or any other factor leading true and reported views to diverge. We find that recipients of labeled grants were significantly more likely to add their support to the letter, with no significant differences for other treatment arms. This result leads us to conclude that, while experimenter demand effects may be driving part of the impacts on self-reported policy preferences, true preferences changed as well.

We find no significant effects of the grants on business profit, business practices, or household welfare, possibly because many grants were disbursed around the COVID-19 shock, when the need to consume rather than invest out of the grants was high. We find minimal impacts of mentorship, either by a refugee or a Ugandan, on attitudes or business outcomes, despite high uptake of both programs.[6] Impacts on attitudes from both programs were significant but small after five months and did not persist. While interruptions related to the COVID-19 pandemic may be partly responsible, these findings suggest that short-term intergroup contact—even cooperative peer-to-peer contact—has small and less persistent impacts

[6]Sixty-three percent of mentees met their mentors at least twice in person before the program was suspended by the COVID-19 pandemic, and then 55% met over the phone at least four times when the program resumed one year later.

on attitudes than direct aid programs with clear messaging.

Why does receiving assistance labeled as redistribution increase Ugandans' support for refugee hosting? We find that Ugandans in this group update their beliefs about the economic impact of hosting refugees. In addition to directly reporting higher beliefs about the economic impact of refugee hosting on both themselves and Uganda more broadly, recipients of labeled aid were more likely to know that Ugandan policy requires sharing international refugee aid with Ugandan hosts, and to associate the aid they received with refugees years after the program had ended.

We further explore whether experimenter demand effects are driving our results using an incentivized dictator game, and find that the labeled cash group donated a larger share of the endowment to an organization supporting refugees. We find no impacts of a brief information campaign opposing child labor, delivered by the same partner NGO, on attitudes toward child labor, indicating that experimenter demand effects are likely to be low overall in our sample. We also find no impact of a priming experiment in which respondents were randomly primed to recall the assistance they had received before eliciting their perceptions of refugees. While some experimenter demand effects are possible, these additional results point to true changes in views.

We do not find that our results are driven by pure wealth effects, since we find limited economic impacts of the programs, and the Information Only arm significantly affected attitudes even without a grant. We also do not find that our results are driven by contact with refugees, as we find no treatment impact on contact with refugees outside the experiment, no impacts of the refugee mentorship arm, and no differential impact depending on the nationality of the NGO staff member who delivered the grant or information.

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 salient. Host countries that restrict refugees' labor market access due to concerns about crowd-out can consider combining integration policies with aid redistribution,[7] and countries that already share foreign aid with the host community could increase support for refugee integration by making existing policies more widely known. More generally, policies that reduce barriers to trade or immigration

[7]In high-income countries, where asylum seekers' labor market access is often limited, redistributing public finances could potentially achieve the same effect. See Dustmann et al. (2017) and Brell, Dustmann and Preston (2020) for reviews of refugee migration and labor market integration in high-income countries.

are likely to benefit some groups more than others or harm certain groups (Autor, Dorn and Hanson, 2013), which can incite political backlash (Dustmann, Vasiljeva and Piil Damm, 2019, Autor et al., 2020).

While redistribution has been proposed as a means to increase support for policies creating winners and losers (Freeman, 2006, Clemens, 2011, Edelberg and Watson, 2022, Lokshin and Ravallion, 2022), there has been little evidence on whether policy preferences respond to these economic incentives. This is especially true in the immigration field, possibly owing to the common finding that attitudes about immigration are primarily determined by host opinions about migrants' generalized impacts—on the national economy, demographic composition, and culture (Tabellini, 2020)—rather than "personal economic circumstances" (Hainmueller and Hopkins, 2014). Research interventions studying natives' attitudes often focus on culture, providing humanizing narratives of refugees and migrants or using persuasive techniques of empathetic listening and perspective-taking (Adida, Lo and Platas, 2018, Kalla and Broockman, 2020). Our findings indicate that economic incentives can influence views about immigration policy regardless of whether the original opposition was economic or cultural in nature.

We contribute to the vast literature studying policy preferences under economic shocks, most of which focuses on high-income countries. Bonomi, Gennaioli and Tabellini (2021) and Grossman and Helpman (2021) study models in which voters weigh both economic and cultural concerns of groups they identify with when evaluating policies. The literature on political responses to immigration has largely focused on, and distinguished between, hosts' economic and cultural concerns (Alesina and Tabellini, 2022). Immigration can provoke a nativist backlash (Halla, Wagner and Zweimüller, 2017, Mayda, Peri and Steingress, 2022), though Aksoy and Ginn (2022) find little evidence for a backlash to refugee arrivals on average in low- and middle-income countries, even in places where refugees have more labor market access. Immigration can also shift boundaries of social groups (Fouka, Mazumder and Tabellini, 2021, Fouka and Tabellini, 2022) and diminish natives' preferences for redistribution (Alesina and Stantcheva, 2020, Alesina, Murard and Rapoport, 2021, Alesina, Miano and Stantcheva, 2023). Trade shocks that displaced US workers in areas exposed to competition with Chinese exporters increased political polarization (Autor et al., 2020), and even exposure to stories about labor-market shocks sharply increases preferences for trade restrictions (Di Tella and Rodrik, 2020). Informing US citizens in a survey experiment

about existing redistribution programs toward workers displaced by trade increases support for international trade (Ehrlich and Hearn, 2014).[8] However, to our knowledge, no study has experimentally tested whether redistribution can affect policy views on immigration, an area where non-economic concerns may play a significant role in determining attitudes. Our paper does so in the context of refugee hosting policies, which affect millions of people every year and remain a contentious policy issue across much of the world.

This paper also contributes to the literature on attitudes toward immigrants, refugees, and internally displaced people more broadly. The majority of this research has focused on public opinion in the US and Europe (Hainmueller and Hiscox, 2007, 2010, Dancygier and Laitin, 2014, Hainmueller and Hopkins, 2014, Bansak, Hainmueller and Hangartner, 2016) with a growing literature in low- and middle-income countries (Alrababa'h et al., 2021). These studies often find that group-based rather than individual concerns determine native attitudes (Hainmueller and Hopkins, 2014), and that cultural rather than material or economic drivers are the strongest predictors (Alesina and Tabellini, 2022). Studies of intergroup attitudes in low-income contexts suggest that refugees may have a positive economic effect without affecting social attitudes (Kreibaum, 2016, Zhou, 2020, Zhou, Grossman and Ge, 2022). Our study shows that economic policy can decrease the perceived social distance between hosts and refugees and reduce measures of resource resentment among hosts.

Within the literature on attitudes toward immigrants is a set of papers studying the impacts of aid on refugee-host relations. In rural Uganda, refugee presence is associated with improved public service delivery for natives and a higher vote share for incumbent local politicians but not with shifts in attitudes toward refugees or refugee policies (Zhou and Grossman, 2022, Zhou, Grossman and Ge, 2022). In Tanzania, however, high inflows of resources to refugees created "resource resentment" among the host community (Zhou, 2019), a phenomenon documented in a wide range of contexts (Adato et al., 2015, López, Arredondo and Salcedo, 2011, Kreibaum, 2016, Pavanello et al., 2016). Lehmann and Masterson (2020) find, in contrast, that aid distributed only to Syrian refugees in Lebanon reduced violence toward refugees, positing that aid indirectly benefited the hosts through increased spending or sharing. In a randomized controlled trial in Ecuador, Valli, Peterman and Hidrobo (2019) show that transfers of grants, food, and vouchers to Colombian refugees and poor members

[8]Similarly, Kim and Pelc (2021) find that—after controlling for trade shocks—counties with more Trade Adjustment Assistance petitions see fewer calls for trade protection.

of the host community increased pro-social attitudes and behaviors of refugees but did not lead to measurable effects on host attitudes.[9] In DR Congo, Quattrochi et al. (2021) find that economic transfers in the form of vouchers to displaced persons and vulnerable members of the host community had no effect on social cohesion. Our study builds on this literature by labeling transfers given directly to the host community as redistribution: that is, as aid-sharing with the host community out of funding from the refugee response.[10]

Our work also contributes to a large literature on the effects of intergroup contact on attitude formation. Expanding on the seminal work by Allport (1954), Mousa (2020), Lowe (2021), and Corno, La Ferrara and Burns (2022) find that collaborative contact can reduce prejudice, which is consistent with the meta-analysis by Paluck, Green and Green (2019) and the findings of Jha (2013) that economic complementarities can improve intergroup relations. Lowe (2021) also shows that adversarial contact—opponents in a cricket match—can increase exclusionary attitudes. In Kampala, Loiacono and Silva-Vargas (2023) find that Ugandan business owners who are randomly offered a subsidized refugee employee for one week employ more refugees eight months later, with the effect driven by pairs in which both have positive attitudes toward the other group at baseline. However, Enos and Gidron (2018) and Zhou and Lyall (2022) find few effects of contact among Israel's Jewish citizens toward Palestinians and among Afghan hosts toward internally displaced people, respectively. Finally, in the Ugandan context, Betts et al. (2023) find a positive correlation between interactions with refugees and an index of positive perceptions toward refugees among Ugandans. Our project experimentally induced short-term, collaborative contact through a mentorship program and builds on this literature by comparing the effects on attitudes to programs focusing on economic incentives.

This project also contributes to the vast literature on small business profitability in low- and middle-income countries. A key argument from Bloom and Van Reenen (2007) and Bloom et al. (2013) is that managerial capital is both important for profitability and lacking in many small businesses in these settings. Brooks, Donovan and Johnson (2018) find that a one-on-one mentorship program in Nairobi, Kenya increased profits of inexperienced business owners more than a formal skills training program that attempted to impart this capital to

[9] A potential explanation of this finding, in light of our results, is that the connection between the transfers and the refugee presence was not clear to hosts.

[10] Our paper also relates to literature on politicians' claiming and receiving credit for development projects, for example, Guiteras and Mobarak (2016), Blattman, Emeriau and Fiala (2018), Evans, Holtemeyer and Kosec (2019), and Lyall, Zhou and Imai (2020).

microentrepreneurs. Cai and Szeidl (2018) and Fafchamps and Quinn (2018) similarly find positive effects on businesses from experimentally expanding the business owners' networks. We find substantial interest in our setting in mentorship programs that promote skill transfer across nationalities but find no measurable impacts of these programs on business outcomes.

2 Refugee Hosting in Uganda

This section describes the setting for our study, focusing on policies toward refugees and host attitudes.

2.1 Refugee Hosting Policies

With over 1.5 million refugees, Uganda hosts the largest population of refugees in Africa, and the sixth largest globally (UNHCR, 2023). The majority of refugees live in one of 11 rural settlements, where they receive monthly food assistance from humanitarian actors and a plot of land to farm. Kampala, the capital city and the site of our study, hosts about 125,000 registered refugees, though the unofficial number is likely significantly higher.[11] Refugees choosing to live in Kampala do not receive the food or land offered in the rural settlements. Nearly all of the refugees in Kampala are in protracted displacement situations, where conflicts in the country of origin have lasted for longer than five years.

