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Can Redistribution Change Policy Views?

Aid and Attitudes toward Refugees

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

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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 2023, more than 42 million refugees and asylum seekers were residing outside their country of origin (UNHCR, 2024*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 atrophy (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 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]

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

[1] Examples of redistributions of policy gains include the Trade Adjustment Assistance program, the H-1B Skills Training Grants, which use visa fees to fund training programs for US citizens, and compensation for residents living near major industrial facilities.

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

[3] Sixty-six percent of refugees live in protracted situations of at least five years (UNHCR, 2024*a*), while 71% of 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 potential medium-term effects of aid: the return to skills, for instance, is higher when refugees are eligible for formal jobs.

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, Huang and Graham, 2018, Verme and Schuettler, 2021, Dhingra, Kilborn and Woldemikael, 2021, Bahar, Ibáñez and Rozo, 2021, Ginn, 2023). 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 increases policy support. We offered these programs to native micro-entrepreneurs 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. Micro-entrepreneurship 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 arm as *Information Only*. 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 to this arm as *Labeled Grant*. 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, 2024).[6] We therefore designed a third program facilitating contact

[4]We use *host community* to describe native-born individuals living in the same country or area as refugees, consistent with humanitarian terminology.

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

[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

between Ugandans and refugees in the form of business mentorship pairings. This treatment arm tests a variant of the contact hypothesis, which is frequently applied in practice with displaced populations (Zhou and Lyall, 2024, Loiacono and Silva-Vargas, 2023). It also serves as a benchmark, allowing us to compare our two economically motivated interventions with one motivated by cultural concerns.

Our experiment included three additional comparison arms to isolate mechanisms. First, we offered a business grant that was not bundled with information on aid-sharing or integration policies in order to isolate any impacts of receiving 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.

Our sample in urban Uganda, however, exhibits relatively high levels of support for refugees at baseline. We conducted a second experiment in rural Kenya—where opposition to refugee integration is greater—in which we distributed smaller grants of about \$7.50.[7] Kenya also hosts a large number of refugees but does not implement a similar aid-sharing bargain, and refugees' labor market access and freedom of movement are significantly restricted. Labeled Grant recipients in Kenya watched a short video explaining that they are receiving the grant because Kenya hosts refugees, and that future aid-sharing between refugees and Kenyans could be possible with increased freedom of movement and labor market access for refugees. We also implemented a grant arm with no information about refugees and a control arm to study mechanisms.

We find that labeled grants 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. We also find large effects of labeled grants on support for refugee integration in Kenya, providing initial evidence that compensation programs of relatively small amounts can influence views even where opposition is high. Receiving information about Uganda's aid-sharing policy, but no grant, created similar but smaller impacts. Receiving an unlabeled business grant also increased support for integration policies in Uganda and Kenya, but by less than a labeled grant.

Do the impacts on self-reported views translate into changes in actual political behavior?

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.

[7] When referring to the Kenya experiment throughout the paper, we note the setting explicitly. If neither country is specified, we are describing the main experiment in Uganda.

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 in Uganda 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. Impacts of mentorship by a refugee on policy views were significant but small on average after nine months and did not persist. These findings suggest that short-term cooperative inter-group contact has less persistent impacts on policy views than direct aid programs explicitly connected to the refugee presence. While our results do not imply that contact of a different nature—such as friendship—would not change views, they are relevant for the many programs that attempt to improve inter-group relations through similar contact-based programs (Paluck, Green and Green, 2019).

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 are most consistent with a credibility channel: the direct transfer makes the accompanying information about aid-sharing more believable by demonstrating it visibly. Our findings also suggest that receiving aid *per se*, even without information about aid-sharing, impacted views by reducing resentment against groups such as refugees perceived to be major beneficiaries of aid. We argue that this reduction in resentment against refugees, together with an association between the grant and the refugee-led implementing organization, explains the effects of unlabeled grants. We build a model to disentangle these channels from a pure wealth effect and find that the role of wealth effects is small. This result is consistent with the large impacts of labeled grants in Kenya, which were small in value.

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 inter-group 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 further test for experimenter demand effects, we implemented a placebo campaign that shared the implementing organization's position on an unrelated 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. In Kenya, we conducted a demand-elicitation exercise following the design of De Quidt, Haushofer and Roth (2018). This activity identifies "demand-free lower bound" treatment impact estimates, which we find to be large and positive for each policy outcome we analyze. 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.

Overall, our findings indicate that redistributing potential surplus can be an effective tool to build political support for policies that create perceived winners and losers, especially when the connection between the policies and the transfers is clear. 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

[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 inter-group relations.

about crowd-out can consider combining integration policies with aid redistribution,[10] 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.[11] The literature on political responses to immigration has largely focused on, and distinguished between, hosts' economic and cultural concerns (Alesina and Tabellini, 2024). 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). While existing work has documented that learning about redistribution policies changes support for free trade among low-income respondents in the short-run (Ehrlich and Hearn, 2014), little is known about the mechanisms underlying this effect, whether it persists, or its applicability to policy views where non-economic concerns play a substantial role like immigration.[12] Our paper investigates the role of redistribution—and the mechanisms underlying it—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-

[10] 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.

[11] 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."

[12] Ehrlich and Hearn (2014) find no impact on support for free trade on average, driven by an increase (decrease) in support among low-income (high-income) respondents. In related work, 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.

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, 2024). Studies of inter-group 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, 2023). Our study shows that economic policy can decrease the perceived social distance between hosts and refugees and reduce measures of 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 social cohesion. Inflows of resources to refugees can create "resource resentment" among hosts, a phenomenon documented in a wide range of contexts (Adato et al., 2015, Pavanello et al., 2016, Zhou, 2019). In contrast, Lehmann and Masterson (2020) find that aid to refugees in Lebanon reduced violence toward refugees, possibly through indirect benefits to natives. Four papers study the effects of transfers to both refugees and natives on social cohesion. In Uganda, Baseler et al. (2024) reproduced the results of labeled grants while delivering the information through a video. In Mozambique, Beltramo et al. (2024) find that transfers from the UN refugee agency led to higher levels of social cohesion but do not study the role of beliefs about the source of the transfers. In Ecuador, Valli, Peterman and Hidrobo (2019) find no impacts from transfers on a broad measure of social cohesion, but do not analyze attitudes toward refugees specifically or inform natives whether the transfers were part of the refugee aid response. Quattrochi et al. (2021) similarly find no impact of aid on general social cohesion in the Democratic Republic of Congo.[13] A potential explanation of the latter two findings, in light of our results, is that the connection between the transfers and the refugee presence was not clear to natives. Zhou, Grossman and Ge (2023) find no effect of refugee presence—together with associated public goods improvements—on attitudes toward migrants in Uganda, but do not identify the impact of transfers conditional on refugee presence.[14] Our study builds on this literature by identifying

[13]Both papers analyze a generalized measure of social cohesion: Valli, Peterman and Hidrobo (2019) incorporate outcomes like "I trust people," "Participation in community association or political group," and "Xenophobia is not an issue," while Quattrochi et al. (2021) analyze outcomes like membership in village associations, theft, and trust. Moreover, in both settings to our knowledge, recipients were not informed whether the assistance was part of the aid response for the displaced. In that framing, natives may perceive that refugees are taking assistance that would otherwise be allocated to them.

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

both the impact of compensation programs for natives and the effect of explicitly linking the transfers with the broader policy bargain.

Our work also contributes to a large literature on the effects of inter-group contact on attitude formation. Expanding on the seminal work by Allport (1954), Mousa (2020), Lowe (2021), Corno, La Ferrara and Burns (2022), and Bursztyn et al. (2024) find that contact can reduce prejudice.[15] In contrast, contact-based programs had few impacts on Israeli Jews' views of Palestinians (Enos and Gidron, 2018) or Afghans' views of internally displaced people (Zhou and Lyall, 2024). On average, interventions targeting ethnic or racial prejudice generate weaker impacts on attitudes: see Paluck, Green and Green (2019) for a meta-analysis. Our study builds on this literature by comparing a collaborative contact program to programs focusing on economic interventions.

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.

2.1 Refugee Policies in Uganda

With over 1.6 million refugees, Uganda hosts the largest population of refugees in Africa, and the sixth largest globally (UNHCR, 2024*c*). The majority of refugees live in one of 11 rural settlements, where they receive assistance from humanitarian actors. Kampala, the capital city and the site of our study, hosts about 147,000 registered refugees, though the unofficial number is likely significantly higher.[16]

[15] 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. Also in Uganda, Betts et al. (2023) find a positive correlation between interactions with refugees and positive perceptions toward refugees.

[16] The official count represents 9% of Uganda's refugee population, and 8% of the Kampala population (UNHCR, 2024*c*, UBOS, 2024).

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). Congolese refugees are largely socially and economically segregated from Ugandan society, despite significant spatial integration (Betts et al., 2017, Monteith and Lwasa, 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 Bargain. Under Ugandan policy, 30% of international aid budgets for refugees is shared with Ugandan host communities (UNHCR, 2018). This policy is in line with the global Comprehensive Refugee Response Framework—a component of the Global Compact on Refugees, adopted by the United Nations General Assembly in 2018—under which a portion of aid for the refugee response is directed to the hosts, and refugees are granted the right to access labor, housing, and education markets. 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.[17] 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.

