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

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Abstract

Using a randomized controlled trial with Ugandan firms, we test whether a one-week workplace internship with a refugee worker shifts employers' labor-demand decisions. The internships doubled firms' subsequent hiring of refugees over the next two years, strengthened owners' support for refugee integration, and improved perceptions of refugees' skills—evidence of sizable prior misperceptions regarding workers typically not considered for employment. Impacts vary with both employers' and refugee workers' baseline attitudes: better firm-refugee matches amplify later demand. These findings highlight how low-cost contact interventions can relax informational frictions and inform labor-market policies for forcibly displaced people. (*JEL*: C93, D83, J15, J70, M51, O15)

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1. Introduction

Do firms systematically underestimate the productivity of marginalized workers, and can temporary exposure correct these misperceptions? This question is central to labor economics and policy design, as employer beliefs shape hiring decisions, wage setting, and overall labor market efficiency. A long-standing literature suggests that firms often hold biased prior beliefs about underrepresented groups, leading to statistical discrimination (Arrow 1973; Phelps 1972) and inefficient hiring practices. More recent literature in economics and political science has shown the positive impact of intergroup contact on reducing bias and improving relationships between different groups (Mousa 2020; Lowe 2021; Ghosh 2022; Bursztyn et al. 2024). However, less is known about whether short-term exposure can meaningfully update employer beliefs and induce persistent changes in labor demand.

This paper provides new evidence regarding this question by leveraging a randomized experiment in Uganda that pairs firms with skilled refugee workers for a one-week subsidized internship. The setting is particularly relevant: Uganda hosts over 1.5 million refugees, one of the largest refugee populations globally, yet many local employers have little prior experience hiring refugees. Our intervention creates exogenous variation in employer exposure to refugee workers, allowing us to test whether firsthand experience shifts beliefs and hiring behavior.

We make three key contributions. First, we provide experimental evidence on the updating of employer beliefs in a setting with high labor market frictions. We show that firms' prior beliefs about refugee workers' productivity are systematically downward-biased. The internship experience leads to Bayesian updating, improving employer perceptions and increasing demand for refugee workers even two years later.

Second, we contribute to the broader literature on statistical discrimination and learning (Farber and Gibbons 1996; Altonji and Pierret 2001). Unlike prior studies that focus on formalized signals (e.g., resumes, degrees), we show that direct workplace exposure serves as an alternative mechanism for overcoming bias. Notably, the effect is heterogeneous: firms with initially positive attitudes toward refugees that are matched with refugee workers who hold positive views about Ugandans, update their beliefs more, whereas firms with more negative views that are randomly matched with more negative refugee workers are not more likely (or are even less likely) to hire.

Third, we document the long-run effects of a temporary intervention on labor market outcomes. Treated firms double their hiring of refugee workers over two years, and these hires are not limited to the specific intern they hosted. Instead, exposure induces a broader shift in hiring practices and network formation, with firms becoming more likely to recruit through refugee-focused job-matching organizations. Importantly, we find no significant crowd-out effects on native hiring, suggesting that updated beliefs expand labor demand rather than displacing local workers.

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 inferior, 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.

We began by testing the practical skills of a sample of 552 refugee jobseekers 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, excluding very few workers who did not pass the exam. Treated firms were incentivized 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 theoretical 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 prior beliefs 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 20 percentage points (pp) (or 28 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 28 pp (equivalent to a 39 per cent decrease).

We extend our baseline learning framework to include attitudes, affecting learning efforts of both the employer and the worker. More positive employers assign workers to more complex tasks from which they learn more. In contrast, more negative employers are more likely to assign less complex tasks and find it harder to learn about the skills of their worker. At the same time, more positive workers exert more efforts on the job as their perceived returns to integration are higher. 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).

Our results find additional support in the social psychology literature. 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).

Finally, and crucially, we find that the effect of one-week exposure intervention on actual hirings is concentrated 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.

Our results suggest that interventions facilitating direct exposure to underrepresented workers may be a cost-effective way to counteract hiring biases, particularly in settings where firms lack prior experience with marginalized groups. Our back-of-the-envelope calculations reveal that each job created by the intervention cost less than 100USD. Our findings have broad implications beyond refugee labor markets. They suggest that short-term exposure programs can serve as a powerful tool to counteract employer biases, and would potentially be applicable to other marginalized groups, such as ethnic minorities or formerly incarcerated individuals. The results also highlight the importance of social interactions in labor markets, complementing work on workplace diversity and team composition.

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 study the effect of an intervention that targets firms' demand for workers from a marginalized group. More generally, this study adds to the 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 hiring a disadvantaged group of workers, by studying the effect of a short-term work relationship.

Second, we connect to the literature on workplace contact and firm productivity (Hjort 2014; Ghosh 2022; Baggio and Cosgel 2024; Afridi et al. 2024; Chakraborty et al. 2024). While these articles show that familiarity with in-group members can enhance effort or cohesion among workers, our paper demonstrates that a short-term workplace intervention (internships) with minority workers can alter employer biases and hiring preferences, without affecting firm productivity. This contributes to the question of how workplace composition can be actively managed to improve employment outcomes for minority workers.

Third, we contribute to the literature on hiring inefficiencies, employer bias and learning by experience (Farber and Gibbons 1996; Altonji and Pierret 2001; Pallais 2014; Benson and Lepage 2024; Macchiavello et al. 2024). Benson and Lepage (2024), in particular, find that hiring discrimination evolves over time as managers form perceptions based on past experiences, particularly when they have negative encounters with minority workers. Our paper complements these findings by showing that even a short workplace intervention can shift employer biases and hiring decisions in the long run. This suggests that employer learning about minority workers is not purely

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

shaped by passive experience but can be actively modified through policy interventions such as internships. Unlike these previous studies that focus either on organic exposure (Afridi et al. 2024) or negative experience-driven bias (Benson and Lepage 2024), our paper suggests a proactive strategy—matching firms with minority workers in a short, low-stakes internship—to reshape employer attitudes and hiring patterns. This has direct policy relevance for refugee labor market integration, a topic not explicitly addressed in the other papers.

Importantly, our findings highlight the role of matching quality (i.e., attitudes of both parties) in determining the effectiveness of workplace contact, an angle not explored in the previous studies. Pallais (2014) focuses on hiring inefficiencies in entry-level labor markets and how the provision of public information about worker ability impacts subsequent employment opportunities. The paper demonstrates that employers under-hire inexperienced workers because they cannot fully capture the benefits of revealing worker productivity. Our paper extends this framework to refugees, a particularly disadvantaged group with preconceived biases against them, rather than just inexperienced workers. Our results suggest that employer misperceptions and biases play a significant role in hiring frictions.

Finally, 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; Caria et al. 2024a; see Becker and Ferrara 2019 for a review). We advance this strand of the literature in several ways. First, on the intervention: while most of the current literature has studied the effect of subsidising job search, providing job trainings and improving skills recognition, our paper proposes a way to improve employer's beliefs by incentivizing exposure to refugee workers. Second, we show that the effect is heterogeneous by type of match, with both employers' and workers' attitudes shaping impacts. Third, we study the long-term effects on firms' demand for refugee workers, investigating the impact on hiring two years after the experiment.

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, describes the main outcomes of the paper and presents a simple theoretical framework to guide the intepretation of the results. Section 5 reports the results of the experiment. Section 6 explores more in details the importance of initial attitudes on the success of matching. Section 7 discusses the policy implications of the results. Finally, Section 8 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 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 to 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

[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

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 in the experiment, on the refugees' and employers' side. We begin by describing our sample of refugee 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 our collaboration with two local refugee-led NGOs, which have access to a wide population of refugees in Kampala. Thanks to their assistance, we list 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 consent to be interviewed during the listing exercise and 1,089 respected the sample requirements. We exclude 108 refugees who 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. 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 of Uganda.[7] This

[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 refugees 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]See Appendix Figure A.2, Panel B. It is reasonable to expect that, compared to refugees who did not attend the test, those who attended would be certain to pass it. Although in fact more experienced,

sample collects refugee workers interested in searching for jobs at Ugandan firms, and is therefore representative of the population of skilled refugee workers that Ugandan employers would consider or ever interact with. In partnership with the Directorate of Industrial Training (DIT) and a large vocational institute in Kampala, we organize one examination week during the second half of April 2021. During this week, DIT official examiners test all the refugees that attended the test, using the DIT's national curriculum.

The test focuses on the practical skills of the workers and varied in length, depending on the occupation chosen by the candidate. For instance, hairdressers are 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 the skills tested for each occupation. The skill is chosen by the examiners and communicated in advance to the participants during an introductory session that takes place a few days before the exam.

The examiners, who are trainers with years of expertise in a specific sector, score 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 attend and complet the test, only 11 people fail the exam, and therefore do not obtain a certificate. For this reason, we drop these workers and focus on the ones who pass the test (541). Due to a second wave of COVID-19, we pause the project until September 2021. However, we successfully track 527 of the original sample (see our detailed timeline in Figure 1, Panel A). Our final sample is composed of 85 per cent Congolese (N=448), 11% Burundians (N=58), 3.61 per cent Rwandans (N=19) and less than 1 per cent 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 remainder speaks English or Luganda (the main language spoken in Kampala) 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 skilled 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

refugee workers who attended the test are not necessarily those who would *ex-ante* perform better at the test, as they obtained the vocational skills working or studying outside Uganda.

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, the refugee unemployment rate is almost five percentage points higher and, even when they are 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 to show the characteristics of potential workers that 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 in working age residing in rural areas.

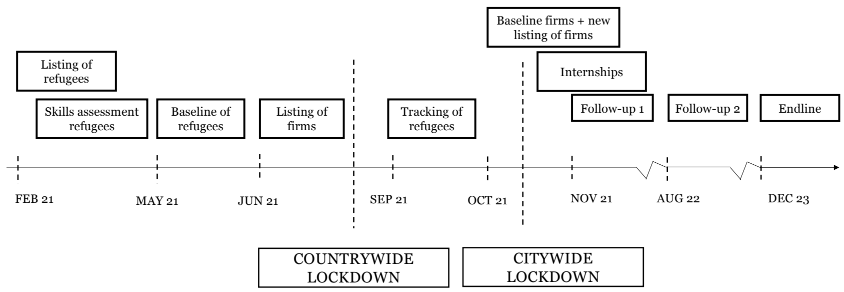
Firms. Our intervention targets local employers. To construct the sample of employers, we listed and interviewed 1,192 firms active in selected sectors in Kampala, using a random walk sampling procedure.[8] A total of 535 firms fulfilled the two criteria for inclusion into our sample: they were owned 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. Importantly, the "willing-to-hire" sample is composed by firms that are more likely to have an open vacancy and desire to expand in the future, smaller and with more experience using internships. The findings of our experiment, therefore, generalize to firms that are interested in hiring new workers. Appendix Table A.3 summarizes the characteristics of the 535 firms whose Willingness to Pay (WTP) is non-negative, compared to the full sample of firms we interviewed. Figure 1, Panel B, maps the location of the firms that belong to our baseline sample in the metropolitan area of Greater Kampala.