Refugees in Kampala have primarily settled in slum areas and ethnic enclaves, and occupy economic niches in informal and formal markets. The majority of the refugee population in Kampala is Congolese, with smaller numbers coming from Somalia, South Sudan, Rwanda, Burundi, and Ethiopia (AGORA, 2018). Monteith and Lwasa (2017) find that Congolese refugees are socially and economically segregated from Ugandan society, despite significant spatial integration (Betts et al., 2017). Congolese refugees are well-known in Uganda for their fabrics, tailoring, and cosmetics, which informs the selection of the industries in our sample.

Under Ugandan policy, 30% of international non-food aid budgets for refugees is shared with Ugandan host communities. 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,

[11] The official 125,000 count represents 8% of Uganda's refugee population, and 8% of the Kampala population (UNHCR, 2023).

housing, and education markets. In Uganda, the aid-sharing policy predates these global agreements and since 2006, refugees can move freely within the country, start businesses and accept jobs, and access primary education and other public services under the Refugees Act 2006.[12] However, there are far fewer aid organizations in Kampala than in the settlement areas (Höök, 2015), and Ugandans in Kampala see little evidence of aid-sharing. This makes it possible to study the impact of aid-sharing on policy preferences in a context where a national aid-sharing rule exists but awareness is low.

2.2 Host Attitudes

Ugandans' attitudes toward hosting refugees are mixed. While a majority generally support current policies, a significant minority express concerns about the economic burden, labor market competition effects, or security threat of hosting refugees (IRC, 2018). Many Ugandans support continued humanitarian assistance to refugees; however, opinions are divided on allowing refugees to work or move freely within the country. As we discuss in Section 3.3, 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. There appears to be no strong association between refugee presence and attitudes toward hosting policy (Zhou, Grossman and Ge, 2022), and refugee presence appears to increase political incumbent support (Zhou and Grossman, 2022).

Although Ugandan policy supports incorporation into host communities, refugees face several barriers to integration, including xenophobia among natives (Krause-Vilmar, 2011, Höök, 2015) and discrimination in labor and housing markets (Bernstein, 2005, Krause-Vilmar, 2011, Höök, 2015). City officials may view urban refugees as illegitimately residing in Kampala due to previous restrictions (Bernstein, 2005, Höök, 2015). We present descriptive statistics on attitudes toward refugees from our baseline survey of Ugandan microentrepreneurs in Section 3.3.

3 Experimental Design

This section provides an overview of our sample, data collection, and experimental arms. Additional details on study design, including intervention scripts, are available in Appendix

[12] This was further institutionalized with the Refugee Regulations of 2010, and the Settlement Transformation Agenda in 2016 that integrated refugee and host community self-reliance into the country's second five-year National Development Plan (NDP2).

B.

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. To be included in the experimental sample, the microentrepreneurs needed to be Ugandans no older than 40, have no more than five years of experience in their sector, and to speak Luganda, English, or Swahili conversationally. We excluded businesses with five or more employees or very high profits or capital. This gave a set of 1,406 microentrepreneurs who form our experimental sample.

We selected tailor and salon owners for several reasons. Both refugees and Ugandans commonly own businesses in these sectors, making the potential competition effects of refugee hosting 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 collaboration with refugees. Finally, both sectors require a stable place of business, which facilitates follow-up survey activities.

3.2 Data Collection Timeline

We conducted the census in October 2019 with 3,414 microentrepreneurs. We then conducted a baseline survey from November–December 2019 with the experimental sample of 1,406 Ugandan microentrepreneurs, 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 suspended 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 an average of 73% of respondents. This share is higher for earlier surveys, with a retention rate of 80% in the midline

survey, 74% in the first in-person endline survey, 76% in the phone endline survey, and 64% in the second in-person endline. Table B3 shows tests of differential attrition across treatment groups. 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 Labeled Grant, Information Only, and Refugee Mentorship are similar to that in Control. We present Lee Bounds for each of our pre-specified outcome domains (see Section 3.6.2 for details) in Tables B4 and B5.

3.3 Summary Statistics

Table 1 displays summary statistics for our experimental sample of 1,406 Ugandan microenterprise owners. The average owner in our sample is 28 years old, has 11 years of education, and has 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 earn an average of USD 37 per month, and about one-fifth of businesses have any employees.[13]

At baseline, few owners are aware of Uganda's aid sharing policy: 19% report that any international aid for refugees is shared with Ugandans. Consistent with the evidence described in Section 2.2, there is high general support for refugee hosting (72% of owners say they support Uganda's hosting of refugees) but mixed views toward extending labor market access or freedom of movement (about 60% of owners say they support these policies). About half of owners say they would support allowing more refuges into Uganda.

Many business owners in our sample mention concerns related to the crowd-out effects of hosting refugees: 78% believe that refugees increase business or housing rents, and 62% believe that refugees increase the prices of other goods they buy. A much smaller share (27%) believe that refugees worsen access to, or quality of, public goods like schools and health facilities. About half of our sample believes that the net economic effect of refugee hosting is positive for Uganda. An additional 29% say that the effect is neutral.

3.4 Interventions

Figure 1 summarizes our sample selection and treatment assignment process. We implement three main interventions to test the impact of aid redistribution on policy preferences and

[13]Monetary values are expressed in 2019 US Dollars (USD). One USD was worth 3,695 Ugandan Shillings at the time of the baseline survey in 2019.

Table 1: Baseline Summary Statistics

	Mean	Standard Deviation	Observations
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
Policy Preferences			
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. Questions on refugees' impact on prices and public goods are asked about Congolese and Somalis, and coded as 1 if either answer is "Yes.". "Don't Know" responses to economic beliefs questions are coded as missing.

beliefs. The first intervention delivers 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. Because awareness of this policy is low at baseline (19% of respondents reported that any international aid for refugees is shared with Ugandans), we expect this treatment arm to change beliefs about the economic impact of hosting refugees. We complement this information delivery with a listening exercise modeled after Kalla and Broockman (2020), in which the NGO staff member—who could be either a refugee or a Ugandan—invites the respondent to share their views of refugees, which the staff member is coached not to interrupt or push back on, and then shares 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. Our interventions were carried out by Young African Refugees for Integral Development (YARID),

a refugee-led non-profit organization in Kampala. YARID develops and manages multiple assistance programs for both refugees and Ugandans, such as skills training programs. Before this project, YARID did not explicitly link its assistance programs to Ugandans to the government's aid-sharing policy but did so randomly for the purpose of this research.

The second intervention is a grant of USD 135, or about 3.5 months of average business profit, delivered with the same information and listening exercise contained in the Information Only arm. The grant is described as an example of aid-sharing: we therefore refer to this treatment as the "Labeled Grant" arm. During an initial meeting, a YARID staff member visits the business owner to inform them about the grant and deliver the information. During the second meeting, the staff member disburses the grant. In the first wave of disbursements before COVID-19, we required that at least 60% of the grant be used for business purposes,[14] and arrange for the staff member to pay directly for business expenses at a shop of the owner's choosing. The remaining balance was disbursed through mobile money. After interventions were resumed in February of 2021, all communication between the implementing partner and respondents was over the phone. This included the information delivery and grant payments, which were sent by mobile money with no in-kind requirements.

The third intervention is a mentorship program that matches business owners with experienced refugee business owners in the same sector.[15] 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). The program was offered for free to business owners in our sample. This design is motivated by the contact hypothesis, in which cooperative relationships are theorized to reduce prejudice between majority and minority group members (Allport, 1954), and by the results of a similar mentorship program which demonstrated large impacts on profits (Brooks, Donovan and Johnson, 2018).[16]

[14]This was motivated by the demonstrated long-run impact of in-kind transfers compared to cash transfers in other contexts (Fafchamps et al., 2014).

[15]Mentors were recruited from the population of eligible Congelese 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. Ideally, mentors would have at least six years of experience and not overlap with the main sample; however, the supply of experienced refugees in three out of four gender-sector cells was too low for a sufficiently powered experiment. We reduced the experience requirement for mentors to three years for male and female salon owners owners and female tailors, and kept the six year requirement for male tailors.

[16]The most common topics of discussion during meetings were customers, skills, equipment and tools, location choices, and suppliers. According to YARID facilitator reports, in 34% of meetings, most of the

In addition to our three main interventions, we include three additional treatment arms to isolate potential mechanisms behind treatment impacts. The first is a business grant identical to the labeled grant, but without any information about refugees or Uganda's aid-sharing policy, which we refer to as the "Grant Only" arm. The second is a mentorship program that matches business owners with an experienced Ugandan business owner in their sector. Mentors were chosen to balance characteristics across nationality groups (see Table B2). This treatment arm allows us to isolate the impact of cooperative contact with refugees from the impacts of mentorship *per se*.[17] YARID 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, we include a pure control group, which did not receive any treatment and was not contacted by YARID.

Interventions were implemented in-person to about 30% of the sample beginning in January 2020. Due to disruptions related to COVID-19, we suspended interventions and restarted all treatments remotely in February 2021. At this time, we dropped the requirement that at least 60% of grants be used for business expenses, and disbursed the full grant through mobile money.[18] We also converted mentorship meetings from in-person to remote. YARID provided up to four facilitated mentorship meetings using three-way calling, regardless of the number of meetings that were held prior to COVID-19.[19] Tables B6 and B7 provide additional information about treatment status before and after COVID-19.

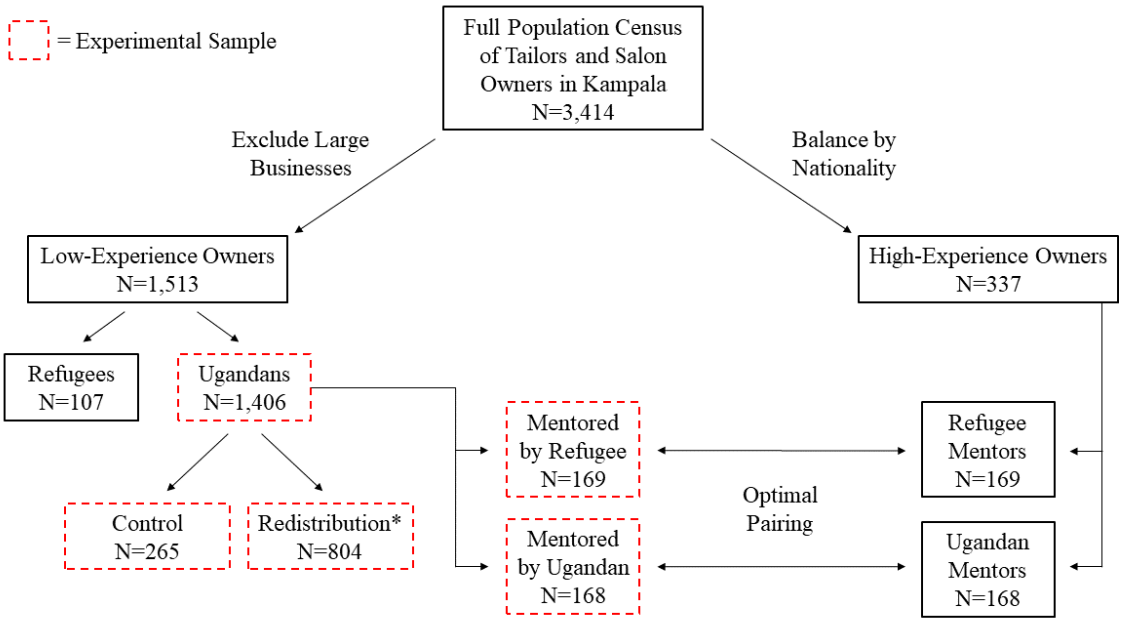
conversation was translated. In 45% of conversations, the facilitator reported that the mentor and mentee had roughly equal control over the conversation.

