WORKPLACE CONTACT AND THE HIRING OF REFUGEES: EVIDENCE FROM RANDOMIZED INTERNSHIPS

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Abstract

Can short-term workplace exposure overcome hiring barriers for underrepresented workers? We randomly assign firms to host refugee job-seekers for one-week internships in Uganda. Treated firms are twice as likely to employ refugees two years later, driven by hiring from the broader community, not just matched workers. Employers revise upward their beliefs about refugees' soft skills, non-contractible attributes difficult to signal through resumes. Effects emerge gradually and persist, suggesting workplace contact reduces perceived risk in employment relationships. Effects concentrate among matches where both employers and refugees enter with positive baseline attitudes, highlighting targeting's importance in program design. (*JEL*: C93, D83, J15, J70, M51, O15)

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1. Introduction

Employers hiring from underrepresented labor pools often lack reliable signals about workers' productivity. In such contexts, skills are imperfectly observed and often inferred from group-level beliefs rather than individual signals. Furthermore, when worker productivity depends on non-contractible traits such as effort, reliability, or trustworthiness, hiring involves a joint investment by firms and workers whose returns unfold over time (Bassi and Nansamba, 2022). Because these investments are costly and their returns depend on the behavior of both sides, new employment relationships require a minimum degree of trust and willingness to cooperate. Pessimistic beliefs about productivity or uncertainty about the other side's willingness to engage can therefore prevent mutually beneficial matches from forming, even when formal barriers to employment are absent (Phelps 1972). These frictions are likely to be most severe for workers whose employment histories are hard to interpret or whose attachment to the labour market is weak, raising the possibility that standard hiring mechanisms fail precisely where such relational investment is most valuable.

When individual signals are weak, even brief observation of a worker on the job generates information that no CV or referral can substitute and where employer reluctance reflects group-level bias rather than individual uncertainty, direct workplace contact may shift attitudes in ways that indirect information cannot. A central question is whether short-term workplace interaction can reduce these hiring frictions. While brief contact may reveal information about productivity, it may also shift employers' willingness to engage, shaping whether firms are willing to hire from underrepresented groups in the future.

We study this question in the context of refugee labour-market integration. 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. This setting is well-suited for studying workplace exposure as a hiring intervention: refugees often possess relevant skills but face severe information barriers. Their employment histories are hard for local employers to interpret and standard screening mechanisms frequently fail to identify viable matches.

This paper studies a randomized internship-matching program designed with the Government of Uganda and two large refugee-led NGOs to create direct workplace exposure between firms and refugee workers. Treated firms are matched with a refugee intern for a short-term placement, during which firms observe worker productivity in real work settings and refugees demonstrate their skills to potential employers. Refugees incur non-trivial participation costs, including commuting time and foregone outside options, while firms invest supervision time and training effort. The program generates informative signals about attributes that are difficult to observe through standard hiring channels, particularly non-contractible traits like reliability, work ethic, and ability to integrate into existing teams.

We build a sample of refugee jobseekers skilled in the manufacturing and services sectors and living in Kampala, the capital of Uganda. We select sectors typically associated with regular employment, including tailoring, food processing, hairdressing, and other light manufacturing and service sectors. While we do not observe the full applicant pool faced by firms, refugees in our sample operate in the same occupational segments as typical Ugandan applicants: small-scale manufacturing and service activities where firms routinely hire workers with limited formal credentials and learn about productivity on the job (Alfonsi et al., 2020).[1] To measure refugees' practical skills, we administered a standardized modular trade assessment developed by the Directorate of Industrial Training (DIT), the statutory body responsible for vocational training standards and certification in Uganda. The DIT modular system evaluates occupation-specific competencies aligned with the national Technical and Vocational Education and Training (TVET) curriculum. After completing the tests, we randomly paired each refugee worker with one Ugandan employer, stratifying by the occupation of the refugee, excluding very few workers who did not pass the exam.

Because participation requires active engagement, the refugees in our study master vocational skills and have prior work experience, yet face substantial barriers to labour-market integration, including limited language proficiency and weak current labour-market attachment. About 70 per cent of the refugees in our sample have work experience in at least one of these sectors, yet less than a quarter has ever had a Ugandan employer.

We find that short-term workplace contact leads to large and persistent changes in firms' labor-demand decisions. Firms randomly assigned to host a refugee intern are substantially more likely to engage again with refugee workers as interns in the short run and are more than twice as likely to employ refugees up to two years after the program. These effects unfold gradually rather than immediately, driven by firms hiring from the broader refugee community rather than simply retaining the matched intern. The delayed emergence and sustained effects suggest that exposure durably reduced perceived risk about refugee workers as a group, making firms willing to consider refugees when hiring opportunities arise, rather than simply revealing the productivity of a single match.

To explain these effects, we examine how workplace exposure shapes employers' beliefs. Following the internship, treated employers revise upward their assessments of refugees' soft skills, such as perceived trustworthiness, with little change in beliefs about technical productivity (hard skills). These belief updates are corroborated by behavior in an incentivized trust game, suggesting that reported attitudes reflect economically meaningful changes in perceived trustworthiness. Because many jobs rely on relational contracts that cannot be

[1]Only 4% of firms in our sample usually check academic or training qualifications and 88% usually test the skills of their candidates on the job.

fully specified ex ante, learning about non-cognitive traits is difficult to achieve through resumes or short informational interventions but requires observing workers in actual work settings.

Importantly, the effects of workplace contact vary substantially across match types. Belief updating and subsequent hiring are concentrated among matches in which both employers and refugee workers enter the interaction with relatively positive baseline attitudes toward cross-group cooperation. Firms with initially favorable attitudes that are matched with workers expressing positive views toward the host community update their beliefs substantially and are more likely to hire. In contrast, firms with more negative initial views that are matched to workers holding similarly negative views exhibit little belief updating and are no more likely (if anything, less likely) to engage in subsequent hiring.

This pattern is difficult to reconcile with a pure learning or screening model. We extend a Bayesian 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 2024). Belief updating and hiring are weak or absent not only when both parties hold negative attitudes, but also when only one party is willing to engage, suggesting that learning requires active cooperation from both sides.

We interpret our findings as evidence of cooperative matching, in which the returns to interaction depend on mutual willingness to invest effort and sustain the relationship. While the differences across match types are not always statistically distinguishable given sample size constraints, the consistent pattern across multiple outcomes suggests that contact-based interventions succeed not by correcting pessimistic beliefs universally, but by enabling cooperation among agents already willing to test a relationship.

Our findings have broad implications beyond refugee labour 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 underrepresented or marginalized groups, such as ethnic minorities or formerly incarcerated individuals.

1.1. **Related Literature.** This paper contributes to the literature on workplace interaction and firm performance (Hjort, 2014; Ghosh, 2022; Baggio and Cosgel, 2024; Afridi et al., 2024; Chakraborty et al., 2024). This work studies how interaction among workers affects effort, coordination, or productivity within existing employment relationships. In contrast, we focus on an earlier and less studied margin: firms' willingness to initiate and repeat employment relationships with workers from underrepresented groups. By studying employer–employee contact rather than worker–worker interaction, we highlight a channel through which workplace exposure can affect labor demand even in the absence of contemporaneous productivity effects.

More broadly, this paper relates to the literature studying intergroup contact as a tool for reducing prejudice and fostering integration between social groups (Paluck and Green, 2009; Broockman and Kalla, 2016; Scacco and Warren, 2018; Rao, 2019; Mousa, 2020; Lowe, 2021; Corno et al., 2022; Bursztyn et al., 2024). A central insight of this literature is that structured contact can durably affect attitudes and behavior. Most existing studies, however, conceptualize contact as an exogenous treatment applied uniformly across participants and focus primarily on attitudinal or short-run behavioral outcomes. Our contribution is to study when contact translates into sustained economic interaction. By examining workplace contact between firms and workers and tracking hiring decisions over a multi-year horizon, we show that the effects of contact depend critically on participants' willingness to engage, rather than on exposure alone.

A large literature emphasizes the role of learning and experience in shaping employers' beliefs and hiring decisions (Farber and Gibbons, 1996; Altonji and Pierret, 2001; Pallais, 2014; Benson and Lepage, 2024; Macchiavello et al., 2024). These studies show that employers update beliefs through interaction, often based on past experiences with minority workers. Our findings add an important qualification to this perspective: learning through experience is not automatic. In our setting, belief updating and subsequent hiring occur only when both firms and workers actively engage in the interaction. This suggests that models of employer learning should account for the endogenous nature of information acquisition when effort, task assignment, and cooperation are not contractible.

Finally, this paper contributes to the literature on refugee labor-market integration (Battisti et al., 2019; Arendt et al., 2021; Fasani et al., 2021, 2022; Caria et al., 2024; Abbiati et al., 2025). Much of this work focuses on improving refugees' employability or reducing hiring costs through training, certification, or subsidies, integration policies that are implicitly designed around a screening or information paradigm. While these interventions can be effective in some settings, they abstract from the fact that key determinants of employment—such as reliability, effort, and willingness to cooperate—are difficult to contract upon and costly to learn without direct interaction. We complement this literature by highlighting a distinct constraint: even when refugees possess relevant skills, firms may be unwilling to engage due to uncertainty about cooperation and relational fit. By showing that short-term workplace exposure can durably affect firms' labor demand, we identify a mechanism through which integration policies can operate beyond skill acquisition alone.

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. Section 5 reports the results of the experiment. Section 6 explores more in details the importance of initial attitudes Finally, Section 7 concludes the paper.

2. Institutional Setting

In this section, we explain why Uganda is a well-suited environment for our purposes. First, we describe the institutional environment of Uganda as a refugee host 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. Approximately 70 per cent of refugees residing in Kampala are of working age - 18-59. Overall, approximately 15 per cent of all refugees of working age in Uganda reside in Kampala.[5]

[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
[5]See Supplemental Appendix Figure S.1, Panel A and B, respectively.

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. We then describe our sample of firms.

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 benchmark refugees' skills, we invited 977 refugees to participate in a practical skills assessment, of whom 552 attended. Importantly, participation in the assessment does not aim to produce a representative sample of the overall refugee population. Rather, it identifies a subset of refugees who are actively searching for wage employment in Ugandan firms and are willing to engage in a formal recruitment process. This is precisely the margin at which employers form hiring beliefs and make employment decisions. Throughout the paper, therefore, the skills assessment should be interpreted as characterizing the distribution of skills among refugees who plausibly reach employers' hiring consideration set, rather than among refugees at large.

In partnership with the 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. Assessments are structured around discrete modules covering core tasks within a trade and are designed to capture applied, job-relevant skills rather than formal credentials or self-reported experience. Tests combine hands-on practical exercises and task-based evaluations, graded using standardized scoring rubrics by certified assessors. This framework allows for comparable measurement of technical proficiency across workers with heterogeneous training backgrounds, including individuals without formal vocational certificates. Appendix Table A.1 sets out the skills tested for each occupation. The skill is chosen by the examiners and

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

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 complete 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.

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.[7] 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 and are able to engage interns at work. Furthermore, the sample of firms who are not interested in hiring a refugee are explicitly more interested in hiring a Ugandan worker as opposed to a refugee one: while close to all the willing firms are interested in hiring a Ugandan as well as a refugee worker, about a third of the unwilling firms are discriminating against refugee workers. Appendix Table A.2 summarizes the characteristics of the 535 firms whose Willingness to Pay (WTP) is non-negative, compared to the the sample of firms unwilling to hire a refugee. Figure 1, Panel B, maps the location of the firms that belong to our baseline sample in the metropolitan area of Greater Kampala.

[7]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.

4. Experimental Design: Matching Firms to Refugee Workers

The experiment tests whether direct workplace exposure to refugee workers increases firms' demand for refugees. For this reason, the unit of randomization is the firm. This section is divided into four 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 describe the intervention, that is, the internships. Third, we describe the main outcomes of interest. Fourth, we discuss a simple theoretical framework to guide the interpretation of our results.

4.1. **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.

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).

We elicit each employer's WTP for the randomly paired refugee worker twice, varying the level of information shared with the employer.[8] 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.1, Panel A and B). Furthermore, we tell employers that they can hire the worker at any time in the four days following the interview.[9]

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

[9]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.1, Panel C and D). We carefully explain that the worker is hypothetical, inviting the employer to imagine that

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. 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 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.

Approximately 45 per cent of the 1,192 firms interviewed at baseline have a non-negative WTP to hire a refugee worker.[10]

We use the second elicitation to randomize the 535 firms with non-negative WTP into treatment and control groups.[11] 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.[12] 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

a worker like the one we are showing is seeking employment at the firm. 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.

[10]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.

[11]Our power calculations are based on the original design of the experiment (see Appendix Figure S.3). About half the treated employers were shown the certificate that the refugee worker obtained on successfully passing the practical skills examination. Appendix Figure S.4 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.

[12]An extensive pilot suggested that the $100,000$UGX 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 $100,000$UGX weekly for their employees as soon as they joined the firm. In the realized data, no control firm received an internship: the one firm with WTP $= 100,000$ was matched to an unavailable refugee.

took place in the control group. 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 $50,000$UGX (that is, about 41 PPP-adjusted USD at 2021 levels).

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.[13] Importantly, there is no difference in any ability measure between refugees who attend at the test versus those who do not. Refugees who do not show up score similarly on measures of grit and self-reported measures of the big 5 psychological traits, suggesting that they possess similar levels of soft skills to those who take up the internship. Appendix Figure A.2 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.

Because around half of assigned refugees do not show up to the internship for reasons largely unrelated to firms' decisions, treatment assignment generates quasi-random variation in actual exposure to a refugee intern. Our main results are reported using intent-to-treat (ITT) estimates that compare outcomes across treatment assignment. Given incomplete take-up, we also report instrumental-variable (IV) estimates that use assignment to treatment as an instrument for realized workplace exposure. These local average treatment effects (LATE) should be interpreted as the effect of exposure among compliers—firms that host a refugee intern when assigned. Under standard IV assumptions (random assignment, exclusion restriction, monotonicity), LATE represents the policy-relevant parameter for scaling the intervention, as it estimates the effect among firms that would comply with the program.

[13]See Appendix Table A.3.

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.4, 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. Appendix Table A.4 assesses attrition at the second follow-up in columns 2 and 5. Finally, for the endline (two years after the experiment) we successfully track 407 firms. We assess balance of attrition at endline in columns 3 and 6. Appendix Table A.5 reports results from a balance test of characteristics between treated and control firms in the baseline sample (Panel A). Panel B shows that baseline characteristics remain balanced among firms interviewed at endline.

4.2. **The internships.** 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).[14] 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.[15] Seven workers are hired after the internship (or 3.9 per cent of the total number of interns). The majority of employers (70 per cent), finally, recommend or would recommend the worker to another firm.

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

[14]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.

[15]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.

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

4.3. **Outcomes.** In this subsection we introduce our outcomes of interest. The goal of the experiment is to study whether exposure to one refugee generates persistent labour demand shifts and the employers update their prior beliefs about refugee workers' productivity.

Our main outcomes of interest measure firms' demand for refugee workers at different time horizons. We focus on two key margins: whether firms offered new internships to refugees at follow-up 2, which captures willingness to re-engage shortly after the initial exposure; and the number of newly hired refugees still employed at endline, which measures sustained employment relationships.

Furthermore, we measure employers' beliefs using self-reported ratings between 1 and 5 to different statements regarding skills of refugee jobseekers: the employer's beliefs about the soft skills (e.g., time management, teamwork, work ethics, trustworthiness and respectfulness) and the hard (e.g., theoretical abilities, practical skills, and performance at work) of a generic refugee worker who may seek employment in the future.[16]

We also measure employers' attitudes toward refugee integration through several complementary outcomes. First, we elicit agreement with the statement "When jobs are scarce, Ugandans should have more right to a job than refugees" (on a 1-5 scale), which captures in-group preferences and support for equal employment rights. Second, we measure willingness to support refugee integration financially by asking employers to donate part of their survey compensation to a refugee-led NGO that provides skills training and employment assistance. This donation measure is collected at both follow-up 1 and follow-up 2, allowing us to assess whether attitudinal changes persist over time. Finally, at endline we construct a binary indicator for active network-building with refugee communities, equal to one if the employer either personally knows someone at a refugee-led organization who can help find refugee workers, or voluntarily registered via SMS during the survey to receive ongoing information about refugee jobseekers from local NGOs. This last measure captures employers' proactive efforts to maintain connections that facilitate future refugee hiring.