There is no centralized framework governing how aid organizations must spend redirected aid. Aid-sharing thus takes the form of both direct assistance, such as cash grants to Ugandans, and public goods investment, such as funding schools and hospitals in areas where both refugees and Ugandans live.[18]

2.2 Attitudes Toward Refugees in Uganda

Ugandans' views toward hosting refugees are mixed. While a majority generally supports current policies, a significant minority expresses concerns about the economic burden, labor market competition effects, or security threat of hosting refugees (IRC, 2018*b*). Many Ugandans support continued humanitarian assistance to refugees; however, opinions are divided on allowing refugees to work or move freely within the country. Uganda ranks 72nd out of 139 countries—close to the median—on Gallup's 2016 Migrant Acceptance Index (Esipova,

[17] 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).

[18] One large actor in this space is the International Rescue Committee (IRC), which operates both cash transfer and public goods programs serving refugees and Ugandans (see, for example, here). Many other organizations implement large cash transfer programs in Uganda, including UNHCR, WFP, UNICEF, and the Ugandan government. Some of these specifically target micro-entrepreneurs.

Fleming and Ray, 2017). As we discuss in Section 3.6, this division in Ugandan public opinion mirrors attitudes documented within our sample, in which we observe high support for hosting refugees in general, but mixed opinions on allowing refugees to work or move freely.

2.3 Refugee Policies and Attitudes in Kenya

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

3 Experimental Design

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

3.1 Sample Selection

We drew our experimental sample from the population of owner-operators of tailor or salon businesses within 10 kilometers of the Kampala city center, which we listed in a censusing exercise described in Appendix B.1. We chose Ugandan micro-entrepreneurs who were no older than 40, had no more than five years of experience in their sector, and who spoke Luganda, English, or Swahili conversationally for inclusion in the experimental sample. We excluded businesses with five or more employees or very high profits or capital. This produced a set of 1,406 micro-entrepreneurs who form our experimental sample.

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

3.2 Data Collection Timeline

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

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

3.3 Interventions in Uganda

We implemented three main interventions to test the impact of aid redistribution on policy preferences and beliefs. Our interventions were carried out by Young African Refugees for Integral Development (YARID), a refugee-led non-profit in Kampala that employs and implements livelihoods and education projects for both refugees and Ugandans. In addition to cash grants, YARID offers services like job training, job placement, English, and computer literacy classes. Before this project, YARID did not explicitly link its assistance programs for Ugandans to the government's aid-sharing policy or conduct information campaigns about refugees targeted to the general public, but did so randomly for the purpose of this research. Figure 1 summarizes our sample selection and treatment assignment process. Appendix B.3

provides details on uptake, which was at least 79% in each treatment arm.

Information Only. The first intervention provided information about Uganda's existing aid-sharing policy, which stipulates that 30% of foreign aid to refugees be shared with the host community through direct transfers or public good provision such as hospitals and schools that Ugandans can access. The script included a specific example of a hospital in Kampala funded partly by international aid for refugees. Participants were visited by a refugee or Ugandan staff member. The script outlined the policy bargain, linking aid-sharing—and the potential benefits to the respondent—with policies that allow refugees to integrate, as the following excerpt shows (full scripts are available in Appendix B.5):

> *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 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.

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.[19] Staff explained that the grant was an example of aid-sharing: we therefore refer to this treatment as the Labeled Grant arm. A YARID staff member first visited the business owner to inform them about the grant and deliver the information. During a second meeting, the staff member paid directly for business expenses at a shop of the business owner's choosing. In the first wave of disbursements before COVID-19, we required that at least 60% of the grant be used for business purposes motivated by

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

the findings of Fafchamps et al. (2014) on similar in-kind transfers. The remaining balance was disbursed through mobile money.

Mentorship by a Refugee. The third intervention was a mentorship program that matched business owners with experienced refugee business owners in the same sector.[20] 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).[21] Many business owners in our sample report little contact with refugees at baseline: when asked to name four people they talk to most about business, 85% of owners named only other Ugandans, and 74% reported contacting zero refugees for social reasons in the past month, suggesting that there is considerable scope for additional contact to change views toward refugees.

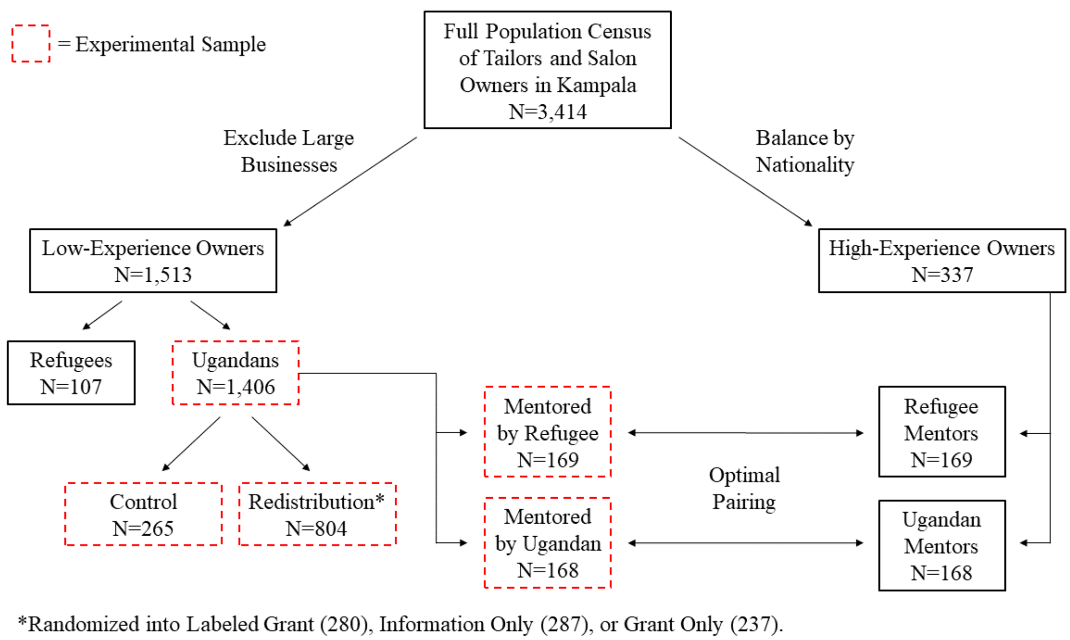
Comparison Arms. In addition to our three main interventions, we included two additional treatment arms and a control group to distinguish mechanisms behind treatment impacts. The first provided a business grant identical to the labeled grant, but delivered by a Ugandan staff member without any information about refugees or Uganda's aid-sharing policy, which we refer to as *Grant Only* 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 more experienced refugees from other impacts of the mentorship program. We assigned only Ugandan staff members to facilitate the Grant Only and Mentorship by Ugandan treatment arms; other treatment arms were facilitated by both Ugandan and refugee staff members. Finally, a pure control group did not receive any treatment and was not contacted by YARID.

COVID-19 Disruptions. Interventions were implemented in-person beginning in January 2020. Due to COVID-19, we paused interventions in March 2020 and restarted all

[20]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.

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

Figure 1: Summary of Study Design



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

treatments remotely in March 2021. At the time of the pause, most business owners had been visited once to inform them of their treatment assignment, but only one-third of grants had been disbursed.[22] We converted mentorship meetings from in-person to remote when they resumed. YARID provided up to four facilitated mentorship meetings using three-way calling, regardless of the number of meetings that were held prior to our pause.

Given that our first follow-up survey was completed before interventions were finished, estimates from our first survey round represent intent-to-treat effects comprising both program effects and anticipation effects. Our remaining three surveys were completed after programs had finished. The switch from in-person to remote mentorship implies that our mentorship arms estimate an average intent-to-treat effect of in-person and remote meetings. In Section 5.3, we consider and reject that the transition to remote mentorship explains the low impacts of refugee mentorship. Finally, the disbursement of grants around COVID-19 complicates attempts to generalize our effect sizes to more typical economic conditions, a point we return to in Section 6.

3.4 Randomization

Within our experimental sample of 1,406 inexperienced Ugandan business owners, we assigned participants randomly to one of six treatment conditions: Control, Grant Only, Information Only, Labeled Grant, Mentored by Refugee, or Mentored by Ugandan. Randomization was conducted using the Stata command *randtreat* within strata defined by gender, sector, and mentor eligibility,[23] and, within each of these cells, median profits and median attitudes towards hosting, computed as the first principal component of support for seven integration policies. We chose treatment probabilities within stratum based on the number of available refugee mentors in that gender-sector cell, and set the probability of assignment to the Ugandan mentorship arm to be equal to that of the refugee-mentorship arm. The remaining sample was divided roughly equally between Labeled Grant, Information Only, Grant Only, and Control. Mentees were paired to mentors within gender-sector cells to minimize within-pair travel distance using a greedy matching algorithm.

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

3.5 Empirical Strategy

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

3.5.1 Estimating Equations

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

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

where y_{it} is an outcome for individual i measured at time t, with $t = 0$ corresponding to

[22]Labeled grant recipients who received their grants before the pause were contacted with a refresher script by YARID when activities resumed.

[23]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.