[17]Business owners were not informed before signing up for the program whether their mentor would be a refugee or a Ugandan. They were told only that that the business owner is in the same industry, of the same gender, and might be of another nationality. Uptake was balanced across the Mentored by Refugee and Ugandan arms.

[18]Business owners were encouraged to invest the money in their business if it was still operating, but this was not enforced. Of the 143 purchases made before COVID-19 in the Grant Only and Labeled Grant groups, 27 (18%) reported buying small tools like scissors, razors, needles and thread, for their salon or for their tailor shop, and 71 (50%) bought assets including chairs, professional grade hair dryers, and sewing machines. Fifty-seven out of 92 salon owners (62%) bought non-durable goods like hair products and cleaning supplies and 23 out of 51 tailors bought fabric (45%). On average 420,000 UGX (Ugandan Shillings, USD 114) was spent on the items and almost no beneficiaries spent more than the 500,000 UGX grant. While 25% spent exactly the minimum and received 200,000 ($54) in cash, 48% spent the entirety of the grant including 8% who used some of their own money to purchase a more expensive item. Out of the 143, 53 (37%) reported they were using the remaining money for business rent and the majority did not disclose what they would spend it on.

[19]Before COVID-19, the conversations lasted an average of 43 minutes. After interventions restarted, the conversations lasted an average of 23 minutes.

Figure 1: Summary of Study Design



Notes: See Appendix B for details on sample selection. Businesses with high capital or profit were excluded from the experimental sample. Potential 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.

3.5 Randomization

We assign treatments randomly within strata defined by gender, sector, and mentor eligibility,[20] and, within each of these cells, median profits and median attitudes towards hosting using the Stata command *randtreat*. 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 Labeled Grant, Information Only, Grant Only, and Control. Table B1 shows balance tests for the set of baseline characteristics displayed in Table 1, plus the baseline value of each domain summary index (see Section 3.6.2). We reject joint orthogonality of our treatment variables at the 10% level for 2 out of 31 baseline variables, suggesting that randomization was effective at creating

[20]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 treatment groups according to the same process used for mentor-ineligible respondents.

balanced treatment groups.

3.6 Empirical strategy

This section briefly describes our strategy for measuring outcomes and identifying treatment effects. Additional details are available in our pre-analysis plan hosted at the AEA RCT Registry (Baseler et al., 2022).

3.6.1 Estimating equations

We estimate intent-to-treat (ITT) effects using the following ANCOVA specification:

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

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 dummies 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 strata fixed effect; 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 between each treatment group and the control group, across individuals and post-treatment survey rounds, conditional on pre-treatment outcome levels and the set of baseline controls selected by double lasso. See McKenzie (2012) for details on the ANCOVA specification in the analysis of experiments.

3.6.2 Measurement and Multiple Hypothesis Testing

Because many of our outcomes of interest represent broad conceptual categories, such as "support for inclusive refugee hosting policies," we organized our outcomes into a series of 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.[21]

Within each pre-specified domain, we report 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 to control for the family-wise error rate in Table A9. This procedure estimates the probability of making one or more type I errors and adjusts for correlation across outcomes. The main body of this paper presents only a subset of our pre-specified analysis; we report the full set of pre-specified outcomes in Online Appendix C.[22]

4 Results

We find that redistributing refugee aid toward Ugandans in the form of a labeled grant—that is, a grant labeled as part of Uganda's broader aid-sharing policy, along with information about that policy—substantially and persistently changes policy preferences in favor of greater support for refugee hosting and inclusive policies such as extending labor market access and freedom of movement. Sharing information about existing redistribution—without any additional grant—has similar, but smaller, impacts. Subsidizing cooperative contact through business mentorship by experienced refugees has no durable impacts on policy preferences or attitudes.

4.1 Policy Preferences

A primary hypothesis of this study is that receiving aid connected to the refugee presence will change policy preferences, as summarized by a pre-specified index. We find that receiving a labeled grant significantly increases support for refugee hosting and inclusive hosting policies, as shown in Table 2. Recipients of labeled grants were 14 pp. more likely to say that they support Uganda's hosting of refugees generally, on a base of 73% (q-val = 0.001; family-wise error rate < 0.001). Labeled grants also increase support for admitting more refugees into Uganda (15 pp. on a base of 52%, q-val = 0.001), support for extending the right to work (14 pp. on a base of 60%, q-val = 0.001), and support for extending freedom of movement

[21] In the Anderson summary index, a component's weight is equal to the sum of its row entries in the inverted covariance matrix of outcomes in its domain.

[22] Online Appendix C can be accessed here.

to refugees (6 pp. on a base of 60%, q-val = 0.07). The impact on our pre-specified domain summary index is 0.35 standard deviations (p-val < 0.001).

Our Information Only treatment—in which owners learn about Uganda's aid-sharing policy and participate in the listening exercise but do not receive a grant—also impacts policy preferences, though by less than receiving a labeled grant (p-val = 0.01). Effect sizes are generally half to two-thirds the size of impacts of the labeled grant. Our Grant Only treatment—which included a business grant but no information about aid-sharing—also impacts policy preferences in the same direction, though by a smaller magnitude (p-val = 0.05), than labeled grants. As we discuss further in Section 5.2, this result is likely due to an implicit labeling of the grants operating through contact with the refugee-led implementing NGO, as unlabeled grant recipients were significantly more likely to report aid associated with refugees compared to control (p-val < 0.01). It may also be due in part to the grant's impact on views about the fairness of aid distribution. We do not believe that wealth effects are driving changes in attitudes, as discussed in Section 5.

Do the impacts on self-reported views reflect changes in real-world behavior? Our main strategy to test for changes in true preferences was to identify a behavior reflecting true policy support, by inducing a naturalistic situation that required business owners 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 either the survey firm or YARID. One year after the interventions were completed, they 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.[23] As shown in Table 2, labeled grant recipients were 10 pp. more likely to respondent affirmatively to the call (on a base of 23%, p-val < 0.01), with no significant differences for other treatment arms.[24] This

[23]The organization is called OneYouth OneHeart Initiative. The letter was described as being addressed to local politicians (Members of Parliament and local councilors, or LC1s) and including a thank you note for allowing refugees to live in Kampala with the right to work. We recorded a one-minute message explaining the campaign. Respondents could press 1 to support the campaign or 2 to oppose the campaign, and their answers were immediately recorded by the phone system. See Appendix Section B.5 for the script. For people who did not answer, we followed up with the same question over SMS and subsequent calls the following day to alternative phone numbers for people who did not answer the phone or respond to the SMS. The campaign was intended to allow respondents to express their policy views without any risks of opposing the government, and only the number of supporters—not names—were not included in the final letter. Call campaigns are not uncommon in this context, and the business owners were not told that the phone call was connected to the intervention they had received.

[24]More detailed results are presented in Table A8. Over 80% of the sample answered the call, and all

Table 2: Policy Preferences

	Supports Refugee Hosting	Supports More Refugees	Supports Right to Work	Supports Freedom of Movement	Policy Preference Index	Supported Phone Campaign^{+}
Labeled Grant	0.137***	0.151***	0.136***	0.059*	0.353***	0.100***
	(0.024)	(0.030)	(0.026)	(0.031)	(0.062)	(0.038)
	[0.001]	[0.001]	[0.001]	[0.067]	[0.000]	[0.008]
Information Only	0.060**	0.101***	0.082***	0.022	0.213***	0.024
	(0.027)	(0.031)	(0.027)	(0.031)	(0.064)	(0.036)
	[0.041]	[0.004]	[0.009]	[0.279]	[0.001]	[0.513]
Grant Only	0.094***	0.115***	0.096***	0.015	0.240***	0.043
	(0.028)	(0.031)	(0.027)	(0.031)	(0.065)	(0.039)
	[0.003]	[0.003]	[0.003]	[0.368]	[0.000]	[0.265]
Mentored by Refugee	0.033	0.064*	0.075**	-0.028	0.108	-0.022
	(0.031)	(0.034)	(0.031)	(0.037)	(0.072)	(0.042)
	[0.196]	[0.071]	[0.028]	[0.267]	[0.133]	[0.603]
Mentored by Ugandan	0.064**	0.040	0.025	-0.066*	0.099	-0.033
	(0.030)	(0.034)	(0.032)	(0.037)	(0.073)	(0.043)
	[0.047]	[0.188]	[0.267]	[0.077]	[0.175]	[0.437]
Observations	3,040	3,038	3,039	3,031	3,051	1,406
Control Mean (Baseline)	0.726	0.515	0.600	0.599	0.000	.
Control Mean (Follow-Ups)	0.746	0.605	0.717	0.540	-0.000	0.230
p-val: Info = Labeled Grant	0.001	0.080	0.029	0.222	0.017	0.043
p-val: Grant = Labeled Grant	0.063	0.232	0.102	0.142	0.052	0.164
p-val: R-Mentee = U-Mentee	0.327	0.495	0.129	0.355	0.901	0.809

Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices and outcomes not pre-specified (denoted with $^{+}$).

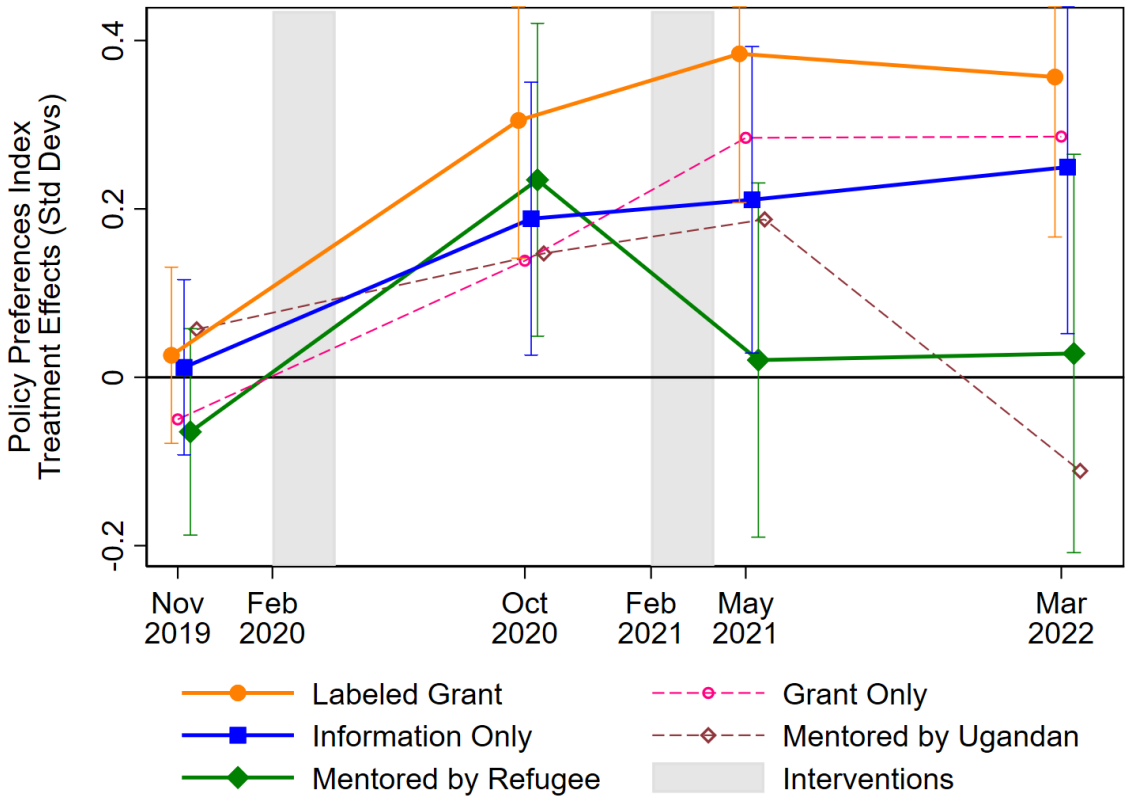
result, together with additional evidence discussed in detail in Section 5, points to a change in true policy preferences rather than effects driven entirely by experimenter demand.