4.4. **Conceptual Framework.** To interpret our results, we develop a simple conceptual framework based on Bayesian learning about worker productivity. Figure 3 provides evidence of substantial uncertainty at baseline: employers expect the typical refugee job-seeker to score below the passing threshold of 65 on the standardized skills assessment, when in fact the

[16]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.

average refugee in our sample scores 84. This 20-point gap suggests that employers hold systematically pessimistic beliefs about refugee workers' abilities.[17]

We interpret our intervention through the lens of Bayesian learning. The one-week internship generates an informative signal about the matched worker's ability, leading employers to update their beliefs. If priors are sufficiently pessimistic relative to the true productivity distribution, exposure should lead to:

(1) **Increased hiring**: Treated employers should be more willing to hire refugees subsequently (tested in Section 5.1).
(2) **Positive belief updating**: Treated employers should revise their assessments of refugee skills upward (tested in Section 5.4).

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. $Treated_i$ 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). 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.

[17] An important caveat: the test scores in Figure 3 come from refugees who voluntarily attended skills assessments (552 of 977 invited, or 56%). These attendees are positively selected on work experience, motivation, and self-reported skill levels (Appendix Figure A.3). The 20-point gap between employer beliefs (mean=64, below the passing threshold of 65) and actual scores (mean=84) therefore reflects two factors: (1) employers' genuine uncertainty about refugees who actively seek employment, and (2) positive selection among test-takers relative to the broader refugee population. While this complicates interpretation of the absolute magnitude of belief bias, three considerations suggest the comparison remains informative for our purposes. First, the refugees who attended skills assessments are precisely the workers relevant for employment matching: those actively seeking wage employment with Ugandan firms and willing to undergo formal evaluation. Employers form hiring beliefs about this margin of job-seekers, not about refugees in general. Second, even if employer beliefs (64) accurately reflected the average skill level of *all* refugees seeking employment (including non-test-takers), the gap with test-takers (84) still indicates that employers systematically underestimate the skills of refugees who demonstrate high motivation, precisely the workers firms would encounter through our internship program. Third, Figure 3 establishes that employers hold uncertain and pessimistic priors even along observable dimensions of productivity (technical skills), but the degree of uncertainty is likely even greater for non-contractible attributes that cannot be assessed through standardized testing, the very attributes we show the internship affects most.

However, using post-double-lasso selection models including pre-registered covariates (or post-randomization slightly unbalanced ones) do not change any of the results.[18]

We report two sets of results. Panel A presents the ITT estimates comparing firms assigned to treatment with control firms. Panel B presents LATE estimates, instrumenting actual exposure to a refugee intern with treatment assignment.[19]

5.1. **Exposure to Refugees Increases Firms' Hiring of New Refugees.** We begin by examining whether the one-week internship affects firms' subsequent hiring behavior. Table 2 reports treatment effects on two key outcomes: firms' willingness to re-engage with refugee workers at follow-up 2 (eight months post-intervention), and the number of new refugees in sustained employment at endline (24 months post-intervention). Importantly, both outcomes explicitly exclude the worker matched during the experimental internship, allowing us to test whether exposure affects firms' demand for refugee workers *generally*, rather than merely facilitating a single match.

5.1.1. *Medium-term effects (8 months).* Columns 1 and 2 examine whether treated firms offer new internships to refugees at follow-up 2. These internships represent firms' willingness to re-engage with refugee workers following the initial experimental exposure. The ITT estimate indicates that treatment increases the probability of offering at least one new refugee internship by 4.5 percentage points ($p = 0.055$), relative to a control mean of 4.3% (column 1). On the intensive margin, treated firms offer 0.065 more refugee internships on average ($p = 0.039$), relative to a control mean of 0.048 (column 2). The LATE estimates in Panel B are roughly twice the magnitude of the ITT effects, consistent with approximately 50% compliance. Firms that actually hosted an intern are 8.2 percentage points more likely to offer new refugee internships ($p = 0.050$) and offer 0.12 more internships on average ($p = 0.035$).

[18]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 S.3. 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).

[19]A potential concern is that firms assigned a refugee who failed to show up might update their beliefs about refugees' reliability, violating the exclusion restriction. To assess this, we compare outcomes for firms assigned a refugee who did not attend (promised but no intern) to control firms. We find no statistically or economically significant differences between these groups across all outcomes, supporting the validity of using assignment as an instrument for exposure. See Appendix Table A.6.

5.1.2. *Long-term effects (24 months).* Columns 5 and 6 examine the number of new refugee workers still employed at endline, approximately two years after the intervention. This outcome captures sustained employment relationships rather than short-term placements. The ITT estimate shows that treatment increases the probability of employing at least one new refugee by 3.4 percentage points ($p = 0.061$), relative to a control mean of 3.7% (column 5). Treated firms employ 0.059 more new refugees on average ($p = 0.026$), relative to a control mean of 0.037 (column 6). The LATE estimates again approximately double the ITT effects: firms that hosted an intern are 6.1 percentage points more likely to employ a new refugee ($p = 0.054$) and employ 0.104 more new refugees on average ($p = 0.023$).

Several features of these results merit emphasis. First, the temporal pattern suggests that the internship produces a durable shift in firms' willingness to engage with refugee workers, rather than a transient response. Second, the experimental internship generated spillover effects beyond the specific worker-firm match, increasing firms' overall demand for refugee labor. Third, the effects appear on both the extensive margin (whether firms hire any refugees) and the intensive margin (how many refugees they hire), suggesting the intervention affects both the decision to hire refugees and the scale of refugee employment.

5.1.3. *No statistically detectable displacement of Ugandan workers.* Columns 3-4 and 7-8 examine whether increased refugee hiring comes at the expense of Ugandan workers. We do not find statistically detectable evidence of displacement. Treatment does not significantly affect either the probability of hiring Ugandan workers or the total number of Ugandans hired, at either follow-up 2 or endline. The point estimates are small and statistically indistinguishable from zero (all p-values > 0.28).

5.1.4. *No change in firm size.* Column 9 examines total employment at endline. The treatment effect is essentially zero ($\beta = 0.001, p = 0.997$), indicating that the intervention did not expand firms' overall workforce. Taken together with the null effects on Ugandan hiring, this suggests that treated firms may have adjusted their hiring composition, employing more refugees and potentially fewer workers from other sources, without changing their total labor demand. We return to this point in Section 5.2, where we examine whether refugee hires perform different tasks or receive different compensation than other workers.

In summary, exposure to a refugee worker through a short-term internship leads to substantial and persistent increases in firms' subsequent hiring of *new* refugee workers. These effects cannot be attributed to retention of the matched worker, indicating that the intervention shifted firms' beliefs or preferences about refugee workers as a group. The next subsection investigates the mechanisms underlying this demand shift.

5.2. **Treated Firms Assign Refugees to More Complex Tasks.** The hiring effects documented in Section 5.1 raise a natural question: are the jobs created for refugees *real* and productive positions, or do they represent short-term, low-skill placements? To address this question, we examine the types of tasks assigned to refugee workers hired by firms post-intervention, comparing them with tasks assigned to Ugandan workers hired over the same period.

We focus on two outcomes measured at endline for all workers hired post-intervention: (i) the maximum complexity of any task assigned, and (ii) the total daily compensation. Task complexity is measured using employers' baseline assessments of each firm-specific task on a five-point scale, where 1 indicates "Very Simple" and 5 indicates "Very Complex". This measure was elicited before treatment assignment and is therefore not mechanically affected by the intervention.

Our analysis compares task assignment across four groups of newly hired workers: (1) refugees hired by treated firms, (2) refugees hired by control firms, (3) Ugandans hired by treated firms, and (4) Ugandans hired by control firms. Critically, *both treated and control firms hired some refugees post-intervention*: treated firms simply hired more, as shown in Table 2. This allows us to examine whether treatment affects not only *how many* refugees firms hire, but also *how* they employ them.

Figure 4 presents the main findings. Panel A shows the maximum task complexity assigned to refugee and Ugandan hires, separately for treated and control firms. Three patterns emerge. First, treated firms assign refugee workers to tasks of similar complexity as Ugandan workers (mean difference is less than 0.10, $p = 0.713$). This suggests that, conditional on hiring, treated firms view refugees as substitutes for Ugandans in terms of job responsibilities. Second, control firms assign refugees to significantly less complex tasks than Ugandans (mean difference $= 0.30$, $p = 0.070$), indicating that control firms—when they do hire refugees—tend to place them in more menial positions. Third, comparing across treatment status, treated firms assign refugees to more complex tasks than control firms do (mean difference $= 0.4$, $p = 0.038$).[20]

[20]One potential concern is that our task complexity measure may not be comparable across occupations—a 3-rated task in tailoring may represent different skill requirements than a 3-rated task in hairdressing. To address this concern, we verify that our results are robust to normalizing task complexity within occupation categories prior to aggregation. We repeat the analysis using normalised percentile ranking and we find that treated firms assign refugees to tasks at the 41st percentile of the within-occupation complexity distribution, compared to the 30th percentile for control firms ($p = 0.07$). This confirms that the treatment effect on task assignment is not driven by differences in the occupational composition of refugee hires across treatment arms. A second concern is that refugees who ultimately participated in internships at treated firms may have been positively selected on unobservable characteristics such as motivation or ability, and that this selection, rather than treatment, drives the task assignment patterns. Three pieces of evidence suggest this concern is unlikely to drive our results. First, as documented in Appendix Figure A.2, refugees' decision to attend the introductory meeting for the internship is primarily predicted by distance to the firm, with no significant relationship to measures of ability (test scores, years of experience), cognitive skills (Raven's matrices), or non-cognitive skills (grit, Big Five traits). Second, Appendix Table A.5 shows that treatment and control firms are balanced on baseline characteristics, including prior experience hiring refugees. Third, the task assignment gap emerges when comparing refugees hired by treated versus control firms *after* the

Panel B examines total daily compensation, calculated as total payment divided by days worked. We find no significant difference in the wages paid to refugee workers by treated versus control firms (mean difference = $151 UGX$, $p = 0.89$). However, there is a substantial, though not statistically significant, gap between wages paid to refugees versus Ugandans within both treatment arms (approximately 20 percent). This pattern is consistent with refugees accepting lower wages than observationally similar Ugandans, potentially due to weaker outside options or lower bargaining power. While this may suggest an improvement in firms' productivity (through lower labour costs), 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.

Taken together, these findings indicate that exposure to a refugee worker leads firms to integrate subsequent refugee hires into more productive roles, assigning them tasks comparable in complexity to those performed by Ugandan workers. This pattern is inconsistent with a purely charitable response or temporary goodwill (Macchi and Stalder, 2023), and instead suggests that treated firms update their beliefs about the productive capacity of refugee workers more broadly.

5.3. **Firms Become More Supportive of Refugees' Integration.** The positive effects of the experiment on hiring extend beyond employment outcomes to employers' expressed support for refugee integration. Table 3 presents evidence that workplace exposure leads employers to become more supportive of integrating refugee jobseekers into the labor market, measured through their attitudes, willingness to invest their own resources, and active network-building behaviors. We begin by examining employers' stated attitudes about labor market prioritization. Column 1 shows that at follow-up 1 (approximately one month after the internships), treated employers are significantly less likely to agree that "when jobs are scarce, Ugandans should have more right to a job than refugees" (Panel A: -0.175 SD, p=0.052; Panel B: -0.313 SD, $p = 0.048$). This represents a meaningful shift away from in-group favoritism and toward support for equal employment opportunities, suggesting that the internship experience durably changed employers' normative views about refugee labor market rights. Beyond stated attitudes, we examine whether employers are willing to back their views with concrete actions. Columns 2 and 3 investigate the experiment's effect on firms' willingness to donate their own money to a nonprofit organization that assists refugees in Uganda by providing skills training and employment support. These donation decisions were elicited during the follow-up surveys, after the internship period had concluded. At both follow-up 1 (column 2, approximately one month post-intervention) and follow-up 2

intervention, not during the internship itself. If selection were driving results, we would expect treated firms to assign complex tasks to the matched intern but not necessarily to *other* refugees hired later, yet we observe precisely the opposite pattern.

(column 3, approximately eight months post-intervention), treated employers donate significantly more compared to control employers. The effects are substantial and concentrated among firms where the internship actually took place (Panel B): exposed firms donate 0.306 SD more at follow-up 1 and 0.388 SD more at follow-up 2. The persistence and growth of donation amounts over time suggests that these are not merely transient responses to the experimental intervention, but reflect genuine shifts in employers' willingness to support refugee integration with their own resources. Most strikingly, column 4 shows that by endline (approximately 24 months after the intervention), treated employers actively engaged in building connections to facilitate future refugee hiring. We measure this using a binary indicator equal to one if the employer either (a) personally knows someone at a refugee-led organization who can help them find refugee workers, or (b) registered via SMS to receive information about refugee jobseekers from a refugee-led NGO operating in Kampala. The registration opportunity was offered during the endline survey, where employers could voluntarily send an SMS to receive ongoing information about skilled refugee workers. Treated employers are significantly more likely to have such connections at endline (ITT: 7.1 percentage points, $p = 0.058$; LATE: 12.5 percentage points, $p = 0.050$). We interpret this finding as evidence of employers actively seeking to expand their access to refugee labor pools. This network-building behavior is particularly important because it suggests potential spillover and amplification effects. Treated firms may serve as bridges between refugee communities and the broader employer network in Kampala, potentially accelerating the diffusion of positive attitudes toward refugee hiring. If treated employers are opinion leaders within their local business communities, as suggested by their willingness to participate in the experimental program, their adoption of pro-refugee hiring practices and their visible connections to refugee-supporting organizations could shift local norms and reduce perceived risks for other employers considering refugee hires. Taken together, these findings demonstrate that exposure through internships not only changes employers' hiring behavior (as shown in Table 2) but also durably shifts their broader attitudes and behaviors toward refugee integration.

5.4. **Exposure Improves Beliefs About Non-Contractible Attributes.** To interpret the sustained hiring effects documented in Table 2, we examine how workplace exposure shapes employers' beliefs about refugee workers. While standard hiring mechanisms rely on observable signals like education or prior experience, many job-relevant attributes, particularly trustworthiness, reliability, and work ethic, are difficult to contract upon and costly to learn without direct interaction. These non-contractible attributes are especially important in small firm settings where employment relationships often involve implicit agreements and where monitoring is limited. Our conceptual framework posits that employers hold uncertain beliefs about refugees' match-relevant productivity along these dimensions, and that short-term workplace contact generates informative signals that lead employers to update their beliefs.

Table 4 presents our main results on belief updating. The table focuses on beliefs about refugees' soft skills and behavioral attributes—the dimensions most relevant for sustaining employment relationships but least observable prior to workplace interaction. The results reveal a clear pattern: exposure leads to significant upward revision of beliefs about refugees' soft skills and perceived trustworthiness, but has no detectable effect on beliefs about technical abilities (hard skills).[21] Column 1 shows that the soft skills index increases by 0.132 standard deviations in the ITT specification ($p = 0.096$) and by 0.236 standard deviations among exposed firms ($p = 0.002$). Looking at individual components, the effects are strongest for trust-related attributes. Columns 2-5 show positive but noisier effects on specific work-related attributes (time management, teamwork, work ethic, respect), with the LATE estimates suggesting economically meaningful shifts ($0.201 - 0.326$ SD) even where statistical significance is limited. Column 6 shows that treated employers rate refugee workers as significantly more trustworthy (ITT: 0.171 SD, $p = 0.083$; LATE: 0.306 SD, $p = 0.074$). Column 7 provides behavioral validation of the reported belief shifts using an incentivized trust game conducted at follow-up 2. Employers were given an endowment and asked how much they would send to an anonymous refugee partner, with amounts tripled before the partner could return some portion. This classic trust game measures employers' willingness to make themselves vulnerable to refugees' reciprocity, providing a revealed-preference measure of perceived trustworthiness that is less subject to social desirability bias than survey responses. Treated employers send 4.5 percentage points more of their endowment to refugee partners ($p = 0.088$), consistent with genuinely increased trust rather than mere experimenter demand effects.