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-stratum fixed effect; Phone_{it} is a dummy indicating whether the survey was completed over the phone (as we attempted to survey any respondents who relocated outside Kampala over the phone); Date_{it} is a linear date-of-survey control; and ϵ_{it} is an error term. Standard errors are clustered at the individual level. We run separate lassos for each dependent variable using the Stata package *pdslasso* (Ahrens, Hansen and Schaffer, 2019) and include all possible controls from the baseline in each. Our treatment effects of interest are given by the coefficient vector β_j and represent the average difference in outcome y_{it} between each treatment group and the control group, across individuals and post-treatment survey rounds, conditional on included controls.

Interpretation of Pairwise Comparisons. Throughout the paper, we focus primarily on treatment impacts of individual arms, relative to Control, or pairwise comparisons between them. Treatment impacts relative to Control are directly informative of programs that could operate at scale. Specifically, the Labeled Grant arm approximates direct transfers to natives with explicit labeling. The Information Only arm approximates awareness campaigns to indirect beneficiaries. The Grant Only arm approximates direct transfers to natives without explicit labeling by a humanitarian organization, which in places that host refugees are often closely associated with refugees themselves or with supporting refugee integration.

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.[24]

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. To reduce survey length, not all outcomes were measured in all surveys; the number of observations may therefore 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

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

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 the family-wise error rate in Appendix Table A15. 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 E, which can be accessed here.

3.6 Summary Statistics

Appendix Table A2 displays summary statistics for our experimental sample of 1,406 Ugandan micro-enterprise owners. The average owner was 28 years old with 11 years of education and 2.4 years of experience running a business in their sector. About two-thirds of owners are women, and tailors and salons are roughly equally represented. Their businesses earned an average of USD 37 per month, and about one-fifth of businesses had any employees.[25]

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

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

3.7 Study Design in Kenya

In Kenya, we selected 7,078 households across 235 villages sampled as part of Barnett-Howell, Baseler and Ginn (2023), a separate project unrelated to refugees. Our settings in Kenya and Uganda differ along several dimensions. First, the *status quo* policies on refugees' work and movement are more restrictive in Kenya than in Uganda. Second, support for policies like these to integrate refugees is lower in the Kenyan than the Ugandan sample. Both points are discussed in Section 2. Third, the Kenyan sample lives in predominantly rural areas,

[25] 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.

compared to the Ugandan sample which lives in the capital city. Fourth, while approximately 8% of residents in Kampala are refugees, few refugees live in the sampled Kenyan counties.

We assigned 50 Kenyan villages to receive grants and 185 villages to a control condition, stratifying on county. Within villages assigned to receive grants, households were evenly randomized to a Grant Only arm or a Labeled Grant arm, stratifying on county and age of the respondent. Households in the Grant Only arm received 1,000 KSh ($7.50) labeled as generic support. Those in the Labeled Grant arm received the same grant and watched a short video made by a refugee-led non-profit, RELON Kenya, explaining that they are receiving the grant because Kenya hosts refugees, and that future aid-sharing between refugees and Kenyans could be part of a national policy bargain for increased freedom of movement and labor market access for refugees (see Appendix B.9 for scripts).

We pre-specified the following design and analysis (Baseler and Ginn, 2024). Data on support for refugee integration were collected shortly after the grants were announced. To minimize the effect of spillovers between the Grant Only and Labeled Grant groups, we include in our estimation sample only households surveyed on the first visit day in each village. This produces a final sample of 5,264 households. We estimate treatment impacts on policy views using (1), excluding y_{i0}, M_{i0}, X_i, θ_t, Date_{it}, and Phone_{it}. For hypothesis tests involving comparisons between either Labeled Grant or Grant Only and Control, we use randomization inference, permuting treatment assignment 2,000 times.[26] For tests comparing Labeled Grant to Grant Only, we compute Huber-White standard errors, as treatment assignment between these two arms was done at the household level.

About one month later, we re-surveyed Grant Only and Labeled Grant households by phone to assess the persistence of treatment impacts and the scope for experimenter demand effects.[27] To do so, we implemented the weak demand treatment of De Quidt, Haushofer and Roth (2018), which attempts to induce a demand effect at least as strong as those implicit in the study design. Specifically, respondents in the Grant Only arm received the following script prior to questions about their policy views:

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

Respondents in the Labeled Grant arm received the same script with "less support" instead of

[26]We use the Stata command *ritest* (Heß, 2017), permuting assignment within randomization strata at the village level.

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

"more support." Comparing responses across these two groups identifies the lower bound of demand-free beliefs, assuming that demand effects created by the explicit script are stronger than other implicit demand effects.[28]

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

3.8 Conceptual Framework and Hypotheses

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

Given positive impacts of Labeled Grant and Information Only on policy support, we next assess impacts on beliefs about the economic impacts of hosting refugees and on cultural attitudes toward refugees, as these are two key "intermediate" outcomes that can influence immigration policy views in theory (Tabellini, 2020, Bonomi, Gennaioli and Tabellini, 2021). As before, we test these hypotheses by comparing treated groups to Control. We present these results in Sections 4.3 and 4.4. As complementary evidence, we test whether economic beliefs and cultural attitudes are mediating impacts on policy views by interacting treatment dummies with indicators for high baseline economic and cultural views in Section 5.2.

We proceed to unpack the mechanisms behind changing policy views, economic beliefs, and cultural attitudes, focusing on two mechanisms identified by the literature on policy

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

preference formation and immigration attitudes—resource resentment against refugees and wealth effects (Hainmueller and Hopkins, 2014, Zhou, 2019)—and two that we contribute to the literature—knowledge of the redistribution policy and the credibility of the policy's implementation, which the labeled grant signals. The Information Only arm isolates the impact of knowledge of aid-sharing. To test the role of credibility, we assess impacts of each treatment on beliefs that aid is in fact shared with Ugandans and on reported trust in implementing organizations. To test the role of resource resentment, we analyze beliefs that refugees receive too much aid compared to Ugandans. As the Grant Only arm does not isolate wealth effects due to an association with refugees and a decrease in resource resentment, we estimate wealth effects with a structural model, test for heterogeneity in treatment impacts by initial wealth, and compare results in Uganda to Kenya, where the grant size was much smaller.

4 Results

We find that redistributing refugee aid toward Ugandans in the form of a labeled grant—that is, a grant labeled as part of Uganda's broader aid-sharing policy, along with information about that policy—substantially and persistently changes policy preferences in favor of greater support for refugee hosting and integration policies. These results replicate in Kenya with grants tied to the refugee presence and the potential for new aid-sharing. Sharing information about existing redistribution in Uganda—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.

4.1 Support for Refugee Integration Policies

We find that receiving a labeled grant significantly increases support for refugee hosting and integration, as shown in Table 1. Recipients of labeled grants were 13 pp. more likely to say that they support Uganda's hosting of refugees generally, on a base of 75% (p-val < 0.01). Labeled grants also increase support for admitting more refugees into Uganda (15 pp. on a base of 61%, p-val < 0.01), extending the right to work (13 pp. on a base of 72%, p-val < 0.01), and extending freedom of movement to refugees (6 pp. on a base of 54%, p-val $=$ 0.04). The impact on our pre-specified domain summary index is 0.36 standard deviations (p-val < 0.01; family-wise error rate < 0.01). Adjustments for multiple hypothesis testing do not affect these conclusions, as shown in Appendix Table E1.

Labeled grants have greater impacts on the integration policy support index among those with less support for integration at baseline—or greater economic or cultural concerns about refugee hosting—as shown in Appendix Table A3, and labeled grant recipients were

Table 1: Support for Refugee Integration Policies

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

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

significantly less likely to indicate strong opposition for integration policies, as shown in Appendix Table A4.[29] These results suggest that aid-sharing may influence policy views even in environments where opposition to integration is stronger, a conclusion we later test directly by examining impacts in Kenya.

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

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

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).[30] As we discuss further in Section 5.1, this result is likely due to an implicit labeling of the grants operating through contact with the refugee-led implementing NGO, together with a reduction in resource resentment against refugees. We do not find evidence of significant wealth effects behind changes in attitudes, as discussed in Sections 5.1.1 and 5.3.

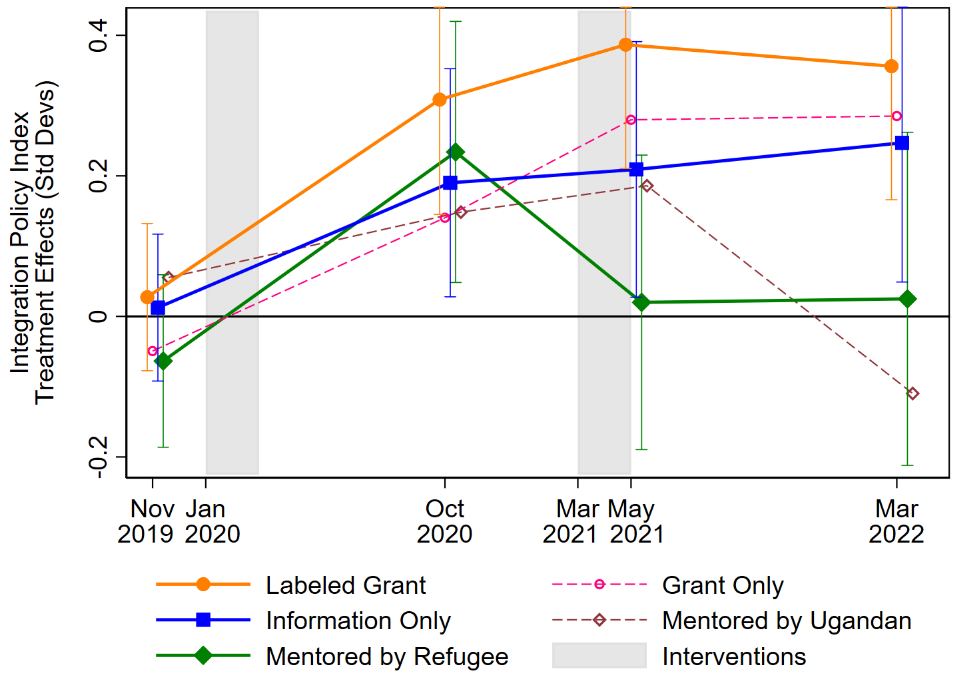
Mentorship by an experienced refugee has much smaller impacts on policy preferences compared to labeled grants. We observe modest increases in support for extending labor market access (8 pp. on a base of 72%, p-val = 0.01), but smaller and statistically insignificant (at the 5% level) impacts on general 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 5.3, we test and reject that additional mentorship meetings would have generated persistent impacts on policy views. This smaller effect is also not due to low perceived value of the program: 71% of mentees reported that they were satisfied with the program in both mentorship arms and 78% said that they learned something from the program that was helpful for their business.