Mentorship by an experienced refugee has much smaller impacts on policy preferences compared to labeled grants. We observe modest increases in support for extending labor market access (8 pp. on a base of 60%, q-val = 0.03), 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 0.11 standard deviations (p-val = 0.13).

Treatment impacts on policy preferences persist for years after the interventions, as shown in Figure 2, which displays treatment impacts estimated separately by survey round. We

treatment groups were equally likely to answer. Of the full sample, 29% responded, and of those 80% supported the campaign.

Figure 2: Treatment Impact on Policy Preferences Index Over Time



Notes: Each line shows the estimated treatment impact on a summary index of preferences for policies supporting inclusive refugee hosting over time, estimated using Equation 1. Nov 2019 corresponds to the baseline survey, Oct 2020 to the midline, May 2021 to the endline, and Mar 2022 to the second endline. We did not collect these measures during the second phone survey. Shaded gray areas show the timing of our interventions, which began in January 2020 and resumed in February 2021 after our pause due to COVID-19. Vertical bars show 95% confidence intervals for each survey round for the Labeled Grant, Information Only, and Mentored by Refugee arms.

see no evidence of attenuation of the treatment effects of labeled grants, unlabeled grants, or information as of the final endline survey in March 2022. Given that interventions began in early 2020 (and resumed in early 2021), this suggests that redistribution (and even information about redistribution) can impact policy views in the long run.

4.2 Beliefs About Economic Impacts of Refugee Hosting

Policy attitudes may change due to updated beliefs about the economic impacts of refugee hosting, a secondary hypothesis of this study. Business owners 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. Respondents were asked if they had received any support, and if so, if they remembered the purpose of the program. 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 17%, q-val = 0.001),[25] and 16 pp. more likely to say refugees have a positive effect on the economy overall (on a base of 50%, q-val = 0.001). They were also more likely to say that refugees benefit them personally, and that refugees have skills (despite the fact that this intervention did not share information about refugees' skills). The impact on our pre-specified domain summary index is 0.3 standard deviations (p-val < 0.001).

Our Information Only and Grant Only treatments also changed beliefs about the economic impacts of refugee hosting, though by less than receiving a labeled grant. 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 only slightly smaller than that among labeled grant recipients. As discussed in Section 5.2, we believe this is due to an implicit labeling of the grant operating through contact with the refugee-led implementing organization. Effect sizes are roughly half the size of impacts of the labeled grant. Mentorship had no discernible impacts on economic beliefs.

4.3 Social Attitudes Toward Refugees

Policy attitudes may change due to updated social attitudes toward refugees, especially through mentorship by a refugee, another secondary hypothesis of this study. We find that labeled grant recipients changed some of their social attitudes toward refugees, as shown in Table 4. We observe a decrease in perceived social distance between respondents and refugees: the labeled grant increases the share who report that they would be comfortable being close friends with a refugee by 7 pp, and marrying a refugee by 13 pp. (q-vals $<$ 0.01). We do not observe significant changes in beliefs about the cultural impact of refugee hosting, or in whether refugees deserve sympathy. The impact on our pre-specified domain summary index is 0.16 standard deviations (p-val = 0.02). As we discuss in Section 5.1, our

[25] Average awareness of aid-sharing is higher in the control group in follow-up surveys than at baseline (37% versus 17%), suggesting that Ugandans are learning about the aid-sharing policy independently of our experiment. We believe this is happening through aid distributed during the COVID-19 pandemic; one percent of the control group had received any assistance in the year proceeding the baseline survey, while 45% reported receiving assistance during COVID-19 lockdowns.

Table 3: Beliefs About Economic Impacts of Hosting Refugees

	Associated Support w Refugees^{+}	Knows About Aid-Sharing	Pos Effect on Economy Overall	Pos Effect on You Personally	Refugees Have Skills	Economic Beliefs Index
Labeled Grant	0.115***	0.148***	0.156***	0.102***	0.111***	0.303***
	(0.016)	(0.032)	(0.035)	(0.035)	(0.041)	(0.071)
	[0.001]	[0.001]	[0.001]	[0.014]	[0.019]	[0.000]
Information Only	0.060***	0.046	0.110***	0.069**	0.026	0.221***
	(0.014)	(0.032)	(0.034)	(0.034)	(0.042)	(0.067)
	[0.001]	[0.213]	[0.009]	[0.093]	[0.492]	[0.001]
Grant Only	0.082***	0.093***	0.106***	0.125***	0.040	0.248***
	(0.015)	(0.033)	(0.036)	(0.036)	(0.044)	(0.071)
	[0.001]	[0.017]	[0.014]	[0.006]	[0.447]	[0.000]
Mentored by Refugee	0.022	-0.051	0.035	-0.032	0.033	0.089
	(0.016)	(0.036)	(0.039)	(0.039)	(0.048)	(0.077)
	[0.085]	[0.213]	[0.447]	[0.453]	[0.466]	[0.245]
Mentored by Ugandan	0.049***	0.012	0.037	0.069*	0.014	0.088
	(0.017)	(0.037)	(0.039)	(0.039)	(0.046)	(0.078)
	[0.004]	[0.591]	[0.447]	[0.138]	[0.800]	[0.260]
Observations	3,061	3,061	2,787	2,906	1,671	3,003
Control Mean (Baseline)	.	0.173	0.503	0.409	0.511	0.000
Control Mean (Follow-Ups)	0.024	0.369	0.423	0.443	0.416	-0.000
p-val: Info = Labeled Grant	0.002	0.002	0.164	0.309	0.033	0.220
p-val: Grant = Labeled Grant	0.083	0.103	0.151	0.504	0.088	0.439
p-val: R-Mentee = U-Mentee	0.179	0.111	0.969	0.012	0.698	0.987

Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices and outcomes not pre-specified (denoted with $^{+}$).

results suggest that impacts on social attitudes toward refugees are driven not by contact with refugees, but indirectly through effects on economic beliefs.

Our Information Only and Grant Only treatments modestly changed social attitudes toward refugees, though the impacts are generally small and inconsistent across outcomes. Mentorship had no discernible impacts on social attitudes.

During our surveys, we conducted a simple dictator game in which the respondent distributed 3,000 UGX (Ugandan Shillings, about $0.80) between themselves, a program that helps refugees in Kampala, and a program that helps Ugandans in need.[26] This offers a financially incentivized measure of altruism toward refugees. Labeled grants increase the proportion donated to refugees by 4 pp. (on a base of 21%, q-val = 0.12). The Grant Only

[26]The base compensation for survey participation was 7,000 UGX for in-person surveys and 3,000 UGX for phone surveys.

Table 4: Social Attitudes Toward Refugees

	Comfortable Refugee Friends	Comfortable Marry Refugee	Prop. Donated Refugees	Pos Effect Culture	Deserve Sympathy	Social Attitudes Index
Labeled Grant	0.073***	0.131***	0.042***	0.005	0.032	0.155**
	(0.027)	(0.039)	(0.015)	(0.032)	(0.041)	(0.065)
	[0.007]	[0.001]	[0.117]	[1.000]	[0.732]	[0.018]
Information Only	0.069**	0.056	-0.001	0.055*	0.034	0.069
	(0.028)	(0.040)	(0.016)	(0.031)	(0.040)	(0.064)
	[0.012]	[0.161]	[1.000]	[0.308]	[0.732]	[0.279]
Grant Only	0.057**	0.071*	0.041***	-0.024	0.094**	0.134**
	(0.028)	(0.041)	(0.016)	(0.033)	(0.041)	(0.066)
	[0.039]	[0.082]	[0.117]	[0.732]	[0.159]	[0.043]
Mentored by Refugee	-0.002	0.061	-0.023	0.028	-0.006	-0.025
	(0.035)	(0.047)	(0.018)	(0.038)	(0.046)	(0.073)
	[0.944]	[0.193]	[0.480]	[0.732]	[1.000]	[0.733]
Mentored by Ugandan	0.036	0.024	-0.003	0.066*	-0.017	0.023
	(0.032)	(0.046)	(0.019)	(0.035)	(0.044)	(0.072)
	[0.268]	[0.604]	[1.000]	[0.256]	[0.959]	[0.752]
Observations	1,942	1,942	3,061	2,612	1,814	3,061
Control Mean (Baseline)	0.782	0.492	0.211	0.708	0.464	0.000
Control Mean (Follow-Ups)	0.817	0.486	0.284	0.690	0.540	0.000
p-val: Info = Labeled Grant	0.883	0.059	0.003	0.104	0.964	0.158
p-val: Grant = Labeled Grant	0.510	0.142	0.953	0.365	0.120	0.741
p-val: R-Mentee = U-Mentee	0.273	0.463	0.336	0.332	0.813	0.522

Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices and outcomes not pre-specified (denoted with $^{+}$).

arm also increased the proportion donated, by 4 pp. (q-val = 0.12). Other treatment arms had no significant effects on the proportion donated.

4.4 Business Outcomes and Household Welfare

None of our treatment arms significantly changed business outcomes or household welfare, as shown in Table 5. Business profit earned over the month preceding the survey was slightly lower among grant recipients and owners mentored by Ugandans, by $2–3 on a base of $40. While somewhat surprising, the impacts are not statistically significant, and may reflect the impact of COVID-19 lockdowns, which reduced the scope for earnings profits while also reducing the incentive to invest (rather than consume) the grant. Impacts on business capital are also noisy: the treatment impact of labeled grants is negative, while the impact of grants alone is positive. Again, none of the effects on capital is statistically significant.