In contrast, Table 5 shows that employers' beliefs about refugees' hard skills, such as theoretical knowledge, practical abilities, and performance speed, do not significantly change following exposure. The hard skills index point estimate is near zero ($0.01 - 0.02$ SD) with tight confidence intervals, and beliefs about refugees' expected test scores actually decline

[21] The absence of significant updating on hard skills beliefs, despite the substantial gap between employers' baseline expectations and refugees' actual test scores documented in Figure 3, warrants explanation. Several factors likely contribute to this pattern. First, the one-week internship may provide a relatively weak signal about technical competence compared to the DIT standardized assessment. Employers observe workers performing firm-specific tasks under supervision, which may not reveal the full range of technical skills measured by the formal test. The Bayesian weight placed on this signal is therefore low relative to the weight on soft skills, which are directly observable through day-to-day workplace interaction. Second, and most importantly, our results suggest that the binding constraint to refugee hiring is uncertainty about non-contractible attributes (trustworthiness, reliability) rather than uncertainty about technical skills. Even if employers somewhat underestimate refugees' hard skills, this misperception appears less consequential for hiring decisions than uncertainty about whether refugees will be reliable, trustworthy employees. The internship primarily resolves the latter uncertainty, explaining why soft skills updating drives subsequent hiring effects.

slightly (though not significantly). This null result on hard skills is informative: it suggests that the hiring effects documented in Table 2 are not driven by firms learning that refugees possess unexpectedly strong technical capabilities, but rather by firms gaining confidence in refugees' reliability and commitment to workplace relationships.

This interpretation helps reconcile several patterns in our data. First, it explains why hiring effects take time to materialize: while beliefs shift quickly (Table 4 measures one month post-intervention), firms may need time to create vacancies and integrate refugees into existing teams, especially given that the intervention did not increase firms' capacity to hire (Appendix Table A.7 shows no effects on firm size or profits). Second, it explains why attitudes (Table 3) persist and even strengthen over time: belief updating about trustworthiness creates a durable willingness to engage with refugee workers when opportunities arise, even if immediate hiring is not feasible. Third, it clarifies the mechanism linking exposure to sustained hiring: by reducing uncertainty about refugees' reliability and commitment to workplace relationships, internships lower the perceived risk of forming employment matches with this group, leading to increased hiring over a multi-year horizon.

Taken together, the evidence in Tables 3 and 4 suggests that workplace exposure operates through multiple complementary channels. First, it provides information about refugees' non-contractible attributes, reducing uncertainty and perceived risk. Second, it shifts employers' attitudes toward refugee integration more broadly, increasing willingness to support and engage with the refugee community. The sustained hiring effects we observe likely reflect both mechanisms operating in tandem: belief updating about trustworthiness makes employers more confident that refugee matches can succeed, while improved attitudes make employers more willing to invest effort in identifying and hiring refugee workers. The heterogeneity analysis in Section 6 provides further support for this interpretation, showing that effects are strongest when both employers and refugees enter the interaction willing to engage.

5.5. **Identifying Sources of Treatment Effect Heterogeneity.** The results presented thus far show substantial average effects on refugee hiring and positive effects on belief updating. However, these average effects mask considerable heterogeneity across firms. Understanding this heterogeneity is important both for interpreting the mechanisms underlying our results and for informing the design of scalable integration programs.

To identify which baseline characteristics predict stronger or weaker treatment effects, we adopt an exploratory machine learning approach across multiple outcomes measured at different time horizons. Specifically, we apply a causal forest algorithm (Athey and Imbens, 2016; Wager and Athey, 2018) to estimate conditional average treatment effects (CATE) as a function of firm, refugee, and match-specific characteristics measured at baseline. While average treatment effects vary across outcomes, the algorithm helps identify whether certain participant characteristics consistently predict larger or smaller effects.[22] The causal forest approach allows us to search across a rich set of covariates without pre-committing to a particular specification.

We feed the algorithm a comprehensive set of baseline variables capturing: (1) firm characteristics (size, sector, management quality, prior experience hiring migrants); (2) employer attitudes toward refugees; (3) refugee characteristics (skills, work experience, language proficiency); (4) refugee attitudes toward the host community; and (5) match-specific variables (geographic proximity, gender concordance). Full details of the algorithm implementation and the complete list of covariates are provided in the Supplemental Appendix SA.1.

The causal forest analysis reveals a striking pattern: the strongest predictor of treatment effect heterogeneity is the interaction between employers' baseline attitudes toward refugees and refugees' baseline attitudes toward Ugandans. Table A.8 presents balance tests comparing firms and refugees in the top 50% of predicted CATE (high predicted effects) versus those in the bottom 50% (low predicted effects). Among all baseline characteristics examined, only two survive multiple hypothesis testing correction (List et al., 2019): employer attitudes and refugee attitudes. Firms with more positive attitudes toward refugee integration are substantially overrepresented in the high-CATE group (84% vs. 64%), as are refugees with more positive attitudes toward locals (87% vs. 5%). Importantly, other dimensions of heterogeneity—including refugee ability (as measured by test scores), firm size, sector, or match quality on observable skills—do not significantly predict treatment effect variation once we account for attitudes. This suggests that willingness to engage cooperatively, rather than technical match quality alone, may be central to whether workplace contact generates learning and sustained hiring.

These exploratory findings motivate a more structured analysis of how treatment effects vary across matches defined by baseline attitudes. In Section 6, we formalize this intuition through an extended conceptual framework that incorporates the role of attitudes in shaping both firms' learning incentives and refugees' work effort. We then present heterogeneity analysis examining how treatment effects on beliefs, attitudes, and hiring differ across four types of matches: (1) positive employer matched with positive refugee (positive match); (2) positive employer with negative refugee; (3) negative employer with positive refugee; and (4) negative employer with negative refugee (negative match).[23]

[22]The primary causal forest was estimated on short-term stated willingness to hire measured at follow-up 1, though we verify that similar patterns emerge when analyzing heterogeneity in beliefs, attitudes, and actual hiring outcomes.

[23]We emphasize one important caveat regarding the heterogeneity analysis that follows. While the machine learning approach helps us identify relevant dimensions of heterogeneity in a data-driven manner, the subsequent analysis of treatment effects by attitude groups should be interpreted cautiously given our sample size. We therefore focus on the overall pattern of results across groups rather than precise point estimates for any single subgroup.

6. Heterogeneity by Baseline Attitudes: When Does Contact Lead to Integration?

The exploratory analysis in Section 5.5 identified baseline attitudes of both employers and refugees as the primary predictor of treatment effect heterogeneity. This section examines how treatment effects vary systematically across matches defined by these attitudes. We begin by developing intuition for why attitudes might matter, then present comprehensive heterogeneity results across beliefs, attitudes, and hiring outcomes.

6.1. **Conceptual Framework: Why Attitudes Matter for Learning and Integration.** To interpret heterogeneity by baseline attitudes, we extend the Bayesian learning framework from Section 4.4 to incorporate two key features: (1) employers' task assignment decisions and learning effort, both of which may depend on their attitudes toward refugees; and (2) refugees' work effort, which may depend on their perceived returns to demonstrating ability. We provide a brief intuitive discussion here; the formal model is presented in Appendix B.

6.1.1. *The role of employer attitudes.* Employers with more positive attitudes toward refugee integration are more willing to experiment with hiring refugees and may be more willing to invest in learning about refugee workers' abilities. This investment can take two forms. First, employers can assign more complex or meaningful tasks during the internship, generating more informative signals about worker productivity. Second, employers can exert greater effort in observing and interpreting worker performance. By contrast, employers with more negative attitudes may assign simpler tasks or invest less attention in evaluation, limiting the information generated during the internship.

6.1.2. *The role of refugee attitudes.* Refugees with more positive attitudes toward the host community perceive greater returns to integration and may exert higher effort during the internship to demonstrate their abilities and secure future employment. Higher effort generates clearer signals of productivity and may also directly shape employers' assessments of non-cognitive skills such as reliability and motivation. Refugees with more negative attitudes, perhaps reflecting past experiences of discrimination, may perceive lower returns to effort and invest less in the relationship, limiting employers' ability to learn.

6.1.3. *Complementarity in attitudes.* These two channels interact. When both parties have positive attitudes, the employer assigns meaningful tasks and observes carefully, while the refugee exerts high effort, creating conditions for productive learning. When both parties have negative attitudes, the employer assigns simple tasks and invests little attention, while the refugee exerts low effort, generating weak or uninformative signals. In intermediate cases where only one party has positive attitudes, the effects are ambiguous: a motivated refugee paired with an inattentive employer, or an attentive employer paired with a disengaged refugee, may generate only moderate learning.

6.1.4. *Defining match types.* Based on this framework, we classify matches into four categories:

(1) **Positive matches**: Both employer and refugee have positive attitudes (above-median attitudes toward the out-group)
(2) **Firm+/Refugee- matches**: Employer has positive attitudes, refugee has negative attitudes
(3) **Firm-/Refugee+ matches**: Employer has negative attitudes, refugee has positive attitudes
(4) **Negative matches**: Both employer and refugee have negative attitudes (below-median attitudes toward the out-group)

We emphasize that "positive" and "negative" refer to *baseline attitudes*—the willingness to engage cooperatively—not to the quality of the interaction itself. This terminology differs from the contact theory literature, where "negative contact" typically describes an unpleasant interaction (Barlow et al., 2012; Paolini et al., 2010). Our focus is on whether participants enter the interaction predisposed to invest in learning and cooperation.

6.1.5. *Testable predictions.* This framework yields several predictions: (1) belief updating should be strongest in positive matches and weakest in negative matches; (2) changes in employer attitudes (e.g., support for refugee rights) should follow similar patterns; (3) hiring effects should emerge gradually over time as employers update beliefs and refugees who demonstrated high effort become available for employment; and (4) the gap between positive and negative matches should be most pronounced for outcomes that depend on sustained cooperation, such as long-term employment relationships.

6.2. **Measurement of Attitudes.** We construct attitude measures separately for employers and refugees, using factor analysis to aggregate responses to multiple survey items.

Employer attitudes are based on two questions: (1) "To what extent do you agree: When jobs are scarce, Ugandans should have more right to a job than refugees?" (5-point scale); and (2) "Do you think that refugees should be allowed to work in Uganda?" (binary). We extract the first factor and classify employers as having "positive attitudes" if their index value is above the median. Positive employers thus exhibit weaker in-group preferences and greater support for refugee labor market participation.

Refugee attitudes are based on four questions measuring perceived cultural proximity and trust toward Ugandans: (1) "Ugandans' culture is different from my own culture" (reverse-coded); (2) "Ugandans discriminate against refugees" (reverse-coded); (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 extract the first factor and classify refugees as having "positive attitudes" if their index value is above the median. Positive refugees thus perceive greater cultural proximity and trust, suggesting higher expected returns to integration effort.

These measures capture willingness to engage cooperatively across group boundaries, rather than broader political ideology or general social attitudes. Full details on index construction are provided in Appendix Tables S.1 and S.2.

6.3. **Evidence from the Internship Experience.** Before examining long-term outcomes, we present descriptive evidence that the quality of the internship experience itself varied systematically by match type. Figure 5 displays key outcomes measured at the first follow-up (approximately one month after the internship).

Panel A shows that employers in positive matches were substantially more likely to report willingness to rehire the same intern (82% vs. 64%, on average, in mixed matches vs. 63% in negative matches), suggesting more favorable experiences. Panel B indicates that supervision was less demanding in positive matches (average rating of 2.4 on a 1–5 scale vs. 2.7, on average, in mixed matches and 2.8 in negative matches), despite employers in positive matches not spending less time supervising (Panel C). This pattern suggests that positive-attitude refugees required less intensive oversight to perform effectively.

Panel D provides suggestive evidence on refugee selection: refugees in positive matches had higher job-finding rates with Ugandan employers in the month prior to the experiment (14% vs. 6% in negative matches), consistent with these refugees being more actively engaged in job search and integration efforts.

Panel E shows that employers in positive matches are more likely to assign more complex tasks during the internship (around a third of firms in positive matches, while less than a quarter in more negative ones). This is consistent with our framework's prediction that positive-attitude employers invest more in learning by assigning tasks that better reveal worker ability.

Taken together, these patterns support the framework's premise: interactions proceeded more smoothly, involved more meaningful work, and generated clearer signals when both parties entered with positive baseline attitudes.

6.4. **Heterogeneity in Belief Updating, Attitude Change, and Hiring.** To examine how treatment effects vary by match quality, we estimate the following specification:

$$\begin{aligned} y_{i1} &= \beta_0 + \beta_1 T \times Positive + \beta_2 T \times (Firm+, Refugee-) \\ &\quad + \beta_3 T \times (Firm-, Refugee+) + \beta_4 T \times Negative + y_{i0} + X_i'\delta + \varepsilon_i \end{aligned} \tag{6.1}$$

where y_{i1} is the outcome of interest measured post-intervention, $T \times Positive$ is an indicator for treated firms with positive baseline attitudes that were matched with a refugee worker holding positive attitudes toward Ugandans, $T \times Negative$ is an indicator for treated firms with negative baseline attitudes matched with a refugee worker holding negative attitudes, $T \times (Firm+, Refugee-)$ is an indicator for matches where the employer has positive attitudes but the refugee has negative attitudes, and $T \times (Firm-, Refugee+)$ is an indicator for matches where the employer has negative attitudes but the refugee has positive attitudes.[24] The baseline value of the outcome y_{i0} is included as a control (ANCOVA specification), and X_i contains strata (refugee occupations) fixed effects. Standard errors are clustered at the refugee level to reflect the experimental design where multiple firms may have been shown the same refugee's profile. Each coefficient β_1 through β_4 captures the treatment effect for the corresponding match type. We test for equality of coefficients across groups to assess whether treatment effects differ significantly by match quality.

Table 6 presents the main heterogeneity results, examining how treatment effects vary across the four match types for outcomes spanning belief updating (columns 1–2), employer attitude change (column 3), and hiring (columns 4–7).

Both the ITT and the LATE estimates provide similar evidence, with all the coefficients on the latter larger and statistically significant. Treatment effects on soft skills beliefs (column 1) are concentrated in positive matches (coefficient = 0.231, $p = 0.024$), with no significant effects in other match types. When comparing equality of coefficients across groups using the LATE estimates, the difference between positive matches and negative matches is statistically significant ($p = 0.010$). Hard skills beliefs (column 2) show no significant effects in the ITT specification (Panel A), but become significant for positive matches in the exposed sample (Panel B), where we also observe a statistically significant difference between positive and negative groups ($p = 0.001$). The point estimates for negative matches are negative for both skill dimensions, suggesting that low-engagement interactions may reinforce pessimistic priors rather than correct them—this offsetting pattern explains why aggregate hard skills effects (Table 5) are null despite positive matches updating positively. This pattern suggests that when internships actually take place, exposure can shift employers' assessments of both technical competencies and non-contractible attributes (trustworthiness, reliability, work ethic). However, consistent with our theoretical framework, such learning requires active engagement from both parties, as evidenced by the concentration of effects among positive matches where both employers and workers entered the interaction willing to cooperate.

[24] Because treatment was randomized after match-type classification, match types are balanced between treatment and control groups (χ^2 test: $p = 0.558$). Equation 6.1 omits main effects for match types because control firms never experienced the matched refugee interaction. Consistent with this, match-type dummies do not significantly predict control group outcomes: relative to Negative matches, neither Positive matches ($p = 0.158$) nor Mixed matches ($p = 0.590$) differ in refugee hiring at endline.

Treatment significantly reduces in-group bias in positive matches (coefficient = -0.316, $p = 0.010$). The effect is stronger when looking at the effect among the exposed sample. Once again, the effect among the positive matches are statistically different from the negative matches, where coefficient is close to zero and insignificant.

We measure hiring outcomes at the second follow-up (approximately 8 months post-intervention), capturing both the extensive margin (any refugee hired) and intensive margin (total number hired). Effects on hiring at least one refugee intern (column 4) and total refugees hired (column 5) are positive and marginally significant across all groups, with no significant differences across groups. The effects are larger and marginally significant in the exposed group.

By endline (24 months post-intervention), clearer patterns emerge. Positive matches show significant increases in both the probability of employing at least one refugee (coefficient = 0.050, $p = 0.061$) and total refugees employed (coefficient = 0.117, $p = 0.047$). The effect on total refugees in positive matches, once again larger in the exposed sample, is marginally significantly different from the effect in negative matches ($p = 0.089$). Mixed matches show intermediate effects that are generally not distinguishable from either extreme.

6.5. **Implications for Contact Theory and Integration Policy.** The contact hypothesis literature has long emphasized optimal conditions for positive intergroup contact: equal status, common goals, intergroup cooperation, and institutional support (Allport, 1954). More recent work has shown that the average effects of contact may shrink when selection into contact is randomized rather than voluntary (Pettigrew and Tropp, 2006; Paluck and Green, 2009; Lowe, 2024). Our results suggest a complementary insight: rather than fighting selection bias, integration programs might strategically leverage it by targeting participants predisposed to cooperative engagement on both sides of the interaction.