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.[31] As shown in Table 1, labeled grant recipients were 10 pp. more likely to support the letter (on a base of 23%, p-val < 0.01),

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

[31]See Appendix B.8 for the script, and Appendix Table A6 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. 30% of the sample responded during the call, and 4% responded to a follow-up SMS.

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



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

with no significant average impacts for other treatment arms.[32] These results, together with additional evidence presented in Section 5.3, 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 final survey in March 2022. Given that interventions began in early 2020 (and resumed in early 2021), this suggests that redistribution can impact policy views in the long run and persist through a large economic shock like COVID-19.

[32] 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.

4.2 Support for Refugee Integration Policies in Kenya

In Kenya, labeled grants substantially increase support for refugee integration. Table 2 reports the same policy support outcomes analyzed in Uganda. Our summary index of integration policy support rises by 0.59 sd ($p < 0.01$) compared to Control, an effect even larger than that in Uganda (0.36 sd). Given that support for refugee integration is lower in Kenya compared to Uganda (see Appendix Figure A1), this finding is consistent with the greater impacts observed among those with less baseline support within Uganda.[33] It also points to a limited role of the size of the grant—as the grant size in Kenya was about 6% of the grant size in Uganda—compared to an "extensive margin" effect of receiving a grant at all. Impacts are large and statistically significant for each component of our summary measure: support for specific integration policies such as freedom of movement or right to work rises by 18–24 pp (p-vals < 0.01). Impacts are large for outcomes with high support in the control group (75% support hosting refugees overall) as well as those with lower support (43% support allowing additional refugees into Kenya). As in Uganda, we observe positive, but smaller, impacts of Grant Only relative to Control. Our summary index measure is 0.18 sd higher in Grant Only ($p < 0.01$). Across outcomes, the impact of Grant Only ranges from about one-quarter to one-third the size of the Labeled Grant impact.

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

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

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

Table 2: Support for Refugee Integration Policies—Kenya

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

Each observation is a household in Kenya. *Immediate Impacts* are measured the same day after grant and information distribution. *Demand-Free Bound* computed using the method of De Quidt, Haushofer and Roth (2018) to identify the lower bound of demand-free treatment effects—labeled grant recipients receive a script attempting to induce negative demand effects, while grant only recipients receive a positive script. These results are measured using follow-up surveys conducted only in Labeled Grant and Grant Only about one month after the first survey. For comparisons between Labeled Grant or Grant Only and Pure Control, standard errors are clustered at the village level and p-values are computed through randomization inference. For comparisons between Labeled Grant and Grant Only, standard errors and p-values are heteroskedasticity-robust. *Lab. Grant = Grant* shows p-values from a regression of the outcome on a *Labeled Grant* dummy estimated on Labeled Grant and Grant Only villages only. Standard errors in parentheses; p-values in brackets. Tests not pre-specified denoted with $^{+}$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Grant Only remain large and similar in low- and medium-support areas, but are close to zero in high-support areas. Notably, impacts of Grant Only relative to Control only appear in low-support areas. This may reflect a greater ability in these areas for aid *per se* to alter views toward groups perceived to be major beneficiaries of aid, a point we return to in Section 5.3.

4.3 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.[34] Business owners who received a labeled grant were 15 pp. more likely to report that international aid for refugees is shared with Ugandans (on a base of 37%, p-val < 0.01),[35] and 16 pp. more likely to say refugees have a positive effect on the economy overall (on a base of 42%, p-val < 0.01). They were also more likely to say that refugees benefit them personally, and that refugees have skills. The impact on our pre-specified domain summary index is 0.3 standard deviations (p-val < 0.01). Adjustments for multiple hypothesis testing do not affect these conclusions, as shown in Appendix Table E5.

Our Information Only and Grant Only treatments also changed beliefs about the economic impacts of refugee hosting. Business owners in the Grant Only treatment arm were 8 pp. more likely than Control business owners to report receiving support linked to the refugee presence, an impact 4 pp. smaller than that among labeled grant recipients. As discussed in Section 5.1, 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.

4.4 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 increased the share who reported that they would be comfortable being close friends with a refugee by 7 pp., and marrying a refugee by 13 pp. (p-vals $<$ 0.01). We do not observe significant changes in beliefs about the impact of refugees on Ugandan culture, or in whether refugees deserve sympathy. The impact on our pre-specified domain summary index is 0.16 standard deviations (p-val $= 0.01$). Adjustments for multiple hypothesis testing do not affect these conclusions, as shown in Appendix Table E10. As we discuss using additional analysis in Section 5.2, impacts on cultural attitudes toward refugees appear to be driven not by contact with refugees, but indirectly through effects on economic

[34] 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 $= 0.04$), for reasons we discuss in Section 5.1.

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

Table 3: Beliefs About Economic Impacts of Hosting Refugees

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

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

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. This finding is consistent with results from Baseler et al. (2024), which finds positive impacts of labeled grants on cultural attitudes and no impacts of a cross-nationality mentorship program involving weekly meetings over six months.

During our surveys, we conducted a simple dictator game in which the respondent dis-

Table 4: Cultural Attitudes Toward Refugees

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

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

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

4.5 Business Outcomes and Household Welfare

Our treatment arms had small and insignificant impacts on business profit, business capital, business practices, and a summary index of household welfare, as shown in Appendix Table A8. 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 profit while also reducing the incentive to invest (rather than consume) the grant. Impacts on business capital are similarly noisy. We find modest impacts of grants and mentorship on our index of business practices—which we modify from McKenzie and Woodruff (2017)—though only the impact of grants alone is statistically significant at the 10% level. We estimate a positive but insignificant impact of grants on a summary index measure of household well-being.[36]

5 Mechanisms Behind Changing Policy Views

Why does learning about aid-sharing—either through new information or by receiving a grant—increase support for refugee integration? In Section 5.1, we discuss the potential mechanisms driving these impacts. In Section 5.2, we investigate whether our interventions acted on economic or cultural concerns about refugees. In Section 5.3, we examine other potential explanations for impacts on policy views which we can rule out by examining additional data—including experimenter demand effects, contact with refugees, reciprocity to the implementing organization, wealth effects, and differential attrition—and discuss whether more intensive inter-group contact would have generated persistent impacts on policy views.

5.1 Unpacking the Effects of Labeled Grants

In this section we present evidence for three mechanisms we find to be driving impacts of labeled grants on policy views: knowledge of the redistribution policy, the credibility of that information, and resource resentment against refugees. We disentangle these channels from wealth effects of the grant using a simple structural model, summarized here and explained in detail in Appendix D.[37]

Knowledge of Aid-Sharing. Learning about Uganda's existing aid-sharing policy through the Information Only arm, without any associated grant, led to significant and persistent impacts on support for refugee integration policies. Additionally, labeled grants had larger impacts on a summary index of support for refugee integration policies compared to unlabeled grants in both Uganda ($p = 0.05$) and Kenya ($p < 0.01$). This indicates that at least part of the impact of labeled grants operates purely through the information provided.

Recipients of unlabeled grants were also more likely to report that international donations to refugees are shared with Ugandans (by 9 pp., $p = 0.01$). Our findings suggest that some of

[36] 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 E16) and so do not make any welfare adjustments.

[37] Wealth effects could, in principle, operate by reducing feelings of scarcity and thus the salience of resource competition with refugees.

these business owners learned that the grant came from a refugee-led organization, lending an implicit labeling of the grant as associated with the refugee presence.[38] Although we intended to minimize associations with refugees in Grant Only, our implementing partner is a well-known refugee-led organization in Kampala, and some grant recipients either already knew about the organization or learned about it after the intervention. We see that owners in 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 A9).[39] These results imply 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. In Section 5.1.1, we estimate a model to recover the wealth effects of grants.

It may also be that labeled grants led participants to seek out additional information about refugees or about the policy—indeed, we observe an increase in reports that business owners are talking to others about refugees by 6 pp. in Grant Only and Information Only (p-vals = 0.10 and 0.12 respectively) and 8 pp. in Labeled Grant ($p = 0.03$), as shown in Appendix Table A9. Compared to Information Only, labeled grants could also affect views by changing beliefs about expected *future* personal benefits of aid-sharing, but, as we discuss in Section 5.1.1, our results are not consistent with a major role for this channel.