Table 5: Business Outcomes and Household Welfare

	Business Profits (USD/Month)	Business Capital (USD)	Business Practices Index	Household Well-Being Index
Labeled Grant	-2.33	-51.4	0.049	0.063
	(2.41)	(43.0)	(0.078)	(0.062)
	[0.332]	[0.232]	[0.536]	[0.312]
Information Only	-0.32	13.9	-0.038	-0.036
	(2.54)	(47.2)	(0.078)	(0.066)
	[0.900]	[0.768]	[0.628]	[0.589]
Grant Only	-1.51	-3.55	0.13*	0.052
	(2.51)	(46.2)	(0.073)	(0.064)
	[0.549]	[0.939]	[0.067]	[0.417]
Mentored by Refugee	1.53	-28.4	0.065	-0.0092
	(2.73)	(47.9)	(0.087)	(0.076)
	[0.574]	[0.554]	[0.451]	[0.904]
Mentored by Ugandan	-1.54	23.7	0.097	0.13*
	(2.82)	(53.1)	(0.080)	(0.068)
	[0.584]	[0.656]	[0.222]	[0.052]
Observations	4,029	2,819	1,942	4,132
Control Mean (Baseline)	39.61	495.56	0.000	0.000
Control Mean (Follow-Ups)	20.69	632.54	0.000	0.000
p-val: Info = Labeled Grant	0.383	0.130	0.262	0.077
p-val: Grant = Labeled Grant	0.722	0.278	0.249	0.845
p-val: R-Mentee = U-Mentee	0.305	0.342	0.725	0.048

Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices and outcomes not pre-specified (denoted with $^{+}$).

We find modest impacts of grants and mentorship on our index of business practices—which we modify from McKenzie and Woodruff (2017)—comprising marketing, buying and stock control, and costing and record keeping, though none is statistically significant. We find suggestive evidence that grants improved household well-being,[27] as summarized in an index comprising income, savings, and qualitative reports of economic hardship (see Table C21 for impacts on the full set of welfare components). However, impacts are small (0.05–0.06 standard deviations) and statistically insignificant.

[27]If treatment is complementary with labor supply, this will reduce welfare impacts of treatment given a positive opportunity cost of owners' time (Agness et al., 2022). We do not find significant differences in time use across treatment groups (see Table C16) and so do not make any welfare adjustments.

5 Potential Mechanisms

Why does learning about aid-sharing—either indirectly through new information or directly by receiving a labeled grant—increase support for refugee hosting? Given that resource and job competition is the primary concern among Ugandans who express negative opinions about hosting refugees (IRC, 2018), redistributing aid has the potential to alleviate this concern. Our results confirm that learning about aid-sharing changes beliefs about the economic impact of hosting refugees. In this section, we discuss the evidence in favor of this interpretation, and consider potential alternative mechanisms, including culturally mediated impacts, contact with individual refugees, experimenter demand effects, and wealth effects.

5.1 Economics or Culture?

Our leading hypothesis is that receiving aid—especially when explicitly labeled as aid-sharing between refugees and Ugandans—leads recipients to update their beliefs about the economic impact of refugees on the host country, which generates increased support for refugee hosting. The results in Table 3 show that labeled grant recipients were much more likely to say that refugees have a net positive economic impact on Uganda and on them personally. As shown in Table A3, changes in support for hosting and integrating refugees—as summarized by an index—are significantly greater among those who expressed greater economic concerns about hosting refugees prior to treatment. This result holds across all treatment groups. As we expect policy preferences to be more sensitive to beliefs about economic impacts among natives with economic concerns about refugee hosting, this finding suggests that changes in economic beliefs are driving impacts on policy views.[28]

An alternative interpretation is that economic beliefs are causally "downstream" from policy views, which may change purely for non-economic (cultural) reasons.[29] For example, receiving a grant from a refugee-led organization may lead owners to feel more altruistic toward refugees via a gift exchange mechanism. Or, the ice-breaker listening exercise may have impacted altruism directly. This greater altruism could potentially impact both policy views and economic beliefs. Disentangling economic from cultural mechanisms driving views

[28] We also find that treatment impacts are greater among those who expressed greater social concerns about refugee hosting. This may be because social and economic concerns about hosting are strongly correlated in our data.

[29] We designate 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.

toward immigration is notoriously difficult, as the two are often highly correlated (Alesina and Tabellini, 2022), as they are in our data.

Nevertheless, several pieces of evidence contradict a purely cultural explanation of our results. Our treatment group that facilitated collaborative intergroup contact through refugee mentorship—which according to the contact hypothesis of Allport (1954) acts directly on perceived social distance—did not change cultural attitudes or policy views. Information about aid-sharing—whether delivered with a business grant or not—did change policy views, but these impacts are not significantly different depending on whether the YARID facilitator was a Ugandan or a refugee. Recipients of labeled grants were also significantly more likely to know that international donations to refugees are shared with Ugandans, suggesting a direct link between economic beliefs and information about aid-sharing. They were also more likely to be familiar with current hosting policies (see Table C4), indicating that the information shared during the intervention was memorable and salient.[30] In follow-up surveys, they were more likely to report receiving aid and to associate that aid with YARID and with refugees in general (see Table A1). If economic beliefs were not directly affected by information about aid-sharing—and were instead an outcome of shifting cultural views—we would not necessarily expect business owners to remember the detailed information shared during the intervention.

Rather, our results suggest that social attitudes about refugees are influenced by economic beliefs.[31] Even though labeled grant recipients did not experience any new contact with refugees—as discussed at greater length in Section 5.3 below—we still observe significant increases in the share of business owners reporting being comfortable being friends with, or marrying, refugees, and an increase in the share allocated to refugees in our dictator game. These impacts are possibly due to rationalization, or "motivated reasoning," about refugees, a process through which a set of views (social beliefs) is formed under the influence of emotions driven by another set of views (economic beliefs).

[30]This interpretation is related to the findings of Bauhoff and Kandpal (2021), who find that pay-for-performance incentives increase the effectiveness of information relative to a flat fee.

[31]The listening exercise embedded in the information script could also have influenced social attitudes. However, this can not explain the impacts on social attitudes of unlabeled grants, which are generally larger than impacts of the information script alone.

5.2 Salience, Trust, and Resource Resentment

The effects of the labeled grant on policy views are generally 50–100% greater than the effects of information about aid-sharing alone. One explanation is that direct receipt of aid may make the accompanying information more believable or salient by acting as a visible demonstration of aid-sharing. Consistent with this, we find that recipients of labeled grants are much more likely to say that international organizations are trustworthy compared to the Information Only arm (19 pp. on a base of 44%, p-val = 0.001), as shown in Table A2. They were also more likely than the Information Only arm to remember that some of the aid coming from the international refugee response is shared with Ugandans, consistent with a salience effect.

A second explanation for the greater treatment effects of labeled grants compared to information alone is that the labeled grant brings a personal benefit that information alone does not. To test this hypothesis, we exploit the fact that our information script focused on hospitals and schools as examples of public goods in Kampala funded by aid coming from the refugee response. If variation in personal economic benefits is explaining the differences in impacts across treatment groups, we expect it to explain variation within group as well. Table A4 shows estimates of heterogeneous treatment effects on policy views (summarized by an index) 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. In no case do we find significant differences in treatment impacts of information alone, although the estimate for hospital use is positive. While this does not rule out the importance of personal economic effects in mediating treatment impacts, it suggests that perceptions about group-level impacts are likely to be key drivers of policy views. This is consistent with the review of the political science literature on views toward immigration in Hainmueller and Hiscox (2010), which concludes that *personal* economic concerns do not appear to be a significant driver of attitudes toward immigration.

Why Did Unlabeled Grants Affect Attitudes? Somewhat surprisingly, receiving a grant without any information about aid-sharing also increases support for refugee hosting. We believe two distinct—though not mutually exclusive—mechanisms explain this result.

First, grant recipients learned that the grant came from a refugee-led organization, lending an implicit labeling of the grant as associated with the refugee presence. Although we intended to minimize associations with refugees in the Grant Only group, our implementing partner is a well-known refugee-led organization in Kampala, and grant recipients may have already known about the organization, or learned about it after the intervention. Indeed, we see that owners in the Grant Only treatment arm were more likely to report receiving support, and to associate that support with YARID and with refugees, than the control group (though less than the Labeled Grant group, as shown in Table A1).

Second, receiving a grant appears to reduce feelings of what Zhou (2019) terms "resource resentment," or negative views toward a group perceived to be receiving unfair levels of support. As shown in Table A2, 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 appears to be driven in part by changing beliefs about the distribution of aid—unlabeled grant recipients were 8 pp. less likely to say that refugees receive more aid than Ugandans (on a base of 71%, p-val $= 0.25$)—and especially by changing attitudes toward international aid organizations: unlabeled grant recipients were much more likely to say that international aid organizations care about them (12 pp, p-val $= 0.05$) and are trustworthy (22 pp, p-val < 0.001). Together, these findings suggest that the receipt of aid can, in itself, reduce feelings of resentment toward groups perceived to be major beneficiaries of aid, such as refugees.[32]

5.3 Contact With Refugees

While contact with the refugee-led organization appears to explain the impact of unlabeled grants on beliefs, we find no evidence of impacts driven by contact with individual refugees, either as mentors or program facilitators as part of our programs, or through increased contact with refugees outside of our programs. Despite COVID-19 interruptions, our mentorship program involved moderate collaborative intergroup contact relative to other experiments that facilitate contact between different ethnic, national, or religious groups (Pettigrew and

[32] A reduction in resource resentment may also be driving part of the treatment effects of labeled grants on policy views, but we do not think this can be the sole explanation. First, labeled grants affect policy views significantly more than unlabeled grants (see Table 2). Second, labeled grant recipients are about as likely to say that refugees receive too much aid compared to control (4 pp. difference on a base of 77%, p-val $= 0.48$). The difference in impacts between labeled and unlabeled grants on beliefs about whether refugees receive too much aid is possibly due to our information treatment making aid toward refugees more salient.

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. Nevertheless, we find few impacts of mentorship on policy preferences, economic beliefs, or social attitudes. We also do not find that contact with a refugee YARID facilitator, relative to a Ugandan YARID facilitator, affects the treatment impacts in Labeled Grant or Information Only arms (see Table A3, Column 2).

We find no impacts of any treatment arm on contact with refugees by choice, as shown in Table C14. This indicates that treatment impacts were not mediated by contact with refugees outside the experiment.

5.4 Experimenter Demand Effects

A potential concern is that the observed change 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. Given that YARID is refugee-led, in part refugee-staffed, and focused on supporting refugees in Uganda, business owners may believe that their chances of receiving future assistance are increased by expressing pro-refugee views.[33] 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. We do not observe treatment impacts on every outcome related to refugee hosting policy, or on economic and social beliefs about refugee hosting. This is inconsistent with extremely strong demand effects, but does not rule out demand effects that appear in some outcomes but not others. Below we discuss aspects of our study design that were intended to minimize demand effects and discuss several results testing whether true preferences were impacted by our treatments.

We designed our study to minimize these potential demand effects as much as possible. Surveys were conducted by a Ugandan-led firm unconnected to YARID. We reminded respondents at the beginning of each survey, and immediately prior to survey modules containing

[33] 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 2). Demand effects could also lead us to *underestimate* treatment impacts on true beliefs, if the control group believes, due to its not yet receiving aid, that it is likely to receive aid in the future.

sensitive questions, that their answers would remain anonymous and would not affect their eligibility for aid. We also explained to grant beneficiaries that the grant they were receiving was a one-time transfer. Nevertheless, it is not possible for us to rule out concerns about demand effects by study design alone. We therefore included several tests to understand whether demand effects are driving our results.

The phone-call campaign discussed in Section 4.1 was conducted by an independent organization and should therefore not be subject to strong experimenter demand effects. That we observe significantly higher support for refugee hosting among labeled grant recipients in this campaign is, in our view, strong evidence of a change in true policy preferences.

