MATCHING WITH THE RIGHT ATTITUDE: THE EFFECT OF MATCHING FIRMS WITH REFUGEE WORKERS

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Abstract

We study the effect of exposure to a refugee worker on firms' hiring of refugees. We conduct a randomized controlled trial randomly matching local firms with refugee workers. The experiment subsidizes local firms to hire a refugee worker for one week. We find that these internships effectively double local firms' hiring of refugee workers two years after the experiment. Moreover, they enhance firms' support of refugees' integration and improve firm owners' beliefs about refugees' skills. This underscores the presence of misperceptions among local firms regarding workers from demographic groups they typically do not consider for employment. The study also identifies significant heterogeneity in the initial attitudes of employers and workers. Notably, the quality of the match between firms and refugee workers has a complementary effect on firms' demand for refugees and enhances the impact of the internships. Overall, these findings hold significant policy implications for countries seeking to create employment opportunities for forcibly displaced people.

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1. Introduction

Refugees constitute one of the world's most vulnerable populations, facing significant barriers to employment that often result in unemployment, underemployment, and poverty (Cortes (2004); Brell et al. (2020)). This situation leads to a loss of potential talent and imposes economic costs on society. The integration of refugees into the labor market can fail for a number of reasons. Refugees may lack the necessary human capital. They may also face entry barriers, because their abilities and skills are largely unknown to the employers, who may perceive them as low, and refugees' culture and norms may differ from those of the destination country, thus increasing the risk that negative attitudes affect the interaction between local employers and refugee workers. With a sufficiently large local labor supply, an individual firm has little incentive to gather information to correct these misperceptions, even if all firms would benefit from a more skilled labor force. This has motivated the design of several labor market policies, including internships and hiring subsidies, aimed at reducing firms' cost of gaining information about disadvantaged workers, such as refugees, to improve their chances of employment and, ultimately, labor market efficiency (Crépon and Premand (2019); Alfonsi et al. (2020); Bandiera et al. (2021); Hardy and McCasland (2023)).

In this paper, we conduct a randomized experiment in Uganda to study the short- and longer-term impact on local-owned and managed firms' willingness to hire refugees after being provided with a skilled refugee worker for free for one week. Uganda is an ideal setting to investigate the labor market integration of refugees. Not only is it the fifth largest refugee host country in the world, but refugees are also allowed to move freely within the country and seek employment. This allows us to focus on the importance of intergroup contact in the workplace.

We began by testing the practical skills of a sample of 552 refugees in the manufacturing and services sectors in Kampala, the capital of Uganda. We chose sectors typically associated with regular employment, including tailoring, food processing, hairdressing, and other light manufacturing and service sectors. About 70 per cent of the refugees in our sample have work experience in at least one of these sectors. On average, they have almost five years of experience in these occupations. We tested in collaboration with the Directorate of Industrial Training, the agency established by the Ministry of Education to be in charge of the vocational education curriculum in Uganda, and two large refugee-led non-governmental organizations (NGOs) based in Kampala.

After completing the tests, we randomly paired each refugee worker with a sample of Ugandan employers, stratifying by the occupation of the refugee. Treated firms were subsidized to offer a week-long internship for free to the paired refugee worker whereas control firms were not. We find a large and significant effect: treated firms hire more than twice as many refugees as firms in the control group and their views towards the integration of refugees (measured with real monetary donations to a non-profit organization and agreement to a set of statements) become, on average, more positive. To explain this result, we use a simple Bayesian learning framework, where local employers have downward-biased prior beliefs about refugees' skills (because of inexperience). The model predicts that the internship would, on average, lead to positive belief updating about refugees' skill sets and an increased labor demand for refugees. Consistent with the model, we first show - using the refugee test data - that local managers have negatively biased priors regarding the skills of the refugee workers at baseline. We then turn to the short-term outcomes of the experiment. We show, consistent with the prediction from the simple Bayesian model, that exposure to a refugee worker through the one-week internship leads firm managers to update their beliefs about refugees' general skills almost immediately, that is: one month after the internships. Yet, firms' short-term demand for a new refugee does not increase on average.

To investigate the mechanisms through which exposure to a refugee worker caused some firms to immediately update their beliefs about refugees' skills, and be more willing to hire them, while others, if anything, became less inclined to do so, we take an agnostic empirical approach and estimate the Conditional Average Treatment Effect (CATE) using a causal forest algorithm (Athey and Wager (2019); Wager and Athey (2018); Davis and Heller (2017)). The method allows us to determine which baseline characteristics are significantly more likely to be associated with heterogeneous treatment effects in the data. The algorithm identifies two predictors: employers' initial attitudes toward refugees - in terms of how supportive they are towards the labor market integration of refugee workers, and refugee workers' attitudes toward locals - in terms of how disenfranchised refugees feel with respect to local Ugandans. We explore the importance of the initial attitudes in the employer-refugee match by estimating the variation in the treatment effect across four groups, distinguished by the attitude of the employer toward refugees and the attitude of the refugee they are matched with toward locals.

We find that firms with a positive attitude toward refugees, and that are (randomly) matched with a refugee with positive attitudes toward locals substantially increase their willingness to hire a (generic) refugee worker one week after the experiment. In particular, treated firms are 11 percentage points (pp) (or 15.5 per cent at the mean) more willing to hire a refugee compared to the control group. By contrast, firms with negative attitudes toward refugees - and that are matched with refugees with similar negative attitudes toward locals - decrease their willingness to pay to hire a refugee by 17.6 pp (equivalent to a 25 per cent decrease). We interpret these findings through the lens of social psychology research. While Allport (1954)'s classical contribution on contact theory predicts that intergroup contact improves the attitudes of the majority in-group (the firms) and increase the willingness to interact with members of the out-group (the refugees), more recent research emphasizes that the intergroup contact can be either positive or negative (Dijker (1987)). Specifically, negative contacts make intergroup differences more salient, inducing a general avoidance of future contact (Paolini et al. (2010); Barlow et al. (2012); Meleady and Forder (2019)). The quality of the interaction therefore affects firms' willingness to hire workers from the minority group going forward and how firms interact with refugee workers - in terms of employment and tasks assigned (Lepage (2022)).

Finally, and crucially, we find that the one-week exposure intervention had a substantial impact on actual hirings in the subgroup of firms that initially held a positive attitude toward refugees and were (randomly) matched with a refugee with positive attitudes toward locals. The effect we estimate can be interpreted as an externality: a match with a refugee with a positive attitude toward locals increases the firm's willingness to hire refugees in general, especially when the firm manager's initial attitudes toward refugees are also positive. Attitudes are complementary and reinforce the effect of contact in the workplace.

1.1. **Related Literature.** We contribute to three strands of literature. First, we relate to work studying the effects of active labor market policies in reducing the entry barriers for disadvantaged workers. Some interventions improve firms' access to information about the quality of job seekers (Bassi and Nansamba (2022); Carranza et al. (2022)), help workers make their skills more accessible to the employers (Pallais (2014); Abel et al. (2020); Abebe et al. (2021)) or adjust workers' and employers' expectations (Bandiera et al. (2021); Abebe et al. (2023)). By contrast, we contribute to this literature studying the effect of an intervention that targets firms' demand for workers from a disadvantaged group. More generally, this study follows and complements several recent field experiments matching jobseekers and firms in urban low-income settings (Groh et al. (2015); Crépon and Premand (2019); Abebe et al. (2019); Alfonsi et al. (2020); Alfonsi et al. (2022); Brown et al. (2022); Hardy and McCasland (2023)).[1] To the best of our knowledge, our study is the first to experimentally lower barriers for firms to hire a disadvantaged group of workers, by incentivizing a short-term work relationship.

Second, we connect to the literature on programs using intergroup contact to foster the integration between different groups (Paluck and Green (2009); Broockman and Kalla (2016); Scacco and Warren (2018); Rao (2019); Mousa (2020); Lowe (2021); Bursztyn et al. (2021); Corno et al. (2022)). Unlike previous research, we experimentally vary intergroup contact and exposure in the workplace. This is crucial and our second main contribution, as it allows us to test if contact works in an economically relevant and potentially costly context such as the workplace.

Third, our paper links to the growing body of work on the labor market integration of refugees and forcibly displaced people (Battisti et al. (2019); Arendt et al. (2021); Fasani et al. (2021); Fasani et al. (2022); see Becker and Ferrara (2019) for a review). While a large majority of papers in this literature focus on rich economies, few studies take place in low- or middle-income economies (Caria et al. (2020); Blair et al. (2022); Baseler et al. (2022)). Furthermore, rigorously evaluated randomized control trials in this area are rare (Schuettler and Caron (2020)). We contribute to this literature by designing and evaluating a labor market experiment in a large low-income country, where refugees are legally allowed to seek employment. This is very important for policy, as the largest majority of refugees worldwide are hosted in low- and middle-income countries.

The remainder of the paper is organized as follows. Section 2 describes the context of this study. Section 3 introduces the samples of refugee workers and Ugandan employers. Section 4 details the experimental design, tests the randomization protocol and describes the main outcomes of the paper. Section 5 reports the results of the experiment. Section 6 discusses the policy implications of the results. Finally, Section 7 concludes the paper.

2. Institutional Setting

In this section, we explain why Uganda is a well-suited environment for our purposes. First, we describe the institutional environment of Uganda as a refugee host

[1]See also Caria et al. (2024) for a review of the literature on barriers to job search and hiring in urban labour markets in low-income countries.

country. Second, we describe the population of refugees in the country, using data from the United Nations High Commission for Refugees (UNHCR) Uganda.

2.1. **The Ugandan Refugee Policy.** Uganda is currently the largest refugee host country in Africa and, as of the end of 2022, one of the five largest in the world. Uganda opened its borders to 7,000 refugees from Poland during the Second World War (Lwanga-Lunyiigo (1993)). Since then, it has always endorsed refugees' integration with an open-door policy. Today, Uganda is considered to be one of the most welcoming refugee host countries in the world.[2] As of 2022, it hosted approximately 1.5 million refugees, the majority of whom came from neighboring countries: South Sudan, the Democratic Republic of Congo, Somalia, and Burundi.[3] The Ugandan Refugees Act 2006 and its subsequent amendment in 2010 allow refugees to move freely within the country. Refugees can seek employment opportunities, and share access to education, health, and other basic services with the local communities. As highlighted by a recent report of the Center for Global Development, Uganda has one of the most open policies towards refugees' rights, both *de jure* and *de facto*, and at similar levels as many OECD countries (Ginn et al. (2022)).

2.2. **Refugees in Uganda.** While the majority of refugees in Uganda live in settlements shared with the host communities in rural areas, approximately 8.5 per cent are registered as dwellers of Kampala, which is the largest urban refugee settlement in the country.[4] Since the target of our experiment is urban refugees, we focus on refugees living in this city. Kampala hosts 44 per cent of all business establishments and almost 50 per cent of non-agricultural jobs in Uganda (Sladoje et al. (2019)). It is therefore the location where most of the skilled refugees belonging to our sample look for employment opportunities (Appendix Figure A.1, Panel A). Approximately 70 per cent of refugees residing in Kampala are of working age - 18-59. Overall, approximately 15 per cent of all refugees of working age in Uganda reside in Kampala (Panel B).

3. Sample Selection: Refugee Workers and Ugandan Employers

In this section, we describe how we select the participants to the experiment, on the refugees' and employers' side. We begin by describing our sample of refugee

[2] "As Rich Nations Close the Door on Refugees, Uganda Welcomes Them", *New York Times*, 2018.
[3] https://data.unhcr.org/en/country/uga, portal accessed in December 2022.
[4] As of January 2024, Kampala hosts 140,442 refugees and asylum seekers. See: https://data.unhcr.org/en/documents/details/106545, accessed in March 2024

workers, which we then match to a sample of local employers. Second, we compare our sample of refugees to a nationally representative sample of Ugandans and to a representative sample of refugees living in rural areas outside Kampala.[5] We then describe our sample of firms. Finally, we compare our firms to a representative sample of businesses in Kampala.

Refugees. Our main treatment is an internship for a refugee worker. Therefore, the first step is to search skilled refugee jobseekers living in Kampala. To the best of our knowledge, there are no publicly accessible datasets on individual refugees' characteristics and their location in Uganda, so we leverage on our collaboration with two local refugee-led NGOs, which have access to a wide population of refugees in Kampala. Thanks to their assistance, we listed 1,478 refugees with the following characteristics: i) declaring not to have a permanent job at someone's firm and ii) actively searching for jobs at the time of our data collection. Of these, 1,109 consented to be interviewed during the listing exercise. We exclude 108 refugees that did not possess any employable skills in any of the nationally recognized vocational sectors. Finally, we exclude four refugees who were skilled in sectors that did not reach a critical number for the test to take place.[6]

To verify their skills, we invited a sample of 977 refugees to perform a test, and 552 attended.[7] In partnership with the Directorate of Industrial Training (DIT) and a large vocational institute in Kampala, we organized one examination week during the second half of April 2021. During this week, DIT official examiners tested all the refugees that attended the test, using the DIT's national curriculum.

The test focused on the practical skills of the workers and varied in length, depending on the occupation chosen by the candidate. For instance, hairdressers were asked to execute a hairstyle on a client, chefs to prepare and serve a beef stew, tailors to produce a short-sleeved shirt, and so forth. Appendix Table A.1 sets out which skill

[5]The Uganda Refugees and Host Communities Household Survey (URHHS) conducted by the World Bank in 2018 is representative of both refugee and host communities for Kampala, and the two largest rural regions outside Kampala. However, sampling was imperfect for refugees in Kampala.

[6]At listing, we asked to list the three most important skills they possess and would be ready to be tested on. Appendix Figure A.2, Panel A, list refugees' preferred skills - by whether individual workers attended the test.

[7]Compared with the refugees who did not attend the test, our sample is composed of more experienced and skilled workers, who were more motivated to gain an internship at a local firm and were also more willing to accept a lower wage. Furthermore, they are more likely to have learned their skills outside Uganda (see Appendix Figure A.2, Panel B).

was tested for each occupation. The skill was chosen by the examiners and communicated in advance to the participants during an introductory session that took place a few days before the exam.

The examiners, who are trainers with years of expertise in a specific sector, scored the performance of each candidate on a 0 to 100 basis, following the national guidelines provided by the DIT. Candidates who score at least 65, successfully pass the test. Of the 552 refugees that attended and completed the test, only 11 people failed the exam, and therefore did not obtain a certificate. For this reason, we drop these workers and focus on the ones who passed the test (541). Due to a second wave of COVID-19, we paused the project until September 2021. However, we successfully tracked 527 of the original sample (see our detailed timeline in Figure 1, Panel A). Our final sample is composed by 85 per cent of Congolese (N=448), 11% of Burundians (N=58), 3.61 per cent of Rwandese (N=19) and less than 1 per cent of South Sudanese (that is, only two individuals). The first languages of 72.86 per cent of refugees in our sample are French, Kiswahili (spoken by Congolese), Kirundi (spoken by Burundians), and Kinyarwanda (spoken by Rwandans). This means that the majority speaks a language that is not common in Kampala. The rest speaks English or Luganda as their preferred language.

Finally, nationally representative data collected by the National Bureau of Statistics (Uganda Refugees and Host Communities Household Survey (URHHS)) shows that refugees in Kampala are poorly integrated in the local labor market. Appendix Table A.2 compares our sample of refugee workers to a sample of Ugandans living in Kampala (Panel A) and a sample of refugees in rural areas outside Kampala (Panel B). The latest national household survey conducted in 2018 shows that 56.7 per cent of Ugandans aged 15 to 65 have a job, while the unemployment rate is equal to 11 per cent. Conversely, refugees' unemployment rate is more than three times that of the locals' rate and, conditional on being employed, refugees earn significantly less than Ugandans. Panel B compares our sample of refugees with a sample of other refugees interviewed in the URHHS. We do so to compare refugees in urban areas such as Kampala with those living in poorer areas and show the characteristics of potential workers Ugandan firms could expect to interact with. To sum up, this table suggests that refugee workers participating in our experiment are more educated and active in searching for jobs than the average refugee residing in rural areas.

Firms. Our intervention targets local employers. To construct the sample of employers, we listed and interviewed 1,192 firms active in selected sectors in Kampala, using a random walk sampling procedure.[8] A total of 535 firms fulfilled the two criteria for inclusion into our sample: they were owned by a Ugandan national and they were willing to hire a refugee worker, at least for free, for a period of one week. We elicit willingness to hire a refugee worker using a Becker-DeGroot-Marschak (BDM) mechanism, which we will describe more detail in Section 4. Figure 1, Panel B, maps the location of the firms that belong to our baseline sample.

Our final sample of firms is positively selected along different dimensions, compared with the average firms in similar sectors in Kampala. Appendix Table A.3 compares the characteristics of the firms belonging to our sample and the ones of firms interviewed in the Manpower survey conducted by Uganda Bureau of Statistics in 2016.[9] Our firms are slightly larger, both in terms of employees and revenues (however not all firms disclosed these figures). They are more likely to be owned by higher-educated people and are more likely to keep management books, albeit less likely to be formal, in the sense of paying taxes to the local tax authority. Additionally, they have been operating for a longer period of time. These differences are not surprising, as our firms stated they are willing to expand in the near future, whereas the representative firm in the Manpower survey is significantly less likely to plan to hire new workers in the future.

4. **Experimental Design: Matching Firms to Refugee Workers**

The goal of the experiment is to study whether increase firms' demand for refugees can be increased by changing their beliefs about refugees' skills. The treatment we study is one short-term, fully subsidized internship with one skilled refugee worker. This section has two parts. First, we describe in detail the implementation of the experiment, that is: how we selected the sample of firms and how we assigned employers to treated and control groups. Second, we outline a simple conceptual framework to guide the interpretation of our results.

