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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 study a Ugandan policy that redistributes 30 percent of foreign aid for refugees to Ugandans while allowing refugees to work and move freely. To test whether compensation influences support for refugee integration, we randomly distribute cash grants to natives that are explicitly labeled as aid shared from the refugee response. We find substantial impacts on policy preferences that persist for at least two years and work through changing beliefs about the economic effects of refugees on Ugandans. Sharing information about public goods funded by the refugee response but not providing a grant has smaller, though still significant, effects. In contrast, we find initially positive impacts of intergroup contact—implemented as business mentorship by an experienced refugee—but these impacts do not persist. Our results indicate that economic interventions can shape policy views even on issues greatly influenced by cultural concerns, such as immigration.

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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: voters may form their policy views based largely on non-economic considerations such as group identity, the costs of a policy may be more salient or visible than the benefits, 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 40 million refugees and asylum seekers were residing outside their country of origin (UNHCR, 2023*a*). 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 in the long run.[3]

[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; the H-1B Skills Training Grants, which use visa fees to fund training programs for US citizens; 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.

[3] Sixty-seven percent of refugees live in protracted situations of at least five years (UNHCR, 2023*a*), while 71% of the 24.2 billion USD spent on Official Development Assistance for refugee situations in 2018–19 went to programs designed for a short-term humanitarian response (OECD, 2021). Marbach, Hainmueller and Hangartner (2018) find 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 limit the

Citizens in countries that host refugees 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 natives.[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, 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 two programs to investigate whether redistributing aid can increase policy support among voters. We offered these programs to native microentrepreneurs in the capital city of Uganda, a country that hosts over one million refugees. Ugandan policy stipulates that 30% of international refugee aid be shared with Ugandan host communities (we refer to this as Uganda's *aid-sharing policy*), but we show that awareness of this policy is low at baseline. Our experiment randomly varied both the direct receipt of assistance and information linking assistance to the refugee presence, allowing us to compare the roles of knowledge about the policy bargain and of direct receipt of aid in driving policy views. Microentrepreneurship is a common source of livelihood for both Ugandans and refugees, and these groups may come into direct competition.

The first program delivered information about Uganda's aid-sharing policy and its connection to policies that facilitate refugees' integration. A staff member—either a refugee or a Ugandan—explained that part of foreign aid for refugees is shared with Ugandans, gave examples of public goods like schools and hospitals funded by international refugee aid, and conducted a listening exercise modeled after Kalla and Broockman (2020), inviting the respondent to share their views toward refugees. We refer to this as the Information Only arm. The second program augmented the information delivery with a business grant of USD 135—representing about 3.5 months of profit on average—and explained that the grant is an example of compensation for Ugandans under the national aid-sharing policy. We refer

potential medium-term effects of aid: 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]See Ash and Huang (2018) for a discussion of the *compact model*, where host-country governments and donors agree on levels of aid and hosting policies jointly, and Tsourapas (2019) for a discussion of how conditional offers of assistance from international donors shape policy for countries hosting Syrians.

to this as the Labeled Grant arm. Both treatments were designed to explicitly link the two components of the policy bargain: integration policies and aid-sharing.

There is substantial evidence, however, that attitudes about immigration are primarily driven by cultural—instead of economic—opposition (Hainmueller and Hopkins, 2014, Alesina and Tabellini, 2022).[6] We therefore designed a third program facilitating contact between Ugandans and refugees, allowing us to compare our two economically motivated interventions with one motivated by cultural concerns. The contact hypothesis is frequently studied—and provides the rationale for many programs aiming to improve intergroup relations—though few studies compare contact with other interventions (Paluck, Green and Green, 2019). Our contact-based intervention matched each microentrepreneur with a more experienced refugee business owner in a one-on-one mentorship program modeled on Brooks, Donovan and Johnson (2018) in neighboring Kenya which found positive impacts on business profits. Meetings were facilitated by a staff member in part to overcome any language barriers.

We implemented our experiment within the tailoring and hairdressing sectors, two common occupations for both Ugandans and refugees. Refugee owners are widely perceived as successful in these sectors and thus may be attractive as potential mentors but also in direct competition with natives. We test whether these programs affect Ugandans' support for refugee hosting and integration policies, beliefs about the economic impacts of refugee hosting, cultural attitudes toward refugees, and economic outcomes in the firm and household.

Our experiment included three additional comparison arms to isolate potential channels. First, we offered a business grant that was not bundled with information on aid-sharing or integration policies to isolate any impacts of the aid itself. Second, we provided mentorship by an experienced Ugandan—balancing refugee and Ugandan mentors across several dimensions to increase comparability—to isolate the impacts of contact with a refugee mentor from other aspects of the mentorship program. Finally, we included a pure control group which did not receive any treatment.

We find that the labeled grant substantially increased Ugandans' support for admitting refugees and for policies that facilitate integration like the right to work and freedom of movement, compared to the control group. These effects persist for at least two years beyond the start of our interventions. Receiving information about Uganda's aid-sharing policy, but no grant, created similar but smaller impacts. Receiving an unlabeled business

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

grant also increased support for integration policies, but by less than a labeled grant.

Do the impacts on self-reported views translate into changes in actual political behavior? An ideal real-world outcome would be voting choices in a referendum on admitting refugees, providing the right to work, or freedom of movement. While measuring such an outcome was not possible in our setting, we designed a proxy for voting behavior by implementing a phone-call campaign that asked each respondent 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 non-profit and the data collection firm to reduce the potential influence of experimenter demand effects 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 average impacts of mentorship, either by a refugee or a Ugandan, on attitudes or business outcomes, despite high uptake of both programs.[7] Impacts of mentorship by a refugee on policy views were significant but small on average after nine months and did not persist. To examine the role of contact intensity, we estimate program impacts within a subset of business owners who were randomly assigned to an earlier launch of their program. Because of COVID-19 interruptions, these business owners had significantly more meetings with their mentors. In this smaller group, we find large initial impacts of refugee mentorship on policy preferences which do not persist in later survey rounds. These findings suggest that short-term cooperative intergroup contact has less persistent impacts on policy views than direct aid programs explicitly connected to the refugee presence.

To understand the mechanisms driving the impacts of labeled grants on policy views, we compare the effects of labeled grants to unlabeled grants and to information alone. Our results suggest that receiving the grant *per se*—even without information about aid-sharing—impacted views through an association between the grant and the refugee-led implementing organization, and by reducing resentment against other groups—such as refugees—perceived to be major beneficiaries of aid. Knowledge of aid-sharing also contributed to the overall impact, as learning about aid-sharing without an associated grant impacted policy views. However, we find that neither the grant nor the information alone completely substitutes

[7]Seventy-two percent of mentees met their mentors at least once in person.

for the labeled grant, which had the greatest impact on views among our treatment arms. This does not appear to be due solely to the private benefit conferred by the grant. Rather, our evidence suggests that the labeled grant amplified the impact of the information by serving as a visible demonstration of aid-sharing, which made the information more salient and credible. We conclude that redistribution is most likely to affect policy views when beneficiaries can clearly see the gains and know to attribute them to the policy.

Cultural attitudes in our setting are a much stronger predictor of policy preferences than economic beliefs, consistent with evidence from other immigration studies across multiple settings (Hainmueller and Hopkins, 2014, Tabellini, 2020).[8] Nevertheless, we find that our economic interventions—grants and information—have larger impacts on policy views, and that these impacts are strongest among Ugandans with *either* economic or cultural concerns about refugees at baseline. Labeled grant recipients were also more likely to report that refugees have a positive economic impact on Uganda and on them personally and to express more positive cultural views toward refugees. Changes in cultural views lag other impacts, which we argue is consistent with cultural attitudes changing as a rationalization of new economic and policy views. Our results are consistent with Jha (2012) and Jha and Shayo (2019), which show that financial innovations—in our context, aid-sharing—can support new political economy equilibria and reduce intergroup conflict by aligning competing groups' incentives.[9] Our findings indicate that economic policy can influence views about immigration regardless of whether opposition is rooted in economic or cultural concerns.

We can reject several potential alternative explanations for our findings. To test whether experimenter demand effects are driving our results, we include a placebo campaign that shared the implementing organization's position on child labor but no additional information on the issue, an incentivized dictator game over donations to an organization supporting refugees, and a survey experiment priming respondents about the aid they received. In no case do we observe evidence of significant experimenter demand effects. The placebo campaign also allows us to rule out effects driven by intrinsic reciprocity to the implementing organization (Finan and Schechter, 2012). We also do not find that our results are driven by contact with refugees outside the programs or as program facilitators, or wealth effects.

Overall, our findings indicate that redistributing potential surplus can be an effective tool to build political support for policies that create perceived winners and losers, especially

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

[9]Our interpretation is also related to that of Jha (2013), which shows that economic complementarities—which our interventions may have made Ugandans more aware of—can improve intergroup relations.

when the connection between the policies and the transfers is clear.[10] Policies that reduce barriers to trade or immigration, for example, 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). In the context of refugee immigration, countries that restrict refugees' labor market access due to concerns about crowd-out can consider combining integration policies with aid redistribution,[11] and countries that already share foreign aid with citizens could increase support for refugee integration by making existing policies more widely known.

Related Literature. 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.[12] 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, Ginn and Malpassi (2022) find little evidence of a backlash to refugee arrivals on average in low- and middle-income countries, even 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 that displaced US workers increased political polarization (Autor et al., 2020), and even exposure to stories about labor-market shocks increases preferences for trade restrictions (Di Tella and Rodrik, 2020). Informing US citizens in a survey experiment about existing redistribution programs for workers displaced by trade increases support for trade (Ehrlich and Hearn, 2014).[13] However, to our knowledge, no study has experimentally tested whether compensation—by redistributing gains—can

[10]Combining a new policy with redistribution to increase support frequently arises in public policy discussions: for examples in immigration, see Freeman (2006), Edelberg and Watson (2022), Lokshin and Ravallion (2022). However, there is little rigorous evidence for whether doing so influences support in practice.

[11]In high-income countries that do not receive foreign assistance but 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.

[12]Ruggie (1982) argues that after 1945, states built political support for openness to international markets by expanding social welfare in the "compromise of embedded liberalism."

[13]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. Gaikwad, Genovese and Tingley (2022) document preferences in the United States and India for compensating those harmed by policies to combat climate change.

affect policy views on immigration, an area where non-economic concerns appear to play a significant role in determining attitudes. Our paper does so in the context of refugee hosting policies, which affect millions of people and remain contentious 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, 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 cultural 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. Our experimental design also uniquely, to our knowledge, allows us to compare the impacts of an intervention acting on economic motives—aid-sharing—with a contact-based intervention thought to act on cultural concerns.

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, 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 of the host community increased pro-social attitudes and behaviors of refugees but did not lead to measurable effects on host attitudes. 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. A potential explanation of these findings, in light of our results, is that the connection between the transfers and the refugee presence was not clear to hosts. Our study builds on this literature by labeling transfers to the host community as redistribution: that is, as aid-sharing with the host community out of funding from the refugee response.[14]

[14] Our paper also relates to literature on politicians' claiming and receiving credit for development projects,

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). 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 positive perceptions toward refugees. 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 interventions. More specifically, our finding that early impacts of intergroup contact do not persist highlights the importance of longer-run follow-ups.

Finally, we contribute to the 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 Kenya increased profits of inexperienced business owners more than a formal skills training program. Cai and Szeidl (2018) and Fafchamps and Quinn (2018) similarly find positive effects on businesses from experimentally expanding the business owners' networks. We find substantial interest in our setting in mentorship programs that promote skill transfer across nationalities, but no measurable impacts of these programs on business outcomes.

2 Overview of Refugee Policies and Attitudes

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

for example, Guiteras and Mobarak (2016), Blattman, Emeriau and Fiala (2018), Evans, Holtemeyer and Kosec (2019), and Lyall, Zhou and Imai (2020).

2.1 Refugee Policies

With over 1.5 million refugees, Uganda hosts the largest population of refugees in Africa, and the sixth largest globally (UNHCR, 2023*b*). 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.[15] 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 industries in our sample.

Aid-Sharing Policy. 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, 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.[16] However, there are far fewer aid organizations in Kampala than in the settlement areas, and Ugandans in Kampala see little evidence of aid-sharing. This makes it possible to study the impact of aid-sharing on policy preferences in a context where a national aid-sharing rule exists but awareness of it is low.

[15] The official 141,000 count represents 9% of Uganda's refugee population, and 8% of the Kampala population (UNHCR, 2023*b*).

[16] 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).

2.2 Natives' Attitudes

Ugandans' views 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. Uganda ranks 72nd out of 139 countries—close to the median—on Gallup's 2016 Migrant Acceptance Index (Esipova, Fleming and Ray, 2017).[17] As we discuss in Section 4, 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.

3 Experimental Design

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

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. We chose Ugandan microentrepreneurs who were no older than 40, had no more than five years of experience in their sector, and who spoke Luganda, English, or Swahili conversationally for inclusion in the experimental sample. We excluded businesses with five or more employees or very high profits or capital. This produced a set of 1,406 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 from refugee integration salient for this population, while also making cross-nationality mentorship feasible. Both sectors require skills that can be taught and developed by a mentor without requiring significant new capital investment. Congolese styles in both sectors are popular among Ugandan consumers, suggesting potential benefits to Ugandan producers from collaboration with refugees. Finally, both sectors require a stable place of business, which facilitates follow-up survey activities.