We conducted several additional tests to assess whether experimenter demand effects are driving our results. The implementing NGO, YARID, conducted a campaign opposing child labor within the Grant Only and Information Only arms of our sample.[34] This campaign only informed the respondent about YARID's views on child labor without offering any other information that could change beliefs about child labor. The script is reproduced in Appendix Section B.6. By comparing the impact of the campaign on expressed views toward child labor in the Grant Only to the Information Only, we can identify whether receiving assistance amplifies demand effects. In follow-up surveys taken after the child labor campaign, we found no impacts on attitudes toward child labor in either the Grant Only or the Information Only arm, as shown in Table A6. This indicates that experimenter demand effects within this group are likely to be low in general, with or without the receipt of assistance.

In a follow-up survey, we conducted a priming experiment 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 Table A7), consistent with limited demand effects in this setting. Additionally, we find significant impacts on the share of an endowment donated to a program supporting refugees in a dictator game (see Table 4), when the respondent had the option to donate to a program supporting refugees, Ugandans, or keep for themselves. Taken together, these results strongly suggest that demand effects are not substantial in this setting, and are not completely driving the treatment impacts we observe.[35]

[34]Like refugee hosting, child labor policies are somewhat, but not extremely, sensitive issues in Uganda. We chose our outcomes for these tests to have similar means to support for refugee hosting.

[35]In a different setting, De Quidt, Haushofer and Roth (2018) find that "typical demand effects are

5.5 Wealth Effects

In theory, changes in attitudes and policy views could be driven by the impact of the grant *per se*, for example by reducing feelings of scarcity and thus the salience of resource competition with refugees. We do not believe wealth effects are driving our results. As shown in Tables 5 and C21, we observe only small treatment impacts on several measures of economic well-being. Moreover, the Information Only treatment, despite containing no grant, significantly impacted policy preferences.

5.6 Crowd-Out Effects of Redistribution

We do not find that redistribution crowds out other sources of policy support such as altruism. We can easily reject full crowding-out: such an effect would lead us to find null or negative treatment impacts of labeled grants or information about aid-sharing on support for refugee hosting, but in fact these impacts are large and positive. We also find evidence pointing against even partial crowding-out. We observe an increase in donations supporting refugees in an incentivized dictator game, consistent with an increase in altruistic feelings toward refugees. We also observe no negative treatment impacts on the share of respondents reporting that most refugees deserve sympathy, and positive treatment impacts on measures of perceived social proximity, such as willingness to socialize with or marry refugees. This suggests that aid-sharing facilitates, rather than crowds out, altruism.

6 Discussion

Many public policies create winners and losers. Redistribution has been proposed as a means to build political support for such policies, but may fail if economic considerations cannot influence voters' preferences. We provide experimental evidence testing the scope of redistribution to influence political views on immigration. This paper experimentally increases awareness of a national policy that connects inclusive refugee hosting with aid-sharing between refugees and hosts. We find that information about aid-sharing, especially when augmented with a business grants labeled as redistribution of foreign aid, leads voters to update their beliefs about the net economic impact of hosting refugees and change their policy views in favor of hosting refugees, extending labor market access, and allowing freedom of movement. These impacts persist for at least two years from the start of our interventions.

probably modest" based on experiments that attempt to induce demand effects in large online samples.

This apparently long-term change in views 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 between refugees and hosts may alleviate some hosts' concerns that the costs of hosting refugees have been placed upon them unfairly. Further exploring how beliefs about fairness influence the attitudes and policy views of hosts is a promising avenue for future research.

Many refugees in protracted situations face significant limitations in the labor market and are forced to rely on humanitarian assistance with little long-run benefits. If refugees could better support their own livelihoods through work, spending on humanitarian assistance could be reallocated to development aid and host communities. While integration of refugees within host communities would likely benefit hosts and refugees on net, host community opposition may make inclusive policies infeasible. Our findings suggest that aid-sharing could contribute to a new political economy equilibrium with greater integration of refugees and more financial support to host communities. This strategy is at the heart of the UN's Global Compact on Refugees and the compact model generally, but the underlying premise linking aid-sharing to political support has not been rigorously tested to our knowledge.

In countries that already share aid, our findings have immediate programmatic implications for organizations supporting both refugees and hosts. Non-profits in these settings can more explicitly tie their interventions to aid-sharing policies and practices to improve social cohesion between refugees and hosts. Many of these organizations 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 is likely minimal.

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

A Additional Tables

Table A1: Recall of Treatments

	Reported Any Support^{+}	Associated Support w YARID^{+}	Associated Support w Data Firm^{+}	Associated Support w Refugees^{+}	Knows About Aid-Sharing
Labeled Grant	0.247***	0.211***	0.084***	0.115***	0.148***
	(0.030)	(0.019)	(0.017)	(0.016)	(0.032)
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Information Only	0.001	0.010	0.024*	0.060***	0.046
	(0.027)	(0.006)	(0.013)	(0.014)	(0.032)
	[0.241]	[0.070]	[0.043]	[0.001]	[0.213]
Grant Only	0.260***	0.180***	0.098***	0.082***	0.093***
	(0.030)	(0.018)	(0.017)	(0.015)	(0.033)
	[0.001]	[0.001]	[0.001]	[0.001]	[0.017]
Mentored by Refugee	0.008	0.030***	0.023	0.022	-0.051
	(0.031)	(0.011)	(0.016)	(0.016)	(0.036)
	[0.241]	[0.006]	[0.085]	[0.085]	[0.213]
Mentored by Ugandan	0.043	0.036***	0.010	0.049***	0.012
	(0.030)	(0.013)	(0.015)	(0.017)	(0.037)
	[0.085]	[0.005]	[0.199]	[0.004]	[0.591]
Observations	3,061	3,061	3,061	3,061	3,061
Control Mean (Baseline)	.	.	.	.	0.173
Control Mean (Follow-Ups)	0.316	0.004	0.036	0.024	0.369
p-val: Info = Labeled Grant	0.000	0.000	0.001	0.002	0.002
p-val: Grant = Labeled Grant	0.675	0.222	0.519	0.083	0.103
p-val: R-Mentee = U-Mentee	0.281	0.642	0.462	0.179	0.111

Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices and outcomes not pre-specified (denoted with $^{+}$).

Table A2: Perceived Fairness of Aid Distribution

	Int'l Aid Is Distributed Fairly^{+}	Refugees Get Too Much Aid^{+}	Refugees Get More Aid^{+}	Int'l Aid Orgs Care About Me^{+}	Int'l Aid Orgs Are Trustworthy^{+}
Labeled Grant	0.062	-0.035	0.018	0.100**	0.164***
	(0.054)	(0.049)	(0.051)	(0.051)	(0.061)
	[0.250]	[0.477]	[0.718]	[0.050]	[0.007]
Information Only	-0.024	-0.077	-0.089*	-0.053	-0.030
	(0.053)	(0.052)	(0.053)	(0.049)	(0.063)
	[0.645]	[0.139]	[0.093]	[0.285]	[0.631]
Grant Only	-0.019	-0.146***	-0.062	0.119**	0.223***
	(0.052)	(0.052)	(0.054)	(0.053)	(0.064)
	[0.723]	[0.005]	[0.254]	[0.025]	[0.000]
Mentored by Refugee	-0.019	-0.048	-0.095	0.010	0.146**
	(0.061)	(0.060)	(0.060)	(0.060)	(0.071)
	[0.762]	[0.425]	[0.117]	[0.874]	[0.041]
Mentored by Ugandan	-0.039	-0.014	-0.013	0.029	-0.013
	(0.058)	(0.057)	(0.059)	(0.060)	(0.071)
	[0.498]	[0.803]	[0.828]	[0.626]	[0.856]
Observations	780	821	821	871	653
Control Mean (Baseline)	.	.	.	.	.
Control Mean (Follow-Ups)	0.308	0.767	0.705	0.325	0.438
p-val: Info = Labeled Grant	0.104	0.416	0.038	0.002	0.002
p-val: Grant = Labeled Grant	0.136	0.033	0.122	0.719	0.341
p-val: R-Mentee = U-Mentee	0.752	0.602	0.205	0.765	0.042

Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices and outcomes not pre-specified (denoted with $^{+}$).

Table A3: Heterogeneity in Treatment Impacts on Policy Preferences

	Female Owner	Refugee Facilitator	Business Profit	Supports Hosting Index	Economic Beliefs Index	Social Attitudes Index	Contact Refugees (Choice)	Contact Refugees (Circumstance)	Knows About Aid-Sharing	Mentor Profit	Worried About Covid
Labeled Grant × X	0.059	0.026	-0.146	-0.337**	-0.335**	-0.281**	0.075	0.146	-0.097		-0.062
	(0.146)	(0.095)	(0.128)	(0.134)	(0.131)	(0.134)	(0.152)	(0.134)	(0.164)		(0.141)
	[0.684]	[0.786]	[0.257]	[0.012]	[0.011]	[0.036]	[0.621]	[0.277]	[0.556]		[0.662]
Labeled Grant	0.289**	0.317***	0.402***	0.523***	0.521***	0.491***	0.285**	0.251**	0.351***	0.335***	0.343***
	(0.123)	(0.096)	(0.092)	(0.112)	(0.106)	(0.106)	(0.133)	(0.104)	(0.072)	(0.066)	(0.109)
	[0.019]	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]	[0.032]	[0.016]	[0.000]	[0.000]	[0.002]
Information Only × X	0.254*	0.081	-0.217	-0.258*	-0.305**	-0.305**	0.082	0.078	-0.035		0.060
	(0.151)	(0.095)	(0.135)	(0.136)	(0.136)	(0.137)	(0.162)	(0.141)	(0.165)		(0.143)
	[0.093]	[0.395]	[0.107]	[0.059]	[0.025]	[0.027]	[0.611]	[0.580]	[0.830]		[0.677]
Information Only	0.021	0.141	0.290***	0.340***	0.366***	0.360***	0.133	0.148	0.200***	0.195***	0.136
	(0.131)	(0.093)	(0.093)	(0.111)	(0.106)	(0.109)	(0.143)	(0.109)	(0.076)	(0.068)	(0.112)
	[0.871]	[0.129]	[0.002]	[0.002]	[0.001]	[0.001]	[0.352]	[0.176]	[0.009]	[0.004]	[0.225]
Grant Only × X	0.048		-0.171	-0.272**	-0.367***	-0.292**	-0.208	-0.097	-0.142		-0.040
	(0.146)		(0.134)	(0.137)	(0.137)	(0.137)	(0.152)	(0.137)	(0.154)		(0.141)
	[0.744]		[0.200]	[0.047]	[0.007]	[0.034]	[0.172]	[0.477]	[0.356]		[0.775]
Grant Only	0.190	0.226***	0.300***	0.377***	0.427***	0.385***	0.380***	0.289***	0.252***	0.226***	0.217**
	(0.122)	(0.068)	(0.091)	(0.109)	(0.108)	(0.103)	(0.129)	(0.106)	(0.077)	(0.068)	(0.107)
	[0.119]	[0.001]	[0.001]	[0.001]	[0.000]	[0.000]	[0.003]	[0.006]	[0.001]	[0.001]	[0.042]
Mentored by Refugee × X	0.031		-0.243	-0.228	-0.318**	-0.201	-0.051	-0.003	-0.033	-0.021	0.120
	(0.166)		(0.149)	(0.150)	(0.151)	(0.149)	(0.163)	(0.155)	(0.179)	(0.106)	(0.156)
	[0.853]		[0.104]	[0.128]	[0.035]	[0.179]	[0.756]	[0.985]	[0.854]	[0.844]	[0.440]
Mentored by Refugee	0.078	0.104	0.211**	0.231*	0.281**	0.228**	0.149	0.105	0.106	0.115	0.034
	(0.139)	(0.074)	(0.101)	(0.118)	(0.119)	(0.114)	(0.138)	(0.123)	(0.083)	(0.088)	(0.119)
	[0.576]	[0.157]	[0.036]	[0.051]	[0.018]	[0.047]	[0.279]	[0.392]	[0.203]	[0.193]	[0.772]
Mentored by Ugandan × X	0.083		-0.399**	-0.170	-0.331**	-0.289*	-0.121	0.026	-0.069	0.005	-0.270*
	(0.164)		(0.157)	(0.154)	(0.154)	(0.152)	(0.173)	(0.157)	(0.172)	(0.110)	(0.149)
	[0.614]		[0.011]	[0.272]	[0.031]	[0.058]	[0.483]	[0.870]	[0.688]	[0.961]	[0.070]
Mentored by Ugandan	0.011	0.072	0.240**	0.166	0.255**	0.229*	0.165	0.057	0.083	0.069	0.210*
	(0.135)	(0.077)	(0.097)	(0.124)	(0.120)	(0.124)	(0.147)	(0.125)	(0.087)	(0.095)	(0.109)
	[0.934]	[0.352]	[0.013]	[0.181]	[0.034]	[0.064]	[0.264]	[0.652]	[0.342]	[0.468]	[0.053]
X	-0.203		0.268**	0.244*	0.345***	0.343***	0.158	0.024	0.124		0.048
	(0.167)		(0.122)	(0.126)	(0.106)	(0.106)	(0.118)	(0.105)	(0.123)		(0.109)
	[0.225]		[0.028]	[0.053]	[0.001]	[0.001]	[0.182]	[0.823]	[0.313]		[0.662]
Observations	3,051	3,051	3,051	3,051	3,051	3,051	3,051	3,051	3,051	3,051	2,851