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

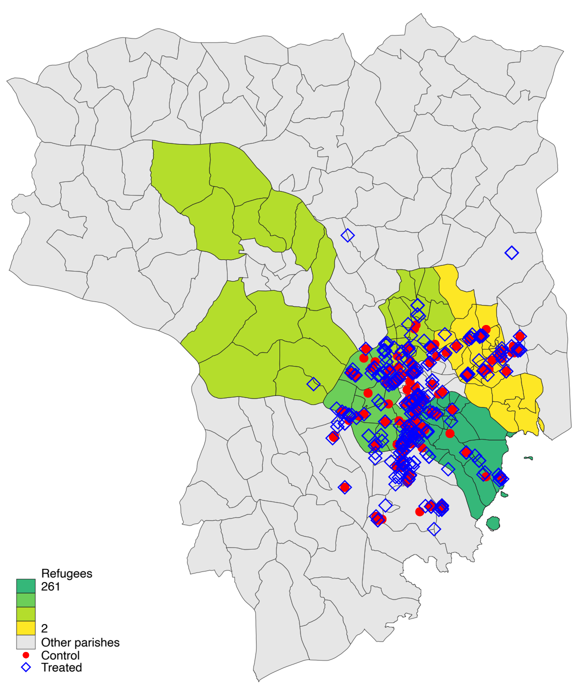
The goal of the experiment is to study whether firms' demand for refugees can be increased by updating their prior beliefs about refugee workers' productivity via costly information acquisition. Our treatment consists of offering firms a subsidized short-term internship with a skilled refugee worker—while employers are informed

[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.

FIGURE 1. Timeline and Firms' Locations



(**A.**) Timeline



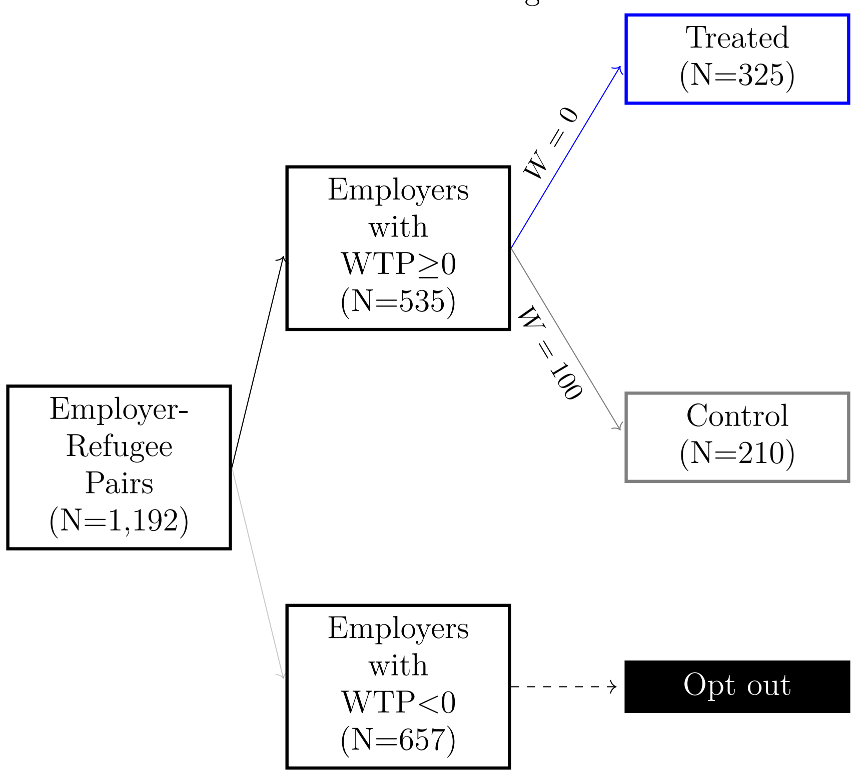
(**B.**) Firms' Locations across Kampala

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 (red 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.

they can hire the worker at no cost, the research team covers the weekly wage paid to the refugee intern. This section is divided into two parts: First, we detail the experimental implementation, including our firm selection process and the methodology for assigning employers to treatment and control groups. Second, we present a simple theoretical 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 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, 81 PPP-adjusted USD at 2021 levels).[9]

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.

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

[9]See Section B in the Appendix for the script we used to elicit employers' WTP.

[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 those 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 with 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.

of vocational skills. The certificate is only shown to the employer, but retained by the research team. 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 worker in the next eight days. See Appendix Figure A.5 for the original registered 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 535 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: $W = 0$UGX and $W = 100,000$UGX.[15] This ensures the allocation of firms to treatment and control is purely random and does not depend on the employer's WTP. This means that no internship took place in the control group. Appendix Figure A.7, Panel A, shows the demand function for a refugee worker at baseline in our sample. As a result of the WTP elicitation, treated firms are asked to offer an unpaid internship to the matched refugee worker. In practice, the refugee interns' salary for their week of work was paid by the research team and it amounted to 50,000UGX (that is, about 41 PPP-adjusted USD at 2021 levels).

[13]The remaining firms are either not interested in hiring any worker (approximately 75 per cent of them) or interested in hiring a worker only if Ugandan (about 25 per cent), suggesting some firms discriminate on the basis of the nationality/refugee status of the worker. Finally, of these 657 firms, more than half say they do not have enough work to hire an intern.

[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. Appendix Figure A.6 shows an example of the certificate. The front page contains demographic information on the candidate, the score obtained on the test and the occupation that was tested. Showing the certificate to the employer increases WTP for the intern by approximately 10%. However, the core results of the experiment, such as the one on beliefs about quality of refugee workers, suggest that the two arms are not distinguishable from each other. Therefore, 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.

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. The salary is paid half at the beginning of the internship and half at the end. 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 attend the introductory meetings. As a consequence, about half of the firms assigned to the treatment group are actually treated (in the sense of receiving a refugee intern). The sample of firms that receives a worker is balanced in terms of random assignment and has similar characteristics to the sample of firms that does not receive the worker. Importantly, there is no difference in any ability measure between refugees who attend at the test versus those who do not. Appendix Figure A.8 explores observable determinants of the refugee workers' take-up of the internships. The sample of refugees who attend the test is slightly older than those who do not attend (34 years of age versus 32). Notably, the largest and most significant determinant 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 may become disappointed with refugees, while others fail to learn anything at all. That is, not only can they not experiment working with a refugee, but also some may revise negatively their beliefs about refugee workers. When refugees fail to present for work, a few employers express dissatisfaction with the research firm and refugees. Our encouragement design may have not affected all treated employers in the same way. In general, some employers behave as control group and do not experience any learning. 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 (although results do not change if we do not include them). 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 takes place about a month after the matching intervention. In this survey, we track 525 firms (attrition is balanced between treatment and control, see Appendix Table A.5, columns 1, and 4). For the second one, which takes place approximately eight months after the intervention, we collect longer term follow-up data from the 474 firms we managed to reach. Finally, for the endline (two years after the experiment) we successfully track 407 firms. 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 take place, but we successfully track 179 firms at the first follow-up. Table 1 describes 179 internships. The median duration of the internship is seven days, as expected. During the internship, employers assigned workers simple and complex tasks (where complexity is measured at baseline using the employer's self-reported scale of 1 to 5 collected for each firm-specific task listed: 1 means "Very Simple" and 5 "Very Complex"). About 40 per cent of the employers pay their interns on average 19,000UGX (about 16USD at PPP-adjusted levels for 2021) for the internship, typically in the form of lunch or transportation (although the worker in most cases does not ask to be paid).[16] On average, each intern works for seven hours a day and managers at the firm spend more than five hours supervising the intern each day. The employers do 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 are willing to rehire the same worker.[17] Seven workers are hired after the internship (or 3.9 per cent of the total

[16]The elicitation exercise proceed only after the respondent correctly answered our comprehension check questions: "*Suppose that the price in the envelope is:* $WTP + / - 5000$. *What will happen?*". Answers options were: 1. I would be able to hire the worker; 0. I would not be able to hire the worker; 888. Do not know. This eliminates any worry that respondents did not understand the WTP elicitation exercise. In practice, given that a worker is not a good, as in many elicitation exercises, but a human being, some employers may have decided to pay their workers regardless.

[17]About 60 per cent of firms who are not willing to hire the same worker again do not have enough work or space, 33 per cent are not satisfied with the skills of that specific worker. A minority (about 5 per cent) says that they are disappointed with refugees.

number of interns). The majority of employers (70 per cent), finally, recommend or would recommend the worker to another firm.

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 come 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. Notice that the dummy "Supervised more than other workers" is created only for firms with at least one employee.

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, more than half of the employers spent more time supervising the intern than any of their other employees.

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 generates persistent labor demand shifts and the employers update their prior beliefs about refugee workers' productivity. 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. Crucially, we do not ask the employers' opinions about a generic refugee, but of a generic refugee *worker*. Specifically, we introduce the section on beliefs as follows: *"For the next set of questions, I want you to think about the typical refugee jobseeker in Kampala."*. We also elicit the employer's beliefs about the score a Ugandan worker would achieve.[18]

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, teamwork, 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.[19]

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.[20] 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.

4.3. **Theoretical Framework.** In this subsection, we provide a simple theoretical framework to interpret the experiment and guide the interpretation of the results. The experiment investigates how exposure, based on observing one refugee for one

[18]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.

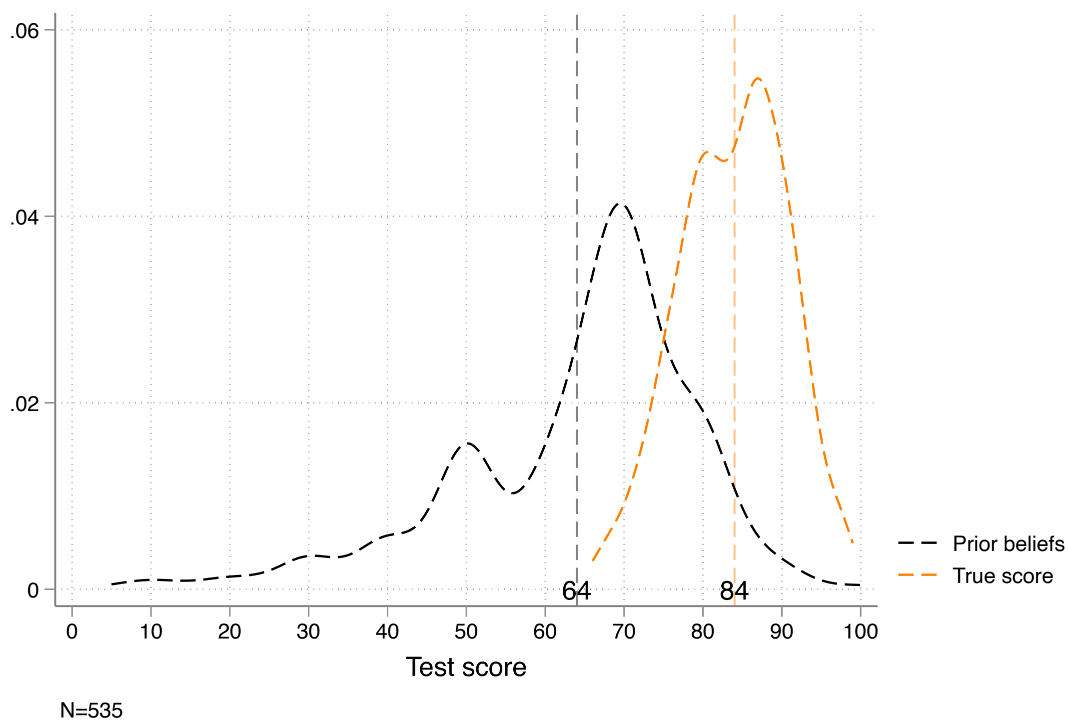
[19]We chose this set of skills after extensive piloting exercises with firms similar to those belonging to our sample. Specifically, we asked pilot firms to rank workers' skills in order of importance for the success of a business like their own.

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

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 regarding one dimension of hard skills of the refugee workers. In Figure 3 we compare the employers' beliefs with the actual scores obtained by the refugee workers in our sample.