[17] Across Uganda, 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).

3.2 Data Collection Timeline

We conducted a microenterprise census in October 2019 and collected basic data on 3,414 owner-operators. We conducted a baseline survey from November–December 2019 with the experimental sample of 1,406 Ugandan 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 paused operations in mid-March 2020, with the interventions only partially complete, due to the COVID-19 pandemic. We conducted a midline survey over the phone in October 2020. We resumed and completed (modified) intervention delivery between March and May 2021. We conducted three additional follow-up surveys after interventions were completed: a phone survey in August 2021 and two in-person surveys in May 2021 and March 2022.

Across our four follow-up surveys, we successfully surveyed 91% of the sample at least once. An indicator for being surveyed in at least one follow-up round is not significantly different across treatment groups, as shown in Appendix Table B4 (joint p-value = 0.46). Our round-by-round retention rates are 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. In an ANCOVA regression, retention rates were 8 percentage points (pp.) higher in Grant Only (p-val < 0.01) and 6 pp. higher in Ugandan Mentorship (p-val $= 0.07$) compared to Control, but rates in Labeled Grant, Information Only, and Refugee Mentorship are similar to that in Control, as shown in Appendix Table B5. We reproduce all of our main results weighting observations by the inverse probability of retention, which is estimated by lasso logistic regression. Results in Appendix Tables B6, B7, B8, and B9 show that our main results hold after adjusting for respondents' propensity to attrit. We also present Lee Bounds for each of our pre-specified outcome domains (see Section 3.5.2 for details) in Tables B10 and B11.

3.3 Interventions

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

Information Only. The first intervention provided information about Uganda's existing aid-sharing policy, which stipulates that 30% of foreign aid to refugees be shared with the host community through direct transfers or public good provision such as hospitals and schools that Ugandans can access. Participants were visited by a refugee or Ugandan staff member. The script outlined the policy bargain, linking aid-sharing—and the potential benefits to the respondent—with policies that allow refugees to integrate, as the following excerpt shows:

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

Because awareness of the aid-sharing policy 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 staff member 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. The full scripts are available in Appendix Section **??**.

Labeled Grant. The second intervention provided 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. Staff explained that the grant was an example of aid-sharing: we therefore refer to this treatment as the Labeled Grant arm. During the first meeting, a YARID staff member visited the business owner to inform them about the grant and deliver the information. During the second meeting, the staff member disbursed the grant. In the first wave of disbursements before COVID-19, we required that at least 60% of the grant be used for businesses[18] and arranged 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.

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

Mentorship by a Refugee. The third intervention was a mentorship program that matched business owners with experienced refugee business owners in the same sector.[19] Mentees and mentors were paired within gender-sector cells to minimize within-pair travel distance using a greedy matching algorithm. The program included up to six in-person meetings between the mentor and mentee, roughly once per week, each facilitated by a YARID staff member who provided guidance and translation if necessary. This design is motivated by the contact hypothesis, in which cooperative relationships are theorized to reduce prejudice between majority and minority group members, and by the results of a similar mentorship program which demonstrated large impacts on profits (Brooks, Donovan and Johnson, 2018).[20]

Comparison Arms. In addition to our three main interventions, we included two additional treatment arms and a control group to distinguish mechanisms behind treatment impacts. The first provided a business grant identical to the labeled grant, but delivered by a Ugandan staff member without any information about refugees or Uganda's aid-sharing policy, which we refer to as the Grant Only arm or the *unlabeled grant*. This arm allows us to isolate impacts of labeling the grant as aid-sharing from impacts generated by the receipt of aid in itself. The second was a mentorship program that matched business owners with an experienced Ugandan business owner in their sector. Mentors were chosen to balance characteristics across Ugandan and refugee mentors (see Appendix Table B3). This treatment arm allows us to isolate the impact of cooperative contact with refugees from other impacts of the mentorship program.[21] 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, a pure control group did not receive any treatment and was not contacted by YARID.

[19]Mentors were recruited from the population of eligible Congolese 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.

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

[21]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 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.

Changes After Covid-19. Interventions were implemented in-person beginning in January 2020. Due to COVID-19, we paused interventions in March 2020 and restarted all treatments remotely in 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.[22] 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.[23] Overall, uptake was at least 77% in each treatment arm. Tables B12 and B13 provide additional information about treatment status before and after COVID-19.

3.4 Randomization

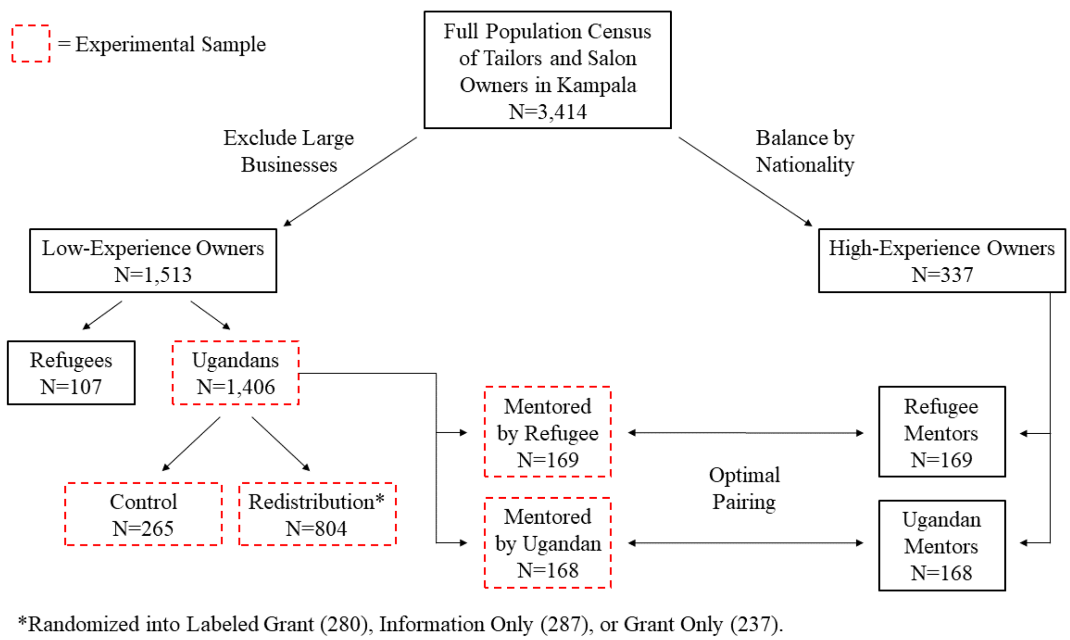
We assign treatments randomly within strata defined by gender, sector, and mentor eligibility,[24] and, within each of these cells, median profits and median attitudes towards hosting using the Stata command *randtreat*. We chose treatment probabilities within stratum based on the number of available 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. Appendix 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 3.5.2). 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 balanced treatment groups. Among individuals surveyed at least once after baseline, 2 out of 31 baseline variables are significantly different at the 10% level, as shown in Appendix Table B2, suggesting that attrition did not generate imbalance.

[22]Business owners were encouraged to invest the grant in their business if it was still operating, but this was not enforced. Of the 143 purchases made before COVID-19 with grants, 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.

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

[24]Respondents in our sample were designated as "mentor eligible" if they had 3–5 years of experience in their sector. Half of these mentor-eligible respondents were randomly assigned to be a mentor; the other half were assigned to one of five treatment groups according to the same process used for mentor-ineligible respondents.

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. 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 Empirical strategy

This section summarizes 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).

3.5.1 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.

3.5.2 Measurement and Multiple Hypothesis Testing

Because many of our outcomes of interest represent broad conceptual categories, such as "support for refugee integration policies," we 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.[25]

We transform survey questions that use Likert scales and other categorical outcomes into binary measures, resolving neutrals towards the smaller group. Monetary values are winsorized at the 1st and 99th percentiles within each survey round and recorded as 0 for firms that are not operating. Due to survey length concerns, not all outcomes were measured in all surveys; the number of observations thus may vary across outcomes.

Within each pre-specified domain, we compute sharpened q-values to control the false discovery rate. This procedure estimates the share of rejected null hypotheses that are false rejections. We indicate outcomes that were not pre-specified with a plus sign ($^+$) and report naive p-values from Equation 1 for these and for the domain summary indices. For hypotheses that we pre-specified as primary, we report Westfall-Young stepdown-adjusted p-values to control for the family-wise error rate in Appendix Table A13. 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, including sharpened q-values, in Online Appendix B.8.[26]

[25] Weights are the sum of row entries in the inverted covariance matrix of outcomes in a domain.
[26] Online Appendix B.8 can be accessed here.

4 Summary Statistics and Correlates of Policy Views

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.[27]

4.1 Baseline Policy Views

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 (58–60% 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 smaller share (27%) believes 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. Many respondents (57%) say that refugees have a neutral impact on culture in Uganda, while 30% say the effect is negative. About 20% say they would be very comfortable marrying a refugee; about 40% say they would be very uncomfortable doing so.

4.2 What Drives Policy Views?

We investigate the baseline drivers of support for refugee integration by running variable-selection lasso regressions on baseline data. Appendix Table A1 presents results for five key measures of support for integration: support for hosting refugees in general, support for admitting additional refugees, support for refugees' right to work, support for freedom of movement for refugees, and a domain summary index of support for refugee integration policies. As potential predictors of policy views, we include seven indices summarizing attitudes toward refugees and two indices summarizing respondents' economic well-being and business profit.

[27]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
Refugee Integration Policy Views			
Aware of Aid-Sharing	0.19	0.39	1,406
Supports Refugee Hosting	0.72	0.45	1,406
Supports More Refugees	0.52	0.50	1,406
Supports Freedom of Movement	0.58	0.49	1,406
Supports Right to Work	0.60	0.49	1,406
Economic Beliefs			
Refugees Increase Rents	0.78	0.41	1,312
Refugees Increase Goods Prices	0.62	0.48	1,313
Refugees Worsen Public Goods	0.27	0.45	1,300
Refugees' Economic Effect is Positive	0.53	0.50	1,334

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

We find that by far the strongest predictor of support for refugee integration at baseline—across all the measures of support for integration policies shown in Appendix Table A1—is cultural views toward refugees. For example, those who view refugees' cultural impacts more favorably (by one standard deviation) are 14 pp. more likely to say they support refugee hosting in general. The coefficients on the three other indices selected in this regression—economic beliefs about refugees hosting, knowledge of hosting policy, and household well-being—are all 0.04, less than one-third the magnitude. This pattern holds for admitting more refugees, supporting refugees' right to work and freedom of movement, and the summary index of support for refugee integration policies, and is consistent with findings from other immigration settings (Hainmueller and Hopkins, 2014, Tabellini, 2020).

5 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 integration policies. Sharing information about existing redistribution—without an additional grant—has similar, but smaller, impacts. Facilitating cooperative contact through business mentorship by experienced refugees has only transient impacts on policy preferences.

5.1 Support for Refugee Integration Policies

We find that receiving a labeled grant significantly increases support for refugee hosting and integration, as shown in Table 2. Recipients of labeled grants were 13 pp. more likely to say that they support Uganda's hosting of refugees generally, on a base of 75% (p-val < 0.001). Labeled grants also increase support for admitting more refugees into Uganda (15 pp. on a base of 61%, p-val < 0.001), extending the right to work (13 pp. on a base of 72%, p-val < 0.001), and extending freedom of movement to refugees (6 pp. on a base of 54%, p-val $= 0.04$). The impact on our pre-specified domain summary index is 0.36 standard deviations (p-val < 0.001; family-wise error rate < 0.001). Adjustments for multiple hypothesis testing do not affect these conclusions, as shown in Appendix Table C1. Labeled grants also affect views at the tails of the distribution: recipients were significantly less likely to indicate strong opposition for integration policies, and more likely to indicate strong support, as shown in Table A3. This result suggests that aid-sharing may influence policy views even in environments where opposition to integration is stronger on average.

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 significantly impacts policy preferences, though by less than receiving a labeled grant (coeff. = 0.22 std. devs.; p-val on comparison to labeled grants = 0.02). 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 than labeled grants (coeff. = 0.25 std. devs.; p-val on comparison to labeled grants = 0.05). As we discuss further in Section 6.1, 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 associate aid with refugees compared to control. 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

Table 2: Support for Refugee Integration Policies

	Supports Refugee Hosting	Supports More Refugees	Supports Right to Work	Supports Freedom of Movement	Integration Policies Index	Supported Phone Campaign^{+}
Labeled Grant	0.133***	0.146***	0.133***	0.062**	0.360***	0.100***
	(0.024)	(0.031)	(0.027)	(0.031)	(0.064)	(0.038)
	[0.000]	[0.000]	[0.000]	[0.043]	[0.000]	[0.008]
Information Only	0.062**	0.097***	0.084***	0.028	0.223***	0.021
	(0.027)	(0.031)	(0.028)	(0.031)	(0.066)	(0.036)
	[0.022]	[0.002]	[0.002]	[0.368]	[0.001]	[0.555]
Grant Only	0.089***	0.121***	0.096***	0.004	0.245***	0.043
	(0.028)	(0.032)	(0.028)	(0.031)	(0.066)	(0.038)
	[0.001]	[0.000]	[0.001]	[0.891]	[0.000]	[0.258]
Mentored by Refugee	0.036	0.058*	0.076**	-0.028	0.120*	-0.012
	(0.031)	(0.035)	(0.030)	(0.037)	(0.072)	(0.042)
	[0.252]	[0.098]	[0.012]	[0.444]	[0.096]	[0.767]
Mentored by Ugandan	0.066**	0.042	0.024	-0.063*	0.101	-0.026
	(0.030)	(0.036)	(0.033)	(0.037)	(0.075)	(0.042)
	[0.029]	[0.241]	[0.461]	[0.087]	[0.177]	[0.537]
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
Labeled Grant = Info Only	0.002	0.079	0.040	0.262	0.019	0.037
Labeled Grant = Grant Only	0.059	0.390	0.122	0.052	0.048	0.157
Labeled Grant = R-Mentee	0.000	0.006	0.036	0.013	0.000	0.010
R-Mentee = Info Only	0.381	0.238	0.780	0.127	0.126	0.420
R-Mentee = U-Mentee	0.346	0.658	0.115	0.398	0.803	0.773

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

are driving changes in attitudes, as discussed in Section 6.3.