This approach differs from traditional contact interventions in two ways. First, most contact experiments randomly assign exposure, treating all participants symmetrically. We show that effects are highly heterogeneous in baseline willingness to cooperate. Second, classic contact theory focuses on the quality of the interaction itself (whether it is pleasant, cooperative, etc.) but says less about how to predict ex ante which interactions will be productive. Our results suggest that baseline attitudes (measurable through simple survey instruments) can help identify promising matches.

An important feature of our setting is that contact occurs within an unequal power relationship: employers have authority over refugees as potential employees. This violates Allport's (1954) condition of "equal status within the contact situation," yet we find no evidence of backlash effects on average. In positive matches, this power asymmetry may even be productive: employers have both the authority to assign meaningful tasks and the incentive (given positive attitudes) to invest in learning. This contrasts with Ghosh (2022)'s finding that contact between Indian workers of different castes reduced productivity while improving attitudes. Our setting may differ because: (1) employers with positive attitudes self-selected into the program—only firms willing to host a refugee intern (WTP ≥ 0) participated, representing approximately 45% of contacted firms—and even within this selected sample, effects concentrate among employers with above-median attitudes (Section 6.4); (2) the internship was short-term with relatively lower stakes; (3) refugees could decline program participation based on general expectations and opportunity costs (primarily distance), though they could not screen individual employers as firm identities were not revealed. More research is needed on when unequal contact can be productive and when equal-status conditions remain critical.

6.5.1. *Targeting vs. scale.* A natural concern with targeting positive-attitude participants is that it may limit program scale. If only 20% of employers have sufficiently positive attitudes, a targeted program reaches far fewer firms than a universal one. However, this concern must be weighed against two considerations. First, if negative-attitude matches produce zero or even negative effects, universal programs waste resources on unproductive matches. Second, positive effects may diffuse through networks: treated employers may recommend refugees to other firms (Table 3, column 3), and successfully integrated refugees may help subsequent refugee cohorts. We cannot directly test these spillover channels, but they suggest that targeting need not imply limited impact.

To inform the feasibility of targeting at scale, we examined data from the Afrobarometer Round 10 survey (2024–2025) conducted in Uganda. Less than one third of Ugandan respondents agree or strongly agree that "when jobs are scarce, employers should prioritize Ugandans over immigrants." This suggests that positive attitudes toward immigrant/refugee employment rights may be relatively common, making targeted programs potentially scalable.

6.5.2. *Comparison to other refugee employment policies.* Our intervention differs from most refugee labor market integration programs, which focus on supply-side constraints: skills training (Battisti et al., 2019), language instruction (Arendt et al., 2021), credential recognition (Fasani et al., 2021), or job search assistance (Caria et al., 2024). These interventions address real barriers but do not directly target employer beliefs or willingness to hire. Our results suggest that demand-side interventions leveraging workplace contact may complement supply-side approaches, particularly when combined with strategic targeting.

Other contact-based integration programs include refugee mentorship or sponsorship by native community members (e.g., Canada's Private Sponsorship Program). These programs share our intervention's emphasis on sustained interpersonal contact but differ in important ways: they involve non-employment relationships, longer time horizons, and typically no experimental variation in matching. Our workplace setting may be particularly effective because: (1) employers have strong incentives to assess worker productivity accurately; (2)

the employment relationship provides structure and repeated interaction; (3) both parties face concrete stakes (wages, productivity) that discipline behavior. However, the short duration of our internships (one week) may limit learning relative to longer-term mentorship programs.

7. Conclusions

Employment relationships that depend on non-contractible traits (trustworthiness, reliability, willingness to cooperate) require mutual investment whose returns unfold over time. When firms lack reliable signals about workers' productivity along these dimensions, pessimistic beliefs or uncertainty about the other side's behavior can prevent mutually beneficial matches from forming. This problem is particularly acute when hiring from underrepresented labor pools, where workers' employment histories are hard to interpret and standard screening mechanisms may fail precisely where relational investment is most valuable.

This paper asks whether short-term workplace interaction can durably change firms' willingness to engage with such workers. We study a randomized internship program in Uganda that matches firms with skilled refugee workers for one-week placements. The intervention is not merely informational: it creates a setting in which both employers and refugees jointly test the feasibility of cooperation in a real workplace environment. Participation requires active engagement from both sides: firms must assign tasks and invest supervision effort; refugees must commute, forgo outside options, and demonstrate their abilities.

We find that this brief exposure generates large and persistent increases in refugee hiring. Treated firms are more than twice as likely to employ refugees up to two years after the internship, with effects emerging gradually rather than immediately. Workplace contact reduces perceived risk and builds trust rather than simply revealing productivity in a one-time match. Following the internship, treated employers revise upward their assessments of refugees' trustworthiness and reliability: the non-contractible attributes difficult to observe through resumes or credentials. These belief updates are corroborated by behavior in an incentivized trust game, become more supportive of refugee integration, and actively build connections to refugee communities.

Critically, these effects are highly heterogeneous in ways that inform both theory and policy. Belief updating and subsequent hiring are concentrated among matches where both employers and refugee workers enter the interaction with positive baseline attitudes toward cross-group cooperation. Firms with initially favorable views matched to refugees expressing trust toward the host community generate strong learning and sustained hiring. In contrast, negative matches (both parties negative) show near-zero effects, while mixed matches show positive but statistically insignificant estimates.

Our findings have several important implications for the design of refugee integration programs. First, not all firms will be interested in or benefit from providing internships to refugee workers. In our setting, about half of the employers we contacted were willing to participate. These firms were more likely to have vacancies, express interest in expansion, and have prior experience with internships. They represent the margin at which integration programs are most likely to succeed: employers who are open to experimentation but lack direct experience with refugee workers. Effective programs should identify and target such firms rather than attempting universal coverage.

Second, not all refugees can participate equally. Credit constraints and transportation costs created substantial barriers: refugees living farther from internship locations were significantly less likely to attend. About half of assigned refugees failed to show up to their scheduled internships, primarily due to distance. Governments and organizations designing integration programs must account for these participation constraints. Providing transportation subsidies or locating programs in refugee-dense neighborhoods may substantially improve take-up and program effectiveness.

Third, the intervention generated spillover effects beyond the specific matches created during the experiment. Treated firms hired refugees from the broader community, not merely the workers they were matched with during the internship. We find no statistically significant displacement of Ugandan workers, though the point estimates are slightly negative, suggesting possible compositional shifts toward refugee hiring. The intervention did not increase firms' overall capacity to hire or improve firm profitability. Thus, the increase in refugee employment appears to reflect treated firms drawing from refugee labor pools they previously excluded from consideration, potentially with modest adjustments in hiring from other sources, rather than overall firm expansion.

The positive reception of these results within the refugee community is noteworthy. After sharing our findings, YARID (Young African Refugees for Integral Development), one of the refugee-led NGOs we partnered with, established an ongoing job placement program to assist refugees in job search and match them with employers. The program provides refugees with training in job search skills and connects them with firms seeking new workers.[25] This organic adoption suggests the intervention addressed a genuine market failure and that the approach can be sustained beyond the experimental setting.

Our results suggest that carefully designed contact interventions can durably shift labor demand for marginalized workers when they identify willing partners on both sides of the market. By highlighting the importance of cooperation, attitudes, and match quality, this paper provides guidance for policies seeking to leverage workplace relationships to improve economic integration.

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

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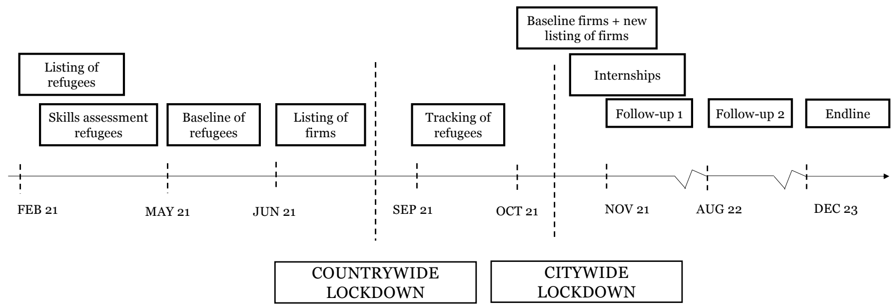
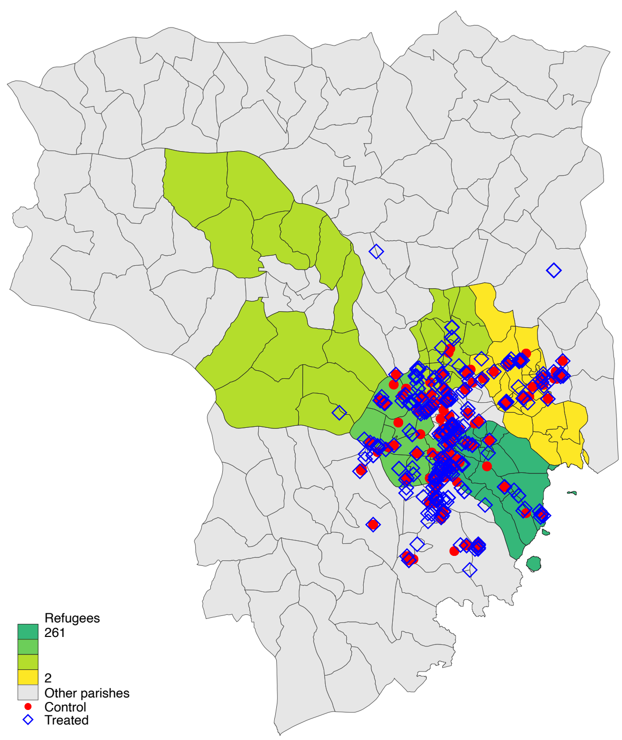
Figure 1. Timeline and Firms' Locations



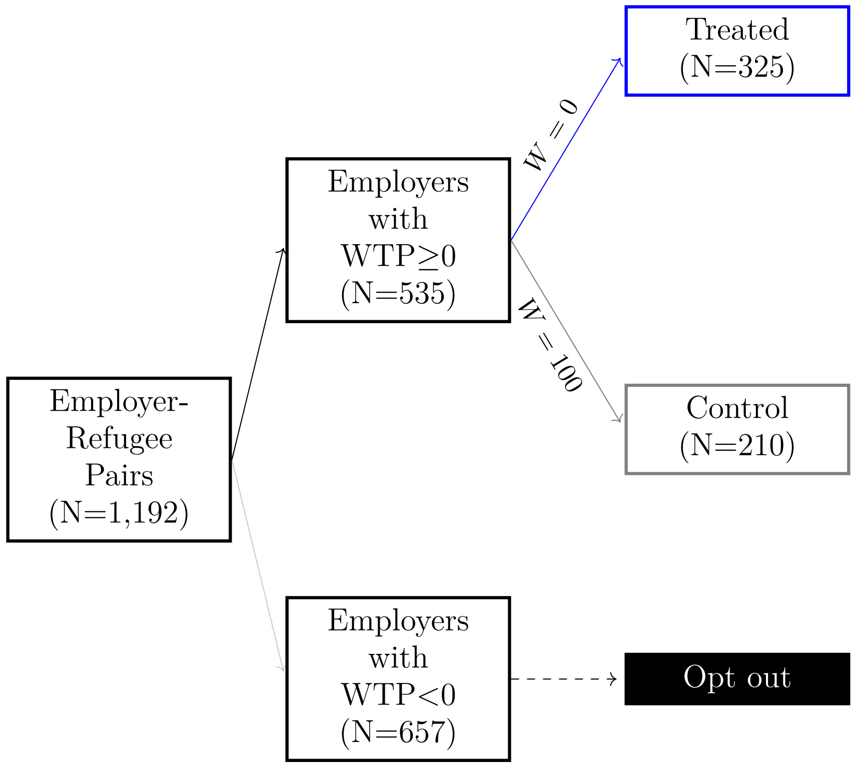
(**A.**) Timeline



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

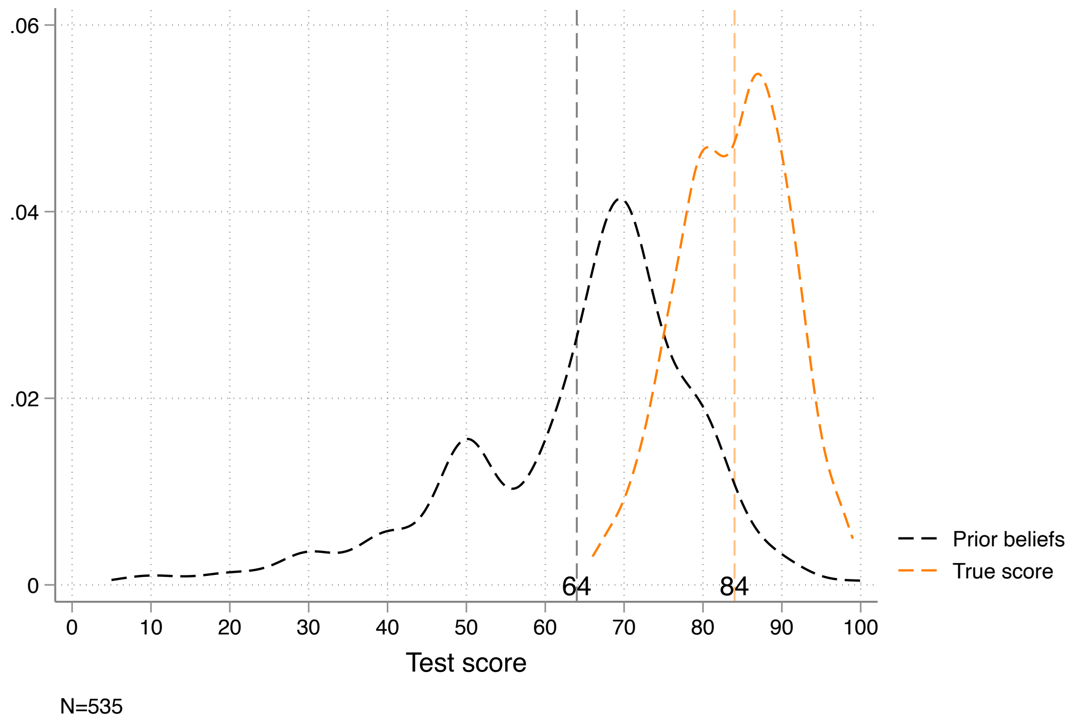
Notes: The timeline (Panel A) illustrates the sequence of events relevant to the study. The map (Panel B) shows the location of firms belonging to our sample, distinguished by treatment (blue diamonds) and control (red dots) status. Each parish is colored based on the number of refugees from our sample. 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.

Figure 2. Design



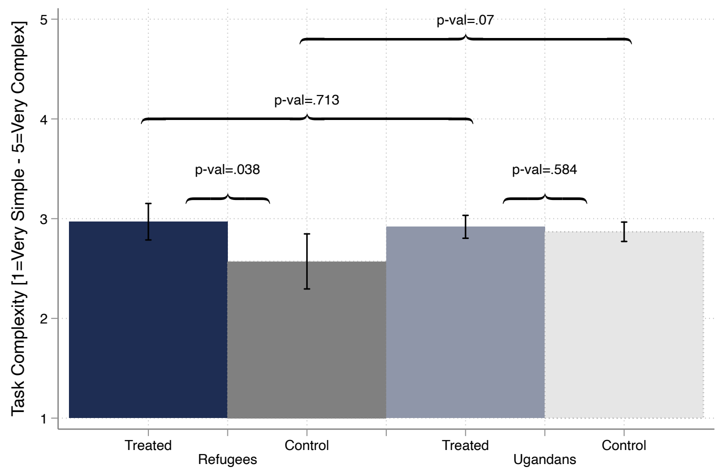
Notes: This figure plots the design of the experiment. We start with a sample of 1,192 pairs. Of these, 535 belong to the final study sample.