FIGURE 3. Firms' Beliefs About Refugees' Ability



Notes: This graph plots the distribution of the employers' beliefs and the real score on the test. The question in the survey is as follows: *"Workers can undertake a modular assessment on some specific skills. The assessment, called "Non-Formal", tests workers' practical skills in specific occupations. At the end of each assessment, they can receive a modular transcript issued by the Directorate of Industrial Training. The modular assessment reports a score associated to the performance of the worker during the test. The score ranges between 0 and 100. The threshold to pass the test is 65. Suppose a refugee job seeker, whom you do not know, does this test for the first time. What is the score you would expect him or her to achieve?"* Baseline sample with 535 firms who are willing to hire a refugee. Dashed black lines represent the employers' beliefs (i.e. self-reported score they think the jobseeker obtained). Dashed orange lines represent the true scores.

Figure 3 evidences two facts. First, employers' beliefs are biased downwards: while refugee workers' average actual score on the test is 84, employers believe a generic refugee worker to score below the passing threshold of 65. Second, employers' beliefs about refugee workers are uncertain, with the variance of beliefs to be higher than the one of the actual score, suggesting that Ugandan employers have weak and imprecise prior beliefs. Taken together, these findings reveal that Ugandan employers have weak and incorrect beliefs regarding the ability of refugee workers.

Suppose the firm's manager's expected utility is given by

$$E(U) = E(\Pi) - \frac{1}{2}g(\eta)Var(\Pi) \tag{4.1}$$

where Π denotes the firm's profits and $g(\eta)$ captures the manager's risk aversion. Profits Π are denoted by $\Pi = \theta + \varepsilon - w$, where θ is the average ability of refugee workers, ε denotes the individual random ability of a worker, distributed following a $N(0, \sigma_\varepsilon^2)$ and w is the market wage, which we assume independent of the worker's individual ability. The employer cannot observe the average group component, but has some prior beliefs about it, and these beliefs are distributed following a $N(m_0, \sigma_0^2)$. Given the employer's inexperience with refugee workers, the employer's prior belief is biased: $m_0 < \theta$ (as evidenced by Figure 3). Under the assumption that the manager's beliefs and the individual random component of ability are independent, Equation 4.1 can re-expressed as

$$E(U) = m_0 - w - \frac{1}{2}g(\eta)(\sigma_0^2 + \sigma_\varepsilon^2) \tag{4.2}$$

If hired by the employer, the worker can produce a signal regarding their ability: $s = \theta + \varepsilon$. After exposure, the employer's beliefs are revised and are now distributed following a $N(m_1, \sigma_1^2)$, where

$$\sigma_1^2 = \sigma_0^2 \frac{\sigma_\varepsilon^2}{\sigma_0^2 + \sigma_\varepsilon^2} < \sigma_0^2 \tag{4.3}$$

$$m_1 = \alpha s + (1 - \alpha)m_0 \tag{4.4}$$

with $\alpha = \sigma_1^2/\sigma_\varepsilon^2$. After exposure, the employer's beliefs will on average be given by $E(m_1) = \alpha\theta + (1 - \alpha)m_0$.

The predictions of this framework are twofold:

Prediction 1. If the employer's beliefs are biased downwards, then exposure will improve the employer's posterior beliefs:

$$E(m_1) > m_0 \tag{4.5}$$

Prediction 2. Given the increased precision in the posterior (see expression 4.3) and improved average posterior beliefs (see expression 4.5), the average expected utility of the employer will increase.

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

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. 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 or Poisson models. However, using post-double-lasso selection models including pre-registered covariates do not change the significance of the results.[21]

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

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

We fail to reject the null of the effect being the same between the two treatment arms. The set of pre-registered covariates comprises: occupation on which the refugee was tested (i.e. our randomization strata), years living in Kampala, age, gender and years of experience in selected sector (the variables included in the CV we show to firms).

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 for conducting a separate analysis is given by the fact that firms that were promised a worker who did not attend the appointment may have experienced a negative effect on their 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 did not measure hiring at baseline, so this table does not run an ANCOVA). We measure total number of refugees hired at two points in time (at eight months and 24 months after the experiment).To study the long-term effect of the internships, we use the endline sample and sum the responses at follow-up 2 and endline to create a unique variable. The main outcomes are then the total number of refugees hired between January 2022 and December 2023 and the extensive margin of hiring, namely whether treatment changes the probability of hiring at least one refugee worker. We additionally investigate whether the effect is driven by recalling the same worker that did the internship with the treated firms or whether firms are hiring new refugees only. Finally, we provide suggestive evidence on the effect of treatment on hiring Ugandan workers. We use OLS to study the effect of treatment on the extensive margin. Given the distribution of the total number of workers characterized by a high number of zeros (75% of firms report zero refugee workers hired by endline), we use a Poisson regression to determine the effect of the experiment on count of workers. In a Poisson regression, the coefficients represent the expected change in the log of the count for a one-unit increase in the predictor variable, holding other variables constant. The upper panel of Table 2 reports the coefficients of both OLS and Poisson regressions. To determine the predicted change in treatment for the outcomes where we use Poisson regressions, we report the incident rate ratios (IRR) minus 1 at the bottom panel of Table 2.[22]

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

[22] $IRR = exp(\beta)$

TABLE 2. Number of Workers Hired

	Refugee hires				Ugandan hires			
	(1) At least 1	(2) Total	(3) Only new	(4) Still employed	(5) At least 1	(6) Total	(7) Still employed	(8) Total employment
Panel A: ITT								
Treated	0.077*	0.588***	0.404*	1.026**	-0.041	-0.097	-0.139	-0.026
	(0.042)	(0.209)	(0.221)	(0.411)	(0.047)	(0.117)	(0.126)	(0.411)
	[0.066]	[0.005]	[0.067]	[0.013]	[0.390]	[0.410]	[0.272]	[0.950]
Firms	407	407	407	407	407	407	407	407
Mean DV	0.200	0.269	0.269	0.037	0.744	2.194	1.069	3.425
Mean Treated	0.279	0.478	0.401	0.093	0.709	1.943	0.866	3.081
Change over mean (%)	39	80	50	179	-5	-9	-13	-1
Regression model	OLS	Poisson	Poisson	Poisson	OLS	Poisson	Poisson	OLS
Panel B: Effect of exposure								
Exposed	0.134**	0.852***	0.601**	1.203**	-0.040	-0.129	-0.211	0.112
	(0.052)	(0.239)	(0.256)	(0.522)	(0.055)	(0.137)	(0.154)	(0.550)
	[0.011]	[0.000]	[0.019]	[0.021]	[0.467]	[0.347]	[0.170]	[0.839]
Firms	299	299	299	299	299	299	299	299
Mean DV	0.200	0.269	0.269	0.037	0.744	2.194	1.069	3.425
Mean Treated	0.324	0.583	0.468	0.108	0.705	1.906	0.827	3.439
Change over mean (%)	67	134	82	233	-5	-12	-19	3
Regression model	OLS	Poisson	Poisson	Poisson	OLS	Poisson	Poisson	OLS

Notes: This table reports the coefficients estimated by equation 5.1 in the upper panel. *Dependent variables*: Columns 1 to 4: a dummy equal to one if at least one refugee worker was hired by endline (col. 1); total number of refugees (col. 2); total number of new refugees (col. 3); total number of refugee workers still employed at endline (col. 4). Columns 5 to 7: a dummy equal to one if at least one Ugandan worker ws hired (col. 5); total number of Ugandan workers (col. 6); total number of Ugandans still employed at endline (col. 7). Column 8 reports the effect of the experiment on total employment at endline (that is, total number of employees employed at the firm 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.

first column reports the effect of the experiment on the extensive margin for refugee workers. Column 1 shows that treated firms are 39% more likely to have hired at least one refugee worker after the internship. Columns 2 to 4 report results on the total number of refugee workers hired. The effect on total number of refugees is substantial and economically meaningful: an increase of 80 per cent over the control mean (col. 2). The increase in number of refugees is not driven up by hiring the same intern. Instead, this result is driven by the number of new refugees hired (col. 3). Excluding the worker matched during the internship, the effect is 50 per cent over the control mean. The comparison with Panel B allows us to demonstrate that the effect is concentrated among firms in which the internship actually took place. First, the effect on the extensive margin is almost twice as large: exposure increases the number of firms hiring at least one worker by about 67 per cent. Second, the total number of refugees hired by treated firms more than doubles compared to the control (col. 2). Importantly, firms that experienced exposure to a refugee intern hire about 82 per cent more new refugees compared to the control group (col. 3). Furthermore, compared to the control group, treated firms are substantially more likely to report refugee workers that are still employed at endline, suggesting that internships help firms establish a durable employment relationship with refugee workers (col. 4).[23] Col. 5 to 7 show that the experiment did not affect hiring of new Ugandan workers. However, this result should be taken with caution, as the point estimate is negative and not negligible. Lastly, col. 8 suggests the absence of any impact on total employment. That is, treatment does not affect total firm size. Taken together, findings from columns 6 to 8 suggest that treated firms could have laid off existing employees (Ugandans or other foreign workers) to welcome new refugee workers.

5.2. **Refugees Perform Equally Complex Tasks as Ugandans.** The effect of exposure to a refugee worker on the total number of refugees still employed at the firm suggests returns on hiring refugee workers are positive. This suggests that employers do not hire refugees simply due to temporary generosity towards marginalized workers (Macchi and Stalder 2023), but rather hire experienced workers for productive purposes. To explore this hypothesis, we investigate work performed by new hires—both refugees and Ugandans—at treated and control firms. At endline, we collect detailed information on all tasks performed by each new worker during their employment. For each task, employers indicate at baseline its level of complexity on a scale from 1

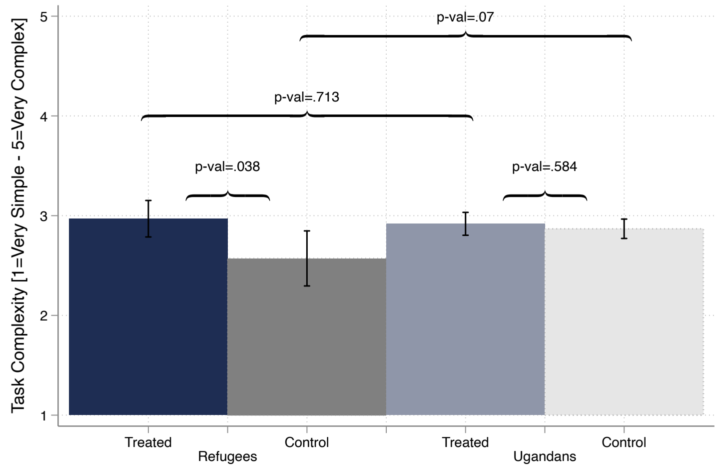
[23]None of the interns employed during the experiment were still employed at endline, suggesting that these new employees are new refugee workers

("Very Simple") to 5 ("Very Complex"). To analyze differences in task complexity, we compute average task difficulty and conduct mean comparison tests across four groups: treated versus control firms, and refugee versus Ugandan hires. Figure 4 focuses on the exposed sample only, excluding interns hired for the experiment, and shows average difficulty of tasks assigned to new refugee and Ugandan hires (Panel A), as well as average daily wage (Panel B). Both panels report p-values from sample means comparison tests. Panel A reveals that treated firms are more likely to assign refugee workers to meaningful and productive tasks compared to control firms (p-val = .038), with task complexity levels at least as high as those assigned to Ugandan workers (p-val = .713). In contrast, control firms assign refugee workers significantly less complex tasks compared to their Ugandan hires (p-val = .07), suggesting that without previous exposure, Ugandan employers tend to assign refugee workers more menial jobs. Panel B examines average daily wage, calculated by dividing total payment by number of days worked. The results show treated firms pay their refugee hires similar wages to control firms. While differences compared to Ugandan hires are not statistically significant, they are substantive: Ugandan hires receive approximately 20 percent higher wages than refugee hires. Wages do not differ significantly between treated and control firms within the same hire group. This suggests treated firms may have learned to hire more experienced and productive refugee workers, paying them their reservation wage, which appears lower than their marginal product of labor. While this may suggest an improvement in firms' productivity, a more direct test fails to find evidence for this. Appendix Table A.7 shows that the internships did not have an effect on firms' profits and profits per worker.