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 72%, p-val = 0.01), but smaller and statistically insignificant (at the 5% level) impacts on general support for hosting, support for admitting more refugees, and support for freedom of movement. The impact on the domain summary index is 0.12 standard deviations (p-val = 0.10). As we discuss in Section 6.4, we test and reject that additional mentorship meetings would have generated persistent impacts on policy views: instead, even mentorship groups that met many times experienced large initial impacts that did not persist.

Do impacts on self-reported views reflect changes in true preferences? To test for changes in true preferences, we sought to induce a naturalistic situation outside of our surveys that required individuals in our sample to make a decision either in favor or not in favor of refugee hosting, similar to voting in a referendum. To do so, we partnered with an organization that was independent of both the survey firm and implementing non-profit. One year after the interventions were completed, that organization conducted a phone-call campaign asking each member of our sample whether they wanted to support a letter to local officials expressing their approval of refugee hosting.[28] As shown in Table 2, labeled grant recipients were 10 pp. more likely to support the letter (on a base of 23%, p-val $<$ 0.01), with no significant average impacts for other treatment arms.[29] These results, together with additional evidence discussed in detail in Section 6, point to a change in true policy preferences rather than effects driven entirely by experimenter demand.

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

5.2 Beliefs About Economic Impacts of Refugee Hosting

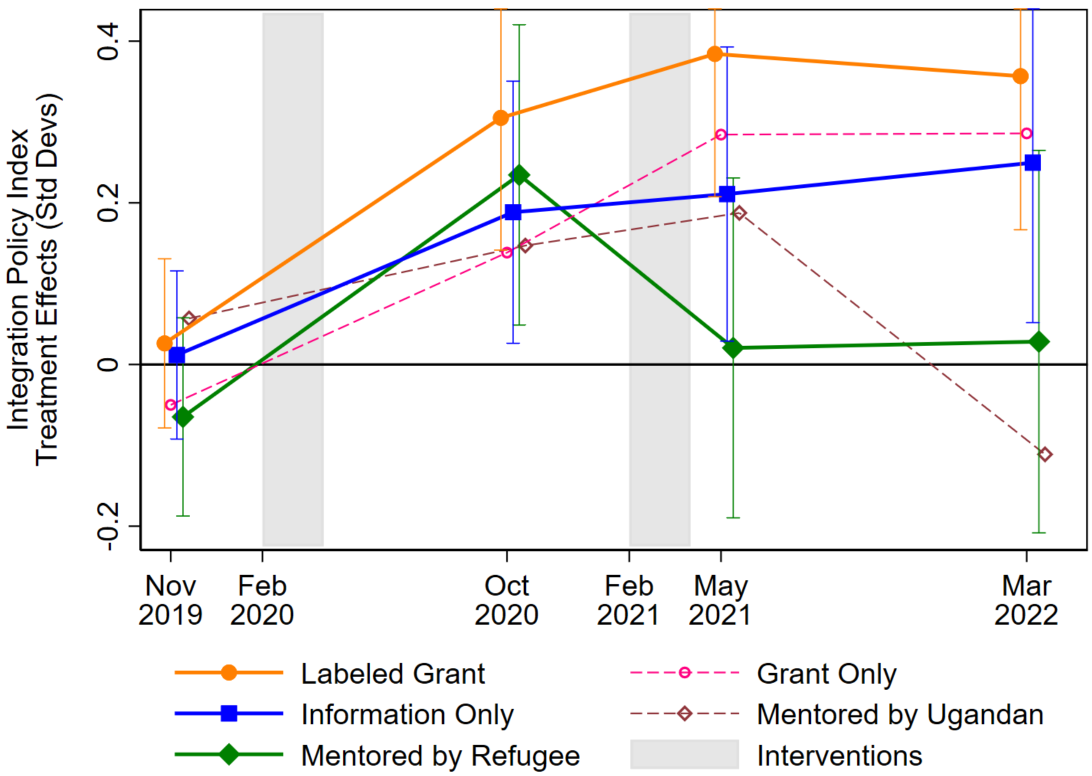
Our interventions may affect policy views by changing beliefs about the economic impacts of refugee hosting. Business owners who received a labeled grant were significantly more likely than control business owners to report receiving support linked to the refugee presence, as shown in Table 3, a necessary "first stage" impact for our hypothesis.[30] Business owners

[28]See Appendix Section B.7 for the script, and Appendix Table A12 for detailed results. The organization is called OneYouth OneHeart Initiative. The letter was described as being addressed to local politicians, thanking them for allowing refugees to live in Kampala with the right to work. The campaign was intended to allow respondents to express their policy views without any risks of opposing the government, as only the number of supporters—not names—were included in the final letter. We recorded a one-minute message explaining the campaign, and respondents could press 1 to support or 2 to oppose. 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. Over 80% of the sample answered the call.

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

[30]To minimize the association between the data firm and the implementer, we did not measure this association explicitly. Instead, we asked respondents about "the purpose" of aid received recently and enumerators coded whether they spontaneously mentioned refugees in their response. Respondents in Grant Only were also more likely to associate support with refugees, though by less than in Labeled Grant (p-val = 0.03), for reasons we discuss in Section 6.1.

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



Notes: Each line shows the estimated treatment impact on a summary index of preferences for policies supporting refugee integration within a given survey wave. We did not collect these measures 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 the Labeled Grant, Information Only, and Mentored by Refugee arms.

who received a labeled grant were 15 pp. more likely to report that international aid for refugees is shared with Ugandans (on a base of 37%, p-val < 0.001),[31] and 16 pp. more likely to say refugees have a positive effect on the economy overall (on a base of 42%, p-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). Adjustments for multiple hypothesis testing to do not affect these conclusions, as shown in Appendix Table C5.

[31] 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	0.082***	0.147***	0.158***	0.093***	0.099**	0.297***
	(0.017)	(0.033)	(0.035)	(0.035)	(0.041)	(0.071)
	[0.000]	[0.000]	[0.000]	[0.009]	[0.016]	[0.000]
Information Only	0.013	0.051	0.116***	0.060*	0.017	0.220***
	(0.012)	(0.032)	(0.035)	(0.034)	(0.042)	(0.069)
	[0.302]	[0.112]	[0.001]	[0.079]	[0.692]	[0.001]
Grant Only	0.039***	0.091***	0.097***	0.107***	0.031	0.212***
	(0.015)	(0.033)	(0.036)	(0.037)	(0.044)	(0.072)
	[0.007]	[0.006]	[0.007]	[0.003]	[0.474]	[0.003]
Mentored by Refugee	-0.001	-0.029	0.035	-0.039	0.012	0.073
	(0.015)	(0.036)	(0.039)	(0.038)	(0.048)	(0.077)
	[0.957]	[0.422]	[0.372]	[0.307]	[0.805]	[0.340]
Mentored by Ugandan	0.019	0.023	0.037	0.056	0.005	0.073
	(0.015)	(0.038)	(0.040)	(0.039)	(0.046)	(0.078)
	[0.198]	[0.536]	[0.344]	[0.148]	[0.916]	[0.347]
Observations	2,171	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	0.031	0.369	0.423	0.443	0.416	-0.000
Labeled Grant = Info Only	0.000	0.003	0.195	0.316	0.040	0.248
Labeled Grant = Grant Only	0.026	0.093	0.073	0.690	0.106	0.231
Labeled Grant = R-Mentee	0.000	0.000	0.001	0.000	0.057	0.003
R-Mentee = Info Only	0.387	0.025	0.032	0.006	0.915	0.046
R-Mentee = U-Mentee	0.256	0.193	0.957	0.017	0.889	0.998

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

Our Information Only and Grant Only treatments also changed beliefs about the economic impacts of refugee hosting. Business owners in the Grant Only treatment arm were 8 pp. more likely than control business owners to report receiving support linked to the refugee presence, an impact only slightly smaller than that among labeled grant recipients. As discussed in Section 6.1, we believe this is due to an implicit labeling of the grant operating through contact with the refugee-led implementing organization. Overall, effect sizes are roughly half to two-thirds the size of impacts of the labeled grant. Mentorship had no discernible impacts on economic beliefs.

5.3 Cultural Attitudes Toward Refugees

Policy attitudes could also change due to updated cultural attitudes toward refugees. We find that labeled grant recipients changed some of their cultural 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. (p-vals $<$ 0.01). We do not observe significant changes in beliefs about the impact of refugees on Ugandan culture, or in whether refugees deserve sympathy. The impact on our pre-specified domain summary index is 0.16 standard deviations (p-val $= 0.01$). Adjustments for multiple hypothesis testing to do not affect these conclusions, as shown in Appendix Table C10. As we discuss using additional analysis in Section 6.2, impacts on cultural attitudes toward refugees appear to be driven not by contact with refugees, but indirectly through effects on economic beliefs and policy views.

Our Information Only treatment modestly changed cultural attitudes toward refugees, though the impacts are generally small and inconsistent across outcomes. Our Grant Only treatment had modest impacts on cultural attitudes, generally of slightly smaller magnitude than impacts of labeled grants. Mentorship had no discernible impacts on cultural attitudes.

During our surveys, we conducted a simple dictator game in which the respondent distributed 3,000 UGX (about \$0.80) between themselves, a program that helps refugees in Kampala, and a program that helps poor Ugandans in Kampala.[32] This offers a financially incentivized measure of positive attitudes toward refugees. Labeled grants increase the proportion donated to refugees by 5 pp. (on a base of 28%, p-val < 0.01). The Grant Only arm also increased the proportion donated, by 4 pp. (p-val $= 0.01$). Other treatment arms had no significant effects on the proportion donated.

5.4 Business Outcomes and Household Welfare

Our treatment arms had small and insignificant impacts on business profit, business capital, business practices, and a summary index of household welfare, as shown in 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 \$21. While somewhat surprising, the impacts are not statistically significant, and may reflect the impact of COVID-19 lockdowns, which reduced the scope for making a profit while also reducing the incentive to invest (rather

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

Table 4: Cultural Attitudes Toward Refugees

	Comfortable Refugee Friends	Comfortable Refugee Spouse	Prop. Donated Refugees	Pos Effect Culture	Deserve Sympathy	Social Attitudes Index
Labeled Grant	0.072***	0.127***	0.045***	-0.000	0.031	0.163**
	(0.027)	(0.039)	(0.015)	(0.032)	(0.040)	(0.066)
	[0.007]	[0.001]	[0.003]	[0.999]	[0.443]	[0.013]
Information Only	0.067**	0.066*	-0.001	0.052*	0.035	0.064
	(0.028)	(0.040)	(0.016)	(0.031)	(0.040)	(0.064)
	[0.016]	[0.097]	[0.934]	[0.094]	[0.380]	[0.317]
Grant Only	0.056**	0.070*	0.041***	-0.025	0.084**	0.126*
	(0.027)	(0.041)	(0.016)	(0.033)	(0.041)	(0.066)
	[0.043]	[0.089]	[0.010]	[0.454]	[0.039]	[0.056]
Mentored by Refugee	0.007	0.051	-0.019	0.024	-0.019	-0.029
	(0.035)	(0.046)	(0.018)	(0.037)	(0.046)	(0.073)
	[0.847]	[0.270]	[0.294]	[0.512]	[0.685]	[0.685]
Mentored by Ugandan	0.038	0.020	-0.002	0.054	-0.021	0.027
	(0.032)	(0.046)	(0.019)	(0.034)	(0.044)	(0.071)
	[0.244]	[0.670]	[0.917]	[0.111]	[0.636]	[0.707]
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
Labeled Grant = Info Only	0.818	0.116	0.001	0.093	0.911	0.101
Labeled Grant = Grant Only	0.487	0.158	0.766	0.454	0.179	0.555
Labeled Grant = R-Mentee	0.036	0.095	0.000	0.510	0.270	0.006
R-Mentee = Info Only	0.061	0.746	0.301	0.449	0.232	0.176
R-Mentee = U-Mentee	0.374	0.533	0.384	0.430	0.965	0.448

Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets and the last five rows display two-sided p-values. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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 at the 10% level. 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 only the impact of grants alone is statistically significant at the 10% level. We find suggestive evidence that grants improved household well-being,[33] as summarized in an index comprising income, savings, and qualitative reports of economic hardship (see Appendix Table C21 for impacts on the full set of welfare components). However, impacts are small (0.04–0.05 standard deviations) and statistically insignificant.

[33]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 Appendix Table C16) and so do not make any welfare adjustments.