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 are most consistent with the direct receipt of aid making the accompanying information more believable by acting as a visible demonstration of aid-sharing.[40] Recipients of labeled grants were more likely than those in Information Only to say that some of the aid from the international refugee response is shared with Ugandans (p-val on comparison < 0.01), as shown in Table 3, which is consistent either with a credibility or with a salience effect. They were also more likely to say that international organizations are trustworthy compared to Information Only (diff. = 20 pp. on a base of 44%, p-val = 0.01), as shown in Appendix Table A10,

[38]The same is true for the Mentored by Ugandan arm, which experienced positive impacts on policy support that did not persist. As shown in Appendix Table A9, business owners offered mentorship by a Ugandan were significantly more likely to report receiving support associated with refugees compared to Control, and may thus have viewed the free program as an example of aid-sharing.

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

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

which is most consistent with a credibility channel.[41]

Resource Resentment. Receiving aid appears to reduce what Zhou (2019) terms *resource resentment*, or negative views toward a group perceived to be receiving unfair levels of aid. As shown in Appendix Table A10, recipients of unlabeled grants were significantly less likely to report that refugees receive too much aid relative to Ugandans (15 pp. on a base of 77%, p-val < 0.01). This may be driven in part by 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 and international aid organizations care about them (by 10–11 pp., p-vals $= 0.09$ and 0.04 respectively) and are trustworthy (by 23 pp, p-val < 0.01). It may also be partly related to changing beliefs about the distribution of aid, as unlabeled grant recipients were 8 pp. less likely to say that refugees receive more aid than Ugandans (on a base of 71%, p-val $= 0.14$).[42]

In Kenya, where unlabeled grants were distributed without any link to refugees and where impacts were measured immediately (shutting down the potential for cross-treatment spillovers), we also observe positive impacts of grants on support for refugee integration. This implies that implicit labeling cannot be the sole factor driving impacts in Grant Only. Together with results in Uganda, this suggests that receiving aid can reduce feelings of resentment toward groups perceived to be major beneficiaries of aid, such as refugees, even when the aid is not explicitly connected to refugees.

5.1.1 Identifying Wealth Effects of Grants With a Model

While the Grant Only arm was intended to isolate wealth effects of the grant, the findings discussed above point to beliefs about aid-sharing and resource resentment against refugees as partly driving impacts of unlabeled grants. To recover the impact of labeled grants net of wealth effects, we build a simple structural model to separate wealth effects from these two channels using data from all four treatment conditions in our grants experiment in Uganda—Labeled Grant, Grant Only, Information Only, and Control. In the model, voters decide whether to support a policy as a function of their wealth, a joint measure of knowledge of aid-sharing and resource resentment, and a labeled-grant fixed effect capturing other channels

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

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

such as beliefs about expected future personal benefits.[43] Intuitively, treatment impacts in the Information Only arm imply an expected relationship between changes in aid-sharing knowledge or resentment and policy views. The deviation from this relationship in Grant Only identifies the average wealth effect of the grant, while the deviation in Labeled Grant identifies wealth effects plus any interaction effects between grants and information operating net of the wealth and awareness channels. Details on the model are in Appendix D.

Our model identifies a wealth effect that is small compared to impacts driven by knowledge of aid-sharing and resource resentment. Across all three policy support measures shown in Table 1 for which Grant Only impacts are significant—overall support of refugee hosting, support for admitting more refugees, and support for labor market access for refugees—estimated treatment impacts of labeled grants net of the wealth channel are 11–12 pp. (p-values < 0.01). As we discuss in Section 5.3, these findings are consistent with results in Kenya, where much smaller grants nevertheless produced large effects on policy views.

5.2 Economic Versus Cultural Beliefs

A large literature examines whether attitudes toward immigrants are driven more by economic or cultural beliefs. We find that cultural concerns are a much stronger predictor of policy views at baseline compared to economic concerns (see Appendix Table A12), consistent with findings from other immigration settings (Hainmueller and Hopkins, 2014, Tabellini, 2020). Nevertheless, we find that policy views respond more to our economic interventions—grants and information—than to our more cultural intervention—contact with a refugee—and that once policy and economic views shift, cultural attitudes follow.

Heterogeneous Impacts By Baseline Economic or Cultural Concerns. We find that all of our interventions had greater impacts on the policy views of Ugandans with *either* above-median economic or cultural concerns about refugee hosting at baseline, as shown in Appendix Table A3.[44] 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

[43] Labeled grants could—even net of wealth and credibility effects—change beliefs about expected *future* personal benefits from refugee hosting relative to information alone, where impacts may be greater on expected future benefits to Uganda broadly. The results of Table 3 do not support this alternative: treatment impacts on beliefs about personal economic benefits and broader economic benefits are roughly proportionate across Labeled Grant and Information Only. Additionally, we do not observe significantly different treatment impacts of Information Only based on whether the respondent uses the public goods mentioned in our script—hospital or schools—a proxy for future personal benefits (see Appendix Table A11). This is consistent with the results of Hainmueller and Hopkins (2014), who find a dominant role for perceived group-level impacts in driving immigration views.

[44] 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.

economic well-being, presented in Appendix Table A13. 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 Appendix Table A12). 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 Appendix Table A13. In the specification that includes all three dimensions of heterogeneity and their interactions, we find that economic concerns predict treatment impacts to the greatest degree across both grant arms and Information Only. Baseline cultural concerns also consistently predict stronger treatment effects across specifications. There is some evidence of stronger treatment effects among those with better baseline economic well-being, which is consistent with our finding that wealth effects are small.

Interpretation. The concentrated treatment impacts among those with economic concerns about refugee integration are unsurprising given that the information was focused on economic policy and the grant is itself an economic intervention. The concentrated impacts among those with cultural concerns are hard to reconcile with a heuristic in which only cultural interventions affect culturally rooted policy opposition. However, they are consistent with Jha (2012) and Jha and Shayo (2019), which show that financial instruments that align incentives toward peaceful coexistence across groups can reduce inter-group 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 5.3, 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 A14,[45] 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 < 0.01 and $= 0.03$

[45] For results in Appendix Table A14, we constrain each summary index to contain only the components asked in each survey round, for comparability over time.

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 26-month surveys (p-val = 0.03). Effects in the Information Only arm display a similar pattern. While other explanations including experimenter demand effects are possible, the delayed timing of these impacts is suggestive of cultural attitudes that are partly rationalized from changing economic and policy views, possibly to reduce the cognitive dissonance involved with holding positive economic but negative cultural views toward refugees.

Overall, our findings imply 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 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.

5.3 Other Potential Mechanisms

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

Experimenter Demand Effects. A potential concern is that the observed 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. We designed our study to minimize potential demand effects: surveys were conducted by a Ugandan-led firm unconnected to YARID, respondents were reminded throughout surveys that their answers would remain anonymous and would not affect their eligibility for aid, and grant recipients were told that the grant was a one-time transfer.

Three pieces of evidence from our main results point to a limited role of demand effects in driving our results. First, we find significant impacts of labeled grants on support of a phone-call campaign which was not explicitly connected to the implementing or survey firms in any way (see Section 4.1). Second, we estimate a positive and statistically significant demand-free lower bound using the method of De Quidt, Haushofer and Roth (2018).[46] Third, we find significant impacts on the share of an endowment donated to a program supporting refugees in an incentivized dictator game (Table 4, Column 4).

In addition to these results, we conducted three further tests of demand effects: a placebo treatment informing grant recipients of YARID's position on an unrelated issue, a survey experiment priming respondents about the grants, and a test of whether labeled grants led respondents to expect future assistance.[47] We find no impacts across these three tests, as discussed in Appendix C.1.

Our tests of demand effects were designed to test whether true (as opposed to simply stated) policy preferences changed. It is possible that demand effects are stronger for questions related to cultural attitudes, and we cannot fully rule this out. However, the lack of treatment effects of our mentorship programs on cultural views—in contrast to positive effects of Information Only, which provided no material support—suggests that the scope for demand effects to influence cultural attitudes is also limited.

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

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

Wealth Effects. Our reduced-form results are consistent with the small wealth effects estimated in the model of Appendix D. We observe similar—if anything, greater—treatment impacts among business owners with higher measures of household well-being at baseline, which is inconsistent with a scarcity channel. We also observe large treatment impacts

[46] This result is especially helpful at ruling out the possibility that participants learned over time about the study's expected results and changed their answers accordingly—perhaps because the same questions were asked multiple times—under the assumption that explicit expectations are at least as impactful as implicit expectations.

[47] The consistent results of Baseler et al. (2024) also help rule out that expectations of future assistance are driving treatment effects. In that study, one group was given a lump-sum grant bundled with information about aid-sharing, while the control group was informed that they would receive a grant in 18 months. The control group did not receive the same information script but is aware that the implementing partner, the International Rescue Committee (IRC), supports refugees in Uganda through program messaging at registration and sign-up events which included both refugees and Ugandans. Therefore, the control group should by design have the highest expectation of future aid and the strongest incentive to overstate their preferences for refugee integration.

of much smaller labeled grants (about $7.50) relative to both unlabeled grants and to a control arm in Kenya. If wealth effects were driving treatment impacts of grants, we would expect much smaller impacts from the small grant in Kenya, whereas impacts in Kenya were generally larger.