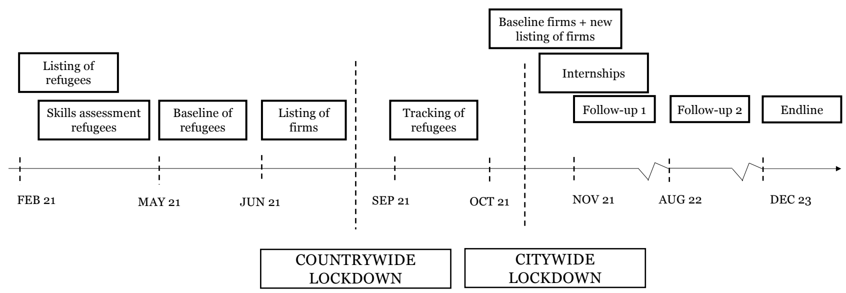
4.1. **Selection into the Experiment, Treatment Assignment and Take-up.**

The experiment focuses on employers who are willing to hire a refugee worker and are

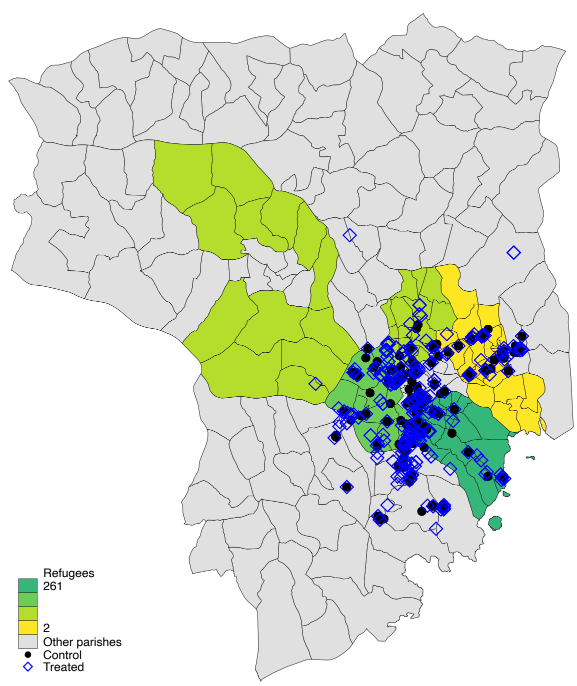
[8] We randomly select a set of neighboring parishes for each day of data collection, based on the Uganda Census of Businesses conducted in 2010. The team leader chooses a landmark and randomly the directions the data collectors are to take to look for respondents. We halted the data collection for one week in October following three terror attacks in the city of Kampala- and resumed when the situation normalized.

[9] *https://www.ubos.org/wp-content/uploads/publications*. Data accessed in July 2019. This survey collected information on the characteristics of Uganda's workforce at employer and employee levels in the formal and informal sectors.

FIGURE 1. Timeline and Firms' Locations



(**A.**) Timeline

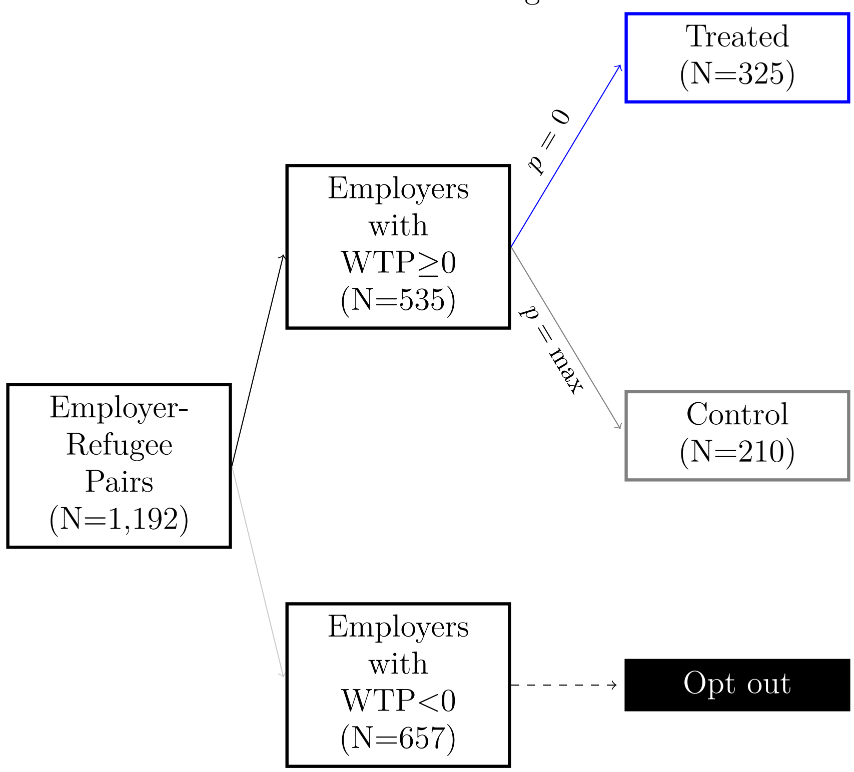


(**B.**) Firms' Locations across Kampala

Notes: The timeline (Panel A) illustrates the sequence of events relevant to the study. The map (Panel B) shows the location of firms belonging to our sample, distinguished by treatment (blue diamonds) and control (black dots) status. Each parish is colored based on the number of refugees from our sample. Darker colors indicate a higher share of refugees living in each parish, while parishes in gray do not host any of the refugees from our sample.

therefore willing to take up the treatment we offer. To elicit the employers' willingness to hire one refugee, we begin by randomly pairing refugees and employers, matching both sides according to the occupation of the refugee worker and the firm's sector. For example, our random algorithm matches refugee cooks with owners of restaurants, beauticians and hairdressers with owners of beauty salons, and so on. Figure 2 summarizes the selection of firms into the experiment and the randomization design.

Figure 2. Design



Notes: This figure plots the design of the experiment. We start with a sample of 1,192 pairs. Of these, 535 belong to the final study sample.

To elicit the employer's WTP for the paired refugee, we use a variation of the BDM elicitation method called Multiple Price List (Becker et al. (1964); Burchardi et al. (2021)). The method consists of a series of take it or leave it offers, where the price (that is, the salary) offered increases at each step. We inform the employers that the salary has already been decided by a computer and has been printed and saved in a sealed envelope which the team will open at the end of the elicitation procedure. We do not inform them of the distribution of this salary, but tell them that the salary is between 0 and $100,000$UGX (that is, approximately 15USD at the nominal

2021 exchange rate). See Section B in the Appendix for the script we used to elicit employers' WTP.

We elicit each employer's WTP for the randomly paired refugee worker twice, varying the level of information shared with the employer.[10] We elicit the first WTP immediately after presenting a document with the profile of the candidate for a one-week internship. The document is a one-page CV containing basic demographic information (a photograph of the worker, gender, age, current address and years since moved to Kampala), years of work experience in the selected occupation and knowledge of languages (see Appendix Figure A.3, Panel A and B). Furthermore, we tell employers that they can hire the worker at any time in the four days following the interview.[11]

If the firm in the treated pair is not interested in hiring the refugee worker (i.e., if the WTP for that specific worker is below 0), we randomly assign the refugee worker to a new firm.[12] The employers with a "negative WTP" (that is, those not willing to hire the refugee worker even for free) opt out of the experiment. We reiterate the process until we obtain the WTP for all treated refugees.

Conditional on the employer's WTP being positive or equal to 0, we then conduct a new WTP elicitation. Following this first elicitation, the research team communicates to a subset (165) of the treated employers that the refugee worker pursued a certificate of vocational skills. To measure whether the certificate affects employers' WTP to hire the worker, we elicit it a second time. We do not show the remaining employers any additional information about the refugee worker. However, we make a more flexible offer to all employers, thus providing the firms with the chance to hire the

[10]Since we have more firms than refugees, multiple employers in the control group may see the profile of the same refugee

[11]To expose the firm owners to the concept of WTP to hire a worker, we begin by the CV of a hypothetical Ugandan worker. For this purpose, we show a CV of one hypothetical worker, a man or a woman, possessing the same characteristics of the real refugee worker randomly assigned to the firm (Appendix Figure A.3, Panel C and D). We carefully explain that the worker is hypothetical, inviting the employer to imagine that a worker like the one we are showing is seeking employment at the firm (see script in the Appendix). We teach the employer the concept of a "random wage" and we ensure that the procedure is clear - by asking comprehension questions at the end of each elicitation. We do not vary the order of the CVs. That is, all the employers first evaluate the profile of the hypothetical worker before that of the real worker.

[12]Younger refugees and who speak better English are more likely to match earlier compared with the rest. By "matching earlier" we mean that the employer(s) they are paired to are more likely to report a non-negative WTP. Refugees assigned to treated pairs and those assigned to control ones are matching with a similar success rate. For more details, see Appendix Figure A.4.

worker in the next eight days. See Appendix Figure A.5 for the original experimental design.

Approximately 45 per cent of the 1,192 firms interviewed at baseline have a non-negative WTP to hire a refugee worker.[13]

We use the second elicitation to allocate approximately 60 per cent of the sample of firms to the treatment group.[14] To do so, we extract a "random salary", W, from a sealed envelope. The random wage determines the outcome of the exercise and allows us to characterize the employers who are willing to take up our treatment. Specifically, if $WTP \geq W$, the employer can hire the refugee worker, otherwise they cannot. In practice though, we have full control of the randomization procedure and extract only two prices: $W = 0\text{UGX}$ and $W = 100,000\text{UGX}$.[15] This ensures the allocation of firms to treatment and control is purely random and does not depend on the employer's WTP. Appendix Figure A.6, Panel A, shows the demand function for a refugee worker at baseline in our sample.

Finally, we facilitate the meeting of the treated firm-refugee pair. Field officers set appointments a few days before the agreed starting date of the internship. The team meets the refugee workers at a prespecified location, which is within walking distance of the firms they are supposed to work for. Firms' take-up of the experiment depends on the refugee's decision to attend the meeting with our field officers. While setting the appointments, the team does not share any information about the firm with the refugee worker. This means that the decision of the refugee worker to attend the appointment does not depend on the characteristics of the firm. If the refugee fails to attend, the internship does not take place.

About 56 per cent of the refugees attended the introductory meetings. As a consequence, about half of the firms assigned to the treatment group were actually treated (in the sense of receiving a refugee intern). The sample of firms that receives a worker

[13]The remaining firms are either not interested in hiring any worker (approximately 35 per cent) or interested in hiring a worker only if Ugandan (about 20 per cent), suggesting some firms discriminate on the basis of the nationality/refugee status of the worker.

[14]Our power calculations are based on the original design of the experiment (see Appendix Figure A.5). About half the treated employers were shown the certificate that the refugee worker obtained on successfully passing the practical skills examination. As the results of the experiment suggest that the two arms are not distinguishable from each other, we pool them into a unique arm to maximize power.

[15]An extensive pilot suggested that the 100,000UGX wage was an unreasonable price for an internship of only one week in the Ugandan small and medium entreprises context. Additionally, fewer than 3 per cent of firms at baseline paid at least 100,000UGX weekly for their employees as soon as they joined the firm.

is balanced in terms of random assignment and has similar characteristics to the sample of firms that did not receive the worker. To sum up, one of the most important determinants of refugees' participation is distance to the business premises. A second consequence of the imperfect take-up, due to some refugees' decision not to attend the meetings, is that some employers become disappointed with refugees. That is, not only can they not experiment working with a refugee, but also some revise negatively their beliefs about refugees. When refugees failed to present for work, employers expressed dissatisfaction with the research firm and the refugees. Examples of comments are *"[...] He was also disappointed with us not giving him a worker"; "He is not happy with us because he told us to match the worker on the day he had agreed with us which was Saturday but up to now he is still waiting for her and no response has been received"; "The firm owner was very disappointed with the worker who was given an internship but didn't show up for work".* Our encouragement design does not affect all treated employers in the same way. As a result, we adjust our preferred specifications in two ways to account for the issues described above. First, we control for firms' location in all specifications. Second, we show results for two different samples: the full sample composed of all firms regardless of whether treatment took place; and the exposed sample, dropping firms that were promised a worker who never showed up.

Appendix Table A.4 reports results from a balance test of characteristics between treated and control firms in the full sample (Panel A) and in the exposed sample (Panel B), where the exposed sample is composed of the firms whose treatment actually took place.

To assess the impact of the intervention, we conduct two follow-up surveys and endline. A first follow-up took place about a month after the matching intervention. In this survey, we tracked 525 firms (attrition is balanced between treatment and control, see Appendix Table A.5, columns 1, and 4). For the second one, which took place approximately 8 months after the intervention, we collected longer term follow-up data using phone calls from the 474 firms we managed to reached to. Appendix Table A.5 assesses attrition at the second follow-up in columns 2 and 5. Finally, we assess balance of attrition at endline in columns 3 and 6.

A total of 182 internships took place, but we successfully tracked 179 firms at the first follow-up. The median duration of the internship was seven days, in line with what employers and workers agreed on. During the internship, employers assigned workers simple and complex tasks (where complexity is measured using a self-reported scale of 1 to 5 collected for each firm-specific task listed at baseline). About 40 per cent of the employers paid their interns on average 19,000UGX (about 4.5USD) for the full week (although the worker in most cases had not asked to be paid). On average, each intern worked for seven hours a day and managers at the firm spent more than five hours supervising the intern each day. The employers did not think that the supervision was too complex (rated on average 2.5 on a scale of 1 to 5), nor communication difficult (on average rated 3). Firms seem quite satisfied with the experience (a median rating equal to 4). Overall, two thirds of the firms who offered internships were willing to rehire the same worker. Seven workers were hired (or 3.9 per cent of the total number of interns). The majority of employers (70 per cent), finally, recommended or would recommend the worker to another firm (Table 1).

Table 1. The Internships

	Mean	Median	SD	Min	Max	N
Agreed days of internship	7.419	7	2.994	1	30	179
Completed days of internship	5.324	7	2.847	1	14	179
Internship was extended	0.101	0	0.302	0	1	179
Hours worked by intern each day	7.331	8	2.637	0	12	179
Intern was paid during internship	0.425	0	0.496	0	1	179
Intern total payment ('000UGX)	19.730	10	21.113	0	140	74
Maximum difficulty of tasks	3.213	3	1.110	1	5	178
Intern supervised by manager	0.911	1	0.286	0	1	179
Daily firm hours spent in supervision	5.771	5	4.135	0	20	179
Supervised more than other workers	0.571	1	0.497	0	1	133
How demanding to supervise this worker	2.553	2	1.250	1	5	179
How difficult communicate with worker	3.335	3	1.302	1	5	179
Overall experience with the worker	3.564	4	1.227	1	5	179
Willing to rehire same worker	0.676	1	0.469	0	1	179
Intern was hired	0.039	0	0.194	0	1	179

Notes: This table reports some summary statistics of the internships that took place. The data comes from the sample of treated firms whose internship took place (N=182), less of employers whom we did not manage to track at follow-up 1.

Taken together, these descriptive statistics show that the internships were short but intense, with the worker present at the business premises for seven hours, five of which the employer spent supervising the worker. Among firms with at least one employee, the employer spent more time supervising the intern than any other employee.

4.2. **Outcomes.** In this subsection we introduce our outcomes of interest. Appendix Table A.6 provides a more detailed description. The goal of the experiment is to study whether exposure to one refugee changes firms' demand for refugees and the employers' beliefs regarding the abilities of refugee workers. Our initial hypothesis is that local employers have erroneous beliefs about the abilities of refugee workers, both in terms of hard and soft skills. At baseline, we collect a measure of employers' beliefs regarding the hard skills of refugee workers by asking what a refugee worker would score on the practical skills examination. We also elicit the employer's beliefs about the score a Ugandan worker would achieve.[16]

Furthermore, we measure employers' beliefs using self-reported ratings between 1 and 5 to different statements regarding skills of refugees: the employer's beliefs about the hard (e.g., theoretical abilities, practical skills, and performance at work) and the soft skills (e.g., time management, team work, and work ethics) of a generic refugee worker who may seek employment in the future; and beliefs regarding how trustworthy and respectful refugee workers are.[17]

Our main outcome of interest is the demand for refugee workers. We measure this using two proxies. The first measure is the number of refugees hired after the experiment. We collect this outcome at follow-up 2, conducted eight months after the intervention, and at endline, conducted approximately two years after the experiment. We collect a second measure of demand for new refugees during the short-term follow-up, approximately one month after the intervention. Specifically, we elicit the employers' WTP to hire a new, hypothetical refugee worker with desirable characteristics in terms of work experience, gender and knowledge of languages. More specifically, we construct CVs with workers having four years of work experience, 26 years of age and good knowledge of both English and Luganda (the main language spoken in Kampala).[18] As a short-term outcome, we use a dummy variable equal to 1 if the firm is willing to hire the new refugee worker at least for free. Not all employers are willing to hire a refugee worker at the first follow-up, either because their WTP is now negative (i.e. they request a positive amount of money to hire the worker) or they are simply no longer interested in refugees.

[16]We randomize the order of the questions so that some employers get to see the question about refugee jobseekers first, and then the question about Ugandans, and vice versa.