Table 5: Business Outcomes and Household Welfare

	Business Profits (USD/Month)	Business Capital (USD)	Business Practices Index	Household Well-Being Index
Labeled Grant	-2.81	-56.3	0.043	0.054
	(2.35)	(44.4)	(0.078)	(0.062)
	[0.232]	[0.205]	[0.583]	[0.385]
Information Only	-0.87	19.3	-0.016	-0.048
	(2.52)	(48.0)	(0.078)	(0.065)
	[0.731]	[0.687]	[0.841]	[0.460]
Grant Only	-1.77	7.82	0.12*	0.041
	(2.52)	(46.8)	(0.073)	(0.064)
	[0.482]	[0.867]	[0.092]	[0.520]
Mentored by Refugee	1.14	-35.2	0.064	-0.025
	(2.83)	(50.6)	(0.088)	(0.077)
	[0.686]	[0.487]	[0.471]	[0.748]
Mentored by Ugandan	-2.35	15.2	0.11	0.11
	(2.74)	(53.6)	(0.081)	(0.068)
	[0.391]	[0.777]	[0.189]	[0.114]
Observations	4,029	2,819	1,942	4,132
Control Mean: Baseline	39.606	495.556	0.000	0.000
Control Mean: Follow-Ups	20.685	632.539	0.000	0.000
Labeled Grant = Info Only	0.393	0.086	0.440	0.063
Labeled Grant = Grant Only	0.645	0.140	0.262	0.818
Labeled Grant = R-Mentee	0.135	0.660	0.811	0.258
R-Mentee = Info Only	0.480	0.289	0.368	0.738
R-Mentee = U-Mentee	0.256	0.366	0.640	0.067

Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets and the last five rows display two-sided p-values. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6 Mechanisms Behind Changing Policy Views

Why does learning about aid-sharing—either through new information or by receiving a grant—increase support for refugee integration? In Section 6.1, we discuss the potential mechanisms driving these impacts. In Section 6.2, we investigate whether our interventions acted on economic or cultural concerns about refugees. In Section 6.3, we examine potential alternative explanations for impacts on policy views—including experimenter demand effects, contact with refugees, reciprocity to the implementing organization, wealth effects, and differential attrition—which we rule out by examining additional data. In Section 6.4, we test and reject that the additional mentorship meetings would have generated persistent impacts on policy views.

6.1 Unpacking the Effect of Labeled Grants

In this section we present evidence for three mechanisms we find to be driving impacts of labeled grants on policy views: information about aid-sharing, the salience and credibility of that information, and the inherent association between aid and the refugee presence created by the refugee-led implementing organization.

Information About Aid-Sharing. Learning about Uganda's existing aid-sharing policy through the Information Only arm, without any associated grant, led to significant and persistent impacts on support for refugee integration policies. This indicates that at least part of the impact of labeled grants operates purely through the information provided.

Association Between the Grant and YARID. Receiving a grant without any information about aid-sharing also increased support for refugee integration. 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 some grant recipients either already knew about the organization or learned about it after the intervention. We see that owners in the Grant Only treatment arm were more likely to report receiving support, and to associate that support with the implementing non-profit and with refugees, than the control group (though less than the Labeled Grant group, as shown in Appendix Table A2).[34] This implies that our Grant Only intervention isolates not only the wealth effect of the labeled grant, but also some of the effect of receiving aid from an organization associated with the refugee presence. As we discuss below, the marginal impact of the label is to link the grant to refugee integration policies and strengthen its association with refugees.

Second, receiving aid appears to reduce what Zhou (2019) terms *resource resentment*, or negative views toward a group perceived to be receiving unfair levels of support. As shown in Appendix Table A4, 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 is likely driven by increased awareness of aid-sharing—as unlabeled grant recipients were 9 pp. more likely to report that aid is shared between refugees and Ugandans compared to control (p-val < 0.01), as shown in Table 3—and by changing attitudes toward aid organizations, as unlabeled grant recipients were more likely to say that local

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

and international aid organizations care about them (by 10–11 pp., p-vals = 0.09 and 0.04 respectively) and are trustworthy (by 23 pp, p-val < 0.001). It may also be partly related to changing beliefs about the distribution of aid, as unlabeled grant recipients were 8 pp. less likely to say that refugees receive more aid than Ugandans (on a base of 71%, p-val = 0.14). Together, these findings suggest that receiving aid can reduce feelings of resentment toward groups perceived to be major beneficiaries of aid, such as refugees.

Salience and Credibility. The effects of the labeled grant on policy views were generally 50–100% greater than the effects of information about aid-sharing alone. Our results suggest that the direct receipt of aid makes the accompanying information more believable or salient by acting as a visible demonstration of aid-sharing.[35] Recipients of labeled grants were more likely than those in the Information Only arm to remember that some of the aid from the international refugee response is shared with Ugandans (p-val on comparison < 0.01), as shown in Table 3. They were also more likely to say that international organizations are trustworthy compared to the Information Only arm (diff. = 20 pp. on a base of 44%, p-val = 0.001), as shown in Appendix Table A4.[36] This indicates that the effects of labeled grants operate in part by amplifying the impacts of knowledge about aid-sharing.

The effects of labeled grants on policy views were generally about 50% greater than the effects of unlabeled grants. This indicates that associating the grant with the refugee presence does not completely substitute for the information provided alongside the labeled grant, which linked the grant to broader national aid-sharing and integration policies. It is also likely that the information provided with the labeled grant strengthened respondents' general association between the grant and the refugee presence. Effects of the labeled grant on indicators for whether the business owner associated any support they received with refugees, knows about aid-sharing between Ugandans and refugees, and believes refugees have a positive impact on the economy overall are also about 50% bigger than impacts of the grant alone (Table 3). The larger effects of the labeled grants are not driven by differences in resource resentment: if anything, labeled grant recipients were more likely to say that refugees receive too much aid compared to unlabeled grant recipients. This difference is possibly due to the information treatment increasing awareness or salience of aid toward refugees compared to receiving a grant alone.

[35] This is related to Bauhoff and Kandpal (2021), who find that incentives in pay-for-performance contracts work by signaling which information is important, making information delivery more effective.

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

6.2 Economic Versus Cultural Beliefs

A large literature examines whether attitudes toward immigrants are driven more by economic or cultural beliefs. While cultural concerns are a much stronger predictor of policy views at baseline compared to economic concerns (see Appendix Table A1), we find that policy views respond more to our economic interventions—grants and information—than to our more cultural intervention—contact with a refugee—and that once policy and economic views shift, cultural attitudes follow.

Heterogeneous Impacts By Baseline Economic or Cultural Concerns. We find that all of our interventions had greater impacts on the policy views of Ugandans with *either* above-median economic or cultural concerns about refugee hosting at baseline, as shown in Appendix Table A5.[37] To further assess the relative importance of economic and cultural views in mediating treatment impacts, we examine specifications that interact treatment dummies separately with baseline measures of economic and cultural views and baseline economic well-being, presented in Appendix Table A6. We include these three possible predictors of treatment impacts because they are well-motivated in the extant literature and because they are the three strongest correlates of policy views at baseline (see Table A1). We focus our discussion on the three grant and information treatment arms—as only those arms had significant average impacts on policy views—but patterns in the mentorship arms are similar. We find that greater economic concerns about refugees at baseline consistently predict stronger treatment effects of both grants and information, even when controlling for cultural concerns, economic-well being, and their interactions with treatment dummies, as shown in Table A6.[38] In the specification that includes all three dimensions of heterogeneity and their interactions, we find that economic concerns predict treatment impacts to the greatest degree across both grant arms and the Information Only arm. Baseline cultural concerns also consistently predict stronger treatment effects across specifications. There is some evidence of stronger treatment effects among those with better baseline economic well-being, which is difficult to reconcile with a model in which individual resource competition is the primary driver of policy views.

Interpretation. The concentrated treatment impacts among those with economic concerns about refugee integration are unsurprising given that the information was focused on economic policy, and the grant is itself an economic intervention. The concentrated impacts among those with cultural concerns are hard to reconcile with a heuristic in which only

[37] Our measures for baseline economic and cultural attitudes are the same pre-specified summary indices we use as outcomes in Tables 3 and 4, using baseline survey questions only.

[38] This is not simply due to a ceiling effect: only 9% of respondents give answers yielding the maximum index value for policy views.

cultural interventions affect culturally rooted policy opposition.[39] However, they are consistent with Jha (2012) and Jha and Shayo (2019), which show that financial instruments that align incentives toward peaceful coexistence across groups can reduce intergroup conflict. In our setting, aid-sharing acts as such an instrument by supporting an equilibrium that dominates the *status quo* for both groups. Less formally, the tendency to divide into tribes—proxied by baseline cultural concerns about refugees—is muted by the introduction of financial incentives for integration.

Why Did Grants Affect Cultural Views? As discussed in Section 6.3, we see no evidence of impacts on contact with refugees which might mediate impacts on cultural attitudes. Rather, our findings suggest that impacts on cultural attitudes appear as a rationalization of changing economic and policy beliefs: once our interventions had changed policy views, cultural concerns were vestigial and could be dropped. As shown in Appendix Table A9,[40] we find that impacts on cultural attitudes lag other impacts: while there were large and significant impacts of labeled grants on preferences for integration and economic beliefs about refugees in the first follow-up survey (0.33 and 0.22 std. devs., p-vals < 0.001 and $= 0.03$ respectively), we find no impact on cultural attitudes at that time (coeff. $= 0.04$ std. devs.). In subsequent surveys, we observe significant impacts of labeled grants on all three of these domains, and can reject that impacts on cultural attitudes were equivalent to estimated first-round impacts in the 16-month survey (p-val $= 0.05$) and the pooled 16- and 28-month surveys (p-val $= 0.03$). Effects in the Information Only arm display a similar pattern. The delayed timing of these impacts is suggestive of cultural attitudes that are partly rationalized from changing economic and policy views, possibly to reduce the cognitive dissonance involved with holding positive economic but negative cultural views toward refugees.

Overall, our findings suggest that a heuristic that economic opposition responds to economic interventions, and cultural opposition to cultural interventions, is misguided. Possibly reflecting this heuristic—and in light of the common finding that immigration views are largely driven by cultural concerns—the majority of the extant experimental literature on immigration attitudes focuses on contact (Mousa, 2020, Loiacono and Silva-Vargas, 2023) or humanizing narratives of immigrants (Adida, Lo and Platas, 2018, Kalla and Broockman, 2020). However, our findings suggest that economic beliefs, cultural attitudes, and policy

[39] While our Labeled Grant and Information Only treatments included a listening exercise which could potentially affect cultural beliefs, the listening exercise does not appear to be driving these impacts, as the Information Only treatment impact on the cultural attitudes summary index is less than half the impact of Labeled Grant (p-val $= 0.10$), while the Grant Only arm—which did not include a listening exercise—did affect cultural beliefs, as shown in Table 4.

[40] For results in Appendix Table A9, we constrain each summary index to contain only the components asked in each survey round, for comparability over time.

views are jointly determined: economic interventions—information and grants—affected all three domains, while the effects of mentorship were transient. We conclude that economic interventions can impact policy views regardless of whether opposition is economically or culturally rooted.

6.3 Alternative Mechanisms Rejected by Our Data

In this section we test and reject several potential alternative explanations for our results: experimenter demand effects, contact with refugees, reciprocity to the implementing organization, wealth effects, and differential attrition.

Experimenter Demand Effects. A potential concern is that the observed change in policy views is 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 the implementing organization, YARID, is refugee-led and in part refugee-staffed, business owners may believe that their chances of receiving future assistance are increased by expressing pro-refugee views.[41] Alternatively, demand effects may be generated by feelings of gift exchange, if respondents who received assistance from YARID viewed the assistance as a *quid pro quo*, and so gave responses they think YARID wanted to hear but do not believe themselves. We do not observe treatment impacts on every outcome related to refugee hosting policy or economic and cultural attitudes about refugee hosting. This is inconsistent with the most extreme demand effects but does not rule out demand effects that appear in some outcomes but not others. The large treatment impacts observed in the Information Only arm are also hard to reconcile with a gift exchange mechanism, as these individuals received no support from YARID. Below we discuss aspects of our study design that were intended to minimize demand effects and discuss several results testing whether true beliefs were impacted by our treatments.

We designed our study to minimize potential demand effects. Surveys were conducted by a Ugandan-led firm unconnected to YARID. We introduced the survey as research on "businesses like yours and the views of citizens" and reminded respondents that their answers would remain anonymous, would not affect their eligibility for aid, and that there would be no direct benefit to survey participation beyond the typical small cash incentive. We also explained to grant recipients that the grant 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.

[41] 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 impacts on true beliefs if the control group believes that it is likely to receive aid in the future.

The phone-call campaign discussed in Section 5.1 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.