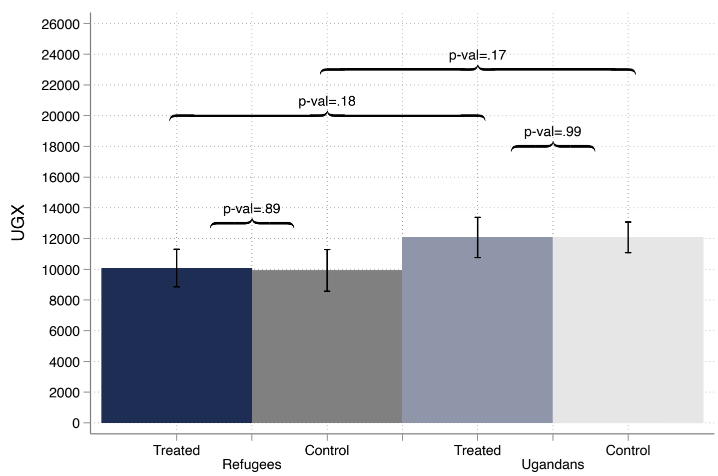
5.3. **Firms Become More Supportive of Refugees' Integration.** The positive results of the experiment on hiring extend beyond employment outcomes to employers' stated support for refugees' integration. The evidence suggests that working together leads employers to become more supportive of integrating refugee jobseekers. We demonstrate this in Table 3. In columns 1 and 2, we investigate the experiment's effect on firms' willingness to donate to a non-profit organization that assists refugees in Uganda by providing skills training and employment assistance.[24] Results show that treated employers are significantly more likely to donate compared to control

[24]At follow-up 1 and 2, we asked the following question: *"We are currently collaborating with a non-profit organization that works with refugees in Kampala. One of their activities is to organize trainings to skill these people and to help them finding jobs and business opportunities in Uganda. Out of the* $5,000 UGX$ *token of appreciation we are going to give you to participate to this survey, how much are you willing to donate to this organization?"*

FIGURE 4. New Hires: Tasks and Payments



(**A.**) Tasks' Average Complexity



(**B.**) Average Daily Wage

Notes: The first graph (Panel A) plots the average difficulty of tasks assigned to refugee and Ugandan hires. The vertical axis reports the average difficulty using a scale 1 to 5. The second graph (Panel B) reports daily wages paid to refugee and Ugandan hires, by treated and control employers. The vertical axis reports payments in Ugandan Shillings. Above the bars, p-values of means comparison tests are reported.

employers (Panel A), with the effect concentrated among firms where the internship took place (Panel B). Treated employers are also more likely to have connections with non-profits that can help them find refugee workers (column 3).[25] We interpret these findings as evidence that the experiment increased firms' support for the labor market integration of refugees. The effect on connections to NGOs supporting refugee jobseekers is suggestive evidence of potential network effects amplifying the effect of exposure. That is, treated firms are more likely to be connected to a larger pool of refugee jobseekers. Columns 4 and 5 further suggest that employers' general views about refugees improved. While 40 per cent of control employers believe that Uganda's cultural life is enriched or very much enriched by refugees, approximately one-third more employers in the treatment group share this view. Finally, over time, 37.5 per cent of control employers report that their general perception of refugees improved, compared to nearly 41 per cent of treated employers who express the same sentiment. Column 6 summarizes endline results in an index calculated following Anderson (2008).

5.4. **Firms Improve Their Beliefs About Refugees, But Short-Term Demand Does Not Change.** Our theoretical framework predicts that exposure should improve employers' beliefs about generic refugee workers. In order to explore this mechanism, 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 refugee workers.

As predicted by our simple theoretical 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 (col. 2), as well as beliefs about their behavior at work (col. 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: $Y = [I_n \otimes T]\beta + \mu$, where Y is a vector of n beliefs outcomes and the square matrix $I_n \otimes T$ collects the Kronecker product of

[25]At endline we asked the following question: *"Do you personally know anyone from refugee-led organizations or other organizations working with refugees you can refer to in case you are looking for a new worker?"*

Table 3. Attitudes Towards Refugees' Integration

	Follow-up 1	Follow-up 2	Endline			
	(1)	(2)	(3)	(4)	(5)	(6)
	Donation to NGO	Donation to NGO	Knows anyone at NGO	Culturally enriched	Improved views	Index
Panel A: ITT						
Treated	0.165*	0.233**	0.041	0.056	0.085	0.213**
	(0.091)	(0.102)	(0.032)	(0.052)	(0.052)	(0.099)
	[0.070]	[0.022]	[0.199]	[0.281]	[0.102]	[0.033]
Firms	525	474	407	407	407	407
Mean DV	-0.000	-0.000	0.094	0.400	0.375	-0.000
Panel B: Effect of exposure						
Exposed	0.255**	0.276**	0.069*	0.146**	0.133**	0.398***
	(0.109)	(0.126)	(0.039)	(0.060)	(0.060)	(0.114)
	[0.020]	[0.030]	[0.083]	[0.016]	[0.027]	[0.001]
Firms	385	343	299	299	299	299
Mean DV	-0.000	-0.000	0.094	0.400	0.375	-0.000

Notes: This table reports the coefficients estimated by equation 5.1. *Dependent variables*: Columns 1 and 2: Donation to a non-profit organization helping refugees, standardized using method described in Anderson (2008), collected at midline and endline. Column 3: 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 4: A dummy equal to 1 if the employer reports that Ugandan culture is enriched or very much enriched by the presence of refugees from other countries, collected at endline. Column 5: A dummy equal to 1 if the employer states that his/her view about refugees improved during the past year, collected at endline. Column 6: An index computed over outcomes at endline (columns 3 to 5), aggregating responses using Anderson (2008). *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 identity matrix and the treatment assignment vector.[26] The coefficient is positive

[26]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.

and significant (p-val=0.053), suggesting that the internships worked in updating the beliefs of the treated employers.

Table 4. Mechanisms

	Learning				Short-term demand
	(1)	(2)	(3)	(4)	(5)
	Hard skills	Soft skills	Behavior	Average stand. effect	WTP ≥ 0
Panel A: ITT					
Treated	-0.056	0.126	0.159	0.060	-0.021
	(0.094)	(0.105)	(0.103)	(0.072)	(0.041)
	[0.550]	[0.228]	[0.126]	[0.409]	[0.610]
Firms	525	525	525	525	525
Mean DV	-0.000	0.000	-0.000		0.709
Panel B: Effect of exposure					
Exposed	0.010	0.271**	0.331***	0.163*	-0.004
	(0.114)	(0.123)	(0.120)	(0.084)	(0.049)
	[0.928]	[0.029]	[0.006]	[0.053]	[0.938]
Firms	385	385	385	385	385
Mean DV	-0.000	0.000	-0.000		0.709

Notes: This table reports the coefficients estimated by equation 5.1. We use beliefs regarding the skills of the refugee worker introduced at baseline as the baseline value of outcome y_i. *Dependent variables*: Indices are computed following Anderson (2008), using the following underlying covariates: theoretical skills (scale from 1 to 5), practical skills (scale from 1 to 5) and performance (scale from 1 to 5) for the index on hard skills (col. 1); work ethics (scale from 1 to 5), time management (scale from 1 to 5) and teamwork ability (scale from 1 to 5) for the index on soft skills (col. 2); trust and respect (both scales from 1 to 5) 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. Short-term demand is proxied by a new WTP elicitation. Col. 5 reports results on 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). *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.

Our theoretical 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 and their willingness to interact with new refugee workers in the near future.

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.[27] 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 4, column 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. The estimated standard errors for the dummy are small, and range between .04 and .049.

In the Appendix, we report the curves for the demand of a new refugee by treatment status.[28] Visually, Appendix Figure A.7, Panel B, shows the demand does not shift differentially across the groups, with no difference between the full sample and the exposed sample.

5.5. **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

[27]There are two further reasons not to use WTP for the new refugee, conditional on WTP being non-negative. 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.

[28]The null effect persists not only on average, as shown in Table 4, column 5, but also across the distribution of the WTP. Kolmogorov-Smirnov tests do not reject the null of equal distributions across the three groups.

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: $\tau(X) = E[Y_{1i} - Y_{0i}|X = x]$, 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.[29] 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. See Appendix Tables A.9 and A.10 for a complete list of variables. 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 ethnic group of the Baganda, which constitute the largest ethnic group in Uganda; 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

[29] The resulting `excess.error` is negligible and equal to $2.79e^{-07}$.

towards Ugandans and Ugandan culture; knowledge of languages; their experience with Ugandan employers in the 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 5 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.11 reports the results from the best linear projector estimation.

Finally, Appendix Figure A.9 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).

Table 5. Heterogeneous Treatment Effects Predicted by Causal Forest

Variable	Low CATE	High CATE	Diff.	MHT p-val
Owner is from majority ethnicity	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.

6. The Role of Employers' and Workers' Attitudes

To understand why attitudes matter, we return to the theoretical framework and extend it to include the role of the employers' attitudes, and then additionally include the role of the workers' attitudes. First, to understand what attitudes means in our context, we begin by explaining more in details how we constructed the indices (see Appendix Table A.9 and Appendix Table A.10 for a full description). Then, we modify the theoretical framework to highlight the role of attitudes and how these shape both firms' incentives to learn and workers' incentives to work.

6.1. **Measuring Attitudes.** To construct the attitudes of employers, we use their 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 ranged on a scale of 1 to 5, where 1 denotes "Strongly Disagree" and 5 "Strongly Agree". We create a binary indicator equal to 1 if the employer's response is below 4 (i.e., they disagree or strongly disagree with prioritizing Ugandans). Additionally, 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, we define a "positive employer" as one who supports refugee labor market integration, demonstrated by having weaker in-group preferences compared to "negative employers".

We construct workers' attitudes as follows. First, we construct binary indicators equal to 1 if the refugee worker agrees or strongly agrees with the following statements: (1) *"Ugandans' culture is different from my own culture*", (2) *"Ugandans discriminate against refugees*", (3) *"I assume that in general, Ugandans have only the best intentions*", and (4) *"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 characterized by perceiving stronger cultural proximity to Ugandans and feeling more integrated, suggesting someone likely to exert greater effort if given the opportunity to demonstrate their skills.

6.2. **Learning and the Role of Attitudes.** Suppose now that the expected utility of the firm is given by the following expression:

$$E(U(\tau, e_f)) = E(\Pi) - \frac{1}{2}g(\eta)Var(\Pi) - \delta_{in-group} - c_{e_f}, \tag{6.1}$$

where:

1. the firm's profit function is given by $\Pi = \tau(\theta + \varepsilon) - w$, where $\tau \in [0, 1]$ indicates the degree of difficulty of tasks assigned to a worker - higher values of τ indicate more complex tasks;
2. the parameter $\delta_{in-group} > 0$ indicates the employer's in-group preferences (their attitudes towards labor market integration of refugees);
3. the parameter c_{e_f} indicates the employer's learning effort costs.

The employer chooses τ and e_f to maximize the expected utility in equation 6.1.

The worker's expected utility is given by $E(U(e_w)) = E(employment|\tau, s) - c(e_w)$, where $E(employment|\tau, s)$ is the expected future employment probability, conditional on the task assigned on-the-job and the learning technology, and the parameter c_{e_w} indicates the worker's on-the-job effort costs.

Including both the employer's and the worker's efforts as well as the difficulty of the task the employer assigns, the signal given by the internship is given by

$$s = \tau(\theta + \varepsilon + \nu(e_f, e_w)), \tag{6.2}$$

where $\nu(e_f, e_w)$ captures measurement error in learning, depending on both the employer's and the worker's efforts, with the variance σ^2_ν decreasing in both effort parameters.