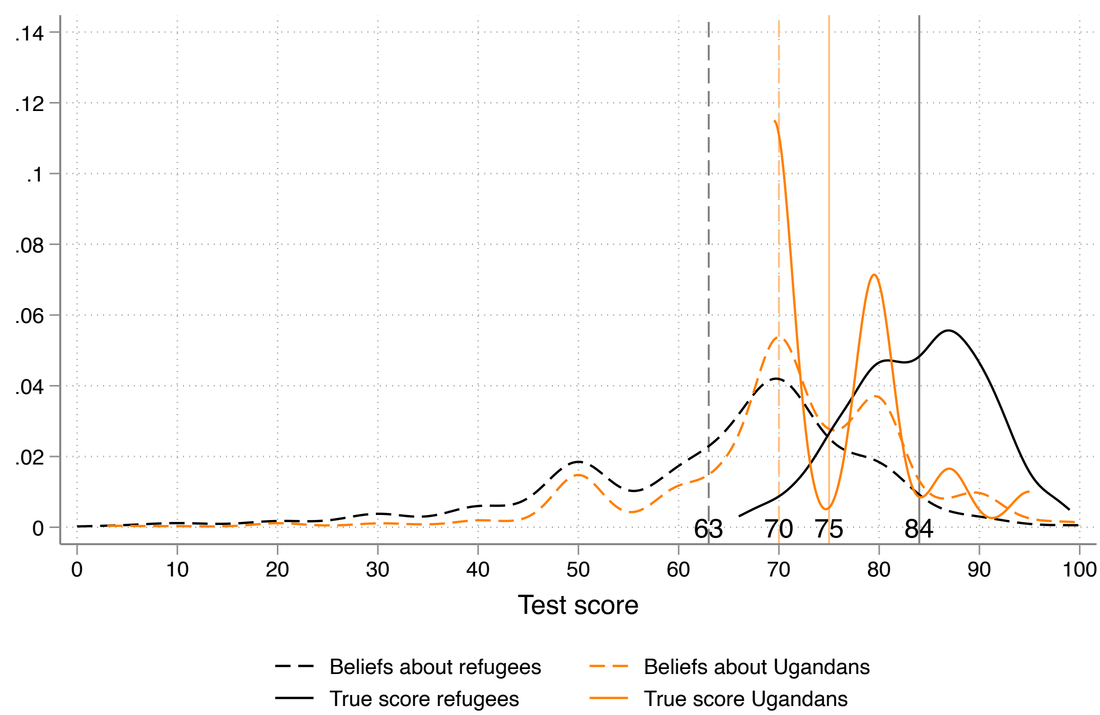
[17]We chose this set of skills after extensive piloting exercises with firms similar to those belonging to our sample. Specifically, we asked pilot firms to rank workers' skills in order of importance for the success of a business like their own.

[18]Employers were not initially aware the profile was one of a hypothetical worker, but we revealed it soon after the elicitation exercise was complete.

4.3. **Conceptual Framework.** We provide a simple conceptual framework to interpret the experiment and guide the interpretation of the results. The experiment investigates how exposure, based on observing one refugee for one week, affects the employer's beliefs about refugees' abilities and their willingness to hire new refugees.

We motivate this framework using data from employers' beliefs. In Figure 3 we compare the employers' beliefs with the actual scores obtained by the refugee workers in our sample. The figure additionally compares employers' beliefs regarding Ugandan workers to a non-random sample of Ugandan workers who took the same test in the last two years at the same test center we worked with. While we do not have access to the exact scores, we use the midpoint of the bins used by the DIT to provide a final result.

FIGURE 3. Firms' Beliefs About Refugees' Ability



Notes: This graph plots the distribution of the employers' beliefs and the real score on the test. Full baseline sample with 1,204 firms. Dashed lines represent the employers' beliefs (i.e. self-reported score they think the jobseeker obtained). Solid lines represent the true scores. Black lines refer to the refugee workers, orange lines refer to Ugandans. Note that Ugandans' scores may not be fully comparable to those of refugees, as the sample we use here to capture "real" scores comprises typically younger and less experienced students.

Figure 3 evidence three facts. First, employers' believe that Ugandan job seekers are significantly better than refugee jobseekers. While, on average, employers believe that Ugandans score 70, they believe that refugees do not pass the test, by assigning an average score of 63 (the threshold to pass the test is 65). Second, their beliefs are biased downwards, and this is particularly true in the case of the refugee workers. Our refugee workers' actual score on the test is 84. Third, employers' beliefs about refugee workers are more dispersed around the mean, compared with beliefs about Ugandan workers, suggesting that Ugandan employers have weak priors regarding refugees. Taken together, these findings reveal that Ugandan employers have weak and incorrect beliefs regarding the ability of refugee workers.

Consider the worker's output contains information regarding the refugee group's mean ability, θ and an individual component ε: $a = f(\theta, \varepsilon)$. If hired by the employer, the worker can produce a signal regarding their ability: $s = a$. The employer cannot observe the average group component, but has some prior beliefs about it. Given the employer's inexperience with refugee workers, the employer's prior is biased: $m_0 < \theta$. The employer's willingness to hire a refugee is a function of the initial beliefs about θ. Furthermore, the firm's utility depends on the expected marginal profit from hiring one refugee. Finally, suppose that firms' profits depend on the worker's output. Given these assumptions, exposure should have a clear impact: first, it affects the employer's beliefs. Specifically, it should increase them, on average, towards the true θ. Consequently, exposure should increase, on average, the employer's willingness to hire new refugees.

Guided by this framework, we turn to the data and test the following two hypotheses: working together increases their demand for new refugees and it improves employers' beliefs.

5. **Results**

This section reports the main results of our study. We establish this estimating the following equation:

$$y_{i1} = \beta_0 + \beta_1 Treated_i + y_{i0} + X_i^{'}\delta + \varepsilon_i, \tag{5.1}$$

where y_{i1} is one of our main outcomes of interest (the demand for new refugees and the beliefs regarding refugees' abilities). $Treated_i$ is a dummy equal to 1 for firms assigned to the treatment group and X_i is a matrix of the randomization strata (the occupations of the refugee workers). The equation always includes area fixed effects to reflect the imperfect compliance caused by the refugees not attending the internships. Appendix Table A.7 explores observable determinants of refugees' take-up of the internships. Whenever possible, we control for the baseline value of the outcome y or its pre-intervention one (therefore, we run an ANCOVA). Standard errors are clustered at the refugee level - to reflect the experimental design where the same refugee might have been presented to multiple firms. In all the estimations, we use OLS. However, using post-double-lasso selection models do not change the results.[19]

We report two separate sets of results. In the first, using the full sample of firms, we show the results of the experiment, that is, the intention to treat (ITT). In the second, using the sample of exposed firms we study the effect of exposure. The core reason of conducting a separate analysis is given by the fact that firms that were promised a worker who did not attend the appointment may have had a negative effect on firms' beliefs regarding refugees. In summary, we cannot instrument exposure with the offer of the treatment because it is not a valid instrument.

5.1. **Exposure to Refugees Increase Firms' Hiring of New Refugees.** We begin by showing that the intervention increased firms' hiring of refugees. Table 2 reports the coefficients estimated by equation 5.1. We measure total number of refugees hired at two points in time (at eight months and 24 months after the experiment). First, we aggregate the responses to create a unique variable. Whenever one observation is missing at any of the two points in time, we use only the observation present. Second, we create a dummy equal to 1 if a firm hired at least one refugee. Third, thanks to the detailed questions we asked, we know if the firm hired the intern we provided them. We are therefore able to provide evidence on whether the results are driven by recalling the same worker.

Table 2 shows that a short-term intervention, more specifically an internship of one week, increases significantly the number of refugees hired by firms, compared to

[19] In the original study design, before eliciting their WTP to hire the refugee worker, we showed to a subsample of the treated firms the refugee's certificate of skills obtained after the test. The results of the two treatment arms are positive and significant, but not statistically distinguishable one from another. We report the original design in Appendix Figure A.5. Furthermore, we rerun specification 5.1 using two dummies instead of one:

$$y_{i1} = \beta_0 + \beta_1 T_1 + \beta_2 T_2 + y_{i0} + X_i^{'} \delta + \varepsilon_i \tag{5.2}$$

Results are not reported but are available upon request. We fail to reject the null of the effect being the same between the two treatment arms.

Table 2. Number of Refugees Hired

	At least 1			Total		
	(1) *Any refugee*	(2) *New only*	(3) *Ugandans*	(4) *Any refugee*	(5) *New only*	(6) *Ugandans*
Panel A: ITT						
Treated	0.073**	0.040	0.018	0.170***	0.114**	-0.061
	(0.034)	(0.033)	(0.043)	(0.056)	(0.055)	(0.212)
	[0.031]	[0.221]	[0.676]	[0.003]	[0.038]	[0.773]
Firms	507	507	507	507	507	507
Mean DV	0.169	0.169	0.617	0.219	0.219	1.816
Panel B: Effect of exposure						
Exposed	0.127***	0.078*	0.032	0.257***	0.171**	-0.075
	(0.043)	(0.041)	(0.051)	(0.075)	(0.072)	(0.251)
	[0.004]	[0.056]	[0.531]	[0.001]	[0.018]	[0.765]
Firms	371	371	371	371	371	371
Mean DV	0.169	0.169	0.617	0.219	0.219	1.816

Notes: This table reports the coefficients estimated by equation 5.1. *Dependent variables*: Columns 1 to 3: dummies equal to 1 if the firm has hired any refugee (col. 1), only new refugees, excluding the worker that completed the internship (col. 2) and Ugandan workers (col. 3). Columns 4 to 6: total number of refugees (col. 4), total number of new refugees (col. 5) and total number of Ugandan workers (col. 6). *Controls*: 15 randomization strata (refugees' occupations: tailor, cook, hairdresser, domestic electrician, arts & crafts maker, painter, baker, motorvehicle mechanic, barber, beautician, hotel staff, plumber, carpenter, leather designer, bricklayer, electronic technician, welder and waiter) and six area fixed effects (dummies identifying the location of the business premises: Central Kampala, Nakawa, Kawempe, Rubaga, Makindye and Wakiso). Standard errors are clustered at the level of the refugee paired with the firm. P-values reported in square brackets. ***, **, *, indicate significance at the 1%, 5%, and 10% levels respectively.

the control group. Panel A shows the ITT effect of the experiment, using the full (non-attrited) sample. Panel B focuses on the effect of exposure, dropping firms that were not treated because the refugee worker did not attend the internship. The first three columns report results on the extensive margin, that is: the number of firms hiring at least one refugee worker. Columns 4 to 6 instead focus on the intensive margin, that is: the total number of refugee workers hired. The ITT effect of the experiment, reported in Panel A, is large and equal to increasing the number of firms hiring at least one refugee by 43 per cent (column 1). The effect on total number of refugees is also substantial and economically meaningful: an increase of 77.6 per cent over the control mean. The result on the intensive margin is driven by the number of new refugees hired, that is excluding the worker matched during the internship (52 per cent). The comparison with Panel B allows to demonstrate that the effect is concentrated among firms for which the internship actually took place. The effect on the extensive margin is equal to 75 per cent. Column 2 now shows that the effect on the number of firms hiring at least one new refugee is significant (pval = 0.056) and equal to 46 per cent. Columns 4 and 5 show that the effect on the intensive margin is substantially larger than the results reported in Panel A. Exposed firms hire 117 per cent more refugees than the control group. Importantly, they hire 78 per cent more new refugees compared to the control group. Finally, columns 3 and 6 show that there is no negative effect on hiring new Ugandan workers, as none of the coefficients is statistically distinguishable from zero.

5.2. **Firms Become More Supportive of Refugees' Integration.** The experiment increases firms' owners' support for refugees' integration. We show this in Table 3. Column 1 asks firms how much they are willing to donate to a non-profit organization that assist refugees in Uganda by providing them skills and helping them find jobs. It shows that treated employers are significantly more likely to donate compare to control ones (Panel A), and the effect is concentrated among firms for which the internship took place (Panel B). Treated employers are also more likely to be connected to non-profits that can eventually help them in finding refugee workers (column 2). Taken together, these two findings suggest that the experiment increased firms' support for labor market integration of refugees.

Column 3 and 4 additionally suggest that employers' general view about refugees improved. While 40 per cent of control employers say that Uganda's cultural life is enriched or very much enriched by refugees, about one-third more employers in the treatment group agree. Finally, over time, 37.5 per cent of control employers declare that their general view of refugees improved, whereas close to 41 per cent of treated employers share this opinion.

5.3. **Firms Improve Their Beliefs About Refugees.** In order to explore mechanisms, we use the short-term follow-up survey and investigate whether firms update their beliefs regarding the skills of refugees and whether this affects firms' demand for hypothetical refugees.

TABLE 3. Attitudes Towards Refugees' Integration

	Donation to NGO	Knows anyone at NGO	Cultural enrich	Views improved
	(1)	(2)	(3)	(4)
Panel A: ITT				
Treated	0.165*	0.041	0.056	0.085
	(0.091)	(0.032)	(0.052)	(0.052)
	[0.070]	[0.199]	[0.281]	[0.102]
Firms	525	407	407	407
Mean DV	0.000	0.094	0.400	0.375
Panel B: Effect of exposure				
Exposed	0.255**	0.069*	0.146**	0.133**
	(0.109)	(0.039)	(0.060)	(0.060)
	[0.020]	[0.083]	[0.016]	[0.027]
Firms	385	299	299	299
Mean DV	0.000	0.094	0.400	0.375

Notes: This table reports the coefficients estimated by equation 5.1. *Dependent variables*: Column 1: Donation to non-profit helping refugees, standardized using method described in Anderson (2008) - collected at follow-up 1. Column 2: A dummy equal to 1 if the employer knows anyone at non-profit organizations who can help in matching with a refugee worker if needed - collected at endline. Column 3: A dummy equal to 1 if the employer reports that Ugandan culture is enriched or very much enriched by the presense of refugees from other countries - collected at endline. Column 4: A dummy equal to 1 if the employer states that his/her view about refugees improved during the past year - collected at endline. *Controls*: 15 randomization strata (refugees' occupations: tailor, cook, hairdresser, domestic electrician, arts & crafts maker, painter, baker, motorvehicle mechanic, barber, beautician, hotel staff, plumber, carpenter, leather designer, bricklayer, electronic technician, welder and waiter) and six area fixed effects (dummies identifying the location of the business premises: Central Kampala, Nakawa, Kawempe, Rubaga, Makindye and Wakiso). Standard errors are clustered at the level of the refugee paired with the firm. P-values reported in square brackets. ***, **, *, indicate significance at the 1%, 5%, and 10% levels respectively.

As predicted by our simple conceptual framework, treated firms improve their beliefs about refugees' skills, especially soft ones. Table 4 reports the results on employers' beliefs. On average, the assignment to treatment does not have any effect on employers' learning (Panel A). Using the exposed sample to determine the effect of exposure, we find that employers update their beliefs upwards: exposure makes them more likely to report a higher rate on refugees' skills, especially soft skills, as well as beliefs about their behavior at work (columns 2 and 3). In the Appendix, we show the effect of exposure on beliefs about each individual skill (Appendix Table A.8).

Column 4 summarizes the effect on learning, computing the average standardized effect of the learning outcomes, averaging the effects in columns 1 to 3, estimating a seemingly unrelated regression system

$$Y = [I_n \otimes T]\beta + \mu \tag{5.3}$$

where Y is a vector of n beliefs outcomes and the square matrix $I_n \otimes T$ collects the Kronecker product of the identity matrix and the treatment assignment vector. Following Kling et al. (2004) and Nyqvist et al. (2019), we collect the estimated coefficient $\hat{\beta}_n$ of the treatment effect on outcome n and standardize it by the standard deviation $\hat{\sigma}_n$ from the control group in outcome n to obtain the standardized coefficient $\tilde{\beta} = \frac{1}{n}\sum_{n=1}^{N}\frac{\hat{\beta}_n}{\hat{\sigma}_n}$ reported in column 4 of Table 4. The coefficient is positive and significant (p-val=0.053), suggesting that the internships worked in updating the beliefs of the treated employers.

Our conceptual framework predicts that employers learn and are therefore more willing to hire new refugees, as soon as immediately after the experiment. We test this prediction, analyzing the effect of exposure on the firms' willingness to hire a new refugee approximately one month after the internship took place. We interpret this measure as the immediate reaction of firms to the internship program.

For this purpose, we show the profile of a new hypothetical refugee worker at follow-up 1. By construction, the new profiles have the same characteristics for all firms (treated and control) in the sample, therefore we can isolate the effect of treatment only.

We repeat the same elicitation conducted at baseline. This time, not all firms in our sample report a non-negative WTP (i.e., some firms are not willing to hire the new worker for any price, including for free). For this reason, our main outcome of interest is a dummy variable equal to 1 if the firm says it is willing to hire the new worker at least for free.[20] While 71 per cent of firms in the control group are willing to hire the worker at a price of 0UGX, we find that treated firms are not more willing to hire a new refugee worker. Table 5 shows that the treatment effect is essentially zero, i.e., we find no evidence that treatment in the full sample (Panel A) or in the group of exposed firms (Panel B) increases firms' demand for a new refugee worker.

[20]There are two further reasons not to use WTP for the new refugee. First, treated firms may have learned that refugees would accept a low wage and are therefore willing to pay a lower wage to hire the worker. Second, control firms that still have open vacancies and are most in need of a skilled worker may have learned through the WTP exercise that increasing their WTP will increase their chances of securing the worker.