To further test whether respondents' answers were influenced by their perceptions of YARID's position—as opposed to the new information provided through our interventions—and whether receiving cash amplifies such an effect, we ran a placebo information campaign on an unrelated political issue, child labor, which shared YARID's position but did not provide any new information.[42] YARID conducted a short campaign opposing child labor within the Grant Only and Information Only arms of our sample. The script was short, and facilitators were instructed to avoid conversations about the issue. Our goal was to inform respondents of YARID's position only in order to test whether knowledge of YARID's position influenced answers. We intentionally excluded information on the issue of child labor, which could have persuaded respondents through other channels besides knowledge of YARID's position. By comparing the expressed views on child labor of the Information Only arm to the Control group (pooled with the Labeled Grant, Mentored by Refugee, and Mentored by Ugandan arms for this specification, which also did not receive the placebo campaign), we test whether knowledge of YARID's view alone affected respondents' expressed preferences, perhaps due to hope for future assistance conditional on "acceptable" answers. In addition, by comparing the impact of the campaign in the Grant Only to the Information Only arms, we can identify whether receiving assistance from YARID amplifies any demand effects, which would complicate our comparison of the Labeled Grant and Information Only arms. The script is reproduced in Appendix Section B.8. In follow-up surveys taken after the child labor campaign, we find no impacts on attitudes toward child labor in either the Grant Only or the Information Only arm, as shown in Appendix Table A10. This indicates that experimenter demand effects within this sample are likely to be low in general, with or without the receipt of assistance.

As a further test of demand effects, we conducted a within-survey 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 Appendix Table A11), consistent with limited demand effects in this setting.[43] To

[42]Similar to refugee hosting, child labor policies are somewhat, but not extremely, sensitive issues in Uganda. We chose our outcomes for these tests to have a similar level of support as refugee hosting.

[43]The priming experiment was conducted only around the questions on refugees presented in Appendix Table A11 and not around our main outcomes on political views to avoid distorting those main outcomes. We believe any demand effects would be equally likely for the selected questions, since respondents were not aware of our primary outcomes of interest.

the extent that people with neutral views are the most sensitive to demand effects, the significant treatment impacts on policy views among those who strongly opposed refugee integration (see Appendix Table A3) indicate a change in true beliefs. Finally, 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 entirely driving the treatment impacts we observe.[44]

Contact With Refugees. We find no evidence that treatment impacts are driven by contact with refugees as program facilitators or through increased contact with refugees outside of our programs, and that impacts driven through contact with refugees as mentors do not persist. 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 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.[45] Nevertheless, we find few persistent impacts of mentorship on policy preferences, economic beliefs, or cultural attitudes. We also do not find that contact with a refugee YARID facilitator, relative to a Ugandan YARID facilitator, affects the treatment impacts in Labeled Grant or Information Only arms, as shown in Appendix Table A5, Column 2.

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

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

[44]In a different setting, De Quidt, Haushofer and Roth (2018) find that "typical demand effects are probably modest" based on experiments that attempt to induce demand effects in large online samples.

[45]In the 28-month survey, 35% of those mentored by a refugee report meeting their mentor after the program ended and 18% report meeting within the 30 days preceding the survey.

between true and reported beliefs; reciprocity could in theory lead owners to update their true beliefs.

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

Wealth Effects. In theory, changes in beliefs could be driven by wealth effects of the grant, for example by reducing feelings of scarcity and thus the salience of resource competition with refugees. We do not find any evidence supporting this channel. As shown in Table 5 and Appendix Table 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. Finally, we observe similar—if anything, greater—treatment impacts among business owners with higher measures of household well-being at baseline (see Appendix Table A6), which is inconsistent with a scarcity channel.

Personal Benefit of the Grant. Our results suggest that labeled grants have a greater impact on policy views than information about aid-sharing alone because the grant acts as a visible demonstration of aid-sharing, increasing the salience and credibility of the information, as described in Section 6.1. A possible alternative is that the grant confers a personal benefit on the respondent, whereas information about existing aid-sharing leads respondents to update their beliefs about economic benefits to other Ugandans only. To test this alternative hypothesis, we exploit the fact that our information script focused on hospitals and schools near where our respondents live as examples of public goods funded by aid coming from the refugee response. If variation in personal economic benefits is explaining the differences in impacts across treatment groups, we would expect it to explain variation within the Information Only group as well. Appendix Table A7 shows estimates of heterogeneous treatment effects on our index summarizing support for refugee integration based on an indicator for hospital use, an indicator for whether the respondent has children who attend school with foreigners (a proxy for whether the school receives funding from the refugee presence), and an indicator for the union of these two measures, with the caveat that these measures were taken after treatment. We do not find significant differences along these measures, although the estimate for hospital use is positive. 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, consistent with the review of the political science literature on views toward immigration in Hainmueller and Hiscox (2010). Moreover, as shown in Table 3, respondents in the Information Only arm were more likely to say that refugees have a positive economic effect on them personally. While this effect is lower than the same effect from the Labeled Grant treatment, this is also true for beliefs about whether refugees benefit the Ugandan economy overall, by a similar magnitude. This finding is consistent with a salience or credibility effect, rather than a difference in personal benefit, driving the greater impacts of labeled grants compared to information alone.

Differential Attrition. Respondents who could not be surveyed after the baseline (9% of the sample) are balanced across treatment groups, as shown in Appendix Table B4. Moreover, the attrition rates pooled across survey rounds and estimated through ANCOVA regression were not significantly different at the 5% level for any treatment arm compared to Control except for Grant Only, where retention was 8 pp. higher, as shown in Appendix Table B5. Retention rates in the pooled specification were modestly higher in Labeled Grant (4 pp., p-val = 0.12) and Mentored by Ugandan (6 pp., p-val = 0.07) compared to Control. Reassuringly, all of our main comparisons of interest are between groups with similar round-by-round attrition rates: Labeled Grant vs. Information Only (p-val = 0.20), Labeled Grant vs. Grant Only (p-val = 0.16), Labeled Grant vs. Mentored by Refugee (p-val = 0.62), Labeled Grant vs. Control (p-val = 0.12), Mentored by Refugee vs. Mentored by Ugandan (p-val = 0.41), and Mentored by Refugee vs. Control (p-val = 0.39). Finally, attrition does not appear to have significantly changed the baseline balance created by randomization (see Appendix Table B1 and Table B2).

Nevertheless, to further assess whether differential attrition is influencing our results, we reproduce all of our main results weighting observations by the inverse probability of round-specific retention, estimated by lasso logistic regression.[46] Results, shown in Appendix Tables B6, B7, B8, and B9 are extremely similar to unweighted results and complement the Lee Bounds presented in Appendix Tables B10 and B11. We conclude that differential attrition is not a significant factor in explaining our main results.

Altruism Crowd-Out. We do not find that redistribution crowds out other sources of policy support such as altruism. We can confidently reject full crowding-out: such an effect would lead us to find null or negative treatment impacts on support for refugee hosting, but in fact these impacts are large, positive, and persistent. We also find evidence against even partial crowding-out. We observe a positive impact of labeled grants on the share donated

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

to 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.

6.4 Could More Contact Produce Persistent Impacts?

We find that our economic intervention—labeled grants—produced large and persistent changes in policy views (see Figure 2), while our contact intervention—mentorship by an experienced refugee—produced initial impacts that diminished substantially after about one year. In this subsection, we analyze whether a more intensive contact intervention, such as additional meetings or more in-person meetings compared to remote meetings, would have produced more persistent impacts. To do so, we exploit a feature of our randomization design in which a random subset of business owners assigned to mentorship started their mentorship meetings earlier. Because of the earlier start date, these business owners had more contact—specifically more in-person contact—with their mentors before the programs were paused due to COVID-19.[47] Within the group assigned to refugee mentors, business owners in the "intensive mentorship" sample met with their mentors in person 3.5 times on average, compared to 1.5 for mentees not in the intensive sample. Including remote meetings, the intensive sample had 5.6 meetings compared to 4.1 in the later sample. See Table B13 for additional summary statistics on mentorship implementation.

We find substantial early impacts on the policy views of business owners who were mentored more intensively by refugees, but these impacts fade out over time, as shown in Appendix Table A9. About 9 months after the meetings began, intensive refugee mentorship had increased our index measure of support for refugee integration by 0.55 sd ($p < 0.001$). This effect falls to 0.22 sd ($p = 0.21$) after 16 months and 0.09 sd ($p = 0.65$) after 28 months. Impacts on beliefs about the economic effects of refugees on Uganda follow a similar pattern, with large initial impacts that fade to insignificance over time. At no point do we observe significant impacts of intensive refugee mentorship on cultural views. Impacts of less intensive refugee mentorship on support for refugee integration are small and positive after 9 months (coeff. = 0.19, $p = 0.07$) but are also smaller and insignificant over time.

Overall, these findings indicate that while contact with refugees produces large initial effects on policy views, these impacts dissipate. Labeled grants, on the other hand, produce

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

large impacts which persist long after the grant is disbursed.

7 Discussion

We provide experimental evidence that compensation—redistributing the aggregate gains from a policy—can influence political views and build support for a policy bargain in the medium-run. While policy bargains are common, we provide evidence that when the connection between an economic intervention and the policy is salient, compensation can persuade voters even when the underlying opposition is cultural in nature. Our primary economic intervention, the labeled grant, in fact has more persistent effects on policy views than our primary cultural intervention, the refugee mentorship.

We find that information about aid-sharing, especially when augmented with a business grant labeled as redistribution of foreign aid, leads Ugandan natives to update their beliefs about the net economic impact of hosting refugees and to 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. 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. The listening exercise that invited respondents to share their personal views and frustrations, including on fairness, may have augmented the effects of the information in the Information Alone and Labeled Grant programs. Further exploring how beliefs about fairness influence the attitudes and policy views of hosts is a promising avenue for future research.

There is reason to believe that our findings could apply in other settings. Uganda ranks close to the median country on Gallup's 2016 Migrant Acceptance Index (Esipova, Fleming and Ray, 2017). Other countries in the region that host significant numbers of refugees and where governments are considering policy bargains similar to the one we study, such as Ethiopia and Kenya, rank similarly.[48] In our setting, we observed the greatest impacts on policy views and attitudes toward refugees among those with negative baseline views: this suggests that programs similar to ours may be impactful even in more hostile environments. Finally, the effects of the non-judgemental listening exercise were first documented in the United States and across a range of issues including, but not limited to, immigration. Our

[48]Uganda ranks 72nd out of 139 countries on the Migrant Acceptance Index. Ethiopia ranks 81st. Kenya ranks 40th.

results suggest that this technique replicates in a second population, and future research could further unbundle the information treatment to analyze which components matter for overall impacts. Altogether, these observations suggest that interventions like information about aid-sharing or labeled grants for hosts could be important programs in areas where tensions are high and where there is potential surplus from policy changes supporting refugees' integration and improved social cohesion generally.

These programs also have the potential to scale. First, many organizations already include host community members in their programs. YARID, for instance, has distributed much larger business grants than the ones we study to refugees and hosts through other programs. Few organizations that we are aware of, however, directly connect assistance to the refugee presence. The marginal cost of delivering this information on top of an existing intervention would likely be minimal, as it was in our context.

Second, additional funding is available for host countries that are willing to facilitate refugees' integration. Donors are frustrated with indefinite short-term programs and willing to make long-term investments that include host communities, as reflected in the UN's Global Compact on Refugees. For instance, the World Bank's Window for Host Communities and Refugees has raised 6.6 billion USD for long-term programming in host countries with "adequate" policy frameworks for refugees (World Bank, 2022). This funding is for development projects that will benefit host communities and serves as a direct incentive for an inclusive policy environment. Uganda, accordingly, is one of the largest beneficiaries, receiving over 500 million USD for public goods in refugee-hosting regions. Our work suggests that an information campaign to connect public good investments to the refugee presence and inclusive policies could benefit refugees without contravening host-country politicians' political incentives.

Finally, our findings could also apply to other public policy issues like economic immigration and international trade. Clemens (2011) estimates that reducing legals barriers to immigration could yield trillions of dollars in global aggregate gains, but this policy often faces strong political opposition. Freeman (2006), Edelberg and Watson (2022) and Lokshin and Ravallion (2022) among others propose strategies to reallocate some of these gains to potential losers and political opponents, including taxation of immigrants and proportional funding of local public goods. Our results suggest that a strategy of redistributing the gains—even when opposition is likely driven by cultural factors—and using the compensation to demonstrate the direct economic benefits of immigrants is a promising avenue to build support for a new political economy equilibrium with higher aggregate welfare.

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

A Additional Tables

Table A1: Baseline Correlates of Support for Refugee Integration

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

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

Table A2: Program Associations and Recall

	Reported Any Support$^+$	Associated Support w YARID$^+$	Associated Support w Data Firm$^+$	Associated Support w Refugees$^+$	Associated YARID w Refugees$^+$	Knows About Aid-Sharing
Labeled Grant	0.239***	0.203***	0.090***	0.082***	0.220***	0.147***
	(0.030)	(0.019)	(0.017)	(0.017)	(0.032)	(0.033)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Information Only	-0.002	0.006	0.023*	0.013	0.171***	0.051
	(0.027)	(0.005)	(0.013)	(0.012)	(0.030)	(0.032)
	[0.929]	[0.285]	[0.075]	[0.302]	[0.000]	[0.112]
Grant Only	0.256***	0.178***	0.103***	0.039***	0.182***	0.091***
	(0.030)	(0.018)	(0.017)	(0.015)	(0.031)	(0.033)
	[0.000]	[0.000]	[0.000]	[0.007]	[0.000]	[0.006]
Mentored by Refugee	0.020	0.027***	0.025	-0.001	0.112***	-0.029
	(0.032)	(0.010)	(0.016)	(0.015)	(0.032)	(0.036)
	[0.534]	[0.008]	[0.107]	[0.957]	[0.001]	[0.422]
Mentored by Ugandan	0.045	0.035***	0.021	0.019	0.143***	0.023
	(0.030)	(0.012)	(0.014)	(0.015)	(0.034)	(0.038)
	[0.136]	[0.004]	[0.153]	[0.198]	[0.000]	[0.536]
Observations	3,061	3,061	3,061	2,171	901	3,061
Control Mean: Baseline	.	.	.	.	.	0.173
Control Mean: Follow-Ups	0.316	0.004	0.036	0.031	0.006	0.369
Labeled Grant = Info Only	0.000	0.000	0.000	0.000	0.242	0.003
Labeled Grant = Grant Only	0.582	0.311	0.549	0.026	0.368	0.093
Labeled Grant = R-Mentee	0.000	0.000	0.001	0.000	0.013	0.000
R-Mentee = Info Only	0.474	0.037	0.880	0.387	0.168	0.025
R-Mentee = U-Mentee	0.454	0.585	0.790	0.256	0.477	0.193

Reports of support—and associations with YARID, data firm, and refugees—are measured without prompting in a question about aid received from NGOs (see Table 3 for question text). *Associated YARID w Refugees* is measured in the final survey in a question explicitly about YARID, but without prompting about refugees. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets and the last five rows display two-sided p-values. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Outcomes that were not pre-specified are denoted with $^+$.