The dependent variable for each column is the policy preferences summary index. Each column title lists the dimension of heterogeneity (X) that is analyzed in the regression. Results estimated through ANCOVA regression with controls for randomization-stratum and survey-wave fixed effects, survey date, an indicator for phone survey, baseline education, and age at baseline. 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: Heterogeneity in Treatment Impacts on Policy Preferences (Public Good Usage)

	Uses Hospitals	Children Go to School With Foreigners	Uses Hospitals Or Schools
Labeled Grant × X	0.158	-0.019	0.092
	(0.117)	(0.117)	(0.132)
	[0.176]	[0.871]	[0.487]
Labeled Grant	0.173**	0.280***	0.199*
	(0.087)	(0.076)	(0.115)
	[0.047]	[0.000]	[0.082]
Information Only × X	0.079	-0.023	0.014
	(0.122)	(0.124)	(0.135)
	[0.517]	[0.852]	[0.920]
Information Only	0.101	0.158**	0.135
	(0.089)	(0.078)	(0.114)
	[0.255]	[0.044]	[0.236]
Grant Only × X	-0.014	-0.128	-0.079
	(0.120)	(0.116)	(0.136)
	[0.908]	[0.272]	[0.561]
Grant Only	0.193**	0.232***	0.240**
	(0.088)	(0.077)	(0.117)
	[0.027]	[0.003]	[0.040]
Mentored by Refugee × X	0.056	-0.012	0.060
	(0.140)	(0.139)	(0.151)
	[0.687]	[0.934]	[0.692]
Mentored by Refugee	0.003	0.044	-0.001
	(0.105)	(0.089)	(0.127)
	[0.978]	[0.619]	[0.993]
Mentored by Ugandan × X	0.091	-0.163	-0.112
	(0.139)	(0.143)	(0.148)
	[0.511]	[0.255]	[0.451]
Mentored by Ugandan	-0.083	0.033	0.052
	(0.103)	(0.088)	(0.125)
	[0.423]	[0.709]	[0.679]
X	-0.040	0.107	0.020
	(0.091)	(0.091)	(0.103)
	[0.661]	[0.238]	[0.847]
Observations	2,499	2,503	2,503

The dependent variable for each column is the policy preferences 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. Results estimated through ANCOVA regression with controls for randomization-stratum and survey-wave fixed effects, survey date, an indicator for phone survey, baseline education, and age at baseline. 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 A5: Heterogeneity in Treatment Impacts on Business Profit

	Female Owner	Business Practices Index	Business Network Size	Mentor Profit	Mentor Experience	Distance to Mentor
Labeled Grant × X	-0.155	-0.072	-0.135			
	(0.132)	(0.122)	(0.120)			
	[0.242]	[0.557]	[0.260]			
Labeled Grant	0.040	-0.032	0.008	-0.063	-0.063	-0.063
	(0.112)	(0.080)	(0.088)	(0.061)	(0.061)	(0.061)
	[0.718]	[0.687]	[0.928]	[0.299]	[0.300]	[0.299]
Information Only × X	-0.165	0.015	0.002			
	(0.137)	(0.131)	(0.129)			
	[0.229]	[0.907]	[0.986]			
Information Only	0.081	-0.040	-0.034	-0.035	-0.034	-0.035
	(0.113)	(0.087)	(0.098)	(0.064)	(0.064)	(0.064)
	[0.477]	[0.647]	[0.727]	[0.586]	[0.591]	[0.585]
Grant Only × X	-0.159	-0.002	-0.123			
	(0.143)	(0.131)	(0.129)			
	[0.265]	[0.990]	[0.344]			
Grant Only	0.081	-0.032	0.042	-0.033	-0.033	-0.033
	(0.121)	(0.086)	(0.098)	(0.065)	(0.065)	(0.065)
	[0.505]	[0.712]	[0.672]	[0.613]	[0.612]	[0.612]
Mentored by Refugee × X	-0.021	-0.056	-0.235*	0.045	-0.019	0.040
	(0.154)	(0.142)	(0.137)	(0.103)	(0.106)	(0.111)
	[0.893]	[0.694]	[0.085]	[0.658]	[0.859]	[0.720]
Mentored by Refugee	0.048	0.050	0.160	0.003	0.037	0.002
	(0.132)	(0.087)	(0.098)	(0.083)	(0.093)	(0.106)
	[0.714]	[0.565]	[0.101]	[0.970]	[0.688]	[0.988]
Mentored by Ugandan × X	-0.278*	0.152	-0.091	0.023	0.071	0.009
	(0.158)	(0.148)	(0.146)	(0.112)	(0.118)	(0.118)
	[0.078]	[0.302]	[0.534]	[0.841]	[0.546]	[0.942]
Mentored by Ugandan	0.089	-0.159*	-0.044	-0.109	-0.132	-0.101
	(0.130)	(0.092)	(0.110)	(0.091)	(0.094)	(0.084)
	[0.494]	[0.086]	[0.687]	[0.231]	[0.158]	[0.233]
X	-0.902***	0.050	0.057			
	(0.156)	(0.094)	(0.092)			
	[0.000]	[0.590]	[0.533]			
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. Results estimated through ANCOVA regression with controls for randomization-stratum and survey-wave fixed effects, survey date, an indicator for phone survey, baseline education, and age at baseline. 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: Impact of Child Labor Information Campaign

	Child Labor Attitudes Index^{+}	No Child Labor Under 15^{+}	No Child Labor Under 17^{+}
Grant Only	-0.071	0.009	-0.056
	(0.095)	(0.047)	(0.049)
	[0.455]	[0.853]	[0.256]
Information Only	0.011	-0.047	0.044
	(0.094)	(0.047)	(0.050)
	[0.910]	[0.322]	[0.376]
Observations	732	731	731
Control Mean	0.000	0.646	0.514
p-val: Grant = Info	0.487	0.343	0.103

Results estimated through OLS regression with baseline controls chosen through double-lasso. Robust standard errors in parentheses; two-sided p-values in brackets. Labeled Grant, Mentored by Refugee, and Mentored by Ugandan groups are pooled with the control. Outcomes that are not pre-specified are denoted with $^{+}$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Within-Survey Priming Experiment

	Primed Outcomes Index	Have Money	Receive More Aid Than Needed	Can Support Themselves	Deserve Sympathy	Have Skills
Primed on Aid Received^{+}	-0.002	0.019	-0.026	0.006	0.018	0.009
	(0.061)	(0.033)	(0.034)	(0.032)	(0.031)	(0.033)
	[0.971]	[0.567]	[0.445]	[0.839]	[0.560]	[0.797]
Observations	1,004	884	857	917	953	890
Control Mean	-0.016	0.549	0.516	0.375	0.559	0.464

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 A8: Full Set of Phone Campaign Outcomes

	Answered Call^{+}	Supported Phone Campaign^{+}	Opposed Phone Campaign^{+}
Labeled Grant	-0.003	0.100***	-0.015
	(0.035)	(0.038)	(0.020)
	[0.937]	[0.008]	[0.446]
Information Only	0.001	0.024	0.025
	(0.034)	(0.036)	(0.021)
	[0.969]	[0.513]	[0.245]
Grant Only	0.031	0.043	0.018
	(0.035)	(0.039)	(0.022)
	[0.384]	[0.265]	[0.425]
Mentored by Refugee	0.024	-0.022	0.010
	(0.039)	(0.042)	(0.022)
	[0.534]	[0.603]	[0.637]
Mentored by Ugandan	0.026	-0.033	0.037
	(0.039)	(0.043)	(0.026)
	[0.505]	[0.437]	[0.159]
Observations	1,406	1,406	1,406
Control Mean (Follow-Ups)	0.804	0.230	0.060
p-val: Info = Labeled Grant	0.905	0.043	0.054
p-val: Grant = Labeled Grant	0.346	0.164	0.138
p-val: R-Mentee = U-Mentee	0.968	0.809	0.338

Results estimated through OLS regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Brackets display sharpened q-values controlling the false discovery rate for individual pre-specified outcomes, and two-sided p-values for summary indices and outcomes not pre-specified (denoted with $^{+}$).

Westfall-Young Stepdown-Adjusted P-Values

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

- Labeled grants will increase support for inclusive hosting.
- Refugee mentorship will increase support for inclusive hosting.
- Labeled grants will increase business profits.
- Refugee mentorship will increase business profits.

Domain 1 contains information on support for inclusive hosting, 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 A9: Westfall-Young Stepdown-Adjusted P-Values for Primary Hypotheses

	Policy Preferences Index	Business Profits
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. WY p-values in brackets.

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

During the listing survey in October of 2019, we surveyed all tailors and hair salons within 10 kilometers of the Kampala city center.[36] We surveyed either the owner of the business or a manger who retains most of the profits since, as the residual claimant on profits, their attitudes are the most relevant for our theory of change.[37]

For the baseline survey in November 2019 through January 2020, we selected a subset of the business 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

[36] 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.

[37] A few businesses pay the owner a flat fee to operate, and then retain the residual earnings. The managers of these firms in the sample and interventions are included because they are the residual claimant on profits. They are included in references to "owners" throughout the paper.)