Table 4. Beliefs About Refugees' Skills

	Hard skills	Soft skills	Behavior	Avg. std. effect
	(1)	(2)	(3)	(4)
Panel A: ITT				
Treated	-0.056	0.126	0.159	0.060
	(0.094)	(0.105)	(0.103)	(0.072)
	[0.550]	[0.228]	[0.126]	[0.409]
Firms	525	525	525	525
Mean DV	-0.000	0.000	-0.000	
Panel B: Exposed sample				
Exposed	0.010	0.271**	0.331***	0.163*
	(0.114)	(0.123)	(0.120)	(0.084)
	[0.928]	[0.029]	[0.006]	[0.053]
Firms	385	385	385	385
Mean DV	-0.000	0.000	-0.000	

Notes: This table reports the coefficients estimated by equation 5.1. *Dependent variables*: Indices computed following Anderson (2008), using the following underlying covariates: theoretical skills, practical skills and speed for the index on hard skills (col. 1); work ethics, time management and team work ability for the index on soft skills (col. 2); trust and respect for the index on behavior (col. 3). Column 4 aggregates the results using the average standardized effect across the underlying components of all the indices. *Controls*: 15 randomization strata (refugees' occupations: tailor, cook, hairdresser, domestic electrician, arts & crafts maker, painter, baker, motorvehicle mechanic, barber, beautician, hotel staff, plumber, carpenter, leather designer, bricklayer, electronic technician, welder and waiter) and six area fixed effects (dummies identifying the location of the business premises: Central Kampala, Nakawa, Kawempe, Rubaga, Makindye and Wakiso). Standard errors are clustered at the level of the refugee paired with the firm. P-values reported in square brackets. ***, **, *, indicate significance at the 1%, 5%, and 10% levels respectively.

The estimated standard errors for the dummy are small, and range between .04 and .049. Note that the point estimate in the full sample is more than five times greater than the point estimate in the exposed sample, suggesting there are firms that are considerably more negative than control ones, among employers whose internship did not take place. Column 2 shows the effect of treatment on the continuous measure of WTP, that is, conditional on WTP being non-negative. Point estimates are not significant and economically irrelevant.

In the Appendix, we report the curves for the demand of a new refugee by treatment status. The null effect persists not only on average, as shown in Table 5, column 2, but also across the distribution of the WTP. Kolmogorov-Smirnov tests do not reject the null of equal distributions across the three groups. Visually, Appendix Figure A.6, Panel B, shows the demand does not shift differentially across the groups, with no difference between the full sample and the exposed sample.

Table 5. Short-term Demand for a New Refugee Worker

	WTP≥ 0	WTP
	(1)	(2)
Panel A: ITT		
Treated	-0.021	-632.801
	(0.041)	(2658.012)
	[0.610]	[0.812]
Firms	525	368
Mean DV	0.709	21301.370
Panel B: Effect of exposure		
Exposed	-0.004	-833.266
	(0.049)	(3080.611)
	[0.938]	[0.787]
Firms	385	273
Mean DV	0.709	21301.370

Notes: This table reports the coefficients estimated by equation 5.1. *Dependent variables*: Column 1: a dummy equal to 1 if the employer is willing to hire the new hypothetical refugee worker (i.e. has a non-negative WTP for the worker); Column 2: WTP for the new worker in UGX, conditional on the WTP being at least equal to 0. *Controls*: 15 randomization strata (refugees' occupations: tailor, cook, hairdresser, domestic electrician, arts & crafts maker, painter, baker, motorvehicle mechanic, barber, beautician, hotel staff, plumber, carpenter, leather designer, bricklayer, electronic technician, welder and waiter) and six area fixed effects (dummies identifying the location of the business premises: Central Kampala, Nakawa, Kawempe, Rubaga, Makindye and Wakiso). Standard errors are clustered at the level of the refugee paired with the firm. P-values reported in square brackets. ***, **, *, indicate significance at the 1%, 5%, and 10% levels respectively.

5.4. **Causal Forest.** To investigate what drives some firms to increase their demand while some others decrease it, we take an agnostic approach. We run a causal forest algorithm and allow the data tell us which covariates are more likely to predict heterogeneous treatment effects. This method will allow us to detect unanticipated results, explore multiple dimensions of heterogeneity, and limit the risks of p-hacking, especially when the heterogeneity analysis is not prespecified (Davis and Heller (2017)).

Causal forest is a machine learning method that allows to predict the heterogeneity in the causal treatment effect. More precisely, it estimates the CATE, that is the average treatment effect conditional on a vector of baseline covariates:

$$\tau(X) = E[Y_{1i} - Y_{0i} | X = x] \tag{5.4}$$

where Y is the outcome of interest and X is a vector of baseline observables. This method emerged with the theoretical work of Athey and Imbens (2016) and Wager and Athey (2018), and the empirical application of the algorithm in Athey and Wager (2019) and Davis and Heller (2017), Davis and Heller (2020). Since then, empirical papers using experiments adopted the causal forest algorithm to investigate heterogeneity in the data (for example, Carlana et al. (2022); Athey et al. (2021)).

First, we run the algorithm on the exposed sample of 385 observations. Given the small sample size, we train the algorithm growing a large number of trees (200,000). This procedure should guarantee the confidence intervals are accurately estimated and is recommended by the creators of the algorithm to obtain stable estimates.[21] Furthermore, we use the so-called "honest approach": we split the training sample in half, with only half of the observations used to grow a tree and the other half used to estimate the treatment effect in each leaf, in mutually exclusive sets. As the covariates fed into the causal forest, we choose firms', workers' and matches' characteristics that may affect firms' willingness to hire a new worker. Using our rich data from the employers' and the refugees' surveys, we construct indices using the first factor from a factor analysis. For each index, we create a dummy equal to 1 if the individual observation has a value greater than the median. Therefore, employers with an index value greater than the median display a high prevalence of the concept represented by the index. We include the following firm- and employer-, refugee- and match-specific variables: the employers' experience with hiring a migrant; a dummy equal to 1 if the employer belongs to the major ethnic group of the Baganda; attitudes towards labor market integration of refugees; the perceived cost of learning about refugees' skills; the willingness to expand their businesses; management quality; current size (in terms of number of employees, number of tasks and number of business premises); a dummy equal to 1 if the firm's sector is manufacturing; beliefs regarding the skills of the matched worker; the worker's ability; attitudes towards Ugandans and Ugandan culture; knowledge of languages; their experience with Ugandan employers in the

[21]The resulting `excess.error` is negligible and equal to $2.79e - 07$.

past; age; country of origin. Finally, we include a dummy equal to 1 if the worker lives in the same neighborhood the business premises are located and if the employer and the worker are the same gender.

Second, we compute the out-of-bag predicted CATE estimate, that is, the predictions produced by the algorithm using trees that do not include observation i. We use it to identify what covariates are associated with heterogeneity in the treatment effect.

Third, once we have obtained the individual predictions, we split the training sample into two groups with respect to the median: observations with a high predicted CATE, belonging to the top 50 per cent, and those with low predicted CATE, belonging to the bottom 50 per cent.

Fourth, we investigate what characteristics are associated with high predicted CATE using two different methods: first, we run a balance test across the two different groups of observations, and correcting the p-value of equality using the method suggested in List et al. (2019). Second, we use a doubly robust estimator to compute the best linear projector of $\tau(X)$ (Chernozhukov et al. (2018)).

Table 6 reports the results of the balance test and the average value of CATE across a variety of characteristics. This table reveals two things. First, there are only two characteristics surviving the correction of the p-values, and therefore significantly associated with a heterogeneous predicted CATE: the employer's attitudes and the refugee's attitudes. Second, the differences between these groups are largest when compared with other characteristics. While 64.2% of the employers with low predicted CATE have positive attitudes towards refugees, 83.9% of those with high predicted CATE have positive attitudes. Furthermore, 86.5 per cent of the employers with high predicted CATE match with a refugee with positive attitudes towards locals, whereas only 5.2 per cent of those with low predicted CATE match with a positive refugee. Appendix Table A.9 reports the results from the best linear projector estimation.

Finally, Appendix Figure A.7, Panel A, depicts a heat map of the predicted CATE across bins of the indices of refugee's attitudes and firm's attitudes. It shows that the better the initial attitudes of both the firm and the refugee, the more positive the firm's predicted CATE (colder colors). And vice versa, the worse their initial attitudes, the lower the predicted CATE (warmer colors).

5.5. **Why Would Employers' Attitudes Matter?** To understand why attitudes matter, we return to the conceptual framework and extend it to include the role of the employers' attitudes, and then additionally include the role of the workers'

Table 6. Heterogeneous Treatment Effects Predicted by Causal Forest

Variable	Low CATE	High CATE	Diff.	MHT pval
Owner is from majority	0.705	0.635	-0.069	0.818
Employer's attitudes	0.642	0.839	0.196	0.000
Firm's initial beliefs	0.430	0.552	0.122	0.192
Employer's learning costs.	0.528	0.490	-0.039	0.970
Firm's willingness to expand	0.269	0.286	0.017	0.918
Firm's quality	0.446	0.521	0.075	0.825
Firm's size	0.523	0.474	-0.049	0.975
Manufacturing sector	0.316	0.339	0.022	0.953
Ever hired a migrant	0.383	0.344	-0.040	0.976
Refugee's ability	0.534	0.469	-0.065	0.908
Refugee's attitudes	0.052	0.865	0.813	0.000
Refugee ever employed by Ugandan	0.275	0.250	-0.025	0.972
Refugee's knowledge of languages	0.161	0.104	-0.056	0.731
Refugee's age	33.565	34.323	0.758	0.951
Refugee is Congolese	0.912	0.849	-0.063	0.499
Neighborhood proximity	0.109	0.120	0.011	0.750
Gender match	0.829	0.792	-0.037	0.963

Notes: This table reports summary statistics for the CATE predicted using a causal forest algorithm. "Low CATE" refers to observations whose predicted CATE is below median. vice versa, "High CATE" refers to observations with predicted CATE above median. The third column collects the coefficient β_1 estimated by the following equation: $y_i = \beta_0 + \beta_1 \mathbb{1}(high) + \varepsilon_i$, where y_i is one of the characteristics included in the causal forest algorithm and $\mathbb{1}(high)$ is an indicator equal to 1 if the predicted CATE is above median. Standard errors are clustered at the level of the refugee paired with the firm. Finally, last column reports the p-value of this coefficient, corrected using a Multiple Hypothesis Testing correction as in List et al. (2019).

attitudes. First, to understand what attitudes means in our context, we begin by explaining how we constructed the indices (see Appendix Table A.10 and Appendix Table A.11 for a full description). To construct the attitudes of the employers, we use the employers' responses to the following question: *"To what extent do you agree with the following statement: When jobs are scarce, Ugandans should have more right to a job than refugees"*. Options were on a scale of 1 to 5, where 1 denotes "Strongly Disagree" and 5 "Strongly Agree". To construct the index we create a dummy equal to 1 if the employer answers below 4. Furthermore, we construct a dummy equal to 1 if the answer to the following question is positive: *"Do you think that refugees should be allowed to work in Uganda?"* Finally, we run a factor analysis and extract the first factor. Therefore, a positive employer is someone who encourages labor market integration of refugees.

One possible way of interpreting the role of attitudes among employers is the following: the supervision of a worker is costly. Additionally, an employer devoting time to a worker on probation will be required to reduce their attention to more profitable activities. This is likely to be happening in micro-, small and medium-sized entreprises such as those in our sample, where managers do not fully delegate responsibilities to other workers (Bassi et al. (2022)). Assuming that employers need to exert effort to learn about the skills of refugees, and that the greater their effort, the more they will learn. An employer decides on their effort weighting the benefit of learning about the productivity of refugees (which is a function of the prior beliefs) and the cost of exerting effort (c). Suppose also that how much effort an employer exerts depends on their initial attitudes towards refugees, δ. The optimal effort level will thus depend not only on the cost of exerting learning effort, c, but also on how easy is to interact with a refugee, that is: δ. Employers with more open views about refugees are more likely to exert more effort than those with less open views. Conversely, employers with negative views (e.g., those that have a very high value of δ) will be less likely to exert effort, and will therefore be less likely to learn. Together, these two assumptions together now predict the distinction between two groups of employers. Positive employers will exert more effort to learn and are going to learn more about refugees. Consequently, their willingness to hire a refugee will increase, given that on average initial beliefs are biased. Conversely, negative employers are less likely to exert effort and learn. Therefore, their willingness to hire a refugee should not change compared to the control group.

5.6. **Why Would Workers' Attitudes Matter?** The causal forest algorithm predicts that workers' attitudes are associated with heterogeneous effects on the demand for new refugees among employers. We construct workers' attitudes as follows. First, we construct dummies equal to 1 if the refugee worker agrees or strongly agrees with the following statements: "*Ugandans' culture is different from my own culture*", "*Ugandans discriminate against refugees*", "*I assume that in general, Ugandans have only the best intentions*", "*Sharing work between Ugandans and refugees is beneficial for both groups*". We interpret the first factor from a factor analysis on these variables as the sense of belonging refugees feel in Uganda. A positive refugee is one who feels a tighter cultural proximity to Ugandans and perceives to be more integrated.

In what follows we conceptualize why these attitudes matter. Suppose that refugees' attitudes affect their efforts at work. Refugees with positive attitudes are more likely to exert effort at work. This affects the employers' learning, who subsequently update more on refugees' skills compared to an employer in control. The opposite happens when a refugee with negative attitudes matches with an employer, who in turn does not learn or learns less compared to the control group.

This extended framework produces two additional predictions:

1) Employers with positive attitudes matching with workers with positive attitudes exert more effort to learn about refugees, learn more because the worker is more motivated on the job and therefore learn more about refugees' skills. Given that exposure is a positive experience, the employers' attitudes improve even more, and become more positive. As a consequence, employers' willingness to hire new refugees unequivocally increases.
2) Employers with negative attitudes matching with workers with negative attitudes do not learn as much. Given the exposure is also a negative experience, the employer may become even more negative about refugees. As a result, their willingness to hire a refugee may decrease.
3) The effect in groups with opposing attitudes is less clear. Two different forces are at play: refugees' effort on the job and employers' effort on learning. Given that neither of the two prevail, the total effect on learning and the demand for new refugees may not be distinguishable from zero.

These predictions are also supported by the literature on social psychology. Specifically, studies have established the opposite role of positive versus negative contact. Allport (1954) already warned that the "wrong kind of contact" could exacerbate perceived differences between groups, "prompting an increase in negative emotions and stereotypes" (McKeown and Dixon (2017)). More recently, empirical work has shown the polarizing effects of positive versus negative contact (Barlow et al. (2012); Paolini et al. (2010)). Our results can be explained by combining insights from economic learning models with social psychology theories on intergroup contact.

5.7. **Quantifying the Heterogeneous Effect of Initial Attitudes.** The causal forest reveals which baseline characteristics of the employers or the match influence heterogeneous effects. In particular, we find two characteristics that can be combined:

the employer's and the refugee's attitudes towards each other. Combining these groups, we estimate the effect of exposure using the following specification:

$$y_{i1} = \beta_0 + \beta_1 T \times Positive + \beta_2 T \times Mixed + \beta_3 T \times Negative + y_{i0} + X_i^{'}\delta + \varepsilon_i \tag{5.5}$$

where $T \times Positive$ is a an indicator for treated positive employers that matched with a positive refugee, $T \times Negative$ an indicator for treated negative employers that matched with a negative refugee, and $T \times Mixed$ is an indicator variable for treated negative (positive) employers that matched with a positive (negative) refugee. Each coefficient tells us the effect of treatment among a specific match. A test of equality between coefficients tells us whether the effect is significantly different across these groups.[22] Finally, X_i contains strata and area fixed effects.

Table 7 presents the heterogeneous treatment effect across the three groups on the willingness to hire the new hypothetical refugee worker, using equation 5.5. Positive matches are more likely to generate an increase in the willingness to hire a new refugee worker. The increase is equal to 11pp, equivalent to an increase of 15.5 per cent over the mean. Vice versa, when the match is negative, the employer's willingness to hire a new worker reduces by approximately 17.6pp, i.e. a reduction of approximately 25 per cent. When testing the equality of coefficients β_1 and β_3, we can reject the null hypothesis that they are equal. The effect on mixed matches is small and not distinguishable from zero.

These results are robust to the method we use to estimate the effect of exposure. Failing to account for model selection may lead to invalid inference (Leeb and Pötscher (2005)). In summary, the finite sample properties of post-model-selection estimators may not be similar to the respective asymptotic distributions. While it is not yet theoretically clear whether standard errors are not correctly specified once we run a regression post-causal forest, there are some methods of addressing this issue. We therefore use a doubly robust estimator to reestimate equation 5.5 and report the results in Appendix Table A.12. These results are stronger than those reported in column 1 of Table 7, suggesting that model specification biased downwards the OLS results. Now, positive matches increase the employers' willingness to hire by about 20pp, that is more than 28 per cent over the mean, while negative matches decrease it by almost 28pp, that is more than 39 per cent. Finally, additional robustness, we

[22]There are two mixed groups, one where the employer has positive attitudes and the refugee worker has negative attitudes, and another one where the opposite is true. Since our conceptual framework predicts that the effect is ambiguous in both these groups, we merge them into one group.

perform randomization inference, conducting 50,000 simulations under the sharp null of no heterogeneous treatment effect. Appendix Figure A.7, Panel B, reports the p-values of β_1 and β_3, as well as the p-value from the test of equality between the two coefficients, using randomized-based inference (RBI). The RBI p-values are similar to those reported in the main regression in Table 7.