Table A3: Strongly Support and Strongly Oppose Inclusive Policies

	Strongly Supports Refugee Hosting^{+}	Strongly Opposes Refugee Hosting^{+}	Strongly Supports More Refugees^{+}	Strongly Opposes More Refugees^{+}	Strongly Supports Freedom of Movement^{+}	Strongly Opposes Freedom of Movement^{+}	Strongly Supports Right to Work^{+}	Strongly Opposes Right to Work^{+}
Labeled Grant	0.096***	-0.057***	0.061**	-0.055**	0.079***	-0.041*	0.078***	-0.067***
	(0.030)	(0.017)	(0.027)	(0.022)	(0.026)	(0.024)	(0.029)	(0.018)
	[0.001]	[0.001]	[0.026]	[0.014]	[0.003]	[0.083]	[0.007]	[0.000]
Information Only	0.027	-0.030*	0.028	-0.047**	0.028	-0.002	0.044	-0.052***
	(0.031)	(0.017)	(0.028)	(0.022)	(0.026)	(0.024)	(0.029)	(0.018)
	[0.379]	[0.087]	[0.316]	[0.035]	[0.281]	[0.933]	[0.125]	[0.004]
Grant Only	0.041	-0.031*	0.037	-0.053**	0.005	-0.019	0.014	-0.053***
	(0.032)	(0.018)	(0.030)	(0.023)	(0.025)	(0.025)	(0.030)	(0.019)
	[0.200]	[0.096]	[0.211]	[0.022]	[0.841]	[0.461]	[0.637]	[0.005]
Mentored by Refugee	-0.015	-0.013	0.006	-0.033	0.033	0.002	0.031	-0.035*
	(0.035)	(0.021)	(0.031)	(0.026)	(0.031)	(0.028)	(0.033)	(0.020)
	[0.671]	[0.526]	[0.837]	[0.208]	[0.280]	[0.941]	[0.356]	[0.086]
Mentored by Ugandan	-0.005	-0.047**	0.007	-0.004	-0.018	0.027	0.010	-0.015
	(0.035)	(0.019)	(0.030)	(0.026)	(0.029)	(0.029)	(0.033)	(0.022)
	[0.895]	[0.013]	[0.820]	[0.890]	[0.530]	[0.345]	[0.755]	[0.492]
Observations	3,040	3,040	3,038	3,038	3,031	3,031	3,039	3,039
Control Mean: Baseline	0.438	0.136	0.236	0.195	0.166	0.157	0.264	0.092
Control Mean: Follow-Ups	0.427	0.110	0.327	0.162	0.232	0.192	0.368	0.114
Labeled Grant = Info Only	0.020	0.063	0.246	0.700	0.047	0.077	0.231	0.305
Labeled Grant = Grant Only	0.085	0.095	0.430	0.923	0.004	0.344	0.030	0.356
Labeled Grant = R-Mentee	0.001	0.023	0.078	0.350	0.138	0.106	0.153	0.059
R-Mentee = Info Only	0.232	0.401	0.488	0.539	0.858	0.879	0.679	0.314
R-Mentee = U-Mentee	0.784	0.104	0.987	0.290	0.113	0.417	0.565	0.340

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

Table A4: Perceived Fairness of Aid Distribution

	Int'l Aid Is Distributed Fairly^{+}	Refugees Get Too Much Aid^{+}	Refugees Get More Aid^{+}	Local Aid Orgs Care About Me^{+}	Int'l Aid Orgs Care About Me^{+}	Int'l Aid Orgs Are Trustworthy^{+}
Labeled Grant	0.056	-0.045	0.001	0.119**	0.086*	0.163***
	(0.053)	(0.049)	(0.051)	(0.056)	(0.051)	(0.061)
	[0.296]	[0.359]	[0.990]	[0.034]	[0.092]	[0.007]
Information Only	-0.033	-0.087*	-0.078	-0.047	-0.067	-0.033
	(0.053)	(0.051)	(0.053)	(0.055)	(0.050)	(0.063)
	[0.527]	[0.091]	[0.140]	[0.385]	[0.178]	[0.600]
Grant Only	-0.012	-0.150***	-0.080	0.100*	0.107**	0.232***
	(0.054)	(0.053)	(0.054)	(0.058)	(0.053)	(0.063)
	[0.823]	[0.004]	[0.139]	[0.085]	[0.044]	[0.000]
Mentored by Refugee	-0.025	-0.068	-0.099*	0.007	0.007	0.139*
	(0.062)	(0.060)	(0.060)	(0.065)	(0.059)	(0.071)
	[0.685]	[0.252]	[0.098]	[0.919]	[0.910]	[0.051]
Mentored by Ugandan	-0.043	-0.020	-0.002	0.045	0.039	-0.002
	(0.057)	(0.057)	(0.059)	(0.064)	(0.059)	(0.070)
	[0.454]	[0.730]	[0.977]	[0.482]	[0.510]	[0.976]
Observations	780	821	821	699	871	653
Control Mean: Baseline	.	.	.	.	.	.
Control Mean: Follow-Ups	0.308	0.767	0.705	0.302	0.325	0.438
Labeled Grant = Info Only	0.093	0.420	0.134	0.002	0.002	0.001
Labeled Grant = Grant Only	0.211	0.045	0.124	0.752	0.692	0.263
Labeled Grant = R-Mentee	0.193	0.700	0.090	0.083	0.184	0.733
R-Mentee = Info Only	0.892	0.765	0.730	0.397	0.212	0.017
R-Mentee = U-Mentee	0.782	0.454	0.130	0.586	0.619	0.068

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

Table A5: Heterogeneity in Treatment Impacts on Support for Refugee Integration Policies

	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	0.058	-0.013	-0.152	-0.297**	-0.307**	-0.280**	0.109	0.180	-0.093		-0.108
	(0.140)	(0.090)	(0.125)	(0.130)	(0.129)	(0.128)	(0.150)	(0.132)	(0.141)		(0.138)
	[0.678]	[0.883]	[0.223]	[0.023]	[0.017]	[0.029]	[0.470]	[0.174]	[0.507]		[0.433]
Labeled Grant	0.320***	0.370***	0.430***	0.537***	0.531***	0.506***	0.281**	0.251**	0.415***	0.362***	0.399***
	(0.119)	(0.091)	(0.089)	(0.109)	(0.104)	(0.101)	(0.133)	(0.103)	(0.069)	(0.064)	(0.106)
	[0.007]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.035]	[0.014]	[0.000]	[0.000]	[0.000]
Information Only × X	0.225	0.064	-0.202	-0.210	-0.294**	-0.291**	0.129	0.112	0.041		0.038
	(0.147)	(0.092)	(0.131)	(0.133)	(0.132)	(0.134)	(0.160)	(0.137)	(0.153)		(0.139)
	[0.127]	[0.491]	[0.124]	[0.113]	[0.026]	[0.030]	[0.419]	[0.414]	[0.788]		[0.786]
Information Only	0.076	0.180**	0.315***	0.346***	0.391***	0.365***	0.124	0.153	0.264***	0.225***	0.180*
	(0.127)	(0.090)	(0.090)	(0.106)	(0.104)	(0.106)	(0.142)	(0.108)	(0.070)	(0.065)	(0.107)
	[0.550]	[0.045]	[0.000]	[0.001]	[0.000]	[0.001]	[0.385]	[0.155]	[0.000]	[0.001]	[0.093]
Grant Only × X	0.012		-0.157	-0.208	-0.349***	-0.313**	-0.153	-0.066	-0.014		-0.063
	(0.142)		(0.130)	(0.133)	(0.135)	(0.133)	(0.152)	(0.135)	(0.148)		(0.140)
	[0.932]		[0.227]	[0.120]	[0.010]	[0.019]	[0.313]	[0.625]	[0.925]		[0.653]
Grant Only	0.241**	0.245***	0.318***	0.366***	0.435***	0.405***	0.347***	0.289***	0.222***	0.249***	0.254**
	(0.118)	(0.066)	(0.089)	(0.107)	(0.107)	(0.101)	(0.130)	(0.106)	(0.068)	(0.066)	(0.106)
	[0.041]	[0.000]	[0.000]	[0.001]	[0.000]	[0.000]	[0.008]	[0.006]	[0.001]	[0.000]	[0.016]
Mentored by Refugee × X	-0.002		-0.208	-0.184	-0.296**	-0.202	0.002	0.043	0.100	-0.040	0.078
	(0.158)		(0.146)	(0.146)	(0.147)	(0.143)	(0.162)	(0.153)	(0.162)	(0.105)	(0.151)
	[0.990]		[0.155]	[0.209]	[0.043]	[0.157]	[0.990]	[0.780]	[0.538]	[0.702]	[0.605]
Mentored by Refugee	0.125	0.122*	0.216**	0.225**	0.286**	0.229**	0.123	0.085	0.063	0.132	0.068
	(0.133)	(0.072)	(0.096)	(0.114)	(0.118)	(0.111)	(0.138)	(0.122)	(0.080)	(0.086)	(0.118)
	[0.349]	[0.092]	[0.025]	[0.049]	[0.015]	[0.038]	[0.370]	[0.489]	[0.428]	[0.127]	[0.562]
Mentored by Ugandan × X	0.069		-0.398***	-0.156	-0.311**	-0.300*	-0.024	0.097	0.189	-0.047	-0.318**
	(0.160)		(0.154)	(0.152)	(0.152)	(0.154)	(0.174)	(0.155)	(0.161)	(0.109)	(0.148)
	[0.667]		[0.010]	[0.304]	[0.041]	[0.052]	[0.893]	[0.531]	[0.242]	[0.664]	[0.032]
Mentored by Ugandan	0.059	0.101	0.269***	0.182	0.271**	0.255**	0.117	0.039	0.035	0.131	0.283***
	(0.132)	(0.075)	(0.096)	(0.122)	(0.119)	(0.126)	(0.148)	(0.126)	(0.086)	(0.092)	(0.106)
	[0.656]	[0.176]	[0.005]	[0.135]	[0.023]	[0.042]	[0.429]	[0.756]	[0.683]	[0.157]	[0.008]
X	-0.180		0.249**	0.255**	0.309***	0.194*	0.109	-0.078	-0.006		0.094
	(0.152)		(0.121)	(0.125)	(0.114)	(0.115)	(0.131)	(0.163)	(0.111)		(0.106)
	[0.235]		[0.039]	[0.041]	[0.007]	[0.092]	[0.404]	[0.631]	[0.958]		[0.377]
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 integration policies summary index. Each column title lists the dimension of baseline heterogeneity (X) that is analyzed in the regression. Results estimated through ANCOVA regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Expanded Treatment Effect Heterogeneity

	Integration Policies Index	Integration Policies Index	Integration Policies Index	Integration Policies Index
Labeled Grant × Pos. Economic	-0.280**	-0.329**		-0.306**
	(0.130)	(0.128)		(0.130)
	[0.032]	[0.010]		[0.019]
Labeled Grant × Pos. Cultural	-0.205		-0.260**	-0.180
	(0.130)		(0.129)	(0.130)
	[0.114]		[0.044]	[0.166]
Labeled Grant × High Well-Being		0.096	0.077	0.106
		(0.128)	(0.128)	(0.126)
		[0.453]	[0.548]	[0.401]
Labeled Grant	0.621***	0.502***	0.462***	0.575***
	(0.120)	(0.121)	(0.118)	(0.132)
	[0.000]	[0.000]	[0.000]	[0.000]
Information Only × Pos. Economic	-0.252*	-0.339***		-0.297**
	(0.135)	(0.131)		(0.133)
	[0.061]	[0.010]		[0.026]
Information Only × Pos. Cultural	-0.210		-0.287**	-0.200
	(0.136)		(0.135)	(0.136)
	[0.121]		[0.033]	[0.143]
Information Only × High Well-Being		0.116	0.115	0.119
		(0.130)	(0.131)	(0.128)
		[0.369]	[0.380]	[0.351]
Information Only	0.468***	0.353***	0.303**	0.421***
	(0.124)	(0.126)	(0.129)	(0.143)
	[0.000]	[0.005]	[0.019]	[0.003]
Grant Only × Pos. Economic	-0.305**	-0.361***		-0.316**
	(0.134)	(0.133)		(0.132)
	[0.023]	[0.006]		[0.017]
Grant Only × Pos. Cultural	-0.235*		-0.302**	-0.224*
	(0.133)		(0.134)	(0.133)
	[0.077]		[0.024]	[0.093]
Grant Only × High Well-Being		0.007	-0.023	0.004
		(0.129)	(0.130)	(0.127)
		[0.959]	[0.856]	[0.977]
Grant Only	0.532***	0.437***	0.408***	0.529***
	(0.122)	(0.121)	(0.118)	(0.134)
	[0.000]	[0.000]	[0.001]	[0.000]
Observations	3,051	3,051	3,051	3,051