Differential Attrition. Respondents who could not be surveyed after the baseline (9% of the sample) are balanced across treatment groups, randomization balance appears to hold among those surveyed after baseline, and our findings are robust to reweighting by the inverse probability of being surveyed in each round. The 95% confidence interval on the lower Lee bound does not cross zero for the impacts of Labeled Grant and Information Only on support for integration policies, although the bounds are wide in some cases. These findings point to a limited role of differential attrition in influencing our main results.

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

Degree and Nature of Inter-Group Contact. Would a more intensive contact intervention, such as additional meetings or more in-person meetings compared to remote meetings, have produced more persistent impacts on policy views? Relatedly, would a program facilitating peer-to-peer interactions have produced durable impacts where our more hierarchical design did not? Evidence from this study as well as a related cross-nationality mentorship experiment in the same setting (Baseler et al., 2024) suggests that the answer is no. In Appendix C.7, we exploit random variation in the start date of the mentorship program to test whether business owners who starting their program earlier—and who therefore had more in-person meetings and more meetings overall—experienced persistent changes in policy views. We find that they did not: while initial impacts on views in this group were large, these impacts faded nearly to zero over time (see Appendix Table A14). Consistent with this finding, Ugandans assigned to the more intensive cross-nationality mentorship groups of Baseler et al. (2024)—which involved weekly in-person meetings with both refugee mentors and refugee peers over six months—did not change their views on refugee integration policies or economic or cultural beliefs. Moreover, the design of Baseler et al. (2024) involved both peer-to-peer and mentee-mentor interactions, implying that the null average impacts of mentorship were likely not driven by the hierarchical design.

Listening Exercise. The treatment impacts in the Information Only group identify the joint effect of the information shared in the script and the listening exercise that invited the respondent to share their views of refugees. While we cannot separate these effects, our experiment in Kenya as well as that of Baseler et al. (2024)—both of which included grants with information about aid-sharing but not a listening exercise—imply that the listening exercise is not necessary for labeled grants to affect policy views.

6 Discussion

We provide experimental evidence from two countries that compensation—redistributing the aggregate gains from a policy—can influence political views and build support for a policy bargain. We find substantial impacts of both a large grant in Uganda and a small grant in Kenya, indicating that both large, targeted and small, distributed compensation policies could, in principle, form part of a policy bargain incorporating aid-sharing and refugee integration. The limited role of wealth effects in driving our findings also supports this view, as does the commonality of this findings to grants disbursed around a large economic shock (COVID-19) and those disbursed in more normal times, in Kenya and in Baseler et al. (2024). While policy bargains are common, we provide evidence that when the connection between an economic intervention and the policy is salient, compensation can influence views even when the underlying opposition is cultural in nature. Our primary economic intervention, the labeled grant, had more persistent effects on policy views than our primary cultural intervention, mentorship by an experienced refuge.

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

Our results are directly informative in two categories of low-to-middle-income countries: those already engaged in aid-sharing between refugees and natives, such as Uganda and Jordan, and those on the margin of adopting major aid-sharing bargains, such as Kenya and Ethiopia (Miller and Kitenge, 2023). Given that a substantial share of our Kenyan sample strongly opposes several integration policies, our findings imply that high levels of support for integration is not a necessary condition for the programs we study to affect policy views. Further research on the scope for aid-sharing to support policy bargains in contexts with even greater opposition to integration than Kenya would, in our view, be valuable. Our finding that labeled grants had the largest impact where baseline views were most negative suggests that this scope may be high. Our results are most directly applicable to low- and middle-income countries, and future research could test compensation programs such as these in high-income contexts. However, the strong statistical relationship between cultural or high-level economic concerns—what Hainmueller and Hopkins (2014) refer to as "sociotropic" concerns—in our data mirrors findings from research on the determinants of immigration attitudes in the US and Europe, suggesting that our findings may extend beyond the contexts we study.

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. We discuss cost by program in more detail in Appendix B.4.

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 allocated 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 1 billion USD in financing for public goods and services 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 redistributing these gains is a promising avenue to build support for a new political economy equilibrium that both improves aggregate welfare and mitigates, or reverses, the policy's harms.

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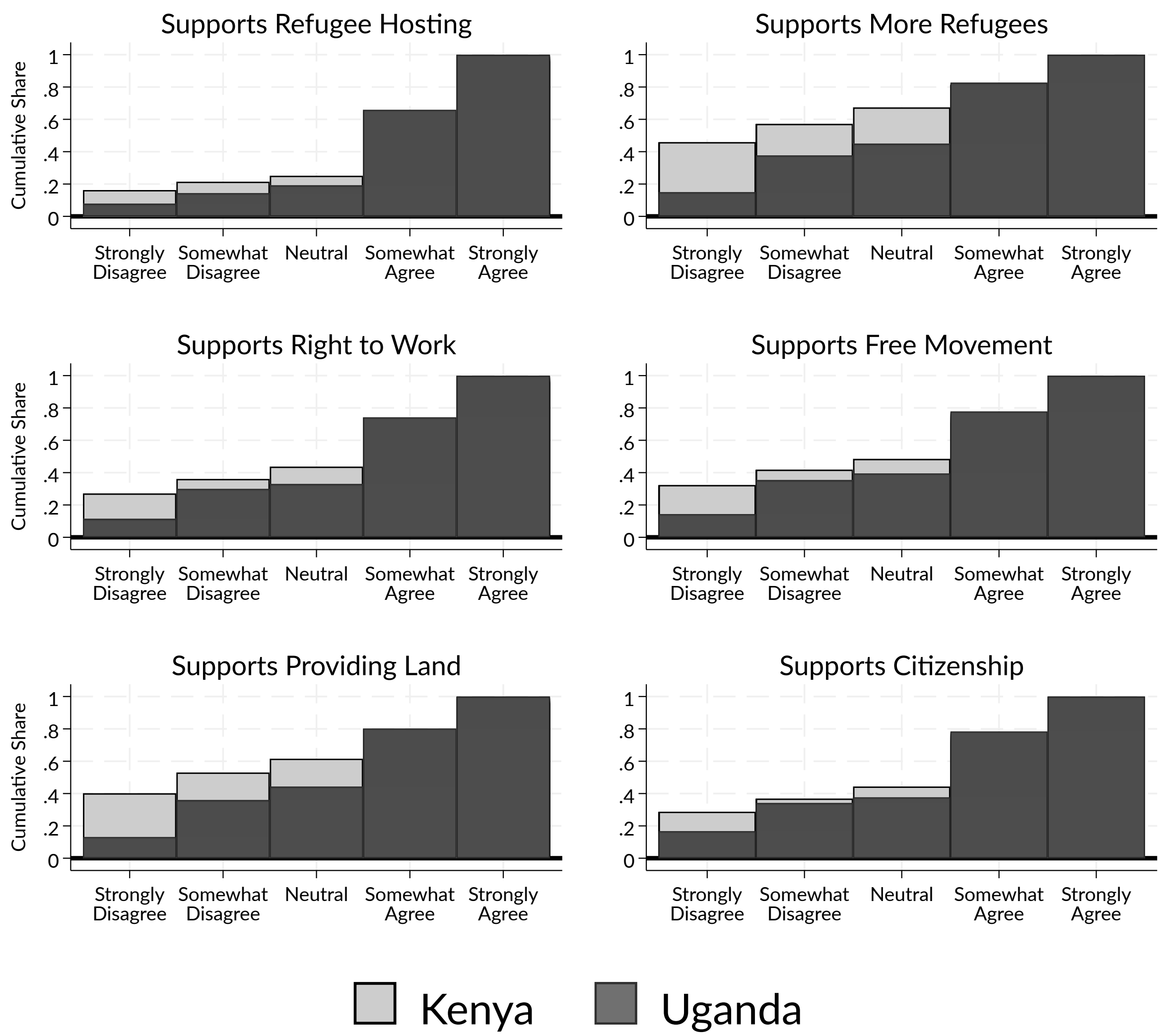
Appendix for "Can Redistribution Change Policy Views? Aid and Attitudes Toward Refugees"

Contents

Online Appendix E link:
https://drive.google.com/file/d/14tPmAq5cNmJoT9T4lC-KDcxoVCWb0tGv/view