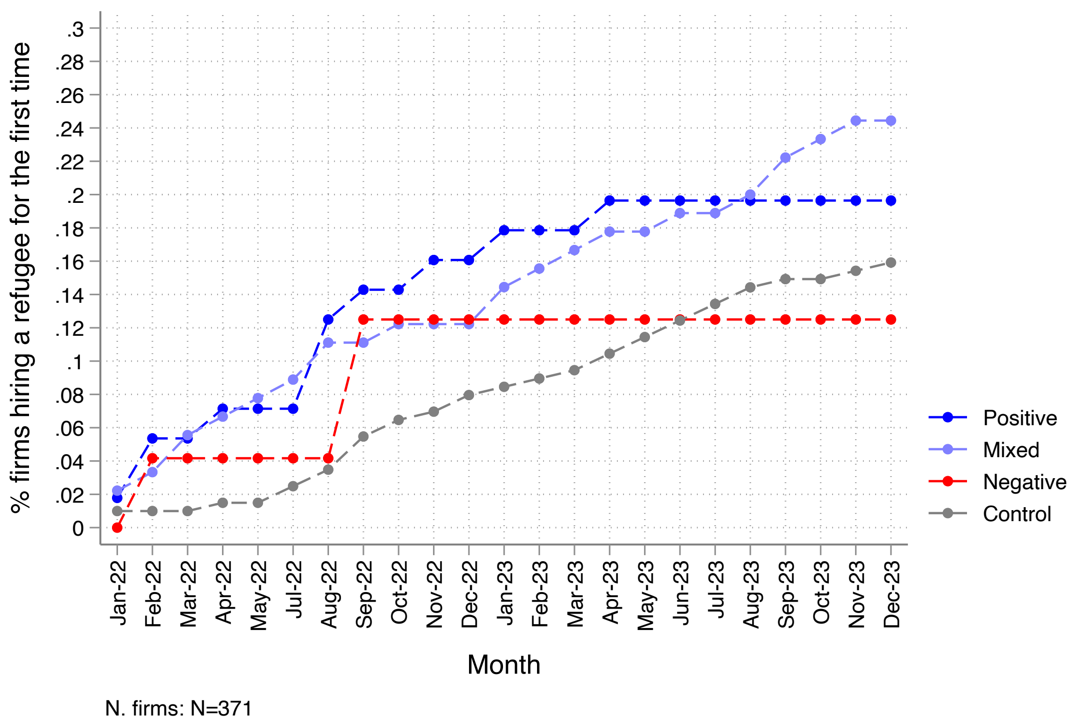
Table 7. Attitudes and Short-term Demand for a New Refugee Worker

	WTP≥ 0	WTP
	(1)	(2)
Exposed × Positive match	0.110*	-806.497
	(0.062)	(4649.798)
	[0.079]	[0.862]
Exposed × Mixed	-0.020	-1581.351
	(0.059)	(3422.419)
	[0.736]	[0.645]
Exposed × Negative match	-0.176*	2870.460
	(0.103)	(7872.258)
	[0.089]	[0.716]
Firms	385	273
Mean DV	0.709	21301.370
H_0: Positive=Mixed	0.065	0.872
H_0: Positive=Negative	0.010	0.684
H_0: Mixed=Negative	0.150	0.574

Notes: This table reports the coefficients estimated by equation 5.5. *Dependent variables*: Column 1: a dummy equal to 1 if the employer is willing to hire the new hypothetical refugee worker (i.e. has a non-negative WTP for the worker); Column 2: WTP for the new worker in UGX, conditional on the WTP being at least equal to 0. *Controls*: 15 randomization strata (refugees' occupations: tailor, cook, hairdresser, domestic electrician, arts & crafts maker, painter, baker, motorvehicle mechanic, barber, beautician, hotel staff, plumber, carpenter, leather designer, bricklayer, electronic technician, welder and waiter) and six area fixed effects (dummies identifying the location of the business premises: Central Kampala, Nakawa, Kawempe, Rubaga, Makindye and Wakiso). Standard errors are clustered at the level of the refugee paired with the firm. P-values reported in square brackets. ***, **, *, indicate significance at the 1%, 5%, and 10% levels respectively.

5.8. **The Persistent Effect of Matching with the Right Attitude.** The heterogeneous effect across groups of attitudes persists over time. Using data from the endline, where we asked detailed questions about when the new refugees were hired, we show that the effect of matching with positive attitudes is consistently higher than the other groups in the 24 months following the experiment. Figure 4 plots the cumulative percentage of firms hiring a *new* refugee worker for the first time after the experiment (that is, disregarding when a firm in the treatment group hired the refugee intern we matched it with). While up to August 2022 (at the time of the second follow-up) 12.5 per cent of the positive matches hired at least one new refugee worker, 11.1 per cent of the mixed matches hired one and only 4.1 per cent in the negative matches did so, compared to 3.5 per cent of the control. By December 2023 (at the time of our endline), the fraction of firms hiring at least one new refugee was higher in the mixed group than in the positive matches. However, when a match was characterized by negative attitudes, the proportion of new firms hiring a new refugee worker was lower than in the control group.

FIGURE 4. Cumulative Share of Firms Hiring New Refugees Over Time



Notes: This graph plots the percentage of firms hiring at least one refugee over time since the end of the experiment (that is, from January 2022). Each color represents a different treatment arm by "quality" of the match. The gray line represents the cumulative probability of hiring at least one refugee among the control firms.

Attitudes are a factor when explaining how refugee jobseekers were hired and what type of work arrangements were in place between firms and newly hired refugees. We investigate two things: i) whether new refugee jobseekers were more likely to be hired without referrals among treated firms compared to control and ii) whether refugees hired by treated employers were more likely to be still employed at the firm and were paid differently compared to workers hired by the control group. Panel B of Table 8, dropping firms for which the internship did not take place, shows three things. First, refugee workers hired by treated employers experiencing a positive match were more likely to be hired without referrals (column 1). That is, employers were more likely to hire refugee jobseekers who walk into the business premises seeking employment, after experiencing a positive match with their intern. Second, employers experiencing a negative match were instead more likely to hire a refugee jobseeker for a short-term period (column 2). Third, these employers pay them less compared to refugee hired by control employers (column 3). The latter findings suggest that workers hired by employers that experienced a negative match were more likely to be employed for temporary low-paid jobs compared to other firms. Panel A shows that there is no difference, on average, between workers hired by treated employers and those hired by control. The bottom panel shows that Ugandan workers are, on average, more likely to be still employed at the firm (62.2 per cent vs 14 per cent) and are paid about 50 per cent more ($15,504$UGX vs $9,464$UGX), suggesting that, on average, local firms are more likely to assign refugee workers to poorly paid temporary jobs.

5.9. **Additional Evidence on the Role of Attitudes.** In this subsection, we take some additional steps to shed light on the complementary role of attitudes.

First, our extended conceptual framework predicts that employers experiencing a positive match are more likely to learn about the refugees' skill set. In line with this prediction, using the beliefs indices described by Appendix Table A.6, and aggregating the coefficients using the average standardized coefficients constructed following equation 5.3, we find that the average positive effect of the exposure is concentrated among the positive matches (Appendix Table A.13). Yet, we fail to reject the null hypothesis of equality of coefficients across heterogeneous matching groups. However, the magnitude of the coefficients suggests the effects are stronger when the match is positive. Column 4 of Appendix Table A.13 suggests the effect is more than twice as great than the effect for the negative group.

Second, we use the data from the internships and provide suggestive evidence that the quality of exposure depends on the initial attitudes of the employer and the worker (Appendix Figure A.8). This figure reports the averages across the three groups of attitudes of different internship outcomes, as well as different refugee characteristics. The figures provide suggestive evidence that when the match is positive, employers

TABLE 8. Employment Outcomes of New Workers Among Firms After the Experiment

	Walk-in	Still employed	Daily payment
	(1)	(2)	(3)
Panel A: Average			
Exposed	0.145	0.009	1.216
	(0.103)	(0.070)	(1.851)
	[0.165]	[0.894]	[0.513]
Panel B: Heterogeneity			
Exposed × Positive	0.353**	0.205	-0.975
	(0.136)	(0.143)	(2.668)
	[0.011]	[0.155]	[0.716]
Exposed × Mixed	0.090	-0.034	3.361
	(0.120)	(0.076)	(2.324)
	[0.456]	[0.655]	[0.152]
Exposed × Negative	-0.045	-0.211***	-4.304**
	(0.174)	(0.058)	(2.003)
	[0.795]	[0.000]	[0.035]
Workers	124	124	124
Firms	52	52	52
Mean DV	0.512	0.140	9.464
Mean among Ugandan workers	0.498	0.622	15.504
H_0: Positive=Mixed	0.053	0.116	0.220
H_0: Positive=Negative	0.030	0.011	0.142
H_0: Mixed=Negative	0.482	0.035	0.006

Notes: This table uses a panel of jobseekers hired by firms after the experiment and drops employers whose internships did not take place. Panel A reports the effect of exposure, estimating equation 5.1 on the subsample of refugee jobseekers hired by firms at endline. Panel B reports the heterogeneous treatment effect, estimating equation 5.5 on the subsample of refugee jobseekers hired by firms at endline. *Dependent variables*: Column 1: a dummy equal to 1 if the worker was hired without referral; Columns 2: a dummy equal to 1 if the worker is still employed at endline; Columns 3: daily wage (inclusive of lunch and transport), in thousands of UGX. *Controls*: 15 randomization strata (refugees' occupations: tailor, cook, hairdresser, domestic electrician, arts & crafts maker, painter, baker, motorvehicle mechanic, barber, beautician, hotel staff, plumber, carpenter, leather designer, bricklayer, electronic technician, welder and waiter) and six area fixed effects (dummies identifying the location of the business premises: Central Kampala, Nakawa, Kawempe, Rubaga, Makindye and Wakiso). Standard errors are clustered at the level of the firm. P-values reported in square brackets. ***, **, *, indicate significance at the 1%, 5%, and 10% levels respectively.

are significantly more willing to hire the same worker, suggesting their experience with the refugee worker was more positive than in the case of the negative match group. Furthermore, firms with positive matches found it less demanding to supervise the worker. These descriptive findings suggest that the internship went significantly better in the group of employers that matched with positive initial attitudes with workers with positive attitudes. Additionally, refugees in the positive matches are also more likely to have been seeking employment prior to the experiment, applying for more positions and being more successful with Ugandan employers. Higher job offer rates from Ugandan employers among refugees in positive matches also suggest that these refugees may have already had better experiences with Ugandan employers in the past. This second set of findings suggests that refugees with positive attitudes matching with the positive employers were also more motivated in providing a better signal of their ability to their employer during the internship.

Finally, we use our longer-term follow-up phone survey to collate employers' views on potential challenges regarding employing refugees, and use it as evidence to support the mechanisms of our experiment. We ask employers in the control group interviewed at follow-up 1 to what extent they agree with a series of statements concerning what could possibly harm firms' and refugees' relationship in the workplace, using a scale of 1 to 5. Appendix Figure A.9 shows the distribution of the ratings for each statement. The figure plots the distribution of the ratings for each statement. We find that about 80 per cent of firms agree or strongly agree that the employer's and the refugee worker's attitudes as well as their interactions are relevant factors for a successful relationship in the workplace. There is also a consistent percentage of firms (65 per cent) that believe or strongly believe that refugees require more training before being given a job. About half of the employers claim that it is difficult to employee a refugee jobseeker because Ugandan employers do not share the same social networks with them. Moreover, less than half believe that language issues are restrictive. Overall, we interpret these results as supportive of the main mechanism of our experiment. Namely, attitudes towards the out-group is a crucial factor in hiring refugees.

6. **Policy Implications and Cost Effectiveness**

This experiment shows that a brief interaction in the workplace with refugee workers can be sufficient to produce long-term effects on local employers' willingness to employ workers from this group. This study has also several policy implications for governments interested in using private sector solutions to affect the labor market integration of refugees.

First, not all firms will be interested in providing internships to refugee workers. About half of the employers we reached out to were interested in joining the experiment, which means firms opt in with heterogeneous characteristics. In fact, in experiments characterized by an encouragement design, participants self-select due to different reasons based on their interest (Karlan and Zinman (2009)). This experiment could be viewed as a selective trial because of our WTP to hire exercise, which reveals which firms are genuinely interested in trialling a refugee worker (see Chassang et al. (2012) for a discussion on selective trials). Thanks to our rich data, we can characterize who these participants are. We provide evidence that such firms are those most likely to be able to hire more refugees after revising their beliefs about these workers. Very few firms have ever hired a migrant (about one-third) and even fewer had employed a refugee before our experiment (about 17 per cent). Lack of experience with these workers may explain why employers have uncertain and weak beliefs about refugees' abilities.

Second, not all the refugees are able to actually attend the internships. This is likely due to severe credit constraints and transportation costs: refugees living further away from the location of the internships are less likely to attend the appointments. Governments interested in investing resources to incentivize internships should take into account the constraints to access the program. For instance, refugees may require financial assistance to move around the city and begin their work engagements.

Third, internships expanded job opportunities among the broader refugee community. This is because treated firms do not automatically hire the same worker they worked with during the experiment (see Table 2). At the same time, we do not find any negative effect on hiring Ugandan workers, which means that firms are starting to hire workers from outside their usual networks, without reducing access to the networks they are already familiar with.

Fourth, results are concentrated, at least in the short and medium term, on the group of employers that already has positive attitudes towards refugees, matching with refugees who already have positive attitudes towards locals. The short-term results for the negative groups are negative and the medium to long-term effects are not distinguishable from zero. This means that the local employers and the refugee workers may benefit from preparatory training before engaging in the internship. This may assist them to adjust their initial attitudes and improve the out-group contact experience. Or, policymakers should match on preexisting attitudes to maximize the return of increasing demand for refugee workers.

Finally, with access to the full cost of the matching program we can compute the cost for each job created. First, during the two years following the experiment, control firms hired a total of 44 refugees and treated firms hired 102 refugees. That is, our program helped firms to hire 58 more refugees. The program's overall cost, inclusive of wages of the field officers (1,929USD), transport and communication costs (877USD), wage subsidies (2,628USD) and management fees (978USD), amounted to 6,413USD.[23] Therefore, the total cost per job created was equal to 111USD and the total cost per firm participating to the experiment and for which we have information at endline (299) was equal to 21USD. While the latter cost is well in line with costs of similar programs described in McKenzie (2017), the cost per job created is significantly lower than in other comparable studies.

7. Conclusions

How to improve the labor market integration of disadvantaged workers such as migrants and refugees is an open question with huge policy implications. Their poor integration has long-term costs on the economies that host them. This is especially true in low-income country settings, where labor markets often do not function well and national resources are already stretched. Refugees face barriers to integration even if they possess experience and employable skills, and even if local institutions support their rights to work. Local employers may have few incentives to hire a refugee, because they may believe they are unskilled and the cost of testing and training a refugee is too high. We design and evaluate an experiment with the goal of facilitating employers learning about workers from this disadvantaged group and helping refugees display their skills to local employers. We find that exposure through a short-term internship is sufficient to stimulate the long-term hiring among firms, over approximately two years after the internship is completed. This is especially true among those employers who experienced a positive match with their intern. The average effect on their willingness to hire a refugee worker in the short term is not statistically distinguishable from zero, but firms on average do update their beliefs and are more supportive of refugees' labor market integration. Finally, this paper opens new questions relevant to the effect of initial attitudes on the employer-worker relationships. What is the outcome of exposure between employers and workers of

[23]We exclude the costs associated with testing the skills of the refugees as well the costs of baseline surveys.

any other group of workers with whom they have rarely interacted? We think understanding whether attitudes play a role regardless of the socio-economic status of the worker is an exciting area for future research.