Figure 3. Firms' Beliefs About Refugees' Ability Are Imprecise

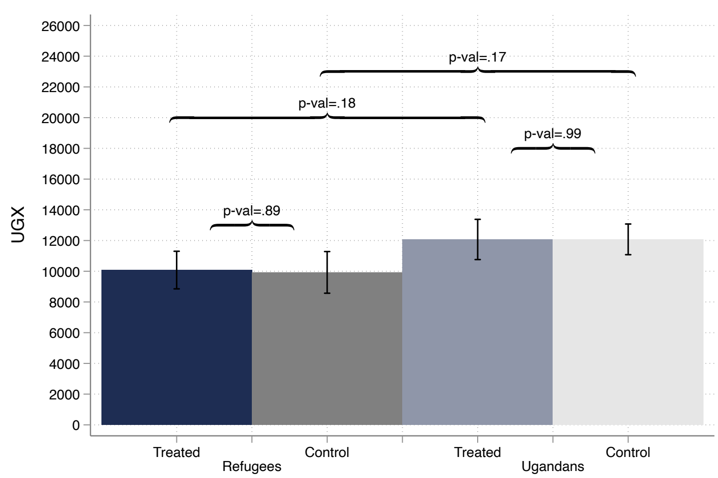


Notes: This graph plots the distribution of employers' baseline beliefs about refugee workers' skills and actual test scores of refugees who attended voluntary skills assessments. **Employer beliefs** (dashed black lines) were elicited by asking: *"Workers can undertake a modular assessment on some specific 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: 535 firms willing to hire a refugee. **Actual test scores** (dashed orange lines) come from refugees who voluntarily attended skills assessments (N=552, 56% of invited refugees).

FIGURE 4. New Hires: Tasks and Payments



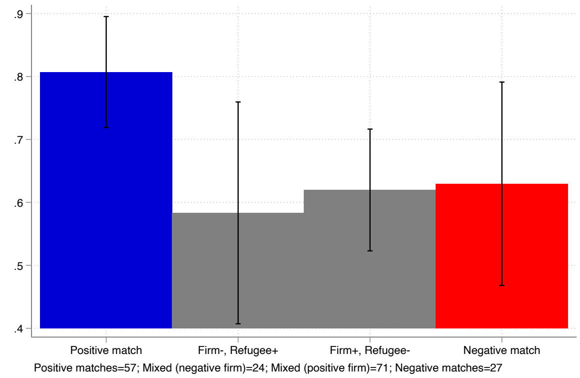
(**A.**) Tasks' Average Complexity



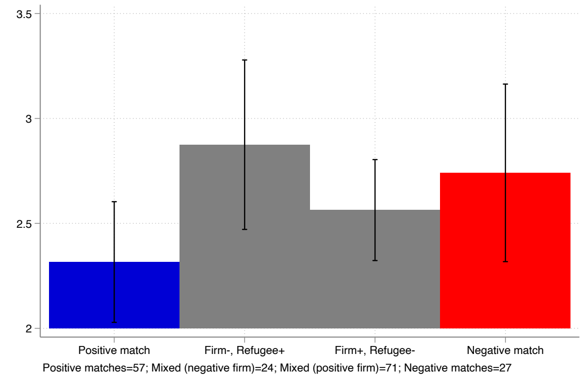
(**B.**) Average Daily Wage

Notes: The first graph (Panel A) plots the maximum 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.

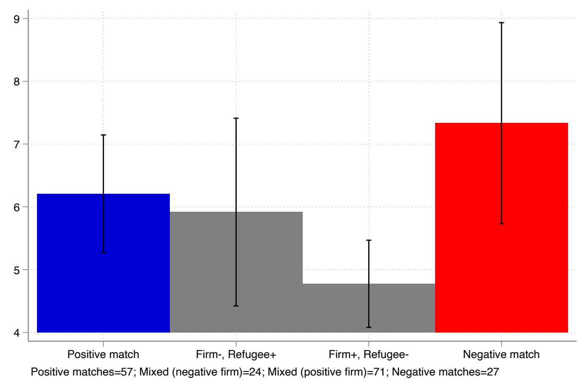
FIGURE 5. Evidence from the Internship



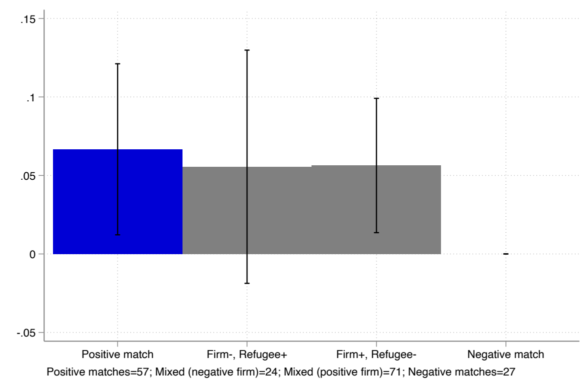
(**A.**) Willingness to hire the same intern



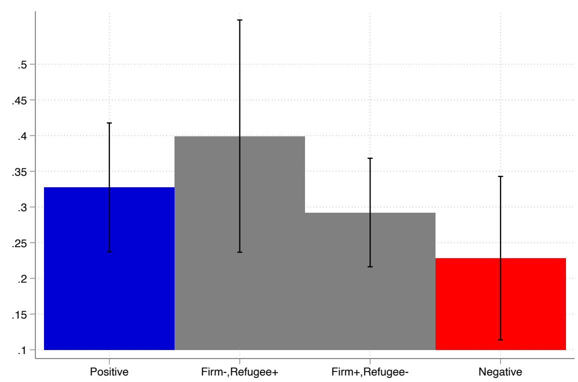
(**B.**) Difficulty of supervision



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



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



(**E.**) Fraction of firms assigning complex tasks

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, less of employers whom we did not manage to track, N=179): Panel A shows the percentage of firms willing to rehire the same intern 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 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, less of employers whom we did not manage to track at follow-up 1 (N=179). Notice that the dummy "Supervised more than other workers" is created only for firms with at least one employee.

Table 2. Exposure Increases Hiring of Refugee Workers

	New internships at follow-up 2				New workers still employed at endline				
	(1) At least 1 refugee	(2) Total refugees	(3) At least 1 Ugandan	(4) Total Ugandans	(5) At least 1 refugee	(6) Total refugees	(7) At least 1 Ugandan	(8) Total Ugandans	(9) Total Employment
Panel A: ITT									
Treated	0.045*	0.065**	0.029	0.106	0.034*	0.059**	-0.055	-0.120	0.001
	(0.023)	(0.031)	(0.038)	(0.116)	(0.018)	(0.027)	(0.053)	(0.123)	(0.402)
	[0.055]	[0.039]	[0.445]	[0.359]	[0.061]	[0.026]	[0.297]	[0.330]	[0.997]
Panel B: LATE									
Exposed	0.082**	0.120**	0.054	0.196	0.061*	0.104**	-0.097	-0.211	0.002
	(0.042)	(0.057)	(0.069)	(0.210)	(0.031)	(0.046)	(0.091)	(0.211)	(0.694)
	[0.050]	[0.035]	[0.437]	[0.350]	[0.054]	[0.023]	[0.285]	[0.318]	[0.997]
Firms	474	474	474	474	407	407	407	407	407
Mean DV	0.043	0.048	0.188	0.398	0.037	0.037	0.550	1.069	3.425

Notes: This table reports treatment effects on refugee and Ugandan hiring, estimated by equation 5.1. Panel A shows intention-to-treat (ITT) estimates; Panel B shows local average treatment effects (LATE), instrumenting actual exposure to a refugee intern with treatment assignment. *Dependent variables*: Columns 1–4 measure new internships offered to refugees (excluding the matched worker from the experiment) at follow-up 2, approximately eight months post-intervention. Column 1: dummy equal to one if firm offered at least one new internship to a refugee; Column 2: total number of new refugee internships offered; Columns 3–4: analogous measures for Ugandan workers. Columns 5–8 measure workers still employed at endline, approximately 24 months post-intervention. Column 5: dummy equal to one if firm employs at least one new refugee (excluding the matched worker); Column 6: total number of new refugees still employed; Columns 7–8: analogous measures for Ugandan workers. Column 9: total employment at endline (all workers, including refugees and Ugandans). All refugee hiring outcomes exclude the worker matched during the experimental internship. *Controls*: 15 randomization strata fixed effects (refugees' occupations: tailor, cook, hairdresser, domestic electrician, arts & crafts maker, painter, baker, motor vehicle mechanic, barber, beautician, hotel staff, plumber, carpenter, leather designer, bricklayer, electronic technician, welder, and waiter). Standard errors in parentheses are clustered at the refugee level. P-values in square brackets. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 3. Exposure Improves Attitudes Towards Refugees' Integration

	Follow-up 1		Follow-up 2	Endline
	(1) Priority to Ugandans	(2) Donation to NGO	(3) Donation to NGO	(4) In contact with NGO
Panel A: ITT				
Treated	-0.175*	0.171*	0.210**	0.071*
	(0.090)	(0.090)	(0.101)	(0.037)
	[0.052]	[0.059]	[0.037]	[0.058]
Panel B: LATE				
Exposed	-0.313**	0.306*	0.388**	0.125**
	(0.159)	(0.159)	(0.183)	(0.064)
	[0.048]	[0.055]	[0.034]	[0.050]
Firms	525	525	474	407
Mean DV	-0.000	0.000	0.000	0.125

Notes: This table reports the coefficients estimated by equation 5.1. *Dependent variables*: Column 1: A standardized index (using Anderson (2008) methodology) measuring agreement with the statement "When jobs are scarce, Ugandans should have more right to a job than refugees" (scale 1-5, where higher values indicate stronger agreement). The index is standardized by the control group mean and standard deviation at follow-up 1 (higher values = stronger in-group preference, so negative coefficients indicate reduced discrimination and increased support for refugee employment rights.) Column 2: Donation amount (in UGX) to a refugee-led nonprofit organization, collected at follow-up 1. Out of the 5,000 UGX survey participation incentive, respondents were asked: "How much are you willing to donate to this organization?" The variable is standardized using the control group mean and SD at follow-up 1. Column 3: Donation amount (in UGX) to a refugee-led nonprofit organization, collected at follow-up 2, standardized similarly. Column 4: A binary indicator equal to 1 if the employer reports engaging in active network-building to access refugee workers, measured at endline (approximately 24 months post-intervention). This variable equals one if the employer either: (a) personally knows anyone from refugee-led organizations or other organizations working with refugees they can refer to when looking for a new worker, OR (b) registered via SMS during the baseline survey to receive ongoing information about refugee jobseekers from YARID (Young African Refugees for Integral Development), a refugee-led organization in Kampala. The SMS registration was voluntary and required employers to send a message including their name, ID number, and preference for refugee or Ugandan workers (or both). Registration indicates active interest in maintaining connections to refugee labor pools. *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). Standard errors are clustered at the refugee level. P-values reported in square brackets. ***, **, *, indicate significance at the 1%, 5%, and 10% levels respectively.

Table 4. Firms Update Beliefs About Refugees' Soft Skills

	Soft skills						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Index	Time management	Team work	Work ethics	Respect	Trust	Trust game
Panel A: ITT							
Treated	0.132*	0.113	0.182*	0.116	0.077	0.171*	0.025*
	(0.079)	(0.096)	(0.105)	(0.096)	(0.101)	(0.099)	(0.015)
	[0.096]	[0.240]	[0.083]	[0.228]	[0.443]	[0.083]	[0.093]
Panel B: LATE							
Exposed	0.236***	0.201	0.326*	0.208	0.138	0.306*	0.045*
	(0.078)	(0.168)	(0.182)	(0.169)	(0.177)	(0.171)	(0.027)
	[0.002]	[0.230]	[0.074]	[0.218]	[0.433]	[0.074]	[0.088]
Firms	525	525	525	525	525	525	474
Mean DV		-0.000	-0.000	-0.000	0.000	0.000	0.491

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*: Column 1 (Index): Standardized index combining all soft skill measures (columns 2-6), constructed following Anderson (2008). The index averages the standardized values of the five individual components, with higher values indicating more positive beliefs about refugees' soft skills: Time management (Column 2): Employers' assessment of refugees' ability to manage their time effectively and complete tasks punctually; Teamwork (Column 3): Employers' assessment of refugees' ability to work cooperatively with colleagues; Work ethics (Column 4): Employers' assessment of refugees' general work ethic and dedication to their jobs; Respect (Column 5): Employers' assessment of how respectful refugee workers are toward employers and colleagues; Trust (Column 6): Employers' assessment of how trustworthy refugee workers are in workplace relationships. All variables in columns 1-6 are standardized using the control group mean and standard deviation at follow-up 1, so coefficients can be interpreted as effect sizes in standard deviation units. The control group mean for each standardized component is zero by construction. Column 7 (Trust game): Proportion of experimental endowment sent to an anonymous refugee partner in an incentivized trust game conducted at follow-up 2 (approximately 8 months post-intervention). At the beginning of the follow-up 2 survey, employers were given 10,000 UGX and told they could send any amount to an anonymous refugee partner. Any amount sent would be tripled before reaching the partner, who could then choose how much to return. *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). Standard errors are clustered at the refugee level. P-values reported in square brackets. ***, **, *, indicate significance at the 1%, 5%, and 10% levels respectively.

Table 5. Beliefs About Hard Skills Do Not Change

	Hard skills				
	(1) Index	(2) Theoretical	(3) Practical	(4) Performance	(5) Score
Panel A: ITT					
Treated	0.010	0.084	-0.055	0.000	-1.748
	(0.086)	(0.096)	(0.097)	(0.099)	(1.256)
	[0.910]	[0.381]	[0.570]	[0.999]	[0.165]
Panel B: LATE					
Exposed	0.017	0.150	-0.098	0.000	-3.125
	(0.098)	(0.168)	(0.170)	(0.174)	(2.218)
	[0.860]	[0.371]	[0.564]	[0.999]	[0.159]
Firms	525	525	525	525	524
Mean DV		-0.000	-0.000	-0.000	63.917

Notes: This table reports the coefficients estimated by equation 5.1. *Dependent variables*: Column 1 presents a standardized index combining beliefs about theoretical knowledge, practical skills, and performance (columns 2-4). Column 5 reports employers' beliefs about refugees' expected test scores (0-100 scale, passing threshold=65). *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). Standard errors are clustered at the refugee level. P-values reported in square brackets. ***, **, *, indicate significance at the 1%, 5%, and 10% levels respectively.

TABLE 6. The Effect of the Experiment Are Concentrated Among Positive Matches

	Index soft skills	Index hard skills	Priority to Ugandans	New internships at follow-up 2		New workers still employed at endline	
	(1)	(2)	(3)	(4) At least 1 refugee	(5) Total refugees	(6) At least 1 refugee	(7) Total refugees
Panel A: ITT							
Treated × Positive match	0.231**	0.099	-0.316***	0.056*	0.070*	0.050*	0.117**
	(0.102)	(0.096)	(0.121)	(0.034)	(0.042)	(0.027)	(0.058)
	[0.024]	[0.300]	[0.010]	[0.099]	[0.100]	[0.061]	[0.047]
Treated × Firm+, Refugee-	0.085	-0.075	-0.075	0.030	0.055	0.033	0.035
	(0.104)	(0.095)	(0.121)	(0.030)	(0.045)	(0.026)	(0.028)
	[0.413]	[0.427]	[0.535]	[0.326]	[0.221]	[0.209]	[0.216]
Treated × Firm-, Refugee+	0.131	-0.063	-0.312*	0.065	0.119	0.037	0.070
	(0.166)	(0.154)	(0.168)	(0.055)	(0.098)	(0.053)	(0.081)
	[0.430]	[0.683]	[0.065]	[0.233]	[0.225]	[0.482]	[0.383]
Treated × Negative match	0.040	-0.119	-0.022	0.038	0.034	0.004	-0.006
	(0.133)	(0.114)	(0.176)	(0.046)	(0.048)	(0.036)	(0.038)
	[0.762]	[0.296]	[0.901]	[0.403]	[0.476]	[0.905]	[0.868]

Notes: See notes to next table.