The updating will now be influenced by the difficulty of the task assigned by the employer and the learning costs. The posterior beliefs are distributed according to the following process $N(m_1(\tau, \nu), \sigma^2_1(\tau, \nu))$, where

$$\sigma^2_1(\tau, \nu) < \sigma^2_0 \tag{6.3}$$

and

$$m_1 = \alpha(\tau, \nu)s + (1 - \alpha(\tau, \nu))m_0. \tag{6.4}$$

This revised theoretical framework produces two testable hypotheses:

Prediction 3: If the match is positive (that is, takes place between an employer with low $\delta_{in-group}$ and a positive worker):

- τ is closer to 1 (that is, tasks of higher difficulty level are assigned to the worker);
- Both the employer and the worker's efforts are high;
- α is higher and therefore learning quality is higher;
- Willingness to interact with new refugee workers increases.

Prediction 4: If the match is negative (that is, takes place between an employer with high $\delta_{in-group}$ and a negative worker):

- τ is closer to 0 (that is, tasks of lower difficulty level are assigned to the worker);
- Both the employer and the worker will exert low levels of effort, thereby decreasing precision of the signal;

- Learning will be limited, if any;
- Demand for new refugees changes a little or does not change at all.

It is more difficult to predict what happens in the mixed groups, that is, where only one agent enters the internship with positive attitudes. Two countervailing forces are at play: refugees' effort on the job and employers' effort on learning. Given that neither of the two prevails, 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 noted 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).

6.3. **Learning is Affected by Attitudes.** In this subsection, we test the predictions of our extended conceptual framework using the internship data. First, we provide suggestive evidence that exposure quality depends on the initial attitudes of both employers and workers. Second, we show how average task complexity varies between groups. Third, using data on employers' beliefs, we test whether firms learn differentially depending on match type. Finally, we quantify heterogeneity in the effect of exposure on short-term demand across positive, mixed, and negative matches.

Figure 5 reports averages of internship outcomes and pre-exposure refugee characteristics across the three attitude groups. The evidence suggests that in positive matches, employers demonstrate significantly greater willingness to hire the same worker (Panel A), indicating a more positive experience compared to negative matches. Furthermore, firms in positive matches found worker supervision less demanding (Panel B), despite employers in negative matches spending more supervision time per worker (Panel C). These descriptive findings suggest that internships proceeded significantly better when employers with positive initial attitudes were matched with workers sharing positive attitudes.

Additionally, refugees in positive matches were more likely to have been actively seeking employment prior to the experiment, having applied for more positions and achieved greater success with Ugandan employers (Panel D). Higher job offer rates from Ugandan employers among refugees in positive matches suggest these individuals may have already experienced better interactions with Ugandan employers. This second set of findings indicates that refugees with positive attitudes who matched with positive-attitude employers were also more motivated to provide stronger signals of their abilities during the internship.

As predicted by our extended theoretical framework, task complexity varies according to match type. Figure 5, Panel E, demonstrates that employers in positive matches assign more complex tasks to refugee workers during the internship.

Dividing the sample of firms by match quality, we estimate the effect of exposure using the following specification:

$$y_{i1} = \beta_0 + \beta_1 T \times Positive + \beta_2 T \times Mixed + \beta_3 T \times Negative + y_{i0} + X_i^{'}\delta + \varepsilon_i \quad (6.5)$$

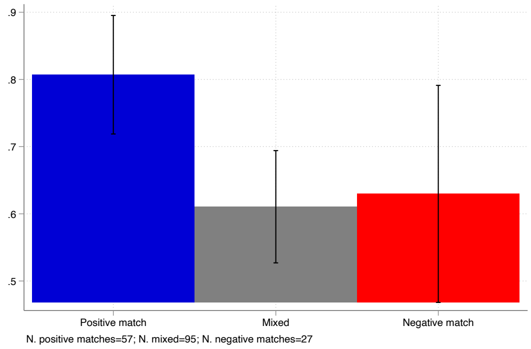
where $T \times Positive$ is a an indicator for treated positive employers that matched with a positive refugee, $T \times Negative$ is an indicator for treated negative employers that matched with a negative refugee, and $T \times Mixed$ is an indicator variable for treated negative (positive) employers 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.[30] Finally, X_i contains strata and area fixed effects.

Our framework further predicts that employers experiencing positive matches are more likely to learn about refugees' skill sets. In line with this prediction, using the beliefs indices described in Appendix Table A.6 and aggregating coefficients using average standardized coefficients, we find that the positive effect of exposure is concentrated among positive matches (Table 6), with limited effects among firms in the negative group. Although we cannot reject the null hypothesis of coefficient equality across heterogeneous matching groups, the magnitude of the coefficients suggests stronger effects when matches are positive. Specifically, Column 1 of Table 6 indicates that the effect for positive matches is more than twice as large as the effect for the negative group. As for the average results, we find that employers in positive matches become more positive of refugee workers' skills, especially soft (col. 3) and behavioral ones (col. 4).

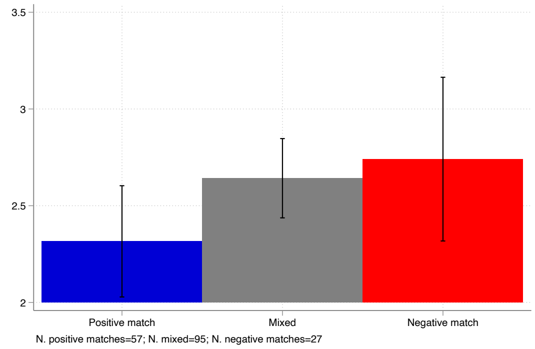
6.4. **Quantifying the Heterogeneous Effect of Initial Attitudes.** To quantify how the effect of exposure on short-term demand for refugees depends on attitudes

[30] 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.

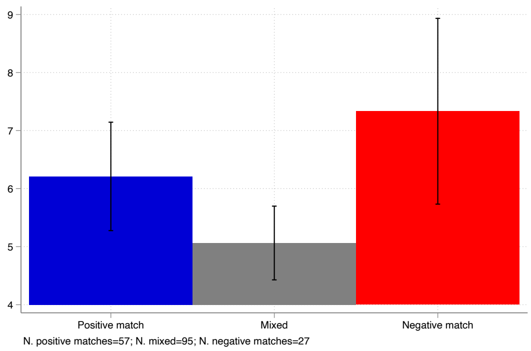
FIGURE 5. Evidence from the Internship



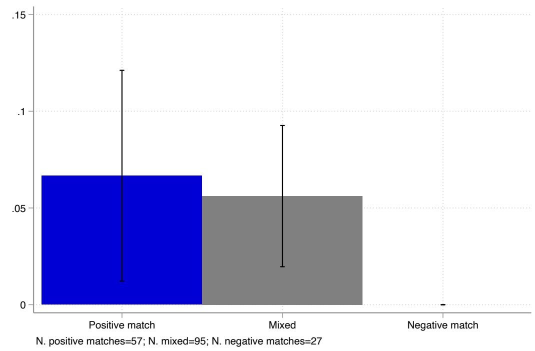
(**A.**) Willingness to hire the same intern



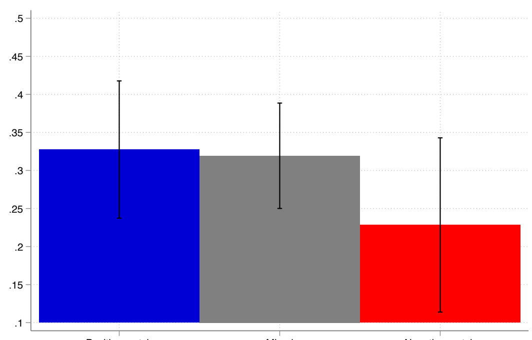
(**B.**) Difficulty of supervision



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



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



(**E.**) Average task difficulty during internship

Notes: The figures display evidence from the internship program involving refugee workers (therefore, the sample is composed by firms for which the internship took place, N=179): 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. Panel E reports fraction of firms assigning complex tasks to the refugee worker matched for the internship. Difficulty is calculated using a scale from 1 to 5, where 1 means "Very Simple" and 5 "Very Complex". We use a dummy equal to 1 if the task is either "Complex" (scale equal to 4) or "Very Complex" (scale equal to 5). All questions were asked at Follow-up 1.

Table 6. Beliefs About Refugees' Skills

	Learning			
	(1)	(2)	(3)	(4)
	Hard skills	Soft skills	Behavior	Average stand. effect
β_1: T $\times$ 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]
β_2: T $\times$ 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]
β_3: T $\times$ 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
p($\beta_1 = \beta_2$)	0.043	0.447	0.057	0.072
p($\beta_1 = \beta_3$)	0.515	0.493	0.183	0.227
p($\beta_2 = \beta_3$)	0.150	0.930	0.791	0.641
Mean DV	-0.000	0.000	-0.000	

Notes: This table reports the coefficients estimated by equation 6.5. *Dependent variables*: Column 1 aggregates the results using the average standardized effect across the underlying components of all the indices. Indices computed following Anderson (2008), using the following underlying covariates: theoretical skills, practical skills and speed for the index on hard skills (col. 2); work ethics, time management and teamwork ability for the index on soft skills (col. 3); trust and respect for the index on behavior (col. 4). *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.

of both the employer and the worker, we use a doubly robust estimator to estimate equation 6.5.[31] The main outcome of interest is a dummy equal to 1 if the WTP to hire a generic refugee worker is non-negative. We measure this outcome at follow-up

[31]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

1 (that is, approximately one month after the internship). Table 7 reports the results. As predicted by our conceptual framework, 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.

TABLE 7. Doubly Robust Post-Causal Forest Estimator on Short-Term Demand

	WTP ≥ 0			
	Beta	SE	Lower CI (95%)	Upper CI (95%)
T × Positive match	.2	.087	.03	.37
T × Mixed	-.053	.065	-.179	.074
T × Negative match	-.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`

6.5. **The Persistent Effect of Matching with the Right Attitude.** The heterogeneous effect across attitude groups persists over time. Using endline data, we demonstrate that the effect of matching with positive attitudes consistently exceeds that of other groups during the 24 months following the experiment. While regressions face power limitations, point estimates suggest a strong effect of matching within a positive attitude group. Table 8 presents results from regressions estimating coefficients from equation 6.5, with refugee-specific outcomes matching those in Table 2. Column 1 shows that effects on the extensive margin concentrate among both positive and mixed groups, with the latter showing the largest predicted change over the mean. Columns 2 through 4, however, reveal that the predicted change over the mean on the intensive margin among positive matches is the largest. Column 4 specifically reports the exposure effect on the number of refugees still employed, suggesting that the effect among positive matches is nearly five times larger than the mean in the control group. This effect is substantially larger compared to the other two groups (though not statistically distinguishable from them).

respective asymptotic distributions. Simple OLS underestimate the heterogeneous effect of attitudes groups on the main outcome of interest, in this case.