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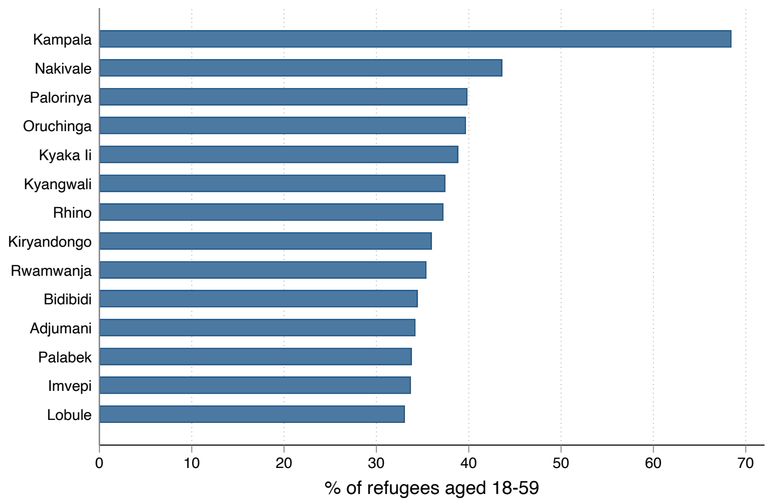
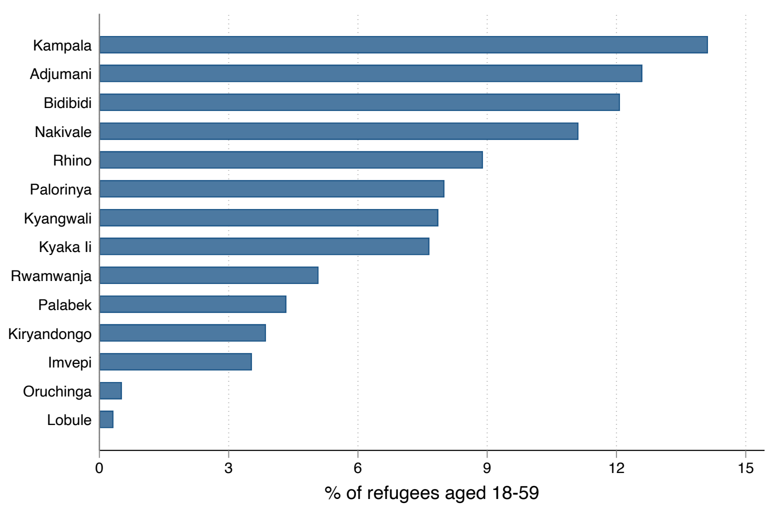
ONLINE APPENDIX

FIGURE A.1. Refugees in Uganda



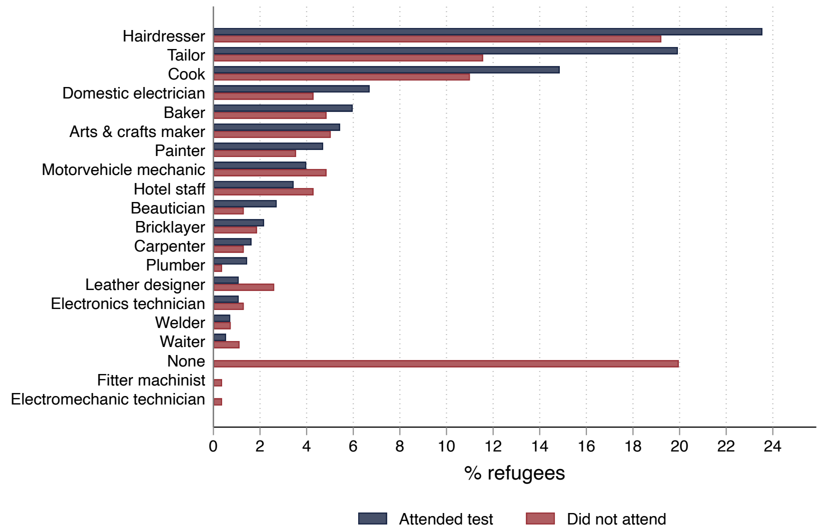
(**A.**) Working-age population of refugees for each settlement



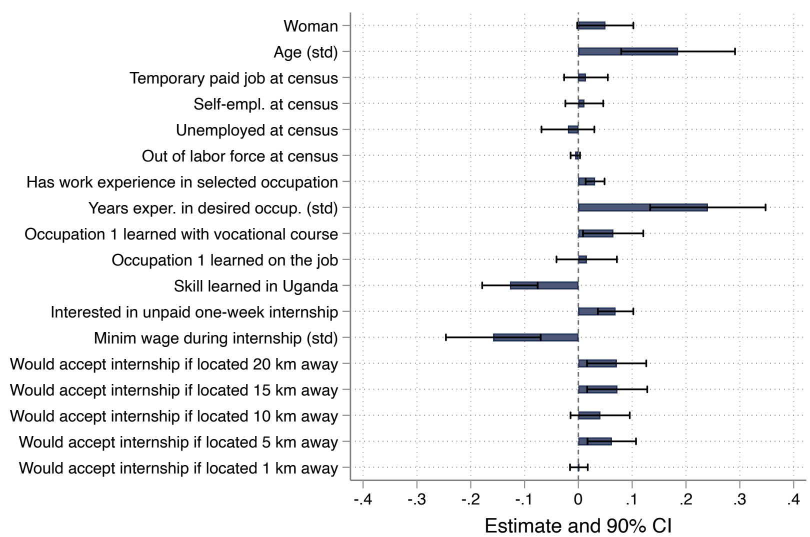
(**B.**) National working-age population of refugees by settlement

Notes: This graph plots descriptive statistics of the refugee population in Uganda as of end of 2022. Data comes from UNHCR Uganda accessed in October 2022: *https://data.unhcr.org/en/country/uga*. Panel (A) shows the distribution of working-age refugees across each registered place of residence. Panel (B) reports the percentage of working-age refugees within each settlement.

FIGURE A.2. Refugees' Skills and Test Attendance



(**A.**) Refugees' skills, by attendance to the test



(**B.**) Refugees who attended the test vs those who did not

Notes: The first graph (Panel A) plots the percentage of refugee workers listed by their skills and exam attendance. Out of 1,088 refugees listed, 977 were invited to the test. Among them, 552 attended the test (dark blue bars), and 425 did not (red bars). Of those who did not attend, 111 were not invited because they either declared no skills or belonged to occupation groups that did not meet the requisite number of participants. The second graph (Panel B) shows the characteristics of refugees by whether they attended the test. Each bar represents a coefficient from the equation: $y_i = \beta_0 + \beta_1 \mathbb{1}(attended_i) + \varepsilon_i$, where y_i is an individual characteristic, and $\mathbb{1}(attended_i)$ is a dummy equal to 1 if refugee i attended the test. The black lines indicate 90% confidence intervals.

FIGURE A.3. CVs of Refugee and Ugandan Workers

Wisdom Karungu

Tel: 07[redacted]15

Resident: Kampala, Nsambya, since: 2015

Age: 34

Expertise: cook

Years of experience as a cook: 8

Gender: Male

Nationality: Congolese

Knowledge of English (self-reported scale 1-5):

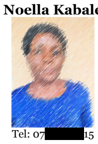
Reading:	*Speaking:*	*Writing:*	*Listening:*
3=Moderately well	2=Not well	2=Not well	3=Moderately well

Knowledge of Luganda (self-reported scale 1-5):

Reading:	*Speaking:*	*Writing:*	*Listening:*
3=Moderately well	3=Moderately well	1=Not at all	3=Moderately well

(**A.**) Real refugee male worker

Noella Kabale



Tel: 07[redacted]15

Resident: Kampala, Masajja, since: 2016

Age: 36

Expertise: cook

Years of experience as a cook: 10

Gender: Female

Nationality: Congolese

Knowledge of English (self-reported scale 1-5):

Reading:	*Speaking:*	*Writing:*	*Listening:*
3=Moderately well	2=Not well	2=Not well	3=Moderately well

Knowledge of Luganda (self-reported scale 1-5):

Reading:	*Speaking:*	*Writing:*	*Listening:*
3=Moderately well	3=Moderately well	1=Not at all	3=Moderately well

(**B.**) Real refugee female worker

John Sabiti

Tel: 07[redacted]15

Resident: Kampala, Nsambya, since: 2015

Age: 34

Expertise: cook

Years of experience as a cook: 8

Gender: Male

Nationality: Ugandan

Knowledge of English (self-reported scale 1-5)**:**

Reading:	*Speaking:*	*Writing:*	*Listening:*
3=Moderately well	2=Not well	2=Not well	3=Moderately well

Knowledge of Luganda (self-reported scale 1-5)**:**

Reading:	*Speaking:*	*Writing:*	*Listening:*
3=Moderately well	3=Moderately well	1=Not at all	3=Moderately well

(**C.**) Hypothetical male worker

Dorcas Mandela

Tel: 07[redacted]15

Resident: Kampala, Masajja, since: 2016

Age: 36

Expertise: cook

Years of experience as a cook: 10

Gender: Female

Nationality: Ugandan

Knowledge of English (self-reported scale 1-5)**:**

Reading:	*Speaking:*	*Writing:*	*Listening:*
3=Moderately well	2=Not well	2=Not well	3=Moderately well

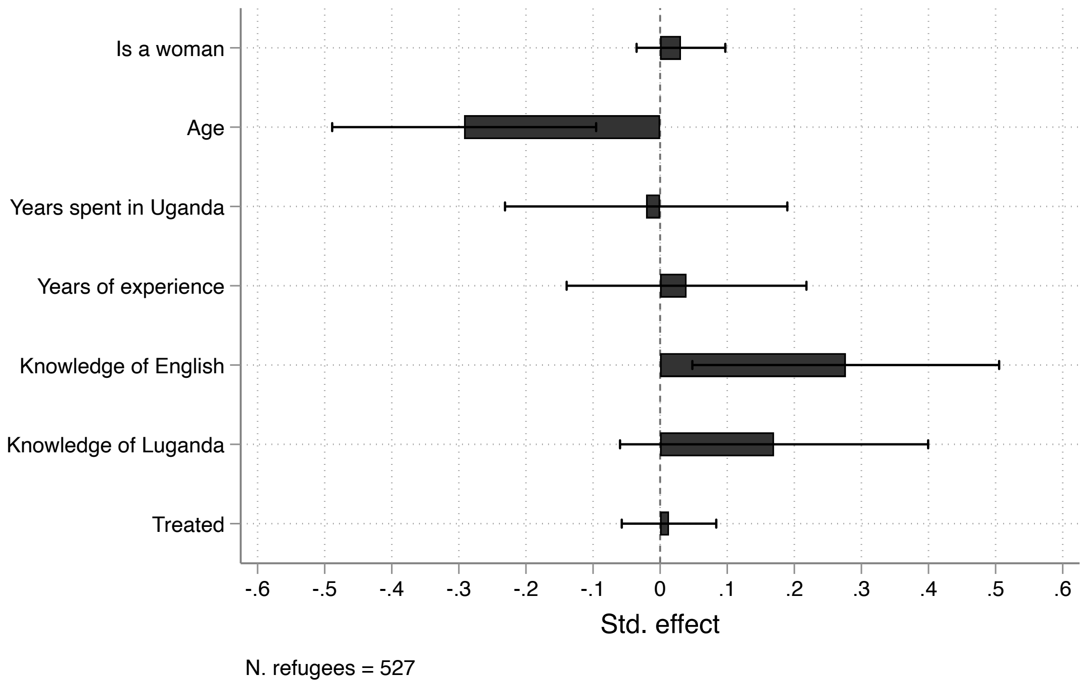
Knowledge of Luganda (self-reported scale 1-5)**:**

Reading:	*Speaking:*	*Writing:*	*Listening:*
3=Moderately well	3=Moderately well	1=Not at all	3=Moderately well

(**D.**) Hypothetical female worker

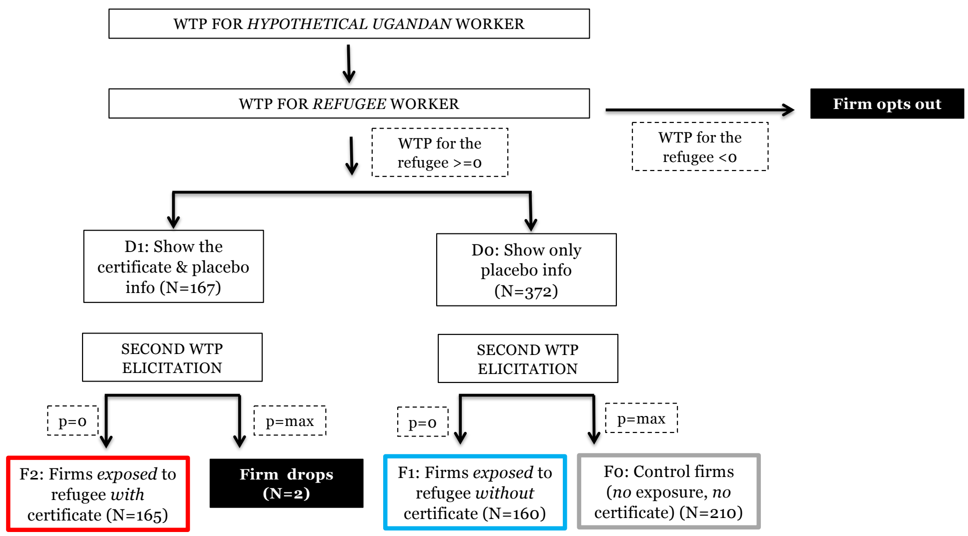
Notes: The figure plots examples of CVs for both real refugee workers and hypothetical local workers. The refugee workers' CVs are based on information provided by the respondents, while the hypothetical local workers' CVs are created to mirror the same structure. Care was taken in the selection of names and images for the local workers to avoid indicating any specific ethnic or tribal affiliation.

Figure A.4. Refugees' Matching Success Rate



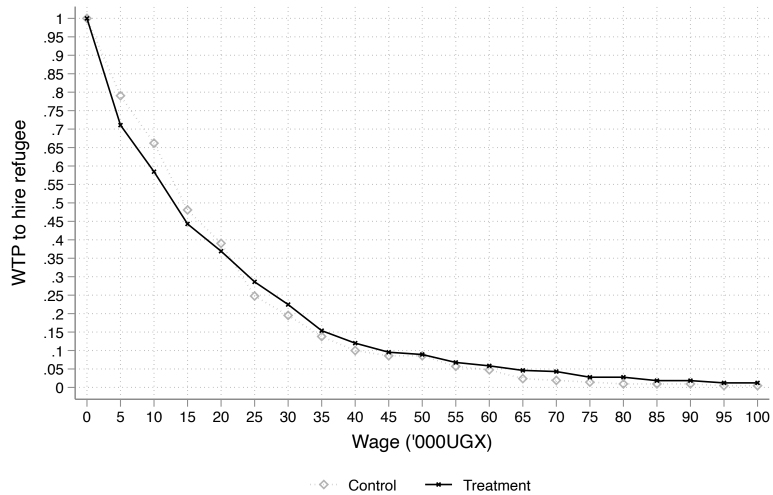
Notes: This graph correlates the characteristics of refugee workers with the average number of firms that are willing to hire them. The graph plots the coefficients from the following specification: $y_i = \beta_0 + \beta_1 \frac{1}{Nfirms} \sum_j^{Nfirms} \mathbb{1}(WTP_i \geq 0) + X_i'\delta + \varepsilon_i$, where y_i is the baseline characteristic of refugee worker i, $Nfirms$ is total number of firms we reached out to (that is: $Nfirms = 1,192$) and $\mathbb{1}(WTP_i \geq 0)$ is an indicator equal to 1 if the WTP to hire refugee i is greater or equal to 0. Additional controls: X_i are occupation fixed effects. Standard errors are clustered at the level of the refugee paired with the firm.

FIGURE A.5. Original Design

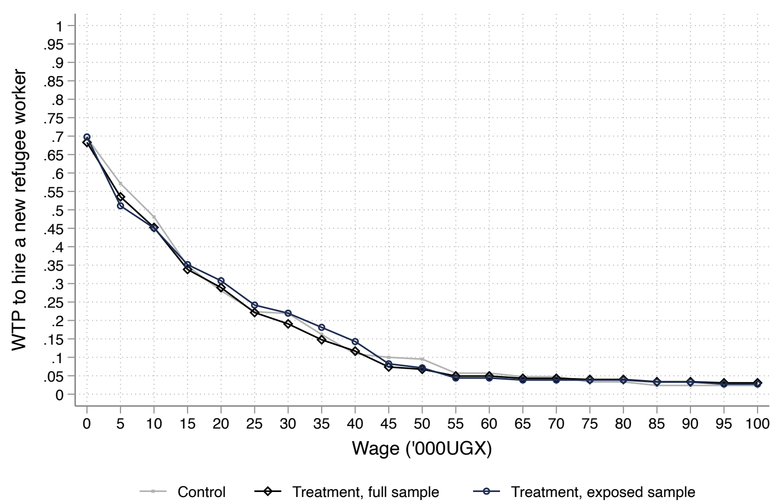


Notes: This graph summarizes the original design of the experiment. In the original design we present the certificate obtained by the matched refugee worker. We drop two employers belonging to the D1 arm to guarantee the incentive compatibility of the BDM mechanism (that is, to guarantee that the likelihood of "winning" the lottery of the random price is strictly lower than 1). The WTP is elicited twice. In the first elicitation we inform the employer that the hiring will happen in four days time. In the second elicitation we provide a slightly desirable increase in the terms of the hiring, informing the employer that the hiring would happen eight days from the baseline.

FIGURE A.6. WTP Curves



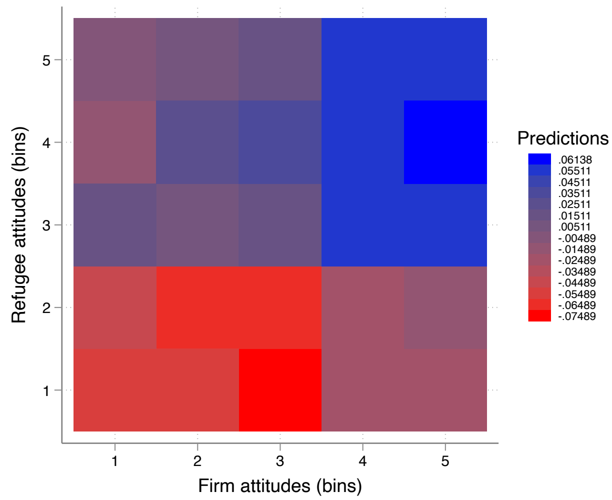
(**A.**) WTP Curves at Baseline



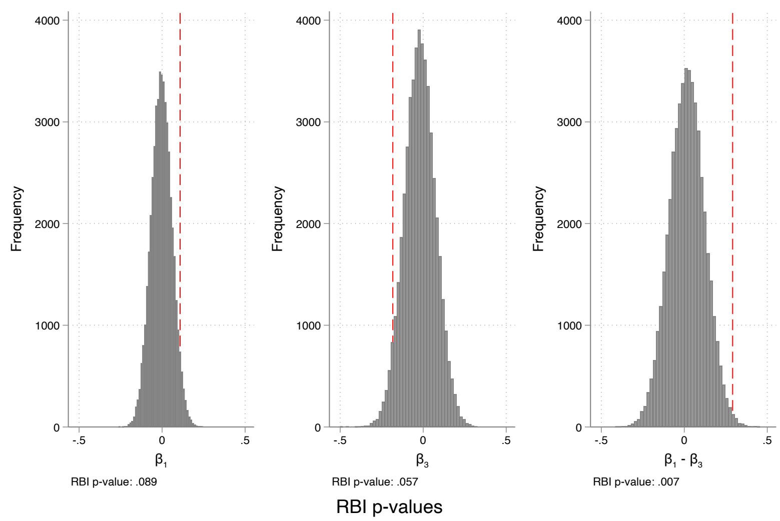
(**B.**) WTP Curves at Follow-up 1

Notes: Panel A (top) plots the Cumulative Distribution Function (CDF) of the Willingness to Pay (WTP) to hire a refugee worker at baseline. The gray line represents the demand among control firms, while the black line represents the demand among firms assigned to treatment. Panel B (bottom) plots the CDF of the WTP to hire a refugee worker at Follow-up 1. The gray line shows the demand among control firms. The black curve with diamonds corresponds to the demand among firms assigned to treatment. The dark blue line with circles excludes firms where the internship did not take place.