TABLE 6, CONT'D. The Effect of the Experiment Are Concentrated Among Positive Matches

	Index soft skills	Index hard skills	Priority to Ugandans	New internships at follow-up 2		New workers still employed at endline	
	(1)	(2)	(3)	(4) At least 1 refugee	(5) Total refugees	(6) At least 1 refugee	(7) Total refugees
Panel B: LATE							
Exposed × Positive	0.441***	0.331**	-0.604***	0.106*	0.132*	0.096*	0.223**
	(0.108)	(0.140)	(0.233)	(0.062)	(0.078)	(0.051)	(0.111)
	[0.000]	[0.018]	[0.010]	[0.088]	[0.089]	[0.059]	[0.044]
Exposed × Firm+, Refugee-	0.157	-0.081	-0.142	0.059	0.108	0.056	0.061
	(0.101)	(0.129)	(0.217)	(0.058)	(0.086)	(0.044)	(0.048)
	[0.122]	[0.526]	[0.514]	[0.311]	[0.210]	[0.198]	[0.204]
Exposed × Firm-, Refugee+	0.201	-0.027	-0.477*	0.101	0.185	0.055	0.104
	(0.130)	(0.174)	(0.272)	(0.084)	(0.154)	(0.076)	(0.117)
	[0.121]	[0.879]	[0.080]	[0.228]	[0.229]	[0.474]	[0.374]
Exposed × Negative	0.065	-0.305*	-0.030	0.068	0.061	0.006	-0.015
	(0.128)	(0.161)	(0.296)	(0.078)	(0.081)	(0.065)	(0.069)
	[0.611]	[0.059]	[0.919]	[0.383]	[0.455]	[0.921]	[0.826]
Firms	525	525	525	474	474	407	407
p($\beta_1 = \beta_2$)	0.016	0.007	0.075	0.523	0.822	0.525	0.164
p($\beta_1 = \beta_3$)	0.104	0.074	0.702	0.966	0.740	0.636	0.447
p($\beta_1 = \beta_4$)	0.010	0.001	0.098	0.672	0.479	0.284	0.089
p($\beta_2 = \beta_3$)	0.755	0.773	0.302	0.658	0.644	0.983	0.707
p($\beta_2 = \beta_4$)	0.513	0.206	0.743	0.919	0.657	0.494	0.329
p($\beta_3 = \beta_4$)	0.410	0.197	0.202	0.760	0.452	0.625	0.386
Mean DV			-0.000	0.043	0.048	0.037	0.037

Notes: This table reports the coefficients estimated by equation 6.1. *Dependent variables*: Indices computed following Anderson (2008), using the following underlying covariates: work ethics, time management and teamwork ability for the index on soft skills (col. 1); theoretical skills, practical skills and speed for the index on hard skills (col. 2). Column 3: standardized answer to the question on whether Ugandans should have priority for jobs when jobs are scarce. Columns 4 and 5: dummy equal to 1 if the firm has ever offered an internship to a new refugee worker and total number of new refugee interns. Columns 6 and 7: a dummy equal to 1 if the firm has at least one refugee still hired at endline and total number of refugee workers hired and still employed at the firm at endline. Panel A presents results for the ITT effects (see previous table). Panel B presents the LATE effects. The bottom rows report p-values from tests of equality of coefficients across groups. *Controls*: 15 randomization strata (refugees' occupations). Standard errors are clustered at the refugee level. P-values reported in square brackets. ***, **, *, indicate significance at the 1%, 5%, and 10% levels respectively.

Appendix A. Additional Figures and Tables

Figure A.1. 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):

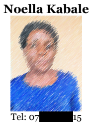
Reading:	*Speaking:*	*Writing:*	*Listening:*
3=Moderately well	2=Not well	2=Not well	3=Moderately well

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)

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)

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)

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

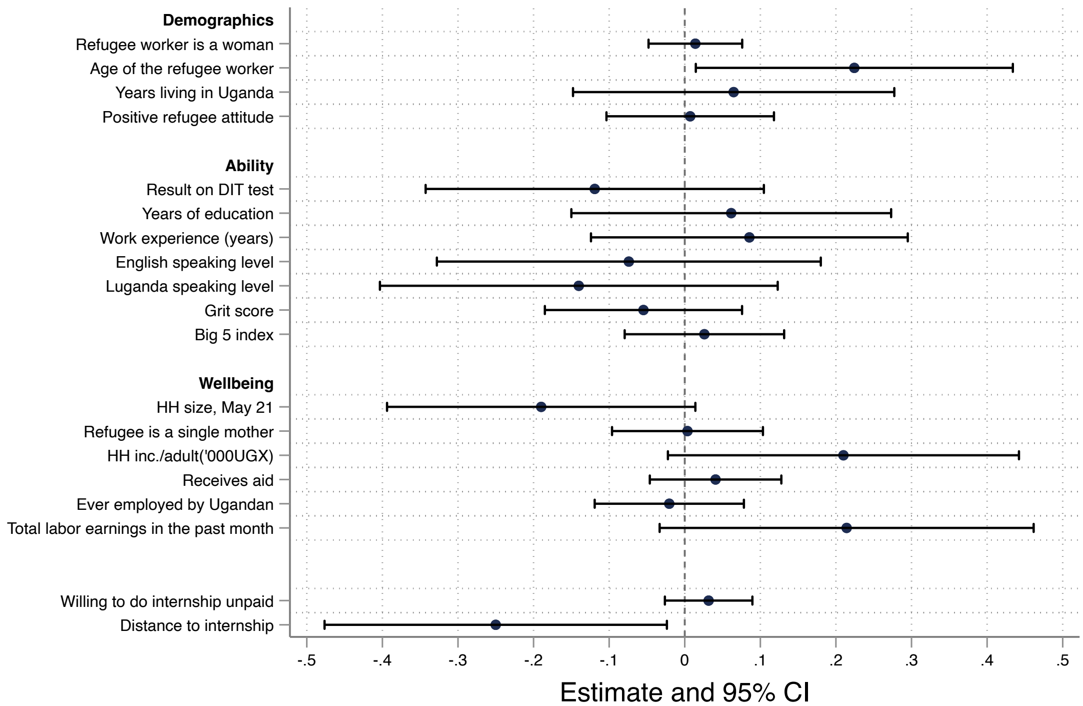
Knowledge of Luganda (self-reported scale 1-5)**:**

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

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

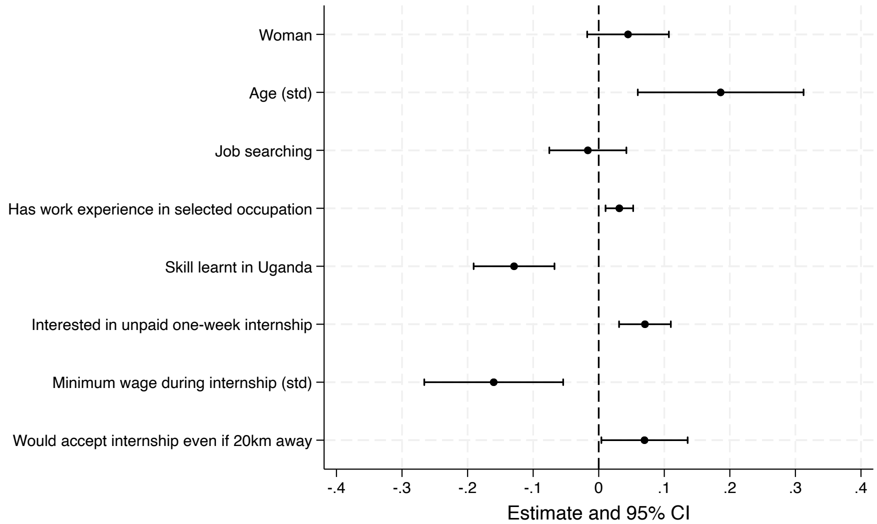
Notes: The figure plots examples of CVs for both real refugee workers and hypothetical local workers. The refugee workers' CVs are based on information provided by the respondents, while the hypothetical local workers' CVs are created to mirror the same structure. Selection of names and images for the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

FIGURE A.2. 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.3. Characteristics of Refugees by Test Attendance



Notes: This graph shows the characteristics of refugees by whether they attended the skills 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.

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. Comparing Firms Willing to Hire a Refugee Intern with Full Sample

	Willing			Unwilling			
Variable	N	Mean	SD	N	Mean	SD	Diff.
Employer is a woman	535	0.570	0.496	657	0.525	0.500	0.045
Firm age	535	7.815	6.644	645	8.135	7.135	-0.320
Firm is formal	535	0.185	0.389	657	0.134	0.341	0.051**
Has a vacancy	535	0.419	0.494	657	0.140	0.347	0.279***
Desires expand in the future	535	0.860	0.348	657	0.642	0.480	0.217***
Employees at baseline	535	2.492	3.147	656	2.825	3.494	-0.333*
Manufacturing sector	535	0.333	0.472	657	0.355	0.479	-0.022
Ever offered internships	535	0.609	0.488	645	0.518	0.500	0.092***
Ever hired a migrant or refugee	535	0.361	0.481	657	0.280	0.449	0.081***
Beliefs about refugees' test score	535	64.131	15.141	657	62.703	15.791	1.428
Supports refugees' empl. rights	535	0.923	0.266	655	0.890	0.313	0.033**
Jobs to locals first	535	3.355	1.268	655	3.362	1.356	-0.007
WTP for local worker, non-neg.	535	0.985	0.121	657	0.347	0.476	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 refugee level. ***, **, *, indicate significance at the 1%, 5%, and 10% levels respectively.

Table A.3. Firms' Take-up of the Internships

Variable	Match	No match	Control	p(Matched=No)	N
Employer is a woman	0.582	0.538	0.581	0.149	535
	(0.495)	(0.500)	(0.495)		
Firm age	7.742	7.510	8.086	0.862	535
	(6.546)	(6.821)	(6.627)		
Firm is formal	0.181	0.182	0.190	0.972	535
	(0.386)	(0.387)	(0.394)		
Has a vacancy	0.423	0.483	0.371	0.420	535
	(0.495)	(0.501)	(0.484)		
Desires expand in the future	0.863	0.839	0.871	0.533	535
	(0.345)	(0.369)	(0.336)		
Employees at baseline	2.615	2.203	2.581	0.163	535
	(3.497)	(2.602)	(3.169)		
Num. of rooms in business premises	1.159	1.182	1.176	0.385	535
	(0.788)	(0.738)	(0.876)		
Number of firms' tasks	3.308	3.350	3.476	0.997	535
	(1.484)	(1.637)	(1.599)		
Manufacturing sector	0.346	0.343	0.314	0.006	535
	(0.477)	(0.476)	(0.465)		
Ever offered internships	0.643	0.650	0.552	0.966	535
	(0.480)	(0.479)	(0.498)		
Ever hired a migrant or refugee	0.357	0.343	0.376	0.625	535
	(0.480)	(0.476)	(0.486)		
Beliefs about refugees' test score	64.390	65.895	62.705	0.462	535
	(14.241)	(14.832)	(16.013)		
Supports refugees' empl. rights	0.934	0.909	0.924	0.287	535
	(0.249)	(0.288)	(0.266)		
Jobs to locals first	3.429	3.336	3.305	0.486	535
	(1.276)	(1.216)	(1.299)		
WTP at baseline	17.445	16.608	16.881	0.709	535
	(20.724)	(20.242)	(17.646)		

Notes: This table reports a balance test of baseline characteristics among treated firms versus control firms. Treated firms are divided into those for which the internship successfully took place and those for which the internship did not take place because the refugee worker did not attend the appointments. Successful matches (*Match*): 182 firms; Unsuccessful matches (*No match*): 143 firms; Control group: 210 firms. First, second and third columns report group means. Fourth column reports the p-value of a test of equality of coefficients β_1 and β_2 from the following linear regression: $y_i = \beta_0 + \beta_1 \mathbb{1}(match) + \beta_2 \mathbb{1}(nomatch) + X_i^{'}\delta + \varepsilon_i$, where characteristic y_i is regressed over an indicator equal to 1 for *Match* firms ($\mathbb{1}(match)$), an indicator equal to 1 for *No match* firms ($\mathbb{1}(nomatch)$), and strata fixed effects ($X_i^{'}$).

Table A.4. 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	0.002	-0.015	0.012	0.003	-0.045	0.024
	(0.011)	(0.029)	(0.039)	(0.012)	(0.034)	(0.045)
Control	0.981	0.886	0.762	0.981	0.886	0.762
Firms	525	474	407	385	343	299

Notes: This table investigates whether attrition at follow-up surveys and endline are differential across treatments. It reports the coefficients for 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. "Control" reports share of firms reached in control at each wave. "Firms" reports number of total firms reached at each wave. ***, **, *, indicate significance at the 1%, 5%, and 10% levels respectively.

Table A.5. Randomization Balance

Variable	Treatment N	Treatment Mean	Treatment SD	Control N	Control Mean	Control SD	Diff.
Panel A: Balance at baseline							
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
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
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: Balance at endline							
Employer is a woman	247	0.530	0.500	160	0.512	0.501	-0.056*
Firm age	247	8.178	6.930	160	8.325	6.554	0.000
Firm is formal	247	0.186	0.390	160	0.169	0.376	0.018
Has a vacancy	247	0.425	0.495	160	0.350	0.478	0.083
Desires expand in the future	247	0.842	0.365	160	0.863	0.345	-0.040
Employees at baseline	247	2.478	3.134	160	2.700	3.196	0.189
Manufacturing sector	247	0.377	0.485	160	0.306	0.462	-0.019
Ever offered internships	247	0.632	0.483	160	0.531	0.501	0.093*
Ever hired a migrant or refugee	247	0.352	0.479	160	0.394	0.490	-0.036
Beliefs about refugees' test score	247	64.729	15.270	160	63.200	14.990	0.976
Supports refugees' empl. rights	247	0.915	0.279	160	0.913	0.283	0.005
Jobs to locals first	247	3.360	1.273	160	3.381	1.293	-0.023
WTP at baseline	247	17.409	21.121	160	16.281	15.863	2.246

Notes: This table reports balance tests of baseline firm characteristics. Panel A shows balance across the full baseline sample (N=535). Panel B shows balance among firms successfully interviewed at endline (N=407, approximately 24 months post-intervention). The table reports observations, means, and standard deviations for treatment and control groups, and the coefficient β_1 from the regression: $y_i = \beta_0 + \beta_1 \text{Treated}_i + X_i'\delta + \varepsilon_i$, where y_i is a baseline characteristic and Treated_i indicates assignment to treatment. X_i' includes randomization strata fixed effects (15 occupation categories). Standard errors are clustered at the refugee level. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A.6. Testing the Exclusion Restriction: Promised, No Intern vs. Control

	Index soft skills	Index hard skills	Priority to Ugandans	New internships at follow-up 2		New workers at endline	
	(1)	(2)	(3)	(4) At least 1 refugee	(5) Total refugees	(6) At least 1 refugee	(7) Total refugees
Promised only	-0.050	-0.106	-0.118	0.011	0.048	0.028	0.040
	(0.091)	(0.085)	(0.118)	(0.026)	(0.044)	(0.025)	(0.030)
	[0.578]	[0.217]	[0.318]	[0.669]	[0.279]	[0.262]	[0.181]
Firms	346	346	346	316	316	266	266
Mean DV			-0.000	0.043	0.048	0.037	0.037

Notes: This table tests the exclusion restriction for the instrumental variables analysis by comparing outcomes between firms that were assigned to treatment but did not receive an intern (because the matched refugee failed to attend, N=143) and control firms (N=210). *Dependent variables*: Column 1: Soft skills beliefs index (standardized). Column 2: Hard skills beliefs index (standardized). Column 3: In-group bias measuring agreement with "When jobs are scarce, Ugandans should have more right to a job than refugees" (standardized, higher values indicate stronger in-group preference). Columns 4-5: New refugee internships offered at follow-up 2 (approximately 8 months post-intervention): dummy for at least one internship and total number. Columns 6-7: New refugee workers still employed at endline (approximately 24 months post-intervention): dummy for at least one hire and total number. All refugee hiring outcomes exclude the worker matched during the experimental internship. The null findings across all outcomes support the validity of the exclusion restriction. *Controls*: 15 randomization strata fixed effects. Standard errors in parentheses are clustered at the refugee level. P-values in square brackets. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A.7. Business Outcomes and Productivity

	Profits			Profits per worker		
	(1) Follow-up 1	(2) Follow-up 2	(3) Endline	(4) Follow-up 1	(5) Follow-up 2	(6) Endline
Panel A: ITT						
Treated	-0.189	0.093	-0.144	-0.021	-0.007	-0.041
	(0.142)	(0.085)	(0.126)	(0.032)	(0.019)	(0.030)
	[0.182]	[0.273]	[0.255]	[0.511]	[0.703]	[0.172]
Firms	456	420	362	456	420	362
Mean DV	0.748	0.514	0.841	0.222	0.149	0.241
Panel B: LATE						
Exposed	-0.343	0.168	-0.247	-0.038	-0.013	-0.071
	(0.251)	(0.150)	(0.212)	(0.056)	(0.034)	(0.051)
	[0.173]	[0.263]	[0.245]	[0.502]	[0.697]	[0.163]
Firms	456	420	362	456	420	362
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 millions 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). Standard errors are clustered at the refugee level. P-values reported in square brackets. ***, **, *, indicate significance at the 1%, 5%, and 10% levels respectively.