A Additional Figures and Tables

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



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

Table A1: Study Timeline

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

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

Table A2: Baseline Summary Statistics

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

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

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

	Female Owner	Refugee Facilitator	Business Profit	Supports Hosting Index	Economic Beliefs Index	Cultural Attitudes Index	Contact Refugees (Choice)	Contact Refugees (Circumstance)	Knows About Aid-Sharing	Mentor Profit	Worried About Covid
Labeled Grant × X	0.06	-0.01	-0.15	-0.30**	-0.31**	-0.28**	0.11	0.18	-0.06		-0.11
	(0.14)	(0.09)	(0.12)	(0.13)	(0.13)	(0.13)	(0.15)	(0.13)	(0.16)		(0.14)
	[0.68]	[0.88]	[0.22]	[0.02]	[0.02]	[0.03]	[0.47]	[0.17]	[0.71]		[0.43]
Labeled Grant	0.32***	0.37***	0.43***	0.54***	0.53***	0.51***	0.28**	0.25**	0.38***	0.36***	0.40***
	(0.12)	(0.09)	(0.09)	(0.11)	(0.10)	(0.10)	(0.13)	(0.10)	(0.07)	(0.06)	(0.11)
	[0.01]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.03]	[0.01]	[0.00]	[0.00]	[0.00]
Information Only × X	0.22	0.06	-0.20	-0.21	-0.29**	-0.29**	0.13	0.11	-0.02		0.04
	(0.15)	(0.09)	(0.13)	(0.13)	(0.13)	(0.13)	(0.16)	(0.14)	(0.16)		(0.14)
	[0.13]	[0.49]	[0.12]	[0.11]	[0.03]	[0.03]	[0.42]	[0.41]	[0.92]		[0.79]
Information Only	0.08	0.18**	0.32***	0.35***	0.39***	0.37***	0.12	0.15	0.23***	0.22***	0.18*
	(0.13)	(0.09)	(0.09)	(0.11)	(0.10)	(0.11)	(0.14)	(0.11)	(0.07)	(0.07)	(0.11)
	[0.55]	[0.05]	[0.00]	[0.00]	[0.00]	[0.00]	[0.38]	[0.15]	[0.00]	[0.00]	[0.09]
Grant Only × X	0.01		-0.16	-0.21	-0.35***	-0.31**	-0.15	-0.07	-0.12		-0.06
	(0.14)		(0.13)	(0.13)	(0.13)	(0.13)	(0.15)	(0.13)	(0.15)		(0.14)
	[0.93]		[0.23]	[0.12]	[0.01]	[0.02]	[0.31]	[0.63]	[0.43]		[0.65]
Grant Only	0.24**	0.24***	0.32***	0.37***	0.43***	0.41***	0.35***	0.29***	0.27***	0.25***	0.25**
	(0.12)	(0.07)	(0.09)	(0.11)	(0.11)	(0.10)	(0.13)	(0.11)	(0.08)	(0.07)	(0.11)
	[0.04]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.01]	[0.01]	[0.00]	[0.00]	[0.02]
Mentored by Refugee × X	-0.00		-0.21	-0.18	-0.30**	-0.20	0.00	0.04	-0.04	-0.04	0.08
	(0.16)		(0.15)	(0.15)	(0.15)	(0.14)	(0.16)	(0.15)	(0.17)	(0.10)	(0.15)
	[0.99]		[0.16]	[0.21]	[0.04]	[0.16]	[0.99]	[0.78]	[0.81]	[0.70]	[0.61]
Mentored by Refugee	0.12	0.12*	0.22**	0.23**	0.29**	0.23**	0.12	0.08	0.13	0.13	0.07
	(0.13)	(0.07)	(0.10)	(0.11)	(0.12)	(0.11)	(0.14)	(0.12)	(0.08)	(0.09)	(0.12)
	[0.35]	[0.09]	[0.03]	[0.05]	[0.02]	[0.04]	[0.37]	[0.49]	[0.11]	[0.13]	[0.56]
Mentored by Ugandan × X	0.07		-0.40***	-0.16	-0.31**	-0.30*	-0.02	0.10	-0.03	-0.05	-0.32**
	(0.16)		(0.15)	(0.15)	(0.15)	(0.15)	(0.17)	(0.16)	(0.17)	(0.11)	(0.15)
	[0.67]		[0.01]	[0.30]	[0.04]	[0.05]	[0.89]	[0.53]	[0.84]	[0.66]	[0.03]
Mentored by Ugandan	0.06	0.10	0.27***	0.18	0.27**	0.26**	0.12	0.04	0.11	0.13	0.28***
	(0.13)	(0.08)	(0.10)	(0.12)	(0.12)	(0.13)	(0.15)	(0.13)	(0.09)	(0.09)	(0.11)
	[0.66]	[0.18]	[0.01]	[0.14]	[0.02]	[0.04]	[0.43]	[0.76]	[0.21]	[0.16]	[0.01]
X	-0.18		0.25**	0.25**	0.31***	0.19*	0.11	-0.08	0.09		0.09
	(0.15)		(0.12)	(0.12)	(0.11)	(0.11)	(0.13)	(0.16)	(0.11)		(0.11)
	[0.23]		[0.04]	[0.04]	[0.01]	[0.09]	[0.40]	[0.63]	[0.44]		[0.38]
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. An observation is a surveyed respondent per post-baseline survey round in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double lasso. Standard errors clustered at the enterprise level in parentheses; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Strongly Support and Strongly Oppose Inclusive Policies

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

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

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

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

An observation is a surveyed respondent per post-baseline survey round in Uganda. Results estimated through ANCOVA regression with baseline controls selected through double lasso. All regressions use entropy balancing (Hainmueller, 2012), matching mean age and educational attainment and support for allowing refugees to work in the Ugandan adult population. Standard errors clustered at the enterprise level in parentheses; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Full Set of Phone Campaign Outcomes

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

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

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

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

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

Table A8: Business Outcomes and Household Welfare

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

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

Table A9: Program Associations, Recall, and Discussions of Refugees

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

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

Table A10: 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	0.06	-0.05	0.00	0.12**	0.09*	0.16***
	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)	(0.06)
	[0.30]	[0.36]	[0.99]	[0.03]	[0.09]	[0.01]
Information Only	-0.03	-0.09*	-0.08	-0.05	-0.07	-0.03
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)
	[0.53]	[0.09]	[0.14]	[0.39]	[0.18]	[0.60]
Grant Only	-0.01	-0.15***	-0.08	0.10*	0.11**	0.23***
	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)	(0.06)
	[0.82]	[0.00]	[0.14]	[0.09]	[0.04]	[0.00]
Mentored by Refugee	-0.03	-0.07	-0.10*	0.01	0.01	0.14*
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)
	[0.69]	[0.25]	[0.10]	[0.92]	[0.91]	[0.05]
Mentored by Ugandan	-0.04	-0.02	-0.00	0.05	0.04	-0.00
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)
	[0.45]	[0.73]	[0.98]	[0.48]	[0.51]	[0.98]
Observations	780	821	821	699	871	653
Control Mean: Baseline	.	.	.	.	.	.
Control Mean: Follow-Ups	0.31	0.77	0.71	0.30	0.33	0.44
Labeled Grant = Info Only	0.09	0.42	0.13	0.00	0.00	0.00
Labeled Grant = Grant Only	0.21	0.05	0.12	0.75	0.69	0.26
Labeled Grant = R-Mentee	0.19	0.70	0.09	0.08	0.18	0.73
R-Mentee = Info Only	0.89	0.77	0.73	0.40	0.21	0.02
R-Mentee = U-Mentee	0.78	0.45	0.13	0.59	0.62	0.07

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

Table A11: Heterogeneity in Impacts on Integration Policies Index (Public Good Usage)

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

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

Table A12: Baseline Correlates of Support for Refugee Integration

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

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

Table A13: Expanded Treatment Effect Heterogeneity

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

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

Table A14: More intensive refugee mentorship does not produce persistent impacts on policy views.

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

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

Westfall-Young Stepdown-Adjusted p-Values

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

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

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

Table A15: 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.[48] 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.[49] We hypothesized that the high concentration and reputational advantage of refugees was desirable for this study to increase the chances for skill transfer and collaboration to emerge from refugee-Ugandan pairs in mentorship.

[48] 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.

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

Table B1: Randomization Balance

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

Baseline surveys of experimental sample in Uganda. First six columns show baseline variable means within treatment groups. Column 7 shows p-values from joint F-tests that means are equal in all treatment groups.

Table B2: Randomization Balance (Among Non-Attriters)

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

Sample includes all baseline individuals of experimental sample in Uganda who were surveyed in at least one follow-up round. First six columns show baseline variable means within treatment groups. Column 7 shows p-values from joint F-tests that means are equal in all treatment groups.

Table B3: Balance of Ugandan and Refugee Mentor Characteristics

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

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

Table B4: Tests of Differential Attrition

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

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

Table B5: Randomization Balance, Kenya Extension

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

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

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

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

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

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

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

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

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

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

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

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

	Household Well-Being Index	Business Profits (USD/Month)	Business Capital (USD)	Business Practices Index
Labeled Grant	0.04	-3.22	-57.22	0.03
	(0.06)	(2.44)	(44.98)	(0.08)
	[0.50]	[0.19]	[0.20]	[0.74]
Information Only	-0.05	-0.60	16.74	-0.02
	(0.07)	(2.67)	(49.25)	(0.08)
	[0.43]	[0.82]	[0.73]	[0.78]
Grant Only	0.03	-2.15	7.91	0.11
	(0.07)	(2.65)	(47.77)	(0.07)
	[0.63]	[0.42]	[0.87]	[0.12]
Mentored by Refugee	-0.04	0.98	-37.11	0.05
	(0.08)	(2.89)	(51.04)	(0.09)
	[0.65]	[0.73]	[0.47]	[0.55]
Mentored by Ugandan	0.10	-2.46	12.51	0.10
	(0.07)	(2.81)	(53.76)	(0.08)
	[0.13]	[0.38]	[0.82]	[0.22]
Observations	4,132	4,029	2,819	1,942
Control Mean: Baseline	-0.03	39.61	495.56	0.05
Control Mean: Follow-Ups	0.00	20.69	632.54	0.00
Labeled Grant = Info Only	0.09	0.27	0.10	0.54
Labeled Grant = Grant Only	0.85	0.64	0.14	0.23
Labeled Grant = R-Mentee	0.27	0.11	0.68	0.75
R-Mentee = Info Only	0.82	0.59	0.30	0.40
R-Mentee = U-Mentee	0.06	0.26	0.37	0.61

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

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

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

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

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

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

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

B.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.[50]

Table B12: Assignment and Actual Treatment Status

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

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

Table B13: Facilitated Mentorship Meetings

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

Source: YARID Administrative data

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

B.4 Cost Effectiveness

Table B14: Cost Effectiveness

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

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

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

B.5 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.6 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.