Figure A.7. Predicted CATE and Randomization-based Inference



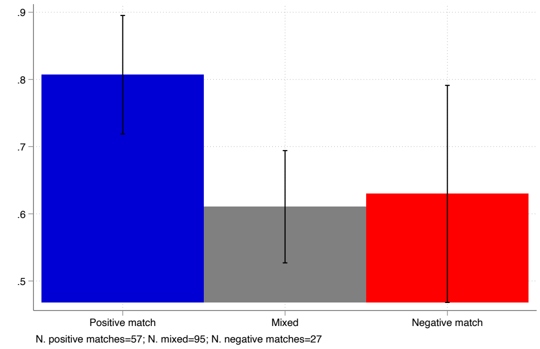
(**A.**) Predicted CATE and attitudes



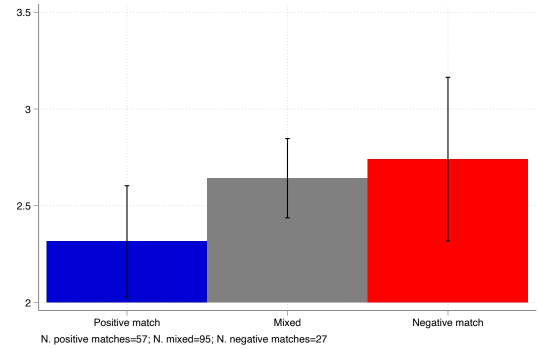
(**B.**) Randomization-based inference on the short-term demand

Notes: Panel A (left) shows a heat map of the predicted Conditional Average Treatment Effect (CATE) across quartiles of the index of attitudes of both the employer (X-axis) and the refugee worker (Y-axis). Colder colors (closer to blue) indicate a more positive effect on the willingness to pay (WTP) to hire a new refugee worker, while warmer colors (closer to red) indicate a lower predicted effect on WTP. Panel B (right) displays the distribution of Randomization-Based Inference (RBI) coefficients estimated by equation 5.5. The first graph (left) shows the distribution of the values of β_1. The middle graph shows the distribution of β_3. The final graph shows the distribution of the t-test of equality between β_1 and β_3. The RBI p-values are reported below each graph.

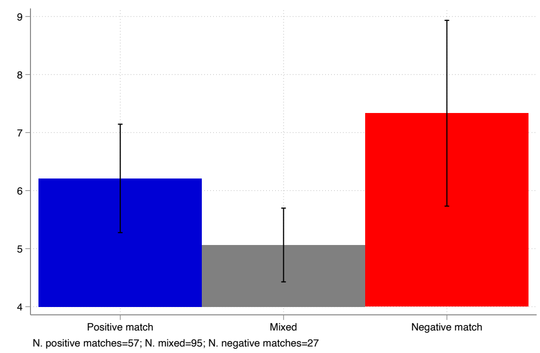
Figure A.8. Evidence from the Internship



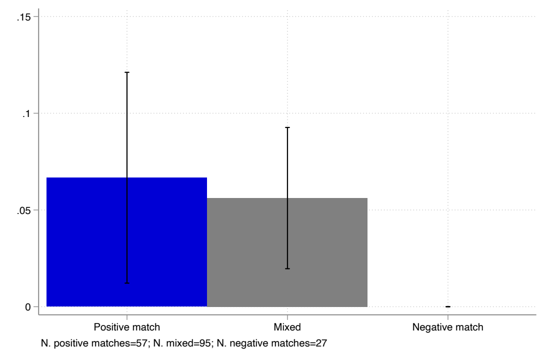
(**A.**) Willingness to hire the same intern



(**B.**) Difficulty of supervision



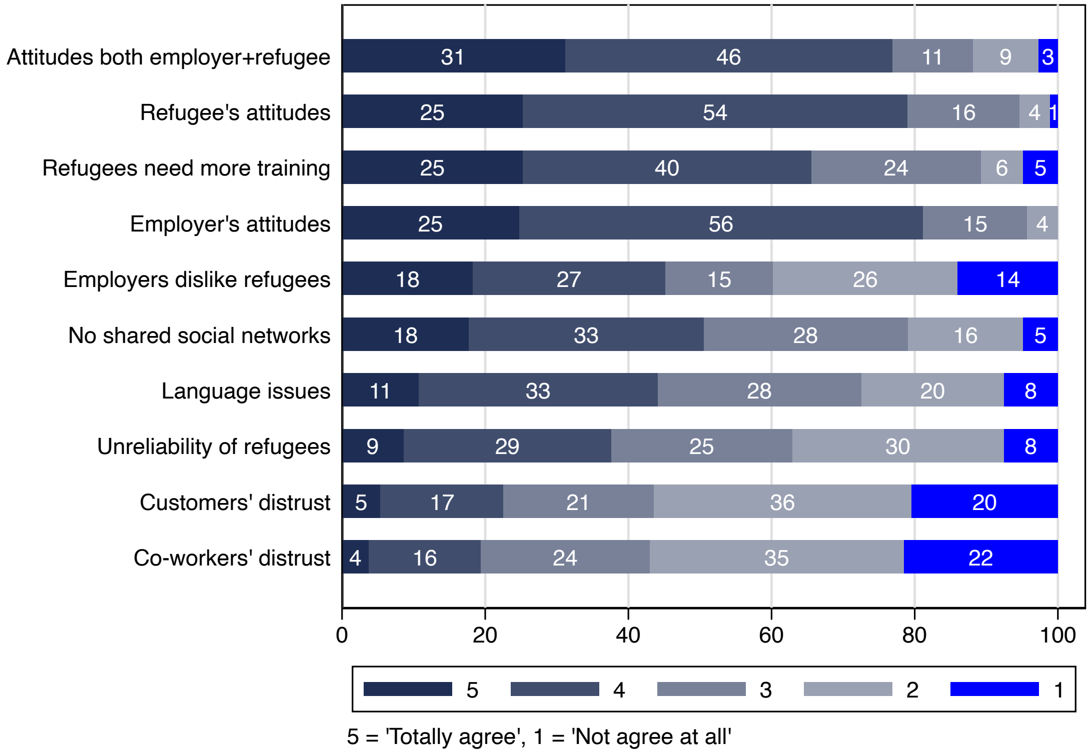
(**C.**) Number of hours of supervision



(**D.**) Job-finding rate with Ugandan employers

Notes: The figures display evidence from the internship program involving refugee workers: Panel A shows the percentage of firms willing to rehire the same intern (including for free). This willingness was elicited similarly to the baseline measurement, with group means represented by the bars and 95% confidence intervals shown by the black lines. Panel B presents the average rating by firms regarding the difficulty of supervising the intern, using a scale from 1 (not demanding) to 5 (very demanding). Panel C illustrates the average number of hours employers spent supervising the intern. Panel D depicts the average success rate of refugee workers in finding jobs with Ugandan employers during the month before the internship, segmented by the quality of matching. All questions were asked at Follow-up 1.

Figure A.9. Employers' concerns in the workplace employing a refugee



Notes: This graph plots Ugandan employers' opinions about what would facilitate or hinder the success of refugees in the workplace. We introduce the section by reading the following: *I will now read a series of statements. I will ask you to tell me to what extent do you agree with them, using a scale of 1 to 5, where 1 denotes "I do not agree at all" and 5 denotes "I agree very much". You can draw from your experience or simply give us your honest opinion about them.* We therefore read the following statements, corresponding to each single item in the graph: *1. A crucial factor in a successful work relationship between a Ugandan employer and a refugee worker is that they both are open to each other and feel comfortable working with someone from a different country*; *2. When working together refugee workers' attitudes and openness towards Ugandan employers is a crucial factor in a successful work relationship*; *3. Refugee jobseekers require more training before starting work at a firm like mine compared with other employees*; *4. When working together Ugandan employers' attitudes and openness towards refugee workers are crucial factors in a successful work relationship*; *5. Ugandan business owners simply do not like to work with refugees, even if a refugee worker is a very good one*; *6. It is hard for a Ugandan employer to give a job to a refugee because he or she does not share the same network*; *7. It is hard to work together with refugees because it is hard to communicate with them*; *8. Refugee workers will terminate their work engagements at short notice (i.e. they are not reliable)*; *9. Customers do *not* trust a refugee worker*; *10. Other employees do *not* fully engage with a refugee worker.*

Table A.1. Skills tested for each occupation

Occupation	**Tested skill**
Baker	Bake a loaf of diabetic bread
Barber	Perform a marine's haircut
Bead artist	Create a set of beaded earrings
Beautician	Apply makeup to a client
Bricklayer	Construct a header bond with attached stretcher
Carpenter	Make a small wooden chair
Cook	Cook rice pilao with beef stew
Domestic electrician	Wire and install two lamps in full conduit
electronic technician	Replace jack pin and mouthpiece on a telephone
Hairdresser	Twist style
Hairdresser	Cornrow style
Hotel receptionist	Take reservations and reserve a room for a guest
Hotel room attendant	Service a hotel room
Knitter	Make a long-sleeved sweater
Leather designer	Make a pair of men sandals
Motorvehicle mechanics	Repair car brakes
Painter	Paint interior walls of a medium-size room
Plumber	Fit and connect pipes
Tailor	Make a casual short-sleeved shirt
Waitron	Perform table food service and customer care
Weaver	Weave a tablecloth
Welder	Make a small metallic window

Notes: This table lists the skills tested for each occupation. Each skill has been chosen by the Directorate of Industrial Training and follows the national vocational education curriculum of Uganda.

Table A.2. Comparing Refugees with Locals Within and Other Refugees Outside Kampala

	Baseline survey			URHHS			
Variable	N	Mean	SD	N	Mean	SD	Diff.
Panel A: Compared with locals							
High. educ.: None	527	0.009	0.097	613	0.020	0.139	-0.010
High. educ.: Primary	527	0.114	0.318	613	0.732	0.443	-0.619***
High. educ.: Secondary	527	0.877	0.329	613	0.235	0.424	0.642***
Employed	527	0.484	0.500	727	0.567	0.496	-0.083***
Unemployed	527	0.159	0.366	727	0.110	0.313	0.049**
Out of labor force	527	0.357	0.479	727	0.322	0.468	0.035
Monthly earnings	255	301.541	294.079	256	609.121	1,091.179	-307.580***
Panel B: Compared with other refugees							
Education: None	527	0.009	0.097	1,320	0.300	0.458	-0.291***
Education: Primary	527	0.114	0.318	1,320	0.227	0.419	-0.113***
Education: Secondary	527	0.877	0.329	1,320	0.033	0.180	0.843***
Employed	527	0.484	0.500	1,772	0.324	0.468	0.159***
Unemployed	527	0.159	0.366	1,772	0.130	0.336	0.030*
Out of labor force	527	0.357	0.479	1,772	0.546	0.498	-0.189***
Monthly earnings	255	301.541	294.079	142	112.014	88.506	189.527***
Years in Uganda	527	6.622	3.714	1,685	4.858	44.381	1.764
Is registered in Uganda	527	0.882	0.322	1,763	0.967	0.178	-0.085***
Received remittances	527	0.476	0.500	1,665	0.127	0.333	0.349***
Total remittances	251	129.335	238.672	184	542.735	1,850.938	-413.401***
Received relief aid	527	0.178	0.383	1,772	0.855	0.352	-0.677***

Notes: This table compares the characteristics of our sample of refugees with a representative sample of Ugandans living in Kampala (Panel A) and a sample of refugees living in rural areas outside Kampala (Panel B), from the most recent wave of the Ugandan Refugees and Host Communities Household Survey (2018). The sample of working-age Ugandans living in Kampala is composed of 727 individuals. Working-age refugees living in rural areas outside Kampala and interviewed in the same survey were 1,772. Our baseline sample of working-age refugees living in Kampala is composed of 527 individuals. The table reports the coefficients of a specification comparing firms across characteristics as follows: $y_i = \beta_0 + \beta_1 \mathbb{1}(baseline)_i + \varepsilon$, where $\mathbb{1}(baseline)_i$ is an indicator equal to 1 if the observation belongs to our baseline sample of firms. ***, **, *, indicate significance at the 1%, 5%, and 10% levels respectively.

Table A.3. Comparing Firms in Sample with Other Firms in Kampala

	Manpower survey			Baseline survey			
	N	Mean	SD	N	Mean	SD	Diff
Respondent is a woman	1045	0.56	0.50	535	0.57	0.50	0.015
Age of the respondent	1036	32.90	10.18	535	34.50	8.37	1.605***
Education: None	1039	0.04	0.20	535	0.02	0.14	-0.025***
Education: Primary	1039	0.31	0.46	535	0.19	0.39	-0.120***
Education: Secondary	1039	0.43	0.50	535	0.46	0.50	0.022
Education: Vocational	1039	0.13	0.33	535	0.21	0.41	0.087***
Education: University	1039	0.06	0.23	535	0.10	0.30	0.047***
Manufacturing sector	1040	0.34	0.47	535	0.33	0.47	-0.005
Firm age	1037	4.45	6.15	535	7.81	6.64	3.366***
Keeps accounting books	1038	0.36	0.48	535	0.64	0.48	0.286***
Has at least one employee	1037	1.00	0.04	535	0.76	0.43	-0.235***
Employees at baseline	1037	1.93	2.09	535	2.49	3.15	0.565***
Revenues past month, M-UGX	1032	0.82	2.33	499	1.88	2.77	1.062***
Expects future increase in size	1045	0.10	0.30	535	0.86	0.35	0.759***
Pays taxes to URA	1036	0.25	0.43	535	0.19	0.39	-0.061***

Notes: This table compares the characteristics of our sample of firms with a representative sample of 1,045 firms in Greater Kampala (a metropolitan area comprising the towns of Kampala and Wakiso). The sample of 1,045 firms comes from the Manpower survey conducted in 2016 in the cities of Kampala, Wakiso and Mukono (World Bank. Uganda National Manpower Survey 2016 - Kampala Informal Sector Survey (UNMPS-ISS 2016). Ref. $UGA\_2016\_NMPS - ISS\_v01\_M$. Downloaded from [$https://microdata.worldbank.org/index.php/catalog/3397study\_desc1674579234511$] in October 2022). For comparability reasons we exclude firms located in Mukono, active in agriculture, health, yransport and retail sectors. The table reports the coefficients of a specification comparing firms across characteristics as follows: $y_i = \beta_0 + \beta_1 \mathbb{1}(baseline)_i + \varepsilon$, where $\mathbb{1}(baseline)_i$ is an indicator equal to 1 if the observation belongs to our baseline sample of firms.***, **, *, indicate significance at the 1%, 5%, and 10% levels respectively.

Table A.4. Randomization Balance

	Treatment			Control			
Variable	N	Mean	SD	N	Mean	SD	Diff.
Panel A: Full sample							
Employer is a woman	325	0.563	0.497	210	0.581	0.495	-0.063**
Firm age	325	7.640	6.659	210	8.086	6.627	-0.321
Revenues past month, M-UGX	298	1.770	2.803	201	2.043	2.710	-0.044
Firm is formal	325	0.182	0.386	210	0.190	0.394	-0.015
Has a vacancy	325	0.449	0.498	210	0.371	0.484	0.077*
Desires expand in the future	325	0.852	0.355	210	0.871	0.336	-0.033
Employees at baseline	325	2.434	3.137	210	2.581	3.169	0.216
Num. of rooms in business premises	325	1.169	0.765	210	1.176	0.876	0.024
Number of firms' tasks	325	3.326	1.551	210	3.476	1.599	-0.073
Manufacturing sector	325	0.345	0.476	210	0.314	0.465	-0.020*
Ever offered internships	325	0.646	0.479	210	0.552	0.498	0.087**
Ever hired a migrant or refugee	325	0.351	0.478	210	0.376	0.486	-0.022
Beliefs about refugees' test score	325	65.052	14.501	210	62.705	16.013	2.126
Supports refugees' empl. rights	325	0.923	0.267	210	0.924	0.266	0.006
Jobs to locals first	325	3.388	1.249	210	3.305	1.299	0.104
WTP at baseline	325	17.077	20.486	210	16.881	17.646	0.916
Panel B: Exposed sample							
Employer is a woman	182	0.582	0.495	210	0.581	0.495	-0.040
Firm age	182	7.742	6.546	210	8.086	6.627	-0.347
Revenues past month, M-UGX	167	1.541	2.090	201	2.043	2.710	-0.258
Firm is formal	182	0.181	0.386	210	0.190	0.394	-0.009
Has a vacancy	182	0.423	0.495	210	0.371	0.484	0.068
Desires expand in the future	182	0.863	0.345	210	0.871	0.336	-0.016
Employees at baseline	182	2.615	3.497	210	2.581	3.169	0.425
Num. of rooms in business premises	182	1.159	0.788	210	1.176	0.876	0.006
Number of firms' tasks	182	3.308	1.484	210	3.476	1.599	-0.025
Manufacturing sector	182	0.346	0.477	210	0.314	0.465	-0.039**
Ever offered internships	182	0.643	0.480	210	0.552	0.498	0.093*
Ever hired a migrant or refugee	182	0.357	0.480	210	0.376	0.486	-0.014
Beliefs about refugees' test score	182	64.390	14.241	210	62.705	16.013	1.455
Supports refugees' empl. rights	182	0.934	0.249	210	0.924	0.266	0.019
Jobs to locals first	182	3.429	1.276	210	3.305	1.299	0.104
WTP at baseline	182	17.445	20.724	210	16.881	17.646	1.235

Notes: This table produces balance checks of baseline characteristics among firms using the full sample (Panel A) and dropping firms for which the internship did not take place (Panel B). The table reports observations, mean and standard deviations for each group in the first six columns. The seventh and last column reports the coefficient β_1 from the following specification: $y_i = \beta_0 + \beta_1 Treat_i + X_i^{'}\delta + \varepsilon_i$, where outcome y_i is a baseline characteristic and $Treat_i$ is an indicator equal to 1 if the firm belongs to the treatment group. $X_i^{'}$ is a matrix of randomization controls (i.e. occupation of the refugee worker) and the area fixed effects. Standard errors are clustered at the level of the refugee paired with the firm. ***, **, *, indicate significance at the 1%, 5%, and 10% levels respectively.