Table A.8. 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 refugee level. Finally, last column reports the p-value of this coefficient, corrected using a Multiple Hypothesis Testing correction as in List et al. 2019.

Appendix B. Conceptual Framework: Learning, Attitudes, and Match Quality

B.1. **Motivation.** Section 4.4 presents a baseline Bayesian learning framework in which exposure generates an informative signal about refugee productivity, leading employers to update their beliefs and increase hiring. While this framework successfully predicts average treatment effects on beliefs and hiring, it cannot explain the sharp heterogeneity we document in Section 6: belief updating and hiring effects are concentrated among matches where both the employer and the refugee worker have more tolerant baseline attitudes toward cross-group cooperation.

In this appendix, we extend the baseline framework to incorporate two key features absent from standard learning models:

(1) **Attitudes affect the cost of investing in learning**: Employers with more negative attitudes toward refugees face higher psychological costs when investing attention and effort in evaluating refugee performance. This could reflect greater mental effort required to bridge cultural or linguistic gaps, discomfort from extended interaction with out-group members, or lower intrinsic motivation to invest in learning about workers they are predisposed to distrust.

(2) **Attitudes affect workers' willingness to demonstrate ability**: Refugees with more negative attitudes toward the host community—perhaps due to past experiences of discrimination or pessimism about integration prospects—face higher costs of exerting effort to demonstrate their abilities. These workers may perceive lower returns to performing well during the internship.

This extended framework generates predictions about which types of matches will produce the strongest learning and hiring effects, consistent with the heterogeneity patterns we document empirically.

B.2. **Setup.**

B.2.1. *Employer Problem.* The employer's expected utility from engaging with a refugee worker is:

$$E(U(\tau, e_f)) = E(\Pi) - \frac{1}{2}g(\eta)Var(\Pi) + \beta V(m_1, \sigma_1^2) - c(\delta) \cdot e_f \qquad \text{(B.1)}$$

where:

- $\Pi = \tau(\theta + \epsilon) - w$ is profit, with θ the average productivity of refugee workers, $\epsilon \sim N(0, \sigma_\epsilon^2)$ the individual-specific productivity component, $\tau \in [0, 1]$ the task complexity assigned, and w the wage
- $g(\eta)$ captures risk aversion

- $V(m_1, \sigma_1^2)$ is the continuation value from holding posterior beliefs (m_1, σ_1^2) about θ, representing the expected benefit of more accurate beliefs for future hiring decisions. We assume $\frac{\partial V}{\partial(1/\sigma_1^2)} > 0$, so that more precise beliefs facilitate better future hiring choices, and $\frac{\partial V}{\partial m_1} > 0$ when $m_0 < \theta$, so that learning refugees are more productive than expected increases future hiring value
- $\beta \in (0, 1)$ is a discount factor
- $c(\delta) > 0$ is the per-unit cost of learning effort e_f, where δ represents the employer's psychological cost of investing effort in relationships with out-group members. Higher δ indicates lower openness to cross-group cooperation and implies higher marginal cost of learning effort: $\frac{\partial c}{\partial \delta} > 0$. This captures the difficulty of engaging cooperatively across group boundaries. For instance, the mental effort required to communicate across cultural or linguistic barriers, or discomfort working closely with individuals from unfamiliar backgrounds.
- $e_f \geq 0$ is the employer's learning effort (time spent supervising, evaluating, and providing feedback)

The first two terms capture the employer's payoff during the one-week internship: expected profit minus the disutility from variance in profit due to risk aversion. The continuation value $\beta V(m_1, \sigma_1^2)$ captures the benefit of learning about refugee productivity for future hiring decisions. This term is central to understanding why employers invest learning effort: while e_f appears as a cost in the immediate objective, it generates benefits through more precise posterior beliefs that improve future hiring outcomes. The employer chooses task complexity τ and learning effort e_f to maximize this full objective, trading off the immediate costs of engagement against both current-period variance reduction and the long-run value of information.

Task complexity affects both expected productivity and the informativeness of the signal about worker ability. Crucially, τ and e_f are complementary in the learning technology: complex tasks generate informative signals only when the employer invests sufficient attention to observe and interpret performance. This complementarity means that employers anticipating low learning effort (due to high δ) optimally assign simpler tasks, as complex tasks would be uninformative without careful supervision.

B.2.2. *Refugee Problem.* The refugee worker chooses effort $e_r \geq 0$ to maximize expected utility:

$$E(U(e_r)) = p(\tau, s) \cdot V_{employment} - c_r \cdot e_r \tag{B.2}$$

where:

- $p(\tau, s)$ is the probability of future employment, which depends on the task complexity assigned (τ) and the signal generated (s)
- $V_{employment}$ is the value of employment to the refugee
- $c_r > 0$ is the per-unit cost of effort, which depends on the refugee's baseline attitudes toward the host community. Refugees with positive attitudes (low c_r) perceive higher returns to integration and are more willing to invest effort in demonstrating their skills. Refugees with negative attitudes (high c_r)—due to past discrimination, distrust, or pessimism about their prospects—exert less effort because they perceive lower returns or face greater psychological costs of engagement.

B.2.3. *Timing and Decision Structure.* We assume the employer observes signals of the refugee's baseline attitudes (which affect c_r) before choosing τ and e_f. In practice, these signals emerge during the initial meeting when the internship is arranged, through the refugee's expressed enthusiasm, communication style, or willingness to discuss integration. The employer and refugee then make their effort choices simultaneously during the internship, with the employer choosing learning effort e_f and the refugee choosing work effort e_r. However, the employer's task assignment τ is made anticipating the equilibrium levels of both e_f (which depends on the employer's own δ) and e_r (which depends on the refugee's c_r, inferred from pre-internship interactions). This sequential-then-simultaneous structure explains why the refugee's type affects the employer's task allocation: employers matched with negative-attitude refugees (high c_r) anticipate low refugee effort and respond by assigning simpler tasks, which in turn reduce their own optimal learning effort.

B.2.4. *The Learning Technology.* The employer observes output during the internship:

$$y = \tau(\theta + \epsilon) + \nu(e_f, e_r) \tag{B.3}$$

where:

- $\tau \in [0, 1]$ is task complexity
- θ is the average productivity of refugee workers (the object the employer wants to learn about)
- $\epsilon \sim N(0, \sigma_\epsilon^2)$ is the individual refugee's deviation from the group mean—a source of noise in learning about θ
- $\nu(e_f, e_r) \sim N(0, \sigma_\nu^2(e_f, e_r))$ is measurement error, where σ_ν^2 decreases in both employer learning effort e_f and refugee work effort e_r: $\frac{\partial \sigma_\nu^2}{\partial e_f} < 0$ and $\frac{\partial \sigma_\nu^2}{\partial e_r} < 0$

Interpretation: Complex tasks (high τ) amplify the productivity component $\tau(\theta + \epsilon)$ relative to the additive noise ν. This makes underlying productivity more visible: a given level of measurement error ν becomes relatively less important when the true signal $\tau(\theta+\epsilon)$ is scaled up by task complexity.

The employer holds prior beliefs about **the group-level productivity parameter** θ, distributed $N(m_0, \sigma_0^2)$. The employer wants to learn about θ because this determines the expected productivity of any refugee they might hire in the future, not just the specific intern.[1]

To infer underlying productivity θ from observed output y, the employer computes productivity per unit of task difficulty:

$$s = \frac{y}{\tau} = (\theta + \epsilon) + \frac{\nu(e_f, e_r)}{\tau} \tag{B.4}$$

The normalization y/τ formalizes the employer's intuitive adjustment for task difficulty: when assigning a complex task (high τ) and observing high output y, the employer doesn't conclude "this worker is exceptionally productive," but rather "this output level is reasonable given the task difficulty." The normalized signal s is an unbiased signal of θ with total noise variance:

$$\sigma^2_{noise} = \sigma^2_\epsilon + \frac{\sigma^2_\nu(e_f, e_r)}{\tau^2} \tag{B.5}$$

This noise has two components. First, σ^2_ϵ represents fundamental uncertainty about whether this particular refugee is representative of the group. This component is irreducible—even with perfect observation ($\sigma^2_\nu \to 0$), the employer cannot know whether this specific refugee is above-average, average, or below-average relative to other refugees. Second, $\sigma^2_\nu(e_f, e_r)/\tau^2$ represents measurement error in observing this refugee's performance. This component decreases when task complexity τ increases (measurement errors become relatively less important), when employer effort e_f increases (reduces σ^2_ν), or when refugee effort e_r increases (reduces σ^2_ν).[2]

[1]If learning were only about the specific match $(\theta + \epsilon)$, employers would have strong incentives to retain workers who performed well during the internship. Yet only 3.9% do so (Table 1), despite 67% reporting willingness to rehire the same worker. This gap suggests that the binding constraint is not information about the specific match but rather the creation of permanent positions. Employers learn that refugees are generally productive (updating beliefs about θ), which makes them willing to consider refugees when future vacancies arise, but they don't immediately create positions to retain the specific intern. This interpretation is consistent with our finding that treated firms hire from the broader refugee community over a two-year period (Table 2) and that total employment doesn't increase (Table 2, column 9).

[2]We assume that refugee effort increases the informativeness of the signal by reducing σ^2_ν. An alternative interpretation is that effort affects the level of output observed, while the employer must infer whether high output reflects high effort or high underlying ability. Our framework abstracts from this inference problem, implicitly assuming employers can distinguish effort from ability with some noise, and that higher effort makes ability more observable. This is consistent with our setting where employers supervise interns closely—Table 1 shows employers spend an average of 5 hours per day supervising—and can observe both output and work process, not just final output.

The employer's posterior beliefs about θ are distributed $N(m_1(\tau, e_f, e_r), \sigma_1^2(\tau, e_f, e_r))$, where:

$$\sigma_1^2(\tau, e_f, e_r) = \frac{\sigma_0^2 \cdot [\sigma_\epsilon^2 + \sigma_\nu^2(e_f, e_r)/\tau^2]}{\sigma_0^2 + [\sigma_\epsilon^2 + \sigma_\nu^2(e_f, e_r)/\tau^2]} \tag{B.6}$$

$$m_1 = \alpha(\tau, e_f, e_r) \cdot s + (1 - \alpha(\tau, e_f, e_r)) \cdot m_0 \tag{B.7}$$

with

$$\alpha(\tau, e_f, e_r) = \frac{\sigma_0^2}{\sigma_0^2 + \sigma_\epsilon^2 + \sigma_\nu^2(e_f, e_r)/\tau^2} = \frac{\sigma_1^2}{\sigma_{noise}^2} \tag{B.8}$$

representing the weight placed on the signal. Higher task complexity, employer learning effort, and refugee work effort all increase the precision of the signal (reduce σ_{noise}^2) and thus increase α.

B.3. **Predictions.** The extended framework with attitudes generates clear predictions about how treatment effects should vary across match types. These predictions follow directly from the comparative statics of the learning technology described above.

Prediction 1: Positive Matches Generate Strong Learning. Consider a match between an employer with positive attitudes (low δ, low cost of engagement) and a refugee with positive attitudes (low c_r, low cost of effort). In such matches:

(1) The employer assigns more complex tasks (τ closer to 1), which reduces the measurement error component σ_ν^2/τ^2 by making measurement noise less important relative to the amplified productivity signal.
(2) The employer invests more learning effort (e_f high), which directly reduces σ_ν^2, as the psychic cost of engagement is low.
(3) The refugee exerts high work effort (e_r high), which directly reduces σ_ν^2, as the perceived returns to demonstrating ability are high.
(4) All three channels reinforce: the total noise variance $\sigma_{noise}^2 = \sigma_\epsilon^2 + \sigma_\nu^2(e_f, e_r)/\tau^2$ decreases substantially, increasing the signal weight $\alpha(\tau, e_f, e_r)$ and the precision of posterior beliefs $1/\sigma_1^2$.

The direction of belief updating depends on the relationship between priors and realized signals. Given that employers hold pessimistic priors about refugee productivity (as documented in Figure 3, where employers expect refugees to score approximately 64 on the standardized skills assessment when actual scores average 84), the increased signal weight $\alpha(\tau, e_f, e_r)$ in positive matches leads to *positive* belief updating. That is, when $m_0 < \theta$ and the signal s reflects true productivity θ, higher precision amplifies the upward revision of beliefs.

Predicted outcomes: Strong positive belief updating about θ (refugees' average productivity); high probability of subsequent hiring from the broader refugee population.

Prediction 2: Negative Matches Generate Limited Learning. Consider a match between an employer with negative attitudes (high δ, high cost of engagement) and a refugee with negative attitudes (high c_r, high cost of effort). In such matches:

(1) The employer assigns less complex tasks (τ closer to 0), which increases the relative importance of measurement error σ_ν^2/τ^2.
(2) The employer invests minimal learning effort (e_f low), which fails to reduce σ_ν^2, as the psychic cost of engagement is high.
(3) The refugee exerts low work effort (e_r low), which fails to reduce σ_ν^2, as the perceived returns to integration are low.
(4) All three channels reinforce negatively: the total noise variance $\sigma_{noise}^2 = \sigma_\epsilon^2 + \sigma_\nu^2(e_f, e_r)/\tau^2$ remains high, yielding a low signal weight $\alpha(\tau, e_f, e_r)$ and imprecise posterior beliefs.

Predicted outcomes: Minimal belief updating about θ; no increase (or potentially decrease) in subsequent hiring.

Prediction 3: Mixed Matches Have Ambiguous Effects. Consider matches where only one party has positive attitudes. Two cases arise:

- **Positive employer, negative refugee**: Employer assigns complex tasks and invests learning effort (high τ, high e_f), but refugee exerts low effort (low e_r) $\Rightarrow$ moderately noisy signal.
- **Negative employer, positive refugee**: Refugee exerts high effort (high e_r), but employer assigns simple tasks and invests little learning effort (low τ, low e_f) $\Rightarrow$ moderately noisy signal.

In both cases, the signal is more informative than in negative matches but less informative than in positive matches. The net effect on belief updating and hiring is ambiguous and depends on which force dominates: the positive employer's task assignment and attention, or the positive refugee's effort investment.

Predicted outcomes: Moderate belief updating; modest increase in hiring (between positive and negative matches).

Comparative Statics Summary. The key comparative statics from the learning technology translate directly into predictions about match-type heterogeneity:

$$\frac{\partial \alpha}{\partial \tau} > 0, \quad \frac{\partial \alpha}{\partial e_f} > 0, \quad \frac{\partial \alpha}{\partial e_r} > 0$$

Because positive attitudes lead to higher τ, e_f, and e_r, they generate more informative signals, stronger belief updating, and larger hiring effects. Crucially, even in positive matches with high τ, e_f, and e_r, the employer faces irreducible uncertainty (σ_ϵ^2) about whether this particular refugee is representative of the broader refugee population. This explains why:

- **Learning is gradual and partial**: The signal contains substantial noise from σ_ϵ^2, so beliefs update partially rather than converging to certainty after one observation.
- **Firms hire from the broader community**: They learned about the group parameter θ, which applies to all refugees, not just the specific match.
- **Effects persist over time**: Updated beliefs about θ guide future hiring decisions whenever opportunities arise.

B.4. **Empirical Implications.** This extended framework generates several testable predictions:

(1) **Task assignment**: Employers with more positive attitudes should assign more complex tasks to refugee interns, particularly when matched with positive-attitude refugees (tested in Figure 5, Panel E).

(2) **Supervision intensity**: Employers with more positive attitudes should invest more time supervising interns (tested in Figure 5, Panel C), though the higher effort levels from both parties may make supervision less psychologically demanding (tested in Figure 5, Panel B).

(3) **Belief updating**: Belief updating should be concentrated among positive matches, with limited updating in negative or mixed matches (tested in Table 6).

(4) **Hiring**: Treatment effects on hiring should be strongest for positive matches, weakest for negative matches, with mixed matches showing intermediate effects (tested in Table 6).

(5) **Match satisfaction**: Positive matches should report higher willingness to continue the relationship, reflecting the more informative signals and positive interaction quality (tested in Figure 5, Panel A).

The heterogeneity analysis in Section 6 provides strong support for these predictions, suggesting that workplace contact operates not as a mechanical information revelation process, but as an interaction whose informativeness depends critically on the willingness and ability of both parties to invest effort in cooperation.