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.

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.7 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.8 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.9 Labeled Grant Script: Kenya

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

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

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

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

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

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

C Details on Tests of Alternative Mechanisms

This appendix presents details on each of the tests of alternative mechanisms summarized in Section 5.3.

C.1 Experimenter Demand Effects

The implementing organization, YARID, is refugee-led and in part refugee-staffed. Business owners may therefore believe that their chances of receiving future assistance are increased by expressing pro-refugee views.[51] Alternatively, demand effects may be generated by feelings of gift exchange, if respondents who received assistance from YARID viewed the assistance as a *quid pro quo*, and so gave responses they think YARID wanted to hear but do not believe themselves.

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

C.1.1 Placebo Information Treatment

To further test whether respondents' answers were influenced by their perceptions of YARID's position—as opposed to the new information provided through our interventions—and whether receiving cash amplifies such an effect, we ran a placebo information campaign on an unrelated political issue, child labor, which shared YARID's position but did not provide any new information. Similar to refugee hosting, child labor policies are somewhat, but not extremely, sensitive issues in Uganda. We chose our outcomes for these tests to have a similar level of support as refugee hosting.[52] 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 influenced attitudes through other channels besides knowledge of YARID's position. Our method makes YARID's stance explicit for a placebo issue but excludes any other information on the issue that could affect respondents' views. Our logic is thus analogous to the demand-elicitation instructions in De Quidt, Haushofer and Roth (2018), in that we assume the effect of the explicit stance in the placebo campaign is at least as strong as the implicit stance in the refugee campaign. The script read by YARID facilitators was:

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

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

Hello, I am [NAME] from YARID. We are an organization that supports people living in Kampala in the areas of small business support, adult education, and women's empowerment. You've been participating in a study and pilot program with us. This call will take about 2 minutes today. Is that ok?

[FOR GRANT ONLY GROUP:] You received 500,000 UGX as part of the project.

We wanted to follow-up with a separate campaign we are running to stop child labor. We believe that children under the age of 15 should not be working, even for their family's business, and should instead be in school. We are calling to deliver the message that YARID takes a strong position against child labor. Thank you for your time today.

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

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

Table C1: Impact of Child Labor Information Campaign

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

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

C.1.2 Priming Experiment

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 C2), consistent with limited demand effects in this setting.[53]

Table C2: Within-Survey Priming Experiment

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

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

C.1.3 Expectations of Future Assistance

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

C.1.4 Other Evidence

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

[53] The priming experiment was conducted only around the questions on refugees presented in Table C2 and not around our main outcomes on political views to avoid distorting those main outcomes. We believe any demand effects would be equally likely for the selected questions, since respondents were not aware of our primary outcomes of interest.

Table C3: Expectations of Future Aid—Kenya

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

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

C.2 Contact With Refugees

We find no evidence that treatment impacts are driven by contact with refugees as program facilitators or through increased contact with refugees outside of our programs, and that impacts driven through contact with refugees as mentors do not persist. Despite COVID-19 interruptions, our mentorship program involved moderate collaborative inter-group contact relative to other experiments that facilitate contact between different ethnic, national, or religious groups (Pettigrew and Tropp, 2006, Mousa, 2020, Corno, La Ferrara and Burns, 2022). High uptake rates suggest that business owners found the mentorship meetings valuable: 80% of owners assigned to mentorship by a Ugandan and 79% of owners assigned to mentorship by a refugee participated in the program by having at least one meeting.[54] 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 A3, Column 2.

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

C.3 Reciprocity to YARID

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

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

consider above. Experimenter demand effects drive gaps between true and reported beliefs; reciprocity could in theory lead owners to update their true beliefs.

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

C.4 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 Appendix Tables A8 and E21, 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 A13), which is inconsistent with a scarcity channel.

C.5 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. 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.[55] Results, shown in Appendix Tables B6, B7, B8, and B9 are extremely similar to unweighted results. As shown in Appendix Tables B10 and B11, the 95% confidence interval on the lower Lee bound does not cross zero for the impacts of Labeled Grant and Information Only on support for integration policies, although the Lee bounds are wide in some cases. We conclude that differential attrition is not a significant factor in explaining our main results.

[55] 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.

C.6 Altruism Crowd-Out

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

C.7 Degree and Nature of Inter-Group Contact

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.[56] 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 A14. About 9 months after the meetings began, intensive refugee mentorship had increased our index measure of support for refugee integration by 0.55 sd ($p < 0.01$). This effect falls to 0.22 sd ($p = 0.21$) after 16 months and 0.09 sd ($p = 0.65$) after 26 months. Impacts on beliefs about the economic effects of refugees on Uganda follow a similar pattern, with large initial impacts that fade to insignificance over time. At no point do we observe significant impacts of intensive refugee mentorship on cultural views. Impacts of less intensive refugee mentorship on support for refugee integration are small and positive after 9 months (coeff. = 0.19, $p = 0.07$) but are also smaller and insignificant over time.

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

D Disentangling Wealth and Information Effects With a Model

In this section, we build a simple structural model to estimate the wealth effect of grants in the presence of implicit labeling in the Grants Only arm.[57] Beside wealth effects, we allow our treatments to affect views either by changing knowledge of aid-sharing or by reducing resource resentment against refugees. Our experimental results suggest that recipients of unlabeled grants perceived the grant to be an example of aid-sharing—likely because our implementing partner was a well-known refugee-led organization—and that grants reduced views that refugees get too much aid.[58] Estimating the wealth effect of grants allows us to recover the marginal impact of the label by computing counterfactual treatment impacts of labeled grants absent any wealth effects.

Consider a set of voters indexed by i deciding whether to support a policy favoring refugee integration. Each voter is exposed through the randomized program $X \in \{LG, G, I\}$—with LG, G, and I denoting Labeled Grant, Grant Only, and Information Only respectively—to a wealth shock Δ_W^X and an awareness shock Δ_A^X relative to voters in a control arm C. We additionally allow for a labeled-grant fixed effect α_{LG}, capturing potential impacts of labeled grants operating independently of the awareness and wealth channels.[59] We choose to model the joint effect of knowledge of aid-sharing and resource resentment—specifically, we estimate treatment impacts on a dummy variable indicating that the respondent knows that aid is shared between refugees and Ugandans, or says that refugees do not receive too much aid compared with Ugandans—which we refer to as "awareness."[60] Preferences over supporting the integration policy are represented by the indirect utility function:

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

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

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

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

[58] The Grant Only arm increased knowledge of aid-sharing by 9 pp. ($p < 0.01$), as shown in Table 3 Column 3. It also reduced the share of respondents reporting that refugees receive too much aid relative to Ugandans by 15 pp. ($p < 0.01$), as shown in Appendix Table A10 Column 2.

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

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

where Δ_A^X and Δ_W^X are treatment impacts on average awareness and wealth respectively and $\bar{c}_0 \equiv E[U_i(\text{Support}_i = 0) - \gamma_W \text{Wealth}_{i0} - \gamma_A \text{Aware}_{i0}]$, with the 0 subscripts denoting baseline levels. Note that random assignment of X implies that $\bar{c}_0$ is equal in expectation across treatment conditions.[61] Without loss of generality, we normalize $\theta = 1$ and let $\tilde{S}^X \equiv \log\left(-\log(1 - E[\text{Support}_i^X])\right)$.[62] This gives:

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

These three differences express three treatment impacts in terms of known quantities—observed policy support and treatment impacts on awareness—and three unknowns: γ_A, $\gamma_W \Delta_W^X$, and α_{LG}. Note that the size of the grant Δ_W^X is the same in G and LG by design. We solve for counterfactual average support by treatment group using (2) and setting the relevant mechanism to zero: for example, setting $\gamma_W = 0$ recovers mean support net of wealth effects.[63] We estimate standard errors using 2,000 bootstrap samples, re-estimating treatment impacts on support and awareness and solving (3) in each under the constraint $\gamma_A \geq 0$.[64] We repeat this for the three binary support measures shown in Table 1 for which Grant Only impacts are statistically significant: support for refugee hosting overall, support for admitting more refugees, and support for labor market access.

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

Interpretation. These results are consistent with our reduced-form analysis. Observed treatment impacts are slightly greater for higher-wealth Ugandans, consistent with limited or negative wealth effects. We observe no significant changes in business profit or household well-being, consistent with limited wealth effects. We also do not find that the degree of expected future personal benefits from aid-sharing predict treatment impacts (see footnote 43). This is consistent with little scope for interactions between information and grants, or α_{LG}, except through impacts on awareness. Instead, labeled grants substantially increased voters' trust in donor institutions, which should affect awareness of aid-sharing by making the information given more credible.

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

[62] Because θ enters each difference in (3) multiplicitavely, it acts only as a scaling factor and does not

Table D1: Parameter Estimates

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

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

influence counterfactual estimates.

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

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