Table A.5. Attrition at Follow-up 1, 2 and Endline

	Full sample			Exposed sample		
	(1)	(2)	(3)	(4)	(5)	(6)
	Follow-up 1	Follow-up 2	Endline	Follow-up 1	Follow-up 2	Endline
Treated	0.004	-0.010	0.008	0.005	-0.041	0.023
	(0.011)	(0.030)	(0.039)	(0.013)	(0.036)	(0.046)
Control	0.981	0.886	0.762	0.981	0.886	0.762
Firms	525	474	407	385	343	299

Notes: This table investigates whether attrition at follow-up surveys and endline are differential across treatments. It reports the coefficients for specification 5.1 where y_i is a dummy equal to 1 if the respondent is attrited at each point in time.***, **, *, indicate significance at the 1%, 5%, and 10% levels respectively.

Table A.6. Summary of Outcome Measures

Category	**Description**
Demand for refugees	1. **Hiring of refugees:** • Dummy: Hired any refugee in the past 2 years • Dummy: Hired only new refugees (excluding those from internships) • Total number of refugee workers hired in the past 2 years • Total number of new refugees hired (excluding internship workers) 2. **WTP for hypothetical worker:** • Dummy: WTP ≥ 0 • Continuous WTP in UGX
Beliefs about skills	1. **Hard skills:** • Expected DIT test score for refugees vs. Ugandan job seekers • Index of theoretical, practical skills, and work performance (Likert scale) 2. **Soft skills:** • Index of time management, teamwork, and work ethics (Likert scale) 3. **Behavioral skills:** • Index of respect and trust (Likert scale)
Attitudes	1. Donation to refugee-led non-profit in UGX 2. Dummy: Knows someone in a refugee-led organization 3. Dummy: Belief that cultural life is enriched by refugees (score 4 or 5) 4. Dummy: Views of refugees have improved in the last year

Table A.7. Refugees' Take-up of the Internships

	Not matched			Matched			
	n	mean	sd	n	mean	sd	Diff
Refugee worker is a woman	143	0.69	0.46	182	0.68	0.47	0.014
Age of the refugee worker	143	32.00	10.41	182	34.26	10.29	2.336**
Years living in Uganda	143	6.67	4.02	182	6.83	3.81	0.260
Years of education	143	11.39	3.87	182	11.71	3.59	0.238
Work experience (years)	143	4.26	7.24	182	4.85	6.25	0.619
English speaking level	143	2.71	1.23	182	2.68	1.05	-0.074
Luganda speaking level	143	2.76	1.24	182	2.65	1.15	-0.140
Positive refugee attitude	143	0.46	0.50	182	0.46	0.50	0.007
HH size, May 21	143	6.06	3.25	182	5.43	2.83	-0.616*
Refugee is a single mother	143	0.39	0.49	182	0.37	0.49	0.004
HH inc./adult('000UGX)	143	121.10	146.90	182	156.75	139.87	30.839*
Receives aid	143	0.17	0.38	182	0.21	0.41	0.041
Ever employed by Ugandan	143	0.29	0.46	182	0.27	0.45	-0.020
Had a business in the past week	143	0.34	0.47	182	0.41	0.49	0.076
Was unemployed in the past week	143	0.22	0.41	182	0.13	0.34	-0.088**
Was out of labor force in the past month	143	0.34	0.47	182	0.36	0.48	0.024
Total labor earnings in the past month	143	137.85	233.05	182	191.66	261.33	49.914*
Willing to do internship unpaid	143	0.91	0.29	182	0.95	0.23	0.032
Distance to internship	143	4.98	2.14	182	4.47	2.27	-0.535**

Notes: This table investigates whether any observable characteristic correlates with the likelihood of matching, both at the refugee and firm level. Using the rich data collected at baseline from both samples, we run the following specification in the sample of refugees matched with treated firms: $y_j = \gamma_0 + \gamma_1 \mathbb{1}(Matched)_j + X_j^{'}\delta + \varepsilon_j$, where the coefficient of interest, γ_1, correlates characteristic y_j with a dummy equal to 1 if the refugee worker j attended the meeting with the firm. The specification uses robust standard errors and controls for strata fixed effect, that is the occupation of the refugee worker. The variables come from the baseline survey with the sample of refugees. Each row is an individual dependent variable from specification.

Table A.8. Beliefs: Individual Components

	Hard skills				*Soft skills*			*Behavioral skills*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Score	Theory	Practice	Perform.	Time	Team	Ethics	Trust	Respect
Panel A: ITT									
Treated	-1.752	0.094	-0.060	-0.011	0.081	0.158	0.114	0.175*	0.094
	(1.231)	(0.096)	(0.097)	(0.101)	(0.096)	(0.108)	(0.099)	(0.102)	(0.101)
	[0.156]	[0.329]	[0.540]	[0.917]	[0.399]	[0.143]	[0.250]	[0.088]	[0.353]
Firms	524	525	525	525	525	525	525	525	525
Mean DV	63.917	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	0.000	0.000
Panel B: Exposed sample									
Exposed	-1.582	0.179	0.022	0.070	0.149	0.328**	0.270**	0.366***	0.197*
	(1.554)	(0.110)	(0.116)	(0.120)	(0.115)	(0.129)	(0.113)	(0.114)	(0.119)
	[0.310]	[0.105]	[0.851]	[0.562]	[0.194]	[0.012]	[0.017]	[0.001]	[0.099]
Firms	384	385	385	385	385	385	385	385	385
Mean DV	63.917	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	0.000	0.000

Notes: This table reports the coefficients estimated by equation 5.1. *Dependent variables*: Test score (i.e. the score between 0 and 100 that a student can achieve on the DIT practical skills examination), theoretical skills, practical skills and speed for the index on hard skills, time management, team work ability and work ethics, trust and respect. *Controls*: 15 randomization strata (refugees' occupations: tailor, cook, hairdresser, domestic electrician, arts & crafts maker, painter, baker, motorvehicle mechanic, barber, beautician, hotel staff, plumber, carpenter, leather designer, bricklayer, electronic technician, welder and waiter) and six area fixed effects (dummies identifying the location of the business premises: Central Kampala, Nakawa, Kawempe, Rubaga, Makindye and Wakiso). Standard errors are clustered at the level of the refugee paired with the firm. P-values reported in square brackets. ***, **, *, indicate significance at the 1%, 5%, and 10% levels respectively.

Table A.9. Best Linear Projector of CATE

	Best Linear Projector of CATE			
	Beta	SE	t-stat	p-value
Intercept	-.47	.356	-1.32	.187
Refugee's ability	-.035	.104	-.334	.739
Refugee's attitudes	.259	.106	2.446	.015
Refugee knowledge of languages	-.158	.167	-.941	.347
Refugee's age	-.001	.006	-.161	.872
Refugee is Congolese	.042	.162	.257	.798
Refugee ever employed by Ugandan	-.039	.128	-.307	.759
Employer's attitudes	.244	.118	2.075	.039
Firm's size	.021	.106	.202	.84
Firm's quality	0	.098	-.003	.997
Firm's beliefs	.028	.107	.264	.792
Firm's perceive cost of learning	-.044	.098	-.448	.655
Firm's expansion plan	-.051	.102	-.498	.619
Employer ever employed migrant	.033	.107	.312	.755
Manufacturing sector	.085	.119	.711	.477
Owner is Muganda	.111	.103	1.074	.284
Employer+refugee live same area	-.226	.154	-1.464	.144
Employer+worker same gender	.173	.132	1.314	.19

Notes: This table reports the best linear projectors estimated using r-command `blp` from the Generalized Random Forest package `grf`. The only two variables with p-values less than 5% are refugee's attitudes (p-val = 0.015) and employer's attitudes (p-val = 0.039).

TABLE A.10. Indices and Variables Used in the Causal Forest (Firms' Characteristics)

Index/Variable	Description
Majority status	Dummy equal to 1 if the firm owner belongs to the majority ethnic group in Uganda (Baganda)
Attitudes	Factor analysis of three dummies: • Agree: "Ugandans should have more rights to jobs." • Strongly agree: "Ugandans should have more rights to jobs." • "No" to allowing refugees to work in Uganda Positive employer = index value below median
Initial skill beliefs	Factor analysis of baseline beliefs on worker's skills (theoretical, practical, performance, etc.). Dummy = 1 if first factor value is greater than median
Learning cost	Factor analysis of: • Days to learn refugee's hard skills • Days to learn refugee's soft skills • Expected DIT test score (dummy = 1 if expected score < 65) Dummy = 1 if first factor value is greater than median
Willingness to expand	Factor analysis of: • Vacancy at baseline • Expected workforce increase in next 5 years Dummy = 1 if index value is greater than median
Firm quality	Factor analysis of: • Business premises ownership • Owner's education • Formality, bookkeeping, separate bank accounts, advertising Dummy = 1 if index value is above median
Firm size	Factor analysis of: • Number of employees at baseline • Total tasks performed • Number of rooms in business premises Dummy = 1 if index value is above median
Manufacturing sector	Dummy = 1 if firm is in manufacturing (e.g., arts and crafts, carpentry, etc.)
Migrant employment	Dummy = 1 if the firm has ever employed a migrant

TABLE A.11. Indices and Variables Used in the Causal Forest (Refugees' Characteristics)

Index/Variable	**Description**
Ability	Factor analysis of: • Worker's test score • Years of experience • Years of education • Cognitive skills (Raven's Progressive Matrices) Dummy = 1 if index value is above median
Attitudes	Factor analysis of: • Agreement with "Ugandans discriminate towards refugees." • Agreement with "Ugandans have the best intentions." • Agreement with "Ugandans and refugees should collaborate." • Agreement with "I see myself similar to a Ugandan." Dummy = 1 if index value is above median
Experience with Ugandans	Dummy = 1 if the refugee worker has ever worked for a Ugandan employer
Language	Self-reported ratings (1 to 5) on English and Luganda knowledge
Age	Refugee's age (continuous variable)
Congolese ethnicity	Dummy = 1 if the refugee worker is Congolese
Neighborhood proximity	Dummy = 1 if the refugee worker and the firm live in the same neighborhood
Gender match	Dummy = 1 if the refugee worker and the firm owner are of the same gender

Table A.12. Doubly Robust Post-Causal Forest Estimator

	Doubly robust estimators			
	Beta	SE	Lower CI (95%)	Upper CI (95%)
Exposed × Positive	.2	.087	.03	.37
Exposed × Mixed	-.053	.065	-.179	.074
Exposed × Negative	-.278	.128	-.53	-.027

Notes: This table reports doubly robust estimation of the heterogeneous treatment effect by attitudes group. The first column reports the estimated coefficient, the second associated standard error. Columns 3 and 4 report lower and upper confidence intervals respectively. We produce these estimates using the r-command `average_treatment_effect` from the Generalized Random Forest package `grf`

Table A.13. Beliefs About Refugees' Skills

	Dependent variable:			
	(1)	(2)	(3)	(4)
	Hard skills	Soft skills	Behavior	Avg. std. effect
Exposed × Positive match	0.244	0.382**	0.586***	0.328***
	(0.169)	(0.176)	(0.169)	(0.119)
	[0.149]	[0.031]	[0.001]	[0.006]
Exposed × Mixed	-0.155	0.226	0.203	0.073
	(0.145)	(0.157)	(0.154)	(0.109)
	[0.286]	[0.152]	[0.188]	[0.501]
Exposed × Negative match	0.112	0.204	0.264	0.142
	(0.160)	(0.222)	(0.206)	(0.130)
	[0.484]	[0.359]	[0.200]	[0.272]
Firms	385	385	385	385
Mean DV	-0.000	0.000	-0.000	
H_0: Positive=Mixed	0.043	0.447	0.057	0.072
H_0: Positive=Negative	0.515	0.493	0.183	0.227
H_0: Mixed=Negative	0.150	0.930	0.791	0.641

Notes: This table reports the coefficients estimated by equation 5.5. *Dependent variables*: Indices computed following Anderson (2008), using the following underlying covariates: theoretical skills, practical skills and speed for the index on hard skills (col. 1); work ethics, time management and team work ability for the index on soft skills (col. 2); trust and respect for the index on behavior (col. 3). Column 4 aggregates the results using the average standardized effect across the underlying components of all the indices. *Controls*: 15 randomization strata (refugees' occupations: tailor, cook, hairdresser, domestic electrician, arts & crafts maker, painter, baker, motorvehicle mechanic, barber, beautician, hotel staff, plumber, carpenter, leather designer, bricklayer, electronic technician, welder and waiter) and six area fixed effects (dummies identifying the location of the business premises: Central Kampala, Nakawa, Kawempe, Rubaga, Makindye and Wakiso). Standard errors are clustered at the level of the refugee paired with the firm. P-values reported in square brackets. ***, **, *, indicate significance at the 1%, 5%, and 10% levels respectively.

Appendix B. **Script WTP**

Introduction to WTP. The purpose of the exercise that will follow is to understand what is your "Willingness To Pay" for some workers. What we mean by this is the most that you would be willing to pay to hire a worker. Please, keep in mind that there are no right or wrong answers. We will just ask some questions to check your understanding. Before moving on with the explanation, I would like you to think about the following situation: imagine a job seeker come to look for a job at your firm. Usually, after getting some information on her, you might already have in mind what you would be willing to pay to hire her. In other words, you might think about what is the maximum price at which you would still hire the worker. Since you do not know the salary at which she would be willing to work for you, the salary you think about is usually your own valuation of the worker. Talking to her, you learn about the actual salary she wants to receive and you decide whether to hire her or not. Your decision will depend on the salary the worker is willing to accept: if the salary is higher than your valuation, you will not hire the worker. If instead the salary is equal or lower than your valuation, you will hire her. We will ask you to form your own valuation about the maximum salary you would pay for one worker looking to work for you for one week of probation. This worker is hypothetical, i.e. s/he does not exist, although his/her characteristics are very similar to the types of workers we have interviewed few months ago. After you have thought about this salary, we will present you a list of 21 possible salaries for this worker for one week of work and we will ask you whether you would be willing to pay each possible salary for her. The salaries range from 0 UGX to 100,000 UGX and increase by 5,000 UGX each time. For example we will ask "Would you be willing to hire this worker for one week under probation if you have to pay her a salary of 10,000UGX?"; "Would you be willing to hire this worker for one week under probation if you have to pay her a salary of 15,000UGX?"; and so on. Once you have answered all these questions, you will be given an envelope with a price like this one [Enumerator: show the envelope]. This price is between 0 and 100,000UGX. The price has been randomly selected by the computer and **I DO NOT KNOW IT, NEITHER I COULD CHANGE IT**. If the maximum salary you agreed to pay in the 21 possible options is higher than the number in the envelope, you will get the worker for a probation period of one week, by agreeing to pay the salary you see in the envelope. Therefore, imagine this worker will start to work for you: at the end of the week, she will expect you to pay the agreed salary. If the maximum salary you agreed to pay is lower than the price in the envelope, you will not be able to work with this jobseeker. Given the mechanism, it is in your best interest to be truthful, meaning to accept to pay salaries up to the maximum amount you are willing to pay for the worker. In this way you will never pay more than the maximum value the worker has for you and you could end up paying less. Moreover, the price you stated will affect your chance of hiring the worker but might not be the price you will actually pay. The price you will pay is fixed and your valuation will not change it. Remember that this worker is hypothetical. However, it is important to us that you take the choices seriously, and do your best to give us the answer you would give if they were real workers.

Multiple Price List.

- Would you be willing to hire this worker for one week under probation, starting up to 4 days from now, if you have to pay her a salary of 0UGX?
  - If no: Are you sure you **don't** want to hire this worker even if for free?
  - If sure: You said you are not willing to hire this worker even if for free. Can you tell us why?
- If yes: Would you be willing to hire this worker for one week under probation, starting up to 4 days from now, if you have to pay her a salary of 5,000UGX?
- Are you sure you **don't** want to hire this worker for 5,000UGX?
- ...
- ...Would you be willing to hire this worker for one week under probation, starting up to 4 days from now, if you have to pay her a salary of 100,000UGX?