B.5. **Connection to Contact Theory.** This framework bridges economics and social psychology by formalizing key insights from contact theory (Allport, 1954):

- **Cooperation condition**: Contact is most effective when both parties are willing to engage. Our model captures this through the effort choices e_f and e_r, whose costs depend on baseline attitudes (δ and c_r).

- **Common goals**: Contact works best when both parties share objectives and are invested in success. In our setting, both must be willing to invest effort for learning to occur: the employer must assign meaningful tasks and observe carefully; the refugee must demonstrate abilities earnestly.
- **Positive vs. negative contact**: The model formalizes why some contact experiences reinforce prejudice (negative matches produce uninformative signals, confirming priors) while others reduce it (positive matches produce informative signals that correct pessimistic beliefs).

Importantly, the framework clarifies that "positive contact" in our setting is not merely about having a pleasant interaction. Rather, it requires both parties to invest effort in a cooperative endeavor where willingness to engage determines how much information the interaction generates—precisely what we observe empirically in the data on task assignment, supervision, belief updating, and subsequent hiring.

B.6. **Limitations and Extensions.** This framework abstracts from several features that may be important in practice:

(1) **Network effects**: Employers who hire refugees may influence other employers' beliefs through social learning or norm diffusion. We find suggestive evidence of this in Table 3 (column 4), where treated firms are more likely to have connections to refugee-led organizations, potentially facilitating information transmission to other employers.
(2) **General equilibrium**: We focus on partial equilibrium employer decisions. Scaling up the intervention could affect refugee reservation wages, Ugandan workers' employment prospects, or the distribution of attitudes in the employer population.
(3) **Dynamic learning**: We model a single learning episode. In practice, employers may learn from repeated interactions with multiple refugees, and refugees may update their effort choices based on past experiences with Ugandan employers. Such dynamic learning could amplify or dampen the effects we estimate.
(4) **Selection**: Our analysis conditions on refugees who are willing to participate in the internship program and firms that express willingness to host an intern. The framework does not model these initial participation decisions, though our empirical analysis (Appendix Figure A.2) shows that refugee participation is primarily determined by distance rather than unobservable motivation.
(5) **Multidimensional productivity**: The model treats productivity as unidimensional. In practice, refugees may have comparative advantages in some tasks but not others, and employers may learn differentially across skill dimensions. Our evidence on soft versus hard skills (Table 4 versus Table 5) suggests that learning is indeed dimension-specific.

SUPPLEMENTAL APPENDIX

Appendix SA.1. Causal Forest

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

Causal forest is a machine learning method that allows to predict the heterogeneity in the causal treatment effect. More precisely, it estimates the CATE, that is the average treatment effect conditional on a vector of baseline covariates: $\tau(X) = E[Y_{1i} - Y_{0i}|X = x]$, 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.[1] 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 S.1 and S.2 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 labour 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

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

number of employees, number of tasks and number of business premises); a dummy equal to 1 if the firm's sector is manufacturing; beliefs regarding the skills of the matched worker; the worker's ability; attitudes towards Ugandans and Ugandan culture; knowledge of languages; their experience with Ugandan employers in the 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). Supplement Appendix Table S.3 reports the results from the best linear projector estimation.

Finally, Supplement Appendix Figure S.5 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 S.1. 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 S.2. Refugees' Characteristics Feeding Causal Forest

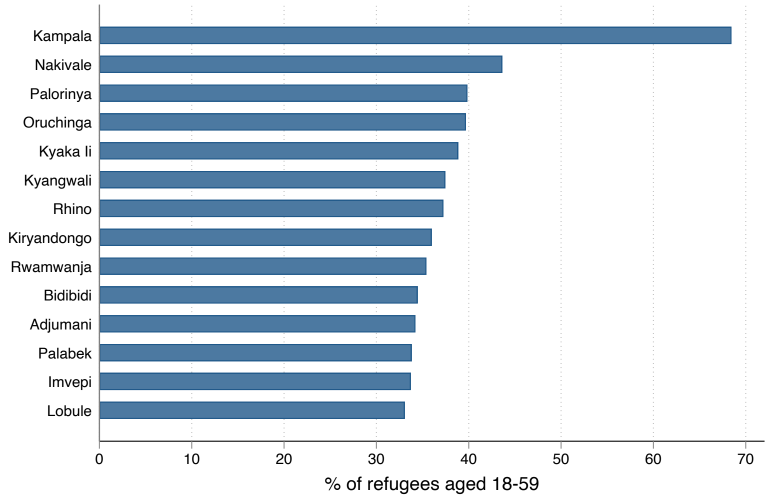
Index/Variable	**Description**
Ability	Factor analysis of: • Worker's test score • Years of experience • Years of education • Cognitive skills (Raven's Progressive Matrices) Dummy=1 if index value is above median
Attitudes	Factor analysis of: • Agreement with "Ugandans discriminate against refugees." • Agreement with "Ugandans have the best intentions." • Agreement with "Ugandans and refugees should collabourate." • 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 S.3. Best Linear Projector of CATE

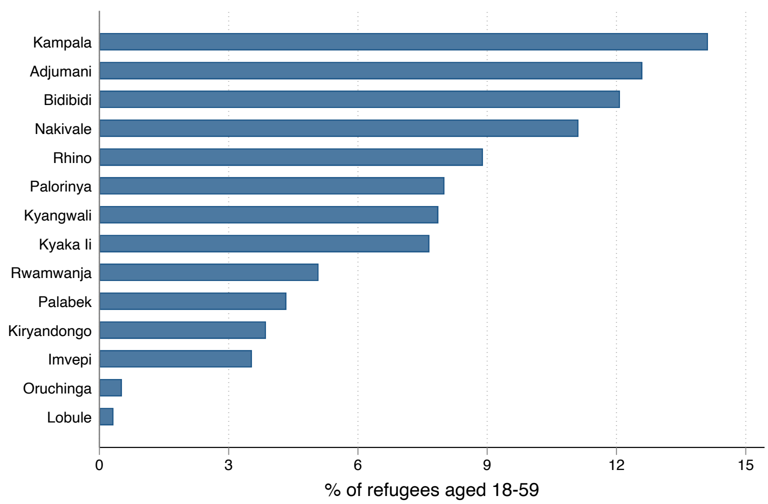
	Best Linear Projector of CATE			
	Beta	SE	t-stat	p-value
Intercept	-.47	.356	-1.32	.187
Refugee's ability	-.035	.104	-.334	.739
Refugee's attitudes	.259	.106	2.446	.015
Refugee knowledge of languages	-.158	.167	-.941	.347
Refugee's age	-.001	.006	-.161	.872
Refugee is Congolese	.042	.162	.257	.798
Refugee 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).

FIGURE S.1. Refugees in Uganda



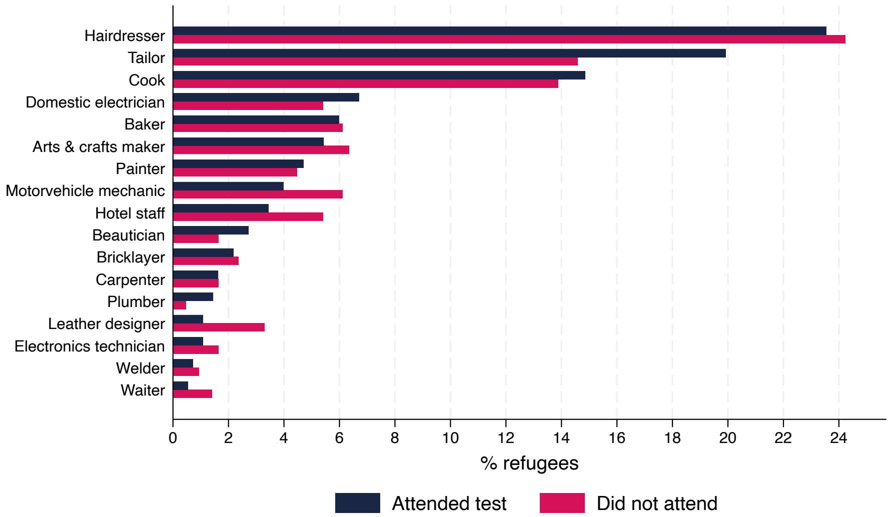
(**A.**) Working-age population of refugees for each settlement



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

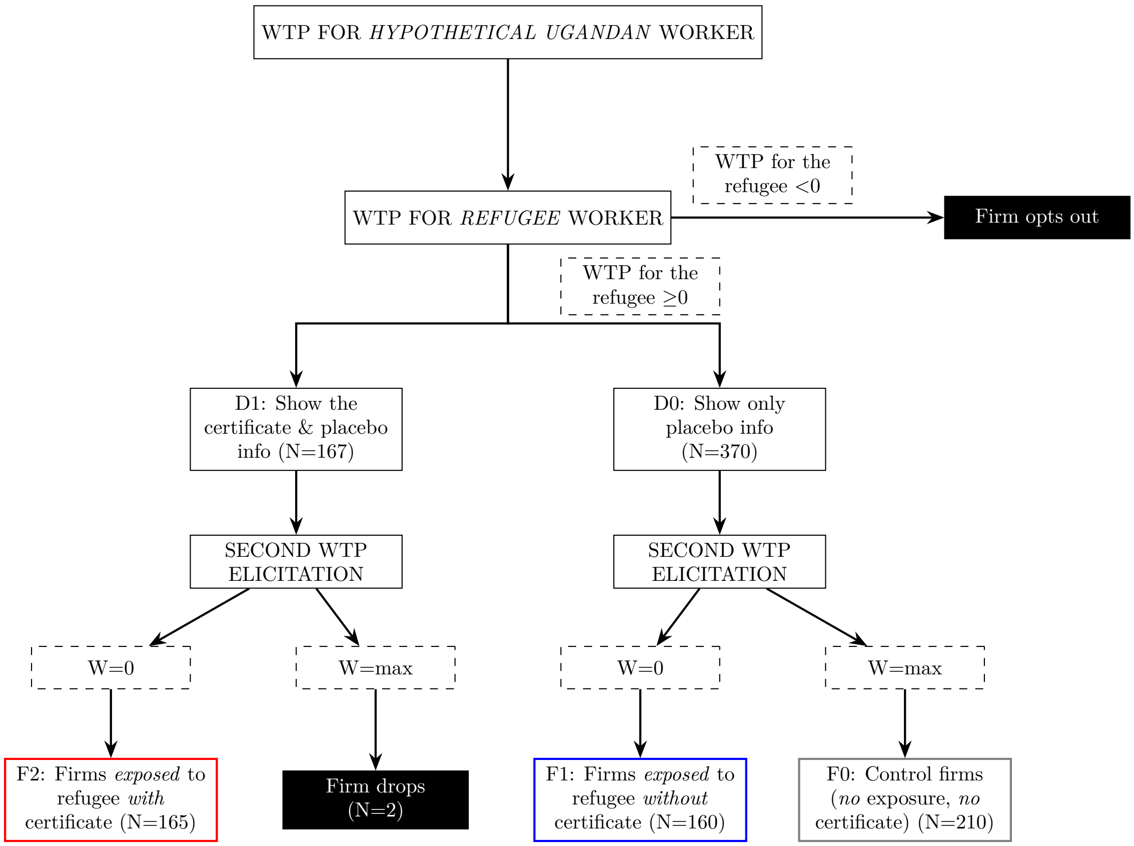
Notes: This graph plots descriptive statistics of the refugee population in Uganda as of end of 2022. Data comes from UNHCR Uganda accessed in October 2022 (see `https://data.unhcr.org/en/country/uga`). Panel (A) shows the distribution of working-age refugees across each registered place of residence.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

Figure S.2. Refugees' Skills by Test Attendance



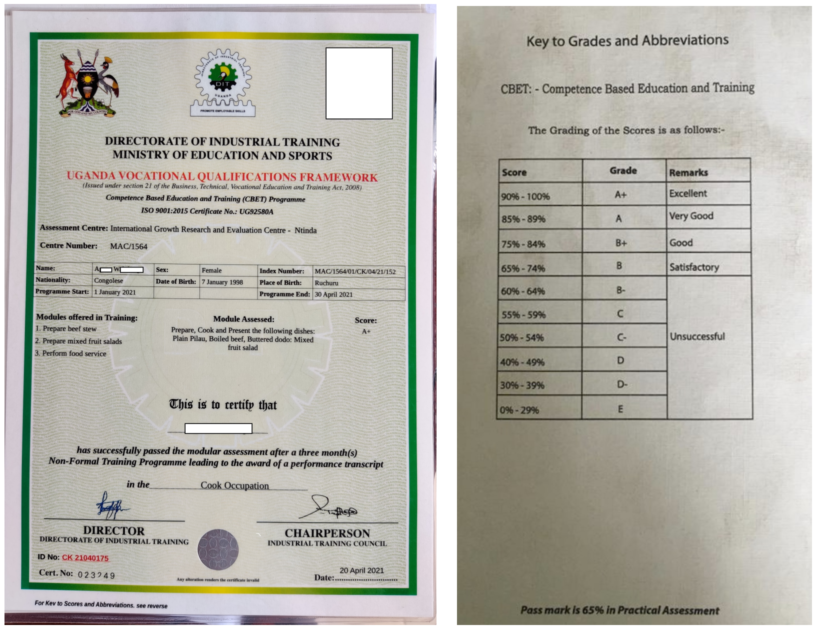
Notes: This graph plots the percentage of refugee workers listed by their skills and exam attendance. Out of 1,088 refugees listed, 977 were invited to the test. Among them, 552 attended the test (dark blue bars), and 425 did not (red bars).

Figure S.3. 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 S.4. 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: 023249

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:-

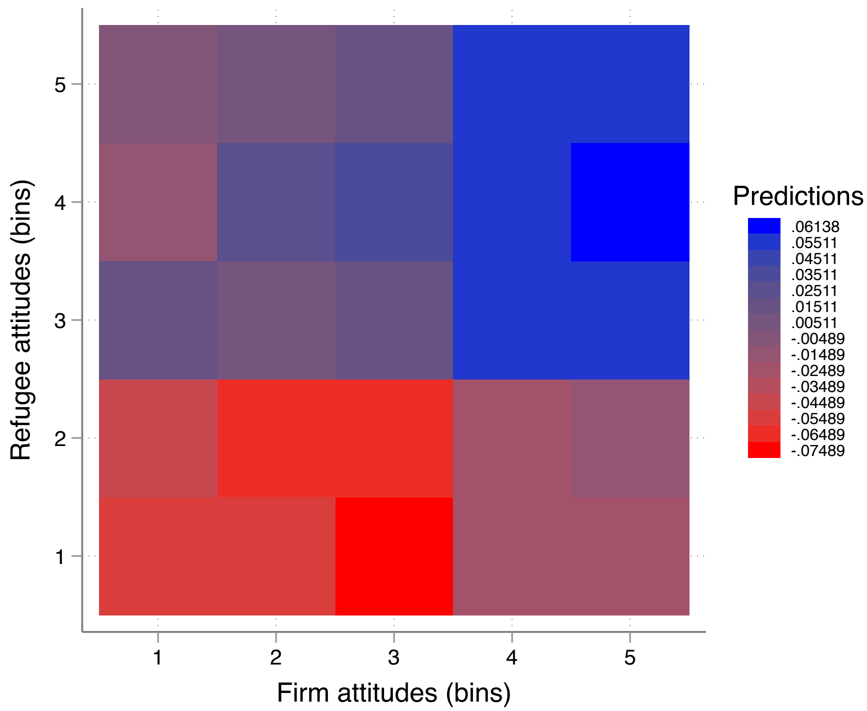
Score	Grade	Remarks
90% - 100%	A+	Excellent
85% - 89%	A	Very Good
75% - 84%	B+	Good
65% - 74%	B	Satisfactory
60% - 64%	B-	Unsuccessful
55% - 59%	C	
50% - 54%	C-	
40% - 49%	D	
30% - 39%	D-	
0% - 29%	E	

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 S.5. Predicted CATE



Notes: This graph shows a heat map of the predicted Conditional Average Treatment Effect (CATE). The causal forest uses median-split dummies for employer and refugee attitudes (as described in Appendix B). For visualization purposes, this heatmap groups observations by quartiles of the underlying continuous attitude indices (X-axis: employer attitudes; Y-axis: refugee attitudes). Within-quartile variation in predicted CATE reflects both the high/low attitude dummies and other covariates in the forest. 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.