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 Table B2).

We chose to 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.[38] We hypothesized 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.

B.2 Tests of Balance and Selective Attrition

Tables B1, B2, B3, B4, and B5 respectively present tests of randomization balance within the experimental sample, mentor characteristic balance across refugees and Ugandans, a test of differential attrition within the experimental sample, and Lee Bounds on treatment impacts for each pre-specified domain (across two tables).

[38] 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.

Table B1: Randomization Balance

	(1) Labeled Grant	(2) Grant Only	(3) Information Only	(4) Mentored by Refugee	(5) Mentored by Ugandan	(6) Control	(7) Joint P-Value
Age (Years)	27.22	28.02	27.37	27.43	27.37	27.34	0.49
Education (Years)	10.89	10.51	10.72	10.57	10.92	10.73	0.41
Experience in Sector (Years)	2.49	2.45	2.47	2.28	2.32	2.21	0.27
Profit (USD/Month)	37.40	36.29	35.32	38.28	36.72	38.21	0.46
Has Any Employees	0.22	0.22	0.25	0.20	0.17	0.25	0.65
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	0.51
Refugees Increase Rents	0.78	0.79	0.75	0.78	0.79	0.80	0.84
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	0.47
Refugees Economic Effect is Positive	0.52	0.54	0.58	0.54	0.50	0.51	0.49
Policy Preferences Index	0.02	0.02	-0.02	-0.08	0.05	0.00	0.55
Knowledge Index	0.20	0.11	0.04	0.16	0.05	0.00	0.14
Economic Beliefs Index	-0.05	-0.09	0.00	0.01	-0.02	0.00	0.82
Economic Perceptions Index	-0.07	0.01	0.00	0.09	0.16	0.00	0.40
Economic Perceptions Index	0.08	0.02	0.14	0.26	0.04	0.00	0.11
Social Attitudes Index	0.01	0.14	0.00	-0.07	0.06	0.00	0.24
Contact Refugees by Choice Index	-0.02	0.01	0.00	0.02	0.12	0.00	0.97
Contact Refugees by Circumst. Index	-0.13	0.09	0.04	0.02	0.04	-0.00	0.13
Business Practices Index	-0.04	-0.05	0.06	-0.07	-0.07	-0.00	0.86
Household Well-Being Index	-0.01	-0.06	-0.07	-0.08	-0.04	-0.00	0.90
General Policy Index	0.19	0.07	0.16	0.13	-0.02	-0.00	0.16
Foreigners: Economic Beliefs Index	0.03	0.08	0.10	0.10	-0.03	0.00	0.74
Foreigners: Social Attitudes Index	-0.03	0.05	0.16	-0.07	0.14	-0.00	0.11
Other Tribes: Contact Index	-0.08	0.01	0.09	-0.01	-0.09	0.00	0.49
Other Tribes: Economic Beliefs Index	0.02	-0.10	0.01	0.00	0.15	0.00	0.35
Other Tribes: Social Attitudes Index	0.02	0.15	0.03	-0.04	-0.02	-0.00	0.26
Gender Role Index	0.01	0.21	-0.07	0.15	0.10	0.00	0.11

Each column shows a baseline variable mean within a given treatment group assignment. P-values testing joint orthogonality recovered from a regression of each variable on the full set of treatment dummies controlling for randomization stratum fixed effects.

Table B2: 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.

Table B3: Test for Differential Attrition

	Surveyed
Labeled Grant	0.044
	(0.028)
	[0.118]
Information Only	0.007
	(0.029)
	[0.805]
Grant Only	0.084***
	(0.029)
	[0.003]
Mentored by Refugee	0.028
	(0.033)
	[0.394]
Mentored by Ugandan	0.056*
	(0.031)
	[0.074]
Observations	5,624
Midline Mean	0.796
In-Person Endline 1 Mean	0.740
Phone Endline Mean	0.762
In-Person Endline 2 Mean	0.641
Joint Orthogonality P-Value	0.040

Results estimated through ANCOVA regression controlling for randomization-stratum and survey-wave fixed effects. 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 B4: Lee Bounds on Treatment Impacts, Domains 1–6.2

	Domain 1	Domain 2	Domain 3	Domain 4	Domain 41	Domain 42	Domain 51	Domain 52	Domain 6	Domain 61	Domain 62
Labeled Grant											
lower	[0.17,0.41]	[-0.27,-0.03]	[0.03,0.34]	[0.02,0.33]	[-0.22,0.23]	[-0.40,0.05]	[-0.38,0.07]	[-0.50,-0.04]	[-0.10,0.19]	[-0.11,0.35]	[-0.17,0.30]
upper	[0.36,0.65]	[-0.05,0.25]	[0.27,0.58]	[0.27,0.60]	[0.18,0.66]	[0.01,0.47]	[0.05,0.50]	[-0.15,0.23]	[0.15,0.46]	[0.31,0.71]	[0.23,0.65]
Observations	1,772	2,139	1,774	1,746	1,228	1,223	1,309	1,283	1,774	1,240	1,240
Information Only											
lower	[0.07,0.33]	[-0.19,0.07]	[-0.08,0.25]	[0.01,0.34]	[-0.29,0.42]	[-0.34,0.41]	[-0.27,0.20]	[-0.24,0.22]	[-0.13,0.20]	[-0.27,0.48]	[-0.24,0.41]
upper	[0.05,0.43]	[-0.18,0.19]	[-0.04,0.28]	[0.09,0.43]	[-0.15,0.36]	[-0.26,0.35]	[-0.22,0.31]	[-0.15,0.23]	[-0.08,0.28]	[-0.20,0.41]	[-0.19,0.39]
Observations	1,804	2,162	1,804	1,780	1,250	1,244	1,328	1,307	1,804	1,264	1,263
Grant Only											
lower	[-0.03,0.23]	[-0.30,-0.05]	[-0.21,0.10]	[-0.17,0.14]	[-0.48,-0.06]	[-0.46,-0.02]	[-0.56,-0.10]	[-0.72,-0.24]	[-0.21,0.06]	[-0.25,0.14]	[-0.44,-0.01]
upper	[0.34,0.60]	[0.09,0.33]	[0.22,0.51]	[0.28,0.60]	[0.18,0.65]	[0.23,0.68]	[0.11,0.57]	[-0.16,0.23]	[0.21,0.49]	[0.37,0.74]	[0.23,0.64]
Observations	1,620	1,965	1,623	1,596	1,116	1,112	1,178	1,157	1,623	1,127	1,127
Mentored by Refugee											
lower	[-0.11,0.19]	[-0.19,0.08]	[-0.35,0.01]	[-0.22,0.14]	[-0.45,0.09]	[-0.46,0.07]	[-0.50,0.03]	[-0.40,0.12]	[-0.30,0.03]	[-0.43,0.10]	[-0.37,0.12]
upper	[0.08,0.43]	[0.01,0.33]	[-0.12,0.22]	[0.02,0.39]	[-0.14,0.38]	[-0.13,0.41]	[-0.15,0.36]	[-0.14,0.31]	[-0.07,0.28]	[-0.08,0.46]	[-0.05,0.46]
Observations	1,411	1,694	1,414	1,387	975	970	1,032	1,013	1,414	986	987
Mentored by Ugandan											
lower	[-0.17,0.12]	[-0.38,-0.10]	[-0.26,0.09]	[-0.29,0.06]	[-0.47,-0.05]	[-0.53,-0.07]	[-0.62,-0.10]	[-0.55,-0.04]	[-0.28,0.02]	[-0.36,0.07]	[-0.40,0.02]
upper	[0.20,0.50]	[-0.01,0.31]	[0.13,0.45]	[0.11,0.45]	[0.06,0.55]	[0.07,0.54]	[0.00,0.51]	[-0.06,0.39]	[0.09,0.39]	[0.16,0.57]	[0.14,0.55]
Observations	1,408	1,697	1,410	1,382	973	972	1,029	1,009	1,410	982	982

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. 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 B5: Lee Bounds on Treatment Impacts, Domains 7–17.1

	Domain 7	Domain 8	Domain 9	Domain 10	Domain 11	Domain 12	Domain 13	Domain 14	Domain 15	Domain 16	Domain 171
Labeled Grant											
lower	[-0.33,0.05]	[-0.38,0.05]	[-0.30,0.07]	[-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	[0.04,0.35]	[-0.01,0.39]	[0.06,0.45]	[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	1,357	1,355	1,357	2,180	2,038	1,226	1,171	1,290	1,215	1,240	844
Information Only											
lower	[-0.63,0.58]	[-0.14,0.32]	[-0.26,0.23]	[-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	[-0.72,0.71]	[-0.25,0.48]	[-0.42,0.40]	[-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	1,378	1,374	1,378	2,208	2,073	1,246	1,180	1,309	1,242	1,264	913
Grant Only											
lower	[-0.42,-0.10]	[-0.48,-0.16]	[-0.26,0.07]	[-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	[0.07,0.38]	[0.03,0.41]	[0.23,0.54]	[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	1,229	1,228	1,229	2,008	1,885	1,112	1,059	1,163	1,106	1,127	786
Mentored by Refugee											
lower	[-0.37,0.04]	[-0.36,0.19]	[-0.28,0.16]	[-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	[-0.07,0.28]	[0.02,0.48]	[0.08,0.47]	[-0.01,0.30]	[-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	1,082	1,081	1,082	1,736	1,618	970	929	1,024	966	987	705
Mentored by Ugandan											
lower	[-0.35,-0.03]	[-0.39,0.00]	[-0.28,0.11]	[-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	[0.01,0.39]	[0.07,0.50]	[0.19,0.54]	[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	1,068	1,067	1,068	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. 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.3 Treatment Roll-Out

The interventions were launched in late January of 2020 and suspended 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 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 percent of the mentorship pairs met at least once, with 23% of those having met all six times. Table B6 presents tabulations of actual treatment status (defined as receiving the grant in Grant Only and Labeled Grant, receiving the information in Information Only, and having at least one mentorship meeting in Refugee and Ugandan Mentorship). Table B7 shows the number of mentorship meetings held by year across Refugee and Ugandan Mentorship arms.

Table B6: Assignment and Actual Treatment Status

	Grant + Canvassing	Grant	Canvassing	Refugee Mentorship	Ugandan Mentorship	Control
Assigned	280	237	287	169	168	265
Treated	230	184	257	133	135	NA

Table B7: Number of Mentorship Meetings by Year

	Number of Meetings							
	0	1	2	3	4	5	6	Total
2020	95	29	129	28	2	6	48	337
2021	107	9	7	27	187	NA	NA	337

B.4 Intervention Delivery Scripts

Information Only Treatment

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?

No, Don't Know Someone	**Yes, They Know Someone**
-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_____ 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 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.

Grant & Information Treatment

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.

Empathetic Listening (Based on Kalla-Broockman Model):

Step 1: Uncover Honest Opinion
What do you think of refugees in Kampala? 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?

No, Don't Know Someone	**Yes, They Know Someone**
-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_____ 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.

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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.

Grant Only Treatment

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.5 Phone Campaign Script (OneYouth OneHeart Initiative)

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.6 Child Labor Campaign Script (YARID)

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.

For Grant Only group and Information Only group

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.