Table 8. Hiring Refugee Workers by Attitudes Groups

	Refugee hires			
	(1)	(2)	(3)	(4)
	At least 1	Total	Only new	Still employed
β_1: T × Positive match	0.127	0.852***	0.696*	1.723**
	(0.081)	(0.311)	(0.360)	(0.728)
	[0.120]	[0.006]	[0.053]	[0.018]
β_2: T × Mixed	0.167**	0.787***	0.516*	0.626
	(0.065)	(0.265)	(0.289)	(0.628)
	[0.011]	[0.003]	[0.074]	[0.319]
β_3: T × Negative match	0.017	0.346	0.036	0.110
	(0.095)	(0.583)	(0.619)	(1.119)
	[0.862]	[0.553]	[0.954]	[0.921]
Firms	299	299	299	299
p($\beta_1 = \beta_2$)	0.666	0.852	0.653	0.175
p($\beta_1 = \beta_3$)	0.349	0.419	0.333	0.195
p($\beta_2 = \beta_3$)	0.156	0.456	0.450	0.650
Mean DV	0.200	0.269	0.269	0.037
Mean Positive	0.326	0.651	0.558	0.186
Mean Mixed	0.355	0.592	0.461	0.079
Mean Negative	0.200	0.400	0.300	0.050
Change in positive match (%)	63	134	101	460
Change in mixed match (%)	84	120	68	87
Change in negative match (%)	8	41	4	12
Regression model	OLS	Poisson	Poisson	Poisson

Notes: This table reports the coefficients estimated by equation 6.5 in the upper panel. *Dependent variables*: A dummy equal to one if firm hired at least one refugee worker (col. 1); total number of refugees (col. 2), total number of new refugees (col. 3) and total number of refugee workers still employed at endline (col. 4). *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. Lower panel reports number of observations (firms at endline), p-values of tests of equality of coefficients across groups, mean of dependent variable in control, raw means in each treatment arm and predicted changes over the mean across different groups: Positive, Mixed and Negative.

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

We use the follow-up 2 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 2 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.10 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 employ 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.

7. 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 for different reasons based on their interest (Karlan and Zinman 2009). This experiment could be viewed as a selective trial due to 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 these participants. 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 have 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 accessing 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 statistically significant 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. The results of our program were met with enthusiasm by the refugee community. After sharing of the results of our experiment, one of the refugee-led NGOs we worked with started a job placement program to assist the refugee community. The program consists in skilling refugee jobseekers in their job search behavior and in matching them with firms looking for new workers.[32]

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 43 refugees. Treated firms hired 118 refugees. That is, our program helped firms to hire 75 more refugees. The program's overall cost,

[32]https://www.yarid.net/job-training-placement-1

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.[33] Therefore, the total cost per job created was equal to 85.5USD and the total cost per firm participating to the experiment and for which we have information at endline (407) was equal to approximately 16USD. 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.

8. Conclusions

How to improve the labor market integration of marginalized workers such as migrants and refugees is an open question with huge policy implications. Their poor integration has long-term costs for 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 mechanisms explaining this result are that firms on average 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 and social interactions on the workplace.

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

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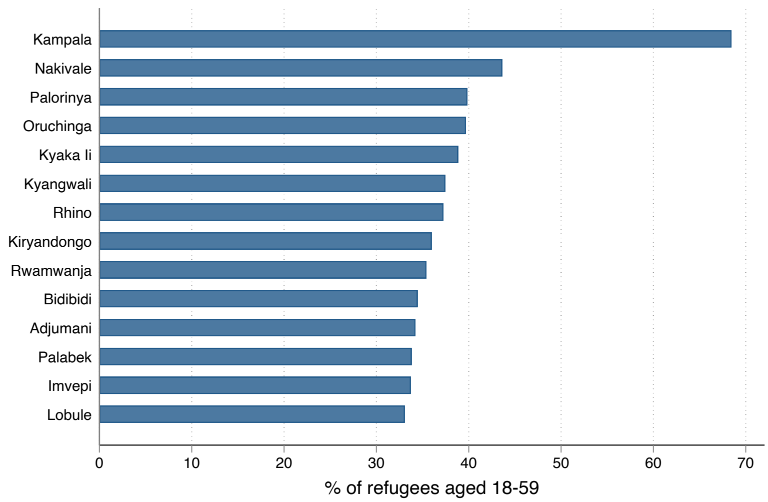
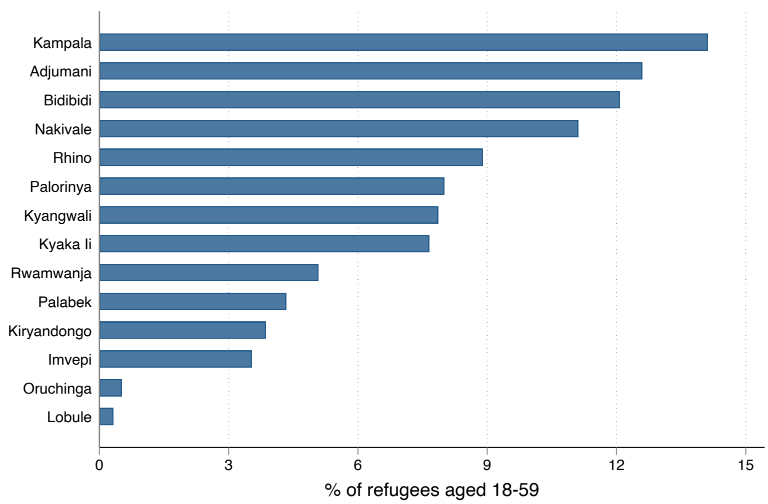
ONLINE APPENDIX

FIGURE A.1. Refugees in Uganda



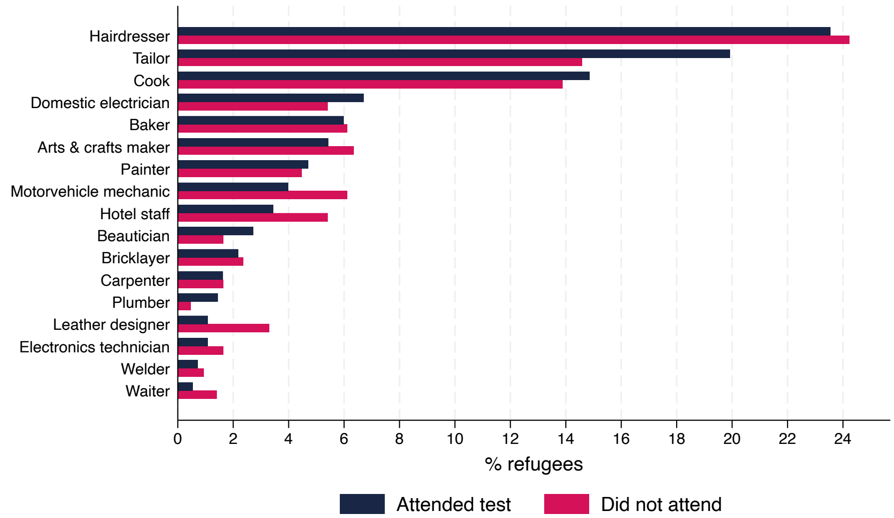
(**A.**) Working-age population of refugees for each settlement



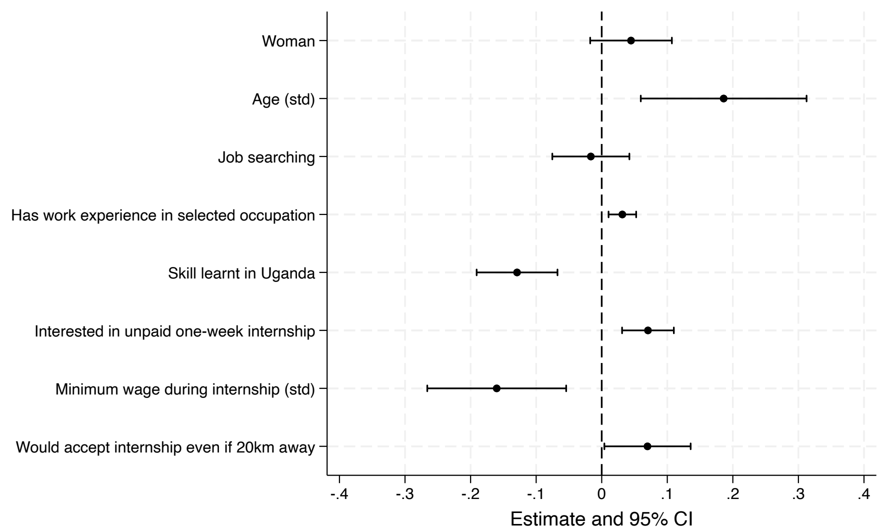
(**B.**) National working-age population of refugees by settlement

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 (see `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 test attendance



(**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). 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 95% 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):

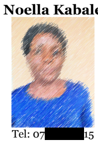
Reading:	*Speaking:*	*Writing:*	*Listening:*
3=Moderately well	2=Not well	2=Not well	3=Moderately well

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 local 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

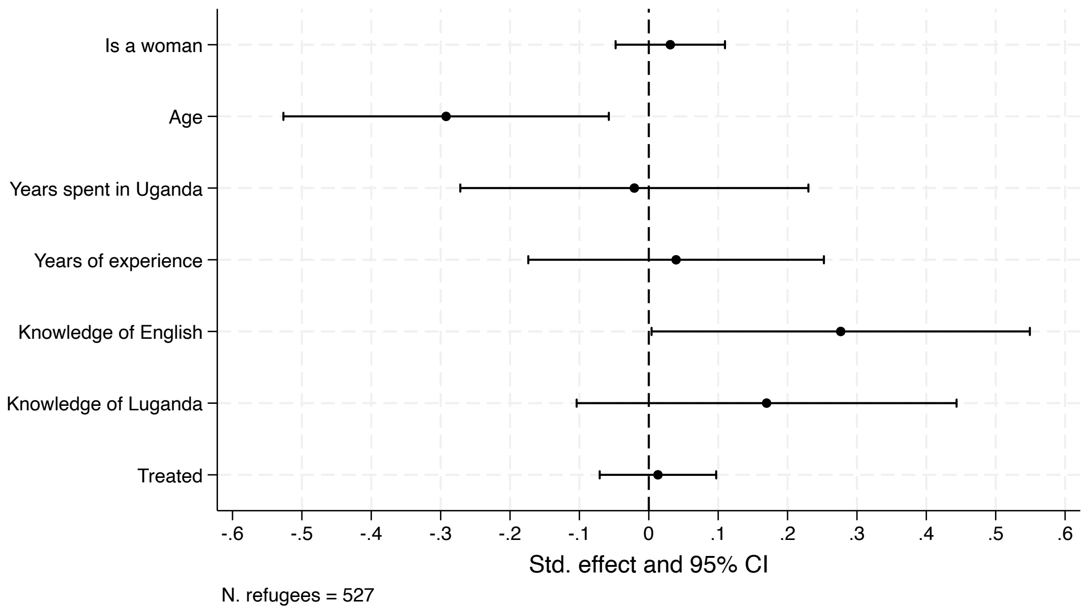
Knowledge of Luganda (self-reported scale 1-5)**:**