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

Table A7: Heterogeneity in Treatment Effects on Integration Policies Index (Public Good Usage)

	Uses Hospitals	Children Go to School With Foreigners	Uses Hospitals Or Schools
Labeled Grant × X	0.138	-0.012	0.091
	(0.141)	(0.140)	(0.156)
	[0.329]	[0.932]	[0.560]
Labeled Grant	0.224**	0.326***	0.244*
	(0.102)	(0.092)	(0.133)
	[0.029]	[0.000]	[0.066]
Information Only × X	0.061	0.049	0.035
	(0.152)	(0.148)	(0.171)
	[0.688]	[0.739]	[0.838]
Information Only	0.145	0.168*	0.153
	(0.112)	(0.097)	(0.148)
	[0.195]	[0.084]	[0.299]
Grant Only × X	0.009	-0.133	-0.045
	(0.148)	(0.143)	(0.170)
	[0.950]	[0.353]	[0.792]
Grant Only	0.198*	0.251***	0.222
	(0.109)	(0.095)	(0.148)
	[0.068]	[0.009]	[0.134]
Mentored by Refugee × X	0.049	-0.029	0.058
	(0.171)	(0.163)	(0.180)
	[0.773]	[0.858]	[0.749]
Mentored by Refugee	-0.002	0.059	-0.012
	(0.128)	(0.107)	(0.150)
	[0.985]	[0.585]	[0.937]
Mentored by Ugandan × X	0.124	-0.168	-0.063
	(0.169)	(0.174)	(0.182)
	[0.465]	[0.334]	[0.729]
Mentored by Ugandan	-0.059	0.070	0.045
	(0.129)	(0.105)	(0.156)
	[0.645]	[0.504]	[0.773]
X	-0.042	0.114	0.020
	(0.112)	(0.107)	(0.127)
	[0.705]	[0.291]	[0.877]
Observations	2,499	2,503	2,503

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

Table A8: 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	-0.175	-0.067	-0.150			
	(0.132)	(0.121)	(0.120)			
	[0.185]	[0.578]	[0.212]			
Labeled Grant	0.047	-0.023	0.017	-0.065	-0.065	-0.064
	(0.110)	(0.077)	(0.086)	(0.060)	(0.060)	(0.060)
	[0.669]	[0.766]	[0.842]	[0.277]	[0.281]	[0.284]
Information Only × X	-0.186	-0.013	0.008			
	(0.137)	(0.130)	(0.128)			
	[0.173]	[0.922]	[0.953]			
Information Only	0.088	-0.025	-0.040	-0.038	-0.038	-0.039
	(0.112)	(0.086)	(0.096)	(0.064)	(0.064)	(0.063)
	[0.435]	[0.769]	[0.678]	[0.546]	[0.555]	[0.537]
Grant Only × X	-0.162	-0.004	-0.131			
	(0.141)	(0.128)	(0.127)			
	[0.249]	[0.973]	[0.302]			
Grant Only	0.073	-0.034	0.048	-0.041	-0.041	-0.040
	(0.119)	(0.083)	(0.097)	(0.064)	(0.064)	(0.063)
	[0.542]	[0.679]	[0.621]	[0.517]	[0.520]	[0.524]
Mentored by Refugee × X	-0.048	-0.068	-0.239*	0.040	-0.006	0.048
	(0.154)	(0.142)	(0.137)	(0.104)	(0.106)	(0.111)
	[0.753]	[0.632]	[0.081]	[0.700]	[0.957]	[0.664]
Mentored by Refugee	0.047	0.045	0.170*	0.004	0.025	-0.006
	(0.130)	(0.084)	(0.099)	(0.082)	(0.092)	(0.105)
	[0.719]	[0.594]	[0.087]	[0.962]	[0.788]	[0.955]
Mentored by Ugandan × X	-0.307**	0.155	-0.091	0.014	0.047	0.010
	(0.155)	(0.144)	(0.145)	(0.114)	(0.117)	(0.118)
	[0.048]	[0.283]	[0.533]	[0.903]	[0.688]	[0.933]
Mentored by Ugandan	0.090	-0.174*	-0.060	-0.124	-0.137	-0.124
	(0.127)	(0.091)	(0.111)	(0.089)	(0.093)	(0.083)
	[0.477]	[0.056]	[0.591]	[0.165]	[0.141]	[0.138]
X	-0.840***	0.084	0.065			
	(0.148)	(0.101)	(0.092)			
	[0.000]	[0.406]	[0.476]			
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 baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: More intensive refugee mentorship does not produce persistent impacts on policy views.

	9-Month Survey			16-Month Survey			28-Month Survey		
	Integration Policies Index	Economic Beliefs Index	Social Attitudes Index	Integration Policies Index	Economic Beliefs Index	Social Attitudes Index	Integration Policies Index	Economic Beliefs Index	Social Attitudes Index
Labeled Grant	0.329***	0.219**	0.036	0.376***	0.384***	0.228**	0.357***	0.230**	0.147
	(0.087)	(0.103)	(0.089)	(0.088)	(0.106)	(0.097)	(0.097)	(0.108)	(0.106)
	[0.000]	[0.033]	[0.687]	[0.000]	[0.000]	[0.019]	[0.000]	[0.033]	[0.163]
Information Only	0.148*	0.248**	-0.037	0.187**	0.245**	0.046	0.246**	0.094	0.182*
	(0.088)	(0.099)	(0.087)	(0.092)	(0.105)	(0.095)	(0.101)	(0.107)	(0.107)
	[0.091]	[0.013]	[0.672]	[0.042]	[0.020]	[0.627]	[0.015]	[0.379]	[0.089]
Grant Only	0.141	0.148	0.048	0.234***	0.261**	0.163*	0.284***	0.330***	0.221**
	(0.092)	(0.105)	(0.092)	(0.091)	(0.106)	(0.096)	(0.098)	(0.109)	(0.112)
	[0.126]	[0.158]	[0.604]	[0.010]	[0.014]	[0.089]	[0.004]	[0.002]	[0.048]
Standard Refugee Mentorship	0.193*	0.040	-0.062	-0.051	-0.098	-0.022	-0.027	-0.029	-0.053
	(0.107)	(0.129)	(0.124)	(0.109)	(0.137)	(0.121)	(0.132)	(0.134)	(0.147)
	[0.070]	[0.754]	[0.617]	[0.643]	[0.472]	[0.859]	[0.837]	[0.825]	[0.717]
Standard Ugandan Mentorship	0.133	0.105	0.065	0.086	0.030	-0.111	-0.074	0.134	0.095
	(0.110)	(0.135)	(0.106)	(0.118)	(0.132)	(0.121)	(0.132)	(0.136)	(0.140)
	[0.226]	[0.439]	[0.539]	[0.467]	[0.820]	[0.357]	[0.577]	[0.323]	[0.499]
Intensive Refugee Mentorship	0.553***	0.522***	0.101	0.221	-0.102	0.080	0.093	0.000	-0.113
	(0.155)	(0.186)	(0.151)	(0.175)	(0.192)	(0.189)	(0.206)	(0.203)	(0.246)
	[0.000]	[0.005]	[0.503]	[0.206]	[0.595]	[0.674]	[0.653]	[0.999]	[0.644]
Intensive Ugandan Mentorship	0.017	0.115	0.117	0.317**	0.222	0.225	-0.267	0.038	-0.042
	(0.162)	(0.174)	(0.153)	(0.132)	(0.176)	(0.148)	(0.223)	(0.202)	(0.199)
	[0.916]	[0.510]	[0.446]	[0.016]	[0.209]	[0.127]	[0.230]	[0.852]	[0.831]
Observations	1,109	1,070	1,119	1,041	1,000	1,041	901	892	901
Refugee = Ugandan (Standard)	0.609	0.672	0.310	0.270	0.408	0.508	0.761	0.288	0.385
Refugee = Ugandan (Intense)	0.008	0.079	0.934	0.620	0.173	0.504	0.205	0.887	0.811

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

Table A10: Impact of Child Labor Information Campaign

	Child Labor Attitudes Index^{+}	No Child Labor Under 15^{+}	No Child Labor Under 17^{+}
Grant Only	-0.077	-0.001	-0.060
	(0.096)	(0.047)	(0.050)
	[0.422]	[0.991]	[0.229]
Information Only	-0.008	-0.038	0.035
	(0.093)	(0.047)	(0.050)
	[0.930]	[0.422]	[0.485]
Observations	732	731	731
Control Mean	0.000	0.646	0.514
Grant = Info	0.559	0.522	0.123

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 A11: Within-Survey Priming Experiment

	Primed Outcomes Index	Have Money	Receive More Aid Than Needed	Can Support Themselves	Deserve Sympathy	Refugees Have Skills
Primed on Aid Received^{+}	-0.002	0.018	-0.031	0.007	0.019	0.009
	(0.061)	(0.033)	(0.034)	(0.032)	(0.032)	(0.033)
	[0.971]	[0.595]	[0.367]	[0.824]	[0.556]	[0.781]
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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Test not pre-specified denoted with $^{+}$.

Table A12: Full Set of Phone Campaign Outcomes

	Supported Phone Campaign^{+}	Opposed Phone Campaign^{+}	Answered Call^{+}
Labeled Grant	0.100***	-0.021	-0.006
	(0.038)	(0.019)	(0.034)
	[0.008]	[0.281]	[0.850]
Information Only	0.021	0.020	0.001
	(0.036)	(0.022)	(0.034)
	[0.555]	[0.345]	[0.966]
Grant Only	0.043	0.013	0.029
	(0.038)	(0.022)	(0.034)
	[0.258]	[0.543]	[0.407]
Mentored by Refugee	-0.012	0.003	0.025
	(0.042)	(0.022)	(0.039)
	[0.767]	[0.905]	[0.524]
Mentored by Ugandan	-0.026	0.034	0.019
	(0.042)	(0.026)	(0.038)
	[0.537]	[0.199]	[0.613]
Observations	1,406	1,406	1,406
Control Mean: Follow-Ups	0.230	0.060	0.804
Labeled Grant = Info Only	0.037	0.044	0.814
Labeled Grant = Grant Only	0.157	0.113	0.312
Labeled Grant = R-Mentee	0.010	0.266	0.416
R-Mentee = Info Only	0.420	0.447	0.539
R-Mentee = U-Mentee	0.773	0.264	0.895

Results estimated through OLS regression with baseline controls selected through double-lasso. Standard errors clustered at the enterprise level in parentheses. Brackets and the last five rows display two-sided p-values. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Outcomes that were not pre-specified are 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 refugee integration policies.
- Refugee mentorship will increase support for refugee integration policies.
- Labeled grants will increase business profits.
- Refugee mentorship will increase business profits.

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

Table A13: Westfall-Young Stepdown-Adjusted p-Values for Primary Hypotheses

	Integration Policies 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B Additional Details on Research Design

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

B.1 Additional Sampling Details

During the listing survey in October of 2019, we surveyed all tailors and hair salons within 10 kilometers of the Kampala city center.[49] We surveyed either the owner of the business or a manger who retains most of the profits.

For the baseline survey in November 2019 through January 2020, we selected a subset of the 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 mentors to three years for male and female salon owners and female tailors, and kept the six year requirement for male tailors. After forming our sample of potential mentors, we observed that the sample was already largely balanced across nationality groups. However, there was a greater number of highly experienced Ugandan potential mentors. We therefore dropped 15 Ugandan potential mentors with 6–10 years of experience, choosing these 15 who had the greatest Mahalanobis distance (defined along business profit, business capital, age, and years of education) compared to refugee mentors with the same level of experience. This produced an equal number of eligible refugee and Ugandan mentors who are largely balanced on these characteristics (see Appendix Table B3).

We chose to 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.[50] 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.

[49]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.

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

B.2 Tests of Balance and Selective Attrition

Tables B1, B3, and B5 respectively present tests of randomization balance within the experimental sample, mentor characteristic balance across refugees and Ugandans, and a test of differential attrition within the experimental sample. Tables B6, B7, B8, and B9 present results from the main text applying inverse probability weights to account for differential attrition. Tables B10 and B11 present Lee Bounds on treatment impacts for each pre-specified domain (across two tables).