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

(**D.**) Hypothetical local female worker

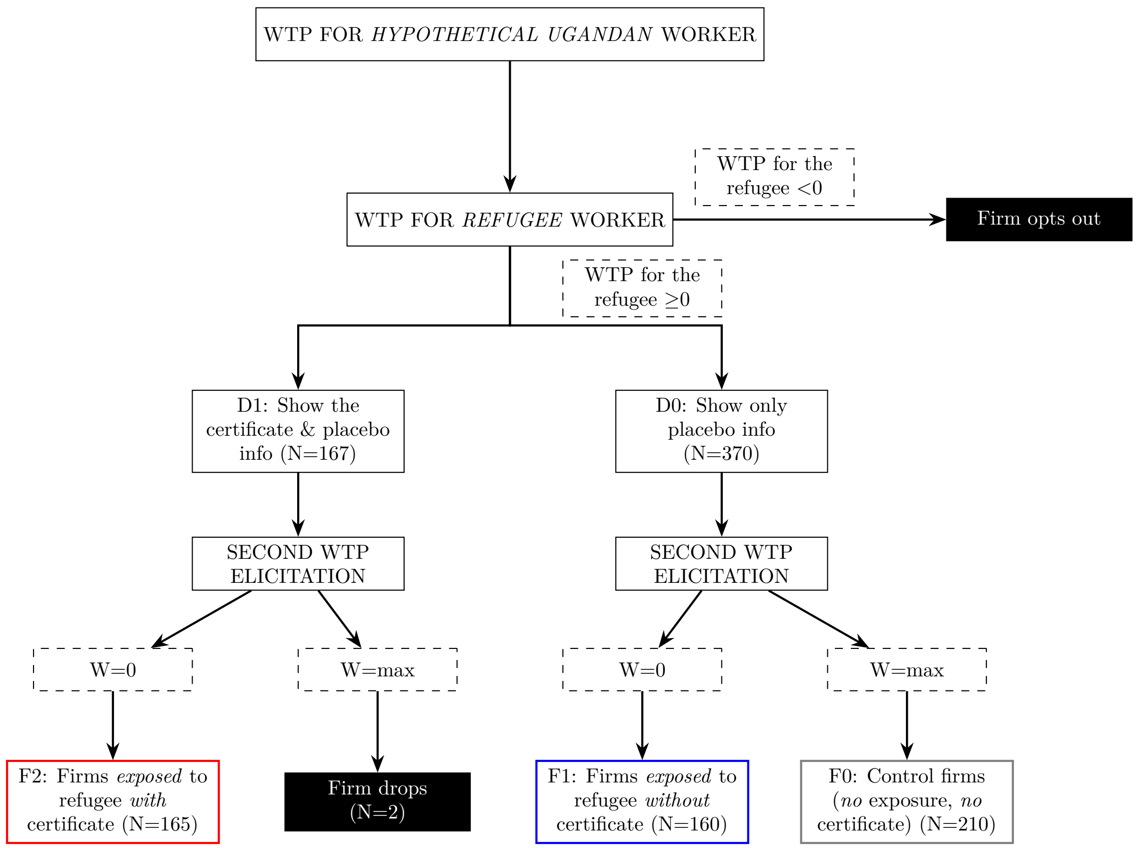
Notes: The figure plots examples of CVs for both real refugee workers and hypothetical local workers. The refugee workers' CVs are based on information provided by the respondents, while the hypothetical local workers' CVs are created to mirror the same structure. Care was taken in the selection of names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

Figure A.4. Refugees' Matching Success Rate



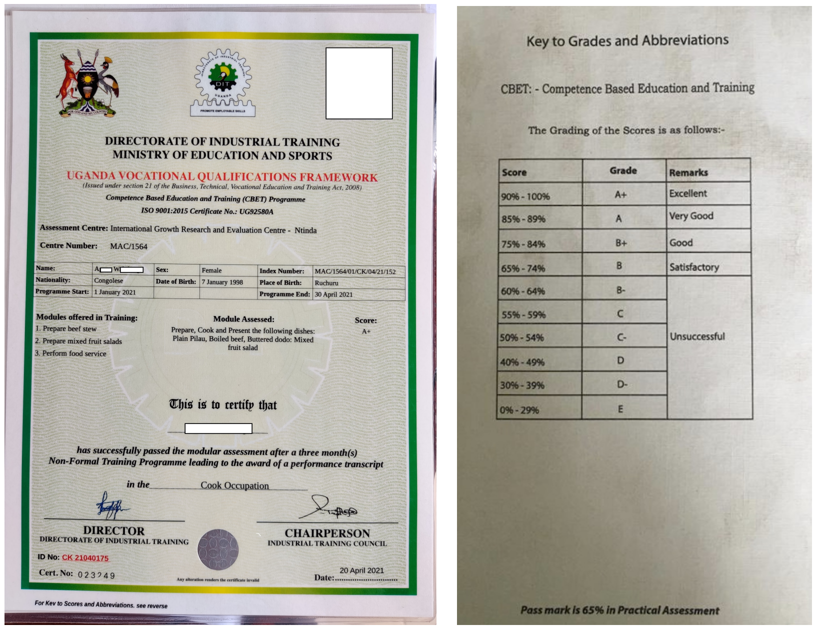
Notes: This graph correlates the characteristics of refugee workers with the average number of firms that are willing to hire them. The graph plots the coefficients from the following specification: $y_i = \beta_0 + \beta_1 \frac{1}{Nfirms} \sum_j^{Nfirms} \mathbb{1}(WTP_i \geq 0) + X_i^{'}\delta + \varepsilon_i$, 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. Example of Certificate



DIRECTORATE OF INDUSTRIAL TRAINING
MINISTRY OF EDUCATION AND SPORTS

UGANDA VOCATIONAL QUALIFICATIONS FRAMEWORK

(Issued under section 21 of the Business, Technical, Vocational Education and Training Act, 2008)

Competence Based Education and Training (CBET) Programme

ISO 9001:2015 Certificate No.: UG92580A

Assessment Centre: International Growth Research and Evaluation Centre - Ntinda

Centre Number: MAC/1564

Name:	A W	**Sex:**	Female	**Index Number:**	MAC/1564/01/CK/04/21/152
Nationality:	Congolese	**Date of Birth:**	7 January 1998	**Place of Birth:**	Ruchuru
Programme Start:	1 January 2021			**Programme End:**	30 April 2021

Modules offered in Training:
1. Prepare beef stew
2. Prepare mixed fruit salads
3. Perform food service

Module Assessed:
Prepare, Cook and Present the following dishes: Plain Pilau, Boiled beef, Buttered dodo: Mixed fruit salad

Score: A+

This is to certify that

has successfully passed the modular assessment after a three month(s) Non-Formal Training Programme leading to the award of a performance transcript

in the Cook Occupation

DIRECTOR
DIRECTORATE OF INDUSTRIAL TRAINING

CHAIRPERSON
INDUSTRIAL TRAINING COUNCIL

ID No: CK 21040175

Cert. No: 023[illegible]49

Any alteration renders the certificate invalid

Date: 20 April 2021

For Kev to Scores and Abbreviations. see reverse

Key to Grades and Abbreviations

CBET: - Competence Based Education and Training

The Grading of the Scores is as follows:-

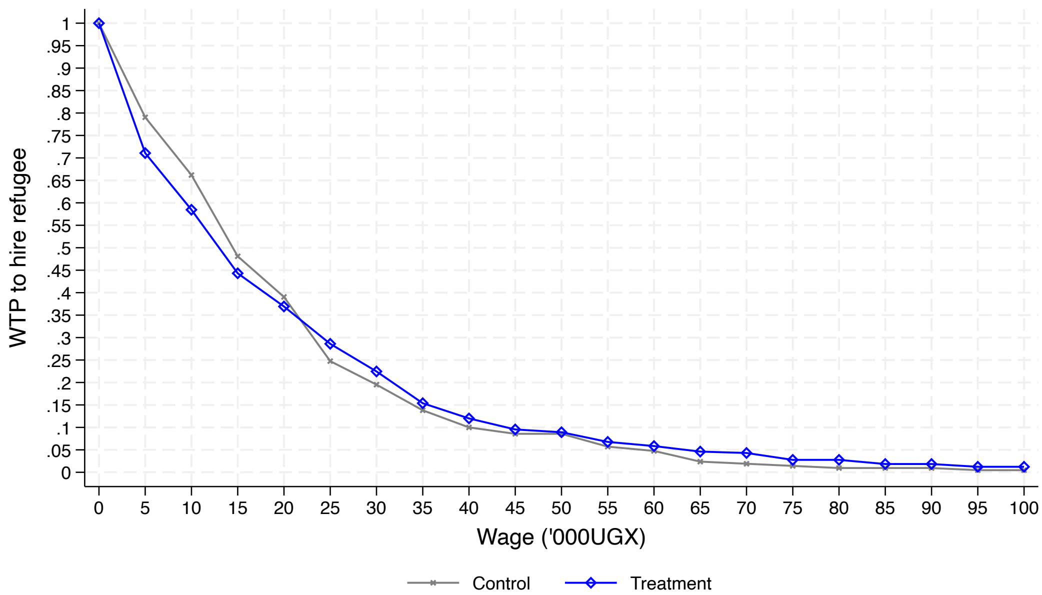
Score	Grade	Remarks
90% - 100%	A+	Excellent
85% - 89%	A	Very Good
75% - 84%	B+	Good
65% - 74%	B	Satisfactory
60% - 64%	B-	Unsuccessful
55% - 59%	C	
50% - 54%	C-	
40% - 49%	D	
30% - 39%	D-	
0% - 29%	E	

Pass mark is 65% in Practical Assessment

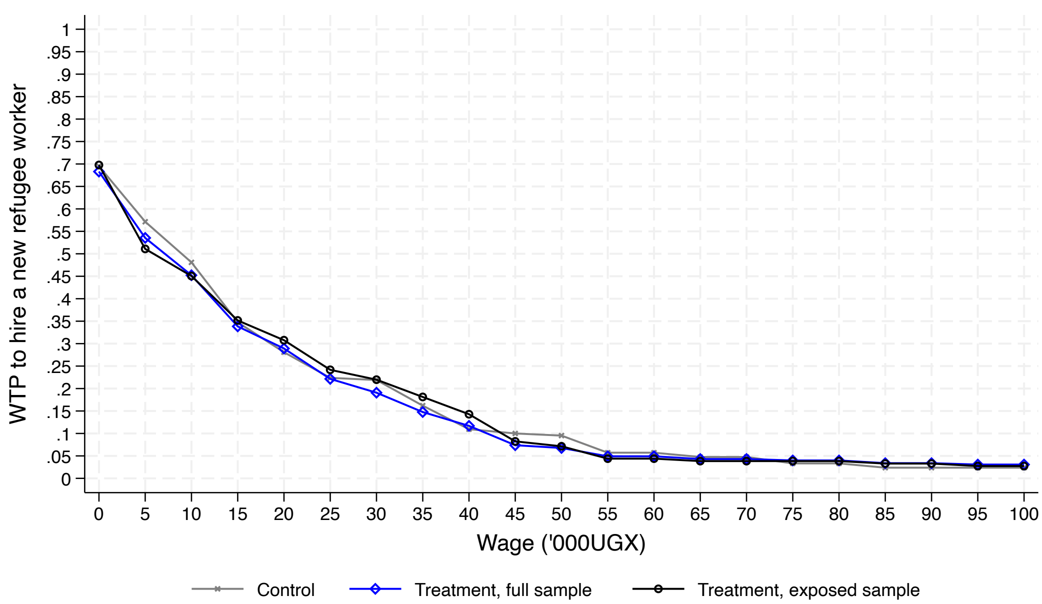
(**A.**) Front Page (**B.**) Back Page

Notes: This picture shows an example of certificate. Panel A (left) shows the front page containing demographic information about the candidate, including the score. Panel B (right) shows the back side and how to interpret the score.