Table B1: Randomization Balance

	(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
Cultural 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: Cultural 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: Cultural 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: Randomization Balance (Among Non-Attriters)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Labeled Grant	Grant Only	Information Only	Mentored by Refugee	Mentored by Ugandan	Control	Joint p-Value
Age (Years)	27.27	28.09	27.52	27.19	27.53	27.67	0.55
Education (Years)	10.97	10.42	10.70	10.63	11.15	10.84	0.31
Experience in Sector (Years)	2.52	2.51	2.46	2.26	2.32	2.28	0.22
Profit (USD/Month)	37.20	37.21	34.46	37.04	36.00	38.29	0.80
Has Any Employees	0.22	0.22	0.27	0.19	0.17	0.26	0.76
Aware of Aid-Sharing	0.23	0.18	0.17	0.22	0.22	0.19	0.56
Supports Refugee Hosting	0.73	0.72	0.69	0.71	0.82	0.73	0.04
Supports More Refugees	0.54	0.55	0.49	0.50	0.57	0.48	0.27
Supports Freedom of Movement	0.56	0.60	0.62	0.54	0.53	0.56	0.66
Supports Right to Work	0.65	0.61	0.58	0.63	0.61	0.60	0.51
Refugees Increase Rents	0.79	0.79	0.78	0.76	0.79	0.81	0.96
Refugees Increase Goods Prices	0.61	0.65	0.64	0.62	0.57	0.62	0.90
Refugees Worsen Public Goods	0.25	0.31	0.29	0.33	0.26	0.27	0.66
Refugees Economic Effect is Positive	0.52	0.55	0.57	0.52	0.47	0.51	0.49
Integration Policies Index	0.04	0.06	-0.00	-0.04	0.05	-0.04	0.61
Knowledge Index	0.25	0.04	0.04	0.17	0.07	-0.02	0.07
Economic Beliefs Index	-0.04	-0.11	-0.00	0.02	-0.10	0.01	0.60
Economic Perceptions Index	-0.05	-0.01	-0.04	0.08	0.13	0.05	0.70
Economic Perceptions Index	0.10	0.00	0.10	0.28	0.05	0.04	0.20
Social Attitudes Index	-0.01	0.15	-0.02	-0.08	0.07	-0.03	0.23
Contact Refugees by Choice Index	-0.02	0.02	0.01	0.10	0.13	-0.05	0.93
Contact Refugees by Circumst. Index	-0.08	0.14	0.05	0.09	0.00	0.06	0.27
Business Practices Index	-0.02	-0.06	0.04	-0.01	-0.07	0.07	0.81
Household Well-Being Index	0.06	-0.06	-0.07	-0.07	-0.04	0.02	0.65
General Policy Index	0.23	0.05	0.10	0.16	-0.02	0.04	0.14
Foreigners: Economic Beliefs Index	0.05	0.06	0.07	0.14	-0.06	-0.01	0.64
Foreigners: Social Attitudes Index	-0.01	0.06	0.15	-0.01	0.14	0.02	0.49
Other Tribes: Contact Index	-0.07	0.03	0.09	0.04	-0.11	-0.03	0.55
Other Tribes: Economic Beliefs Index	-0.04	-0.11	0.00	-0.02	0.13	-0.03	0.48
Other Tribes: Social Attitudes Index	0.05	0.17	0.05	0.05	0.03	0.04	0.54
Gender Role Index	0.04	0.23	-0.05	0.18	0.06	0.04	0.28

Sample includes all baseline individuals who were surveyed in at least one follow up round. 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 B3: Balance of Ugandan and Refugee Mentor Characteristics

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

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

Table B4: Test for Differential Attrition (Ever Surveyed)

	Ever Surveyed^{+}
Labeled Grant	0.002
	(0.024)
	[0.925]
Information Only	-0.039
	(0.026)
	[0.126]
Grant Only	0.013
	(0.024)
	[0.583]
Mentored by Refugee	-0.010
	(0.027)
	[0.713]
Mentored by Ugandan	0.001
	(0.026)
	[0.965]
Observations	1,406
Mean	0.913
Joint Orthogonality p-Value	0.464

Outcome is whether the individual was surveyed in any post-baseline survey round. Results estimated through linear regression controlling for a randomization-stratum fixed effect. Robust standard errors in parentheses. Brackets and the last five rows display two-sided p-values. Outcomes not pre-specified are denoted with $^{+}$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B5: Test for Differential Attrition (Round-by-Round)

	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

Outcome is whether the individual was surveyed in a given post-baseline survey round. Results estimated through ANCOVA regression controlling for randomization-stratum and survey-wave fixed effects. Standard errors clustered at the enterprise level in parentheses. Brackets and the last five rows display two-sided p-values. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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

	Supports Refugee Hosting	Supports More Refugees	Supports Right to Work	Supports Freedom of Movement	Integration Policies Index	Supported Phone Campaign
Labeled Grant	0.131***	0.147***	0.129***	0.065**	0.355***	0.100***
	(0.025)	(0.031)	(0.027)	(0.031)	(0.065)	(0.038)
	[0.000]	[0.000]	[0.000]	[0.034]	[0.000]	[0.008]
Information Only	0.056**	0.097***	0.083***	0.036	0.222***	0.018
	(0.028)	(0.031)	(0.028)	(0.032)	(0.067)	(0.036)
	[0.043]	[0.002]	[0.003]	[0.259]	[0.001]	[0.623]
Grant Only	0.089***	0.127***	0.094***	0.012	0.249***	0.044
	(0.028)	(0.032)	(0.028)	(0.031)	(0.066)	(0.038)
	[0.001]	[0.000]	[0.001]	[0.694]	[0.000]	[0.248]
Mentored by Refugee	0.028	0.052	0.074**	-0.025	0.111	-0.004
	(0.032)	(0.036)	(0.031)	(0.038)	(0.074)	(0.042)
	[0.374]	[0.144]	[0.019]	[0.504]	[0.134]	[0.915]
Mentored by Ugandan	0.058*	0.036	0.015	-0.072*	0.084	-0.024
	(0.031)	(0.037)	(0.034)	(0.037)	(0.077)	(0.042)
	[0.061]	[0.326]	[0.654]	[0.053]	[0.275]	[0.557]
Observations	3,040	3,038	3,039	3,031	3,051	1,406
Control Mean: Baseline	0.726	0.515	0.600	0.599	0.029	.
Control Mean: Follow-Ups	0.746	0.605	0.717	0.540	-0.000	0.230
Labeled Grant = Info Only	0.001	0.076	0.062	0.324	0.026	0.029
Labeled Grant = Grant Only	0.072	0.486	0.159	0.075	0.073	0.167
Labeled Grant = R-Mentee	0.000	0.005	0.051	0.014	0.000	0.017
R-Mentee = Info Only	0.370	0.187	0.750	0.106	0.115	0.599
R-Mentee = U-Mentee	0.366	0.666	0.084	0.257	0.730	0.668

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

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

	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	0.129***	0.145***	0.160***	0.092**	0.100**	0.298***
	(0.016)	(0.033)	(0.035)	(0.036)	(0.041)	(0.071)
	[0.000]	[0.000]	[0.000]	[0.010]	[0.015]	[0.000]
Information Only	0.068***	0.050	0.116***	0.056	0.014	0.212***
	(0.014)	(0.033)	(0.035)	(0.035)	(0.042)	(0.069)
	[0.000]	[0.126]	[0.001]	[0.103]	[0.731]	[0.002]
Grant Only	0.087***	0.091***	0.100***	0.108***	0.037	0.216***
	(0.015)	(0.034)	(0.037)	(0.037)	(0.044)	(0.072)
	[0.000]	[0.007]	[0.006]	[0.003]	[0.401]	[0.003]
Mentored by Refugee	0.039**	-0.028	0.038	-0.038	0.019	0.079
	(0.016)	(0.036)	(0.040)	(0.038)	(0.048)	(0.077)
	[0.015]	[0.437]	[0.339]	[0.314]	[0.697]	[0.306]
Mentored by Ugandan	0.048***	0.020	0.039	0.057	0.011	0.078
	(0.016)	(0.038)	(0.040)	(0.039)	(0.046)	(0.079)
	[0.002]	[0.589]	[0.327]	[0.151]	[0.814]	[0.323]
Observations	3,061	3,061	2,787	2,906	1,671	3,003
Control Mean: Baseline	0.000	0.173	0.503	0.409	0.511	0.026
Control Mean: Follow-Ups	0.024	0.369	0.423	0.443	0.416	-0.000
Labeled Grant = Info Only	0.002	0.004	0.182	0.281	0.033	0.199
Labeled Grant = Grant Only	0.040	0.110	0.082	0.658	0.133	0.244
Labeled Grant = R-Mentee	0.000	0.000	0.001	0.000	0.077	0.004
R-Mentee = Info Only	0.128	0.030	0.040	0.008	0.930	0.071
R-Mentee = U-Mentee	0.622	0.225	0.982	0.018	0.878	0.986

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

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

	Comfortable Refugee Friends	Comfortable Refugee Spouse	Prop. Donated Refugees	Pos Effect Culture	Deserve Sympathy	Social Attitudes Index
Labeled Grant	0.069***	0.125***	0.045***	-0.002	0.029	0.166**
	(0.026)	(0.039)	(0.015)	(0.033)	(0.040)	(0.067)
	[0.009]	[0.001]	[0.003]	[0.950]	[0.469]	[0.013]
Information Only	0.062**	0.070*	-0.001	0.052*	0.029	0.067
	(0.027)	(0.040)	(0.016)	(0.031)	(0.040)	(0.066)
	[0.022]	[0.081]	[0.944]	[0.095]	[0.468]	[0.307]
Grant Only	0.053*	0.074*	0.042***	-0.028	0.079*	0.129*
	(0.027)	(0.041)	(0.016)	(0.034)	(0.041)	(0.067)
	[0.050]	[0.070]	[0.008]	[0.413]	[0.055]	[0.056]
Mentored by Refugee	0.004	0.056	-0.020	0.020	-0.030	-0.032
	(0.034)	(0.047)	(0.018)	(0.038)	(0.047)	(0.074)
	[0.905]	[0.231]	[0.284]	[0.604]	[0.519]	[0.668]
Mentored by Ugandan	0.033	0.014	-0.001	0.049	-0.026	0.025
	(0.032)	(0.046)	(0.019)	(0.035)	(0.045)	(0.074)
	[0.307]	[0.761]	[0.966]	[0.156]	[0.556]	[0.738]
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	0.044
Control Mean: Follow-Ups	0.817	0.486	0.284	0.690	0.540	0.000
Labeled Grant = Info Only	0.775	0.158	0.001	0.081	0.998	0.108
Labeled Grant = Grant Only	0.495	0.213	0.809	0.445	0.211	0.551
Labeled Grant = R-Mentee	0.040	0.135	0.000	0.565	0.188	0.006
R-Mentee = Info Only	0.072	0.771	0.289	0.382	0.190	0.162
R-Mentee = U-Mentee	0.418	0.410	0.345	0.449	0.940	0.461

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

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

	Business Profits (USD/Month)	Business Capital (USD)	Business Practices Index	Household Well-Being Index
Labeled Grant	-3.22	-57.2	0.026	0.042
	(2.44)	(44.9)	(0.079)	(0.062)
	[0.187]	[0.203]	[0.744]	[0.499]
Information Only	-0.60	16.7	-0.022	-0.052
	(2.67)	(49.2)	(0.079)	(0.066)
	[0.824]	[0.734]	[0.783]	[0.434]
Grant Only	-2.15	7.91	0.11	0.032
	(2.65)	(47.7)	(0.074)	(0.066)
	[0.418]	[0.868]	[0.121]	[0.629]
Mentored by Refugee	0.98	-37.1	0.054	-0.036
	(2.89)	(51.0)	(0.090)	(0.079)
	[0.734]	[0.467]	[0.546]	[0.651]
Mentored by Ugandan	-2.46	12.5	0.100	0.10
	(2.81)	(53.7)	(0.081)	(0.069)
	[0.380]	[0.816]	[0.216]	[0.135]
Observations	4,029	2,819	1,942	4,132
Control Mean: Baseline	39.606	495.556	0.048	-0.033
Control Mean: Follow-Ups	20.685	632.539	0.000	0.000
Labeled Grant = Info Only	0.266	0.100	0.537	0.087
Labeled Grant = Grant Only	0.642	0.139	0.226	0.850
Labeled Grant = R-Mentee	0.114	0.676	0.752	0.271
R-Mentee = Info Only	0.587	0.299	0.395	0.822
R-Mentee = U-Mentee	0.260	0.367	0.613	0.060

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

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

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

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

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

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

Each cell shows a 95% confidence interval for an upper or lower Lee bound. Lee bounds estimated using only the control group and one treatment group. 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 paused on March 20, 2020 due to COVID-19. At the time of the suspension, YARID had visited: 82% of Information Only, 75% of Grant Only and Labeled Grant for the first meeting to explain the program and 33% of those groups for the second meeting to disburse the grant, and 83% of the mentorship treatment arms. Seventy-two percent of the mentorship pairs met at least once, with 23% of those having met all six times. Table B12 presents tabulations of actual treatment status (defined as receiving the grant in Grant Only and Labeled Grant, receiving the information in Information Only, and having at least one mentorship meeting in Refugee and Ugandan Mentorship). Table B13 shows the number of mentorship meetings held by year across Refugee and Ugandan Mentorship arms.

Table B12: Assignment and Actual Treatment Status

	Labeled Grant	Grant Only	Information Only	Mentored by Refugee	Mentored by Ugandan	Control
Assigned	280	237	287	169	168	265
Treated	230	184	257	133	135	.
Percentage	82	78	90	79	80	.

Each cell shows the number of respondents who were assigned to, and actually treated with, a given treatment arm.

Table B13: Facilitated Mentorship Meetings

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

Source: YARID Administrative data

B.4 Information Only Script

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

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

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

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

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

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

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

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

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

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

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

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

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

B.5 Labeled Grant Script

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

[**IDENTICAL EMPATHETIC LISTENING ACTIVITY HERE**]

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

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

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

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

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

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

B.6 Grant Only Script

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.7 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.8 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 and Information Only groups
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.