FIGURE A.7. WTP Curves



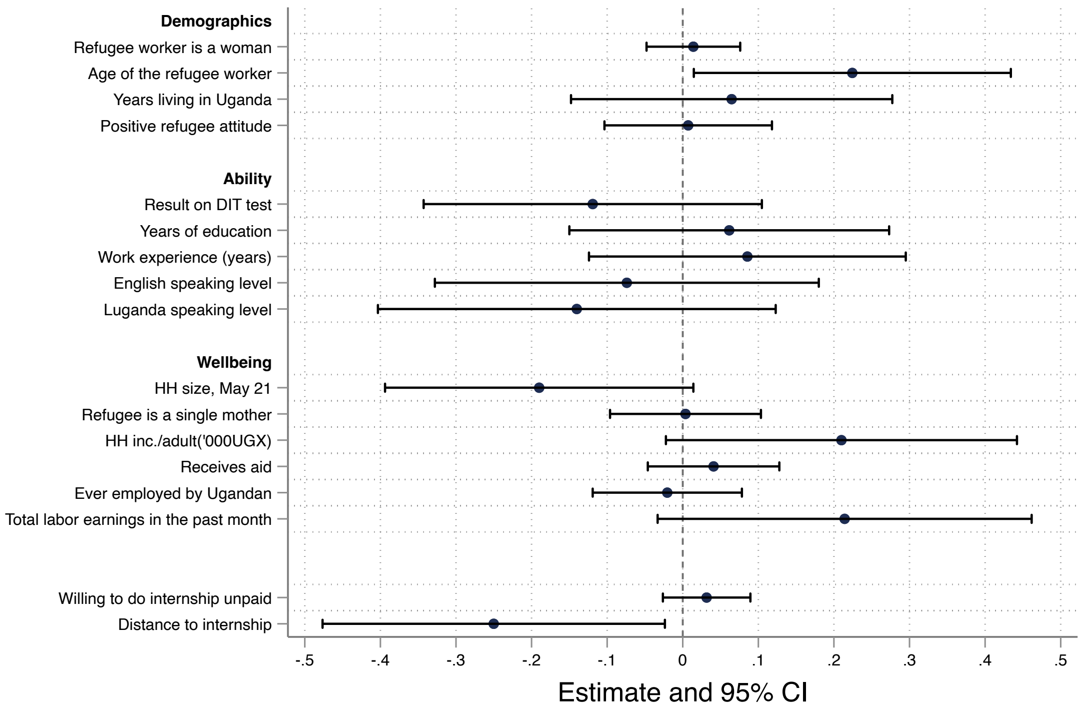
(**A.**) WTP Curves at Baseline



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

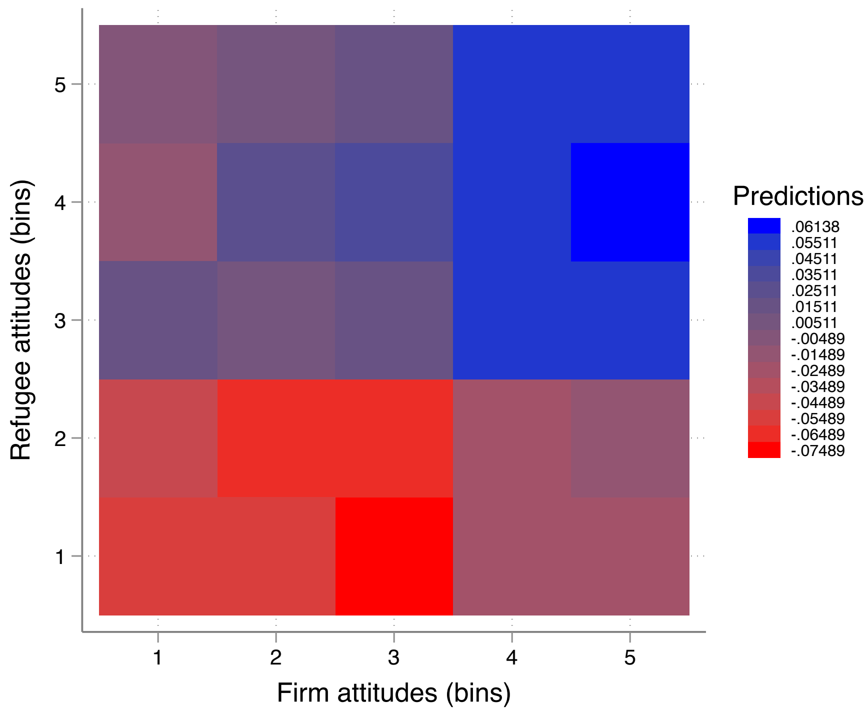
Notes: Panel A (top) plots the Cumulative Distribution Function (CDF) of the Willingness to Pay (WTP) to hire a refugee worker at baseline. The gray line represents the demand among control firms, while the black line represents the demand among firms assigned to treatment. Panel B (bottom) plots the 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.8. Refugees' Characteristics and Take-up of the Internships



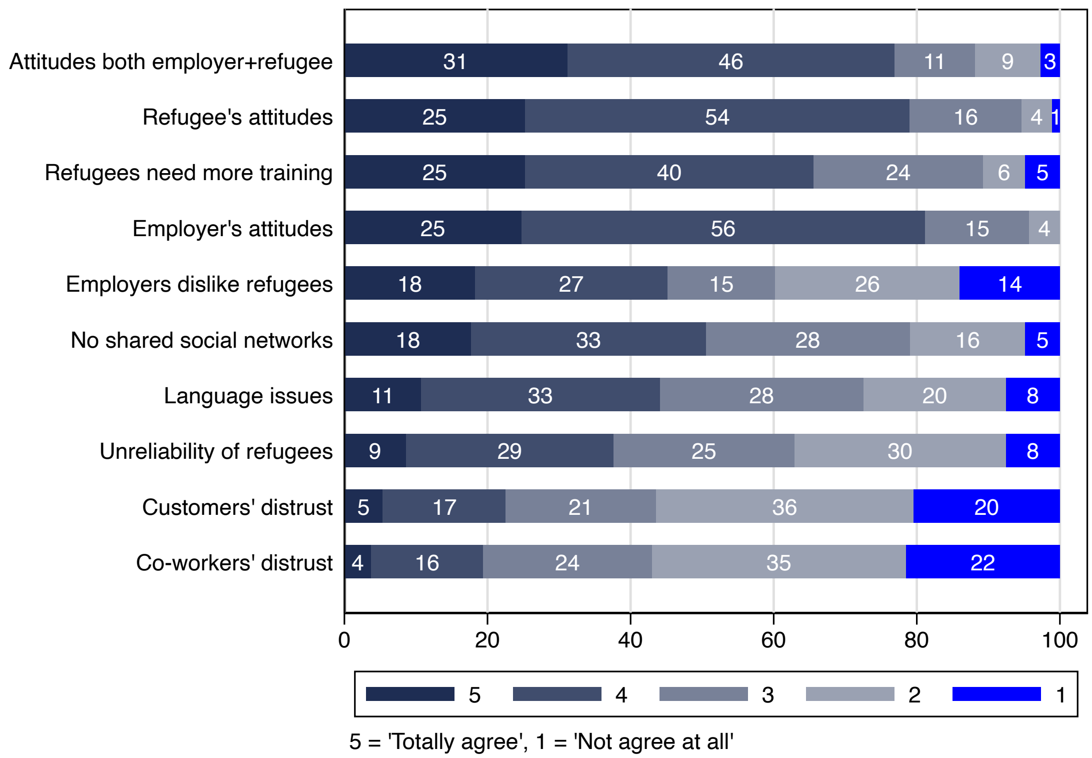
Notes: This graph investigates whether any observable characteristic correlates with the likelihood of matching, both at the refugee and firm level. Using the rich data collected at baseline from both samples, we run the following specification in the sample of refugees matched with treated firms: $y_j = \gamma_0 + \gamma_1 \mathbb{1}(Matched)_j + X_j^{'}\delta + \varepsilon_j$, 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.

FIGURE A.9. Predicted CATE



Notes: This graph 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.

FIGURE A.10. 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 bread suitable for diabetic people
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 sandals for men
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
Waiter	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. Refugees versus 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 Willing to Hire a Refugee Intern with Full Sample

	Willing			Full Sample			
Variable	N	Mean	SD	N	Mean	SD	Diff.
Employer is a woman	535	0.570	0.496	1,192	0.545	0.498	0.045
Firm age	535	7.815	6.644	1,180	7.990	6.915	-0.320
Firm is formal	535	0.185	0.389	1,192	0.157	0.364	0.051**
Has a vacancy	535	0.419	0.494	1,192	0.265	0.442	0.279***
Desires expand in the future	535	0.860	0.348	1,192	0.740	0.439	0.217***
Employees at baseline	535	2.492	3.147	1,191	2.675	3.346	-0.333*
Manufacturing sector	535	0.333	0.472	1,192	0.345	0.476	-0.022
Ever offered internships	535	0.609	0.488	1,180	0.559	0.497	0.092***
Ever hired a migrant or refugee	535	0.361	0.481	1,192	0.316	0.465	0.081***
Beliefs about refugees' test score	535	64.131	15.141	1,192	63.344	15.513	1.428
Supports refugees' empl. rights	535	0.923	0.266	1,190	0.905	0.293	0.033**
Jobs to locals first	535	3.355	1.268	1,190	3.359	1.317	-0.007
WTP for local worker, non-neg.	535	0.985	0.121	1,192	0.633	0.482	0.638***

Notes: This table produces balance checks of baseline characteristics comparing firms selecting into the experiment because their WTP is non-negative to the full sample of firms. 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: $y_i = \beta_0 + \beta_1 Willing_i + \varepsilon_i$, where outcome y_i is a baseline characteristic and $Willing_i$ is an indicator equal to 1 if the firm belongs to the group of firms with non-negative WTP for the refugee intern at baseline. 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.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	535	535	535	392	392	392

Notes: This table investigates whether attrition at follow-up surveys and endline are differential across treatments. It reports the coefficients for the following specification: $y_i = \beta_0 + \beta_1 Treated_i + \varepsilon_i$ where y_i is a dummy equal to 1 if the respondent is found at each survey-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 2 years after the end of the experiment • Total number of refugee workers hired in the 2 years after the end of the experiment • Total number of new refugees hired (excluding workers that did the internship and eventually got hired) • Total number of refugee workers hired in the past 2 years and still hired at the firm 2. **WTP for hypothetical worker:** • Dummy: WTP ≥ 0
Beliefs about skills	1. **Hard skills:** • Expected DIT test score for refugees vs. Ugandan job seekers (score 0 to 100) • Index of theoretical, practical skills, and work performance (Likert scale, scale 1 to 5) 2. **Soft skills:** • Index of time management, teamwork, and work ethics (Likert scale, scale 1 to 5) 3. **Behavioral skills:** • Index of respect and trust (Likert scale, scale 1 to 5)
Attitudes Towards Refugees' Integration	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. Business Outcomes and Productivity

	Profits			Profits per worker		
	(1)	(2)	(3)	(4)	(5)	(6)
	Follow-up 1	Follow-up 2	Endline	Follow-up 1	Follow-up 2	Endline
Panel A: ITT						
Treated	-0.191	0.082	-0.150	-0.024	-0.006	-0.040
	(0.142)	(0.089)	(0.127)	(0.031)	(0.022)	(0.030)
	[0.178]	[0.357]	[0.240]	[0.455]	[0.790]	[0.182]
Firms	456	420	362	456	420	362
Mean DV	0.748	0.514	0.841	0.222	0.149	0.241
Panel B: Effect of exposure						
Exposed	-0.179	0.032	-0.087	-0.024	-0.020	-0.033
	(0.143)	(0.094)	(0.148)	(0.030)	(0.018)	(0.036)
	[0.211]	[0.730]	[0.560]	[0.416]	[0.274]	[0.358]
Firms	336	310	270	336	310	270
Mean DV	0.748	0.514	0.841	0.222	0.149	0.241

Notes: This table reports the coefficients estimated by equation 5.1. *Dependent variables*: Columns 1 to 3: business profits in the 30 days prior the survey (follow-up 1, follow-up 2 and endline, respectively). Columns 4 to 5: a proxy for productivity of the firm, i.e. profits per worker. All outcomes are in thousands Ugandan Shillings. *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.8. Beliefs: Individual Components

	Hard skills				Soft skills			Behavioral skills	
	(1) Score	(2) Theory	(3) Practice	(4) Perform.	(5) Time	(6) Team	(7) Ethics	(8) Trust	(9) 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 performance (e.g. speed) for the index on hard skills, time management, teamwork 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. Firms' Characteristics Feeding Causal Forest

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 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 greater than median
Willingness to expand	Factor analysis of: • Vacancy at baseline • Expected workforce increase in next 5 years Dummy=1 if index 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 ever employed a migrant

TABLE A.10. Refugees' Characteristics Feeding Causal Forest

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 against 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.11. 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).

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 has 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, NOR CAN I 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?