

# Right to Work and Refugee Economic Integration: Experimental Evidence from Ethiopia\*

## Pre-Analysis Plan

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## 1 Project Summary

By mid-2024, the global refugee population exceeded 43 million, with approximately 73 percent residing in low- and middle-income countries (UNHCR, 2024). Protracted displacement affects 66 percent of refugees, with average displacement durations ranging from 10 to 26 years (UNHCR, 2024). Despite this extended timeframe, refugees face substantial legal barriers to employment in most host countries (Ginn, Resstack, Dempster, Arnold-Fernández, Miller, Guerrero Ble, and Kanyamanza, 2022). These restrictive policies often stem from government efforts to protect host communities from perceived adverse effects of expanding the labor supply. At the same time, these restrictions impose dual costs: refugees remain dependent on humanitarian assistance while host economies forgo potential productivity gains and tax revenues.

This project aims to provide the first experimental evidence on the causal effects of granting refugees the right to work (through work permits and business licenses) on their economic integration. In collaboration with Ethiopia's Refugees and Returnees Service (RRS), we are implementing a large-scale randomized controlled trial across four refugee camps, in two different regions of the country, randomly assigning support for obtaining work permits and business licenses to 8,000 refugees. Ethiopia provides an ideal setting for this research: it hosts over 1 million refugees and has recently established a legal framework for refugee work rights that remains largely unimplemented due to administrative barriers.

The project aims to make three primary contributions. First, we hope to provide experimental evidence on the direct effects of work authorization on the employment, earnings, and welfare of refugees and their households. Second, we expect to examine mechanisms including formalization of existing work, allocation decisions across job types and sectors, and psychosocial impacts. Third, our design will enable analysis of heterogeneous effects across camps (which host different refugee populations), gender, baseline mental health, and potential skills-market match, generating policy-relevant insights for diverse implementation contexts.

This pre-analysis plan focuses on the first phase of a broader research program: evaluating the direct effects of work permits on refugees' in-situ economic activities within and around camps. We are concurrently developing a complementary second phase that will examine location-specific work permits, which would facilitate refugees to relocation to designated host communities for employment.

## 2 Experimental Design

### 2.1 Sampling and Recruitment of Participants

Our population of interest is recognized refugees living in four government refugee camps across Ethiopia. Three camps are located in Benishangul-Gumuz region (Bambasi, Sherkole, and Tsore) and one camp is located in the Somali region (Sheder). The selection of these refugee camps was based on a comprehensive field scoping activity that assessed camps across five regions including Gambela, Amhara, Afar, Benishangul-Gumuz, and Somali regions. Initially, five camps were considered for the study, but Aw Barre in Somali Region was subsequently dropped as most residents had already started receiving work permits and business licenses after our scoping mission but before the implementation of the study.

Given distinct implementation environments across camps – including variation in local labor markets, refugee demographics, and administrative capacity – we conduct all analyses separately by camp while also reporting pooled estimates with camp fixed effects for overall program effects.

As economic behavior is a key focus of our research, we define eligible individuals as recognized refugees aged 18 to 65 years who expressed interest in receiving work permits or business licenses during our census listing of each camp. A household is eligible for our study if it contains at least one individual meeting these criteria.

The sampling protocol follows a two-stage design. First, we enumerate all households within each camp through door-to-door listing. To ensure accurate population coverage, we conduct a complete census rather than relying on administrative records, which often contain inaccuracies due to population mobility.

Second, for our baseline survey, we will construct our sample from this listing data by selecting a random sample of eligible households in each camp, where a household is eligible if it includes at least one eligible member, as defined above. From each eligible household, we will sample up to two eligible individuals (working-aged refugees interested in work permits or business licenses). If the household head is eligible, they will be included by default as one of the two sampled individuals. If the household head is not eligible, we will randomly select up to two other eligible individuals from the household. When a household has only one eligible member, only that individual will be included in our sample.

## **2.2 Design, Interventions, and Treatment Assignment**

### **Overall Experimental Design**

Building on our close collaboration with the Government of Ethiopia's Refugees and Returnees Service (RRS) and the World Bank, our project implements a randomized controlled trial to evaluate the impact of work permits and business licenses on outcomes of refugees and their households across four camps in Ethiopia (Bambasi, Sherkole, Tsore, and Sheder). We randomly assign eligible refugee households to treatment status. Treated households will then have one eligible member randomly selected to receive support for obtaining work permits or business licenses. Baseline data collection is scheduled to take place from May to July 2025. Randomization and implementation will follow immediately after the conclusion of the baseline data collection. After that, we aim to conduct twelve monthly high-frequency phone check-ins with selected participants and an in-person endline survey approximately in October 2026, after the end of the monthly check-ins.

### **Intervention**

While Ethiopia has established the legal framework for refugees' right to work, significant implementation barriers remain: widespread lack of awareness among refugees about their rights, obstacles to obtaining required documentation, and unclear administrative procedures for permit issuance.

As preparation for our study, our team conducted an awareness campaign in all selected camps (Bambasi, Sherkole, Tsore, and Sheder). Working closely with RRS and refugee community leaders, we informed refugees of their legal rights to work, engage in self-employment, and start businesses under Ethiopian law. This campaign disseminated information about the study within refugee communities through flyers and community discussions. A second awareness campaign will be launched after the baseline and right before the intervention, expanding the target audience to include not only refugees in the four camps but also nearby communities and local RRS field offices. This campaign will focus on informing refugees about upcoming project activities and providing detailed guidance on the procedures for applying for work permits and business licenses. Additionally, it will engage communities and RRS officials to ensure a broader understanding of relevant regulations and upcoming interventions.

The randomized treatment in this study is the provision of a comprehensive support package to help refugees apply for and obtain work permits or business licenses. This package includes administrative assistance to obtain required documentation (biometric ID, passport photos, support letter, tax ID), facilitation during application procedures with RRS

and the Ministry of Labor and Skills, and payment of administrative fees associated with the application process.

The intervention is designed for scalability. We work directly with RRS to build administrative capacity and operational protocols that extend beyond the study sample. We support capacity building by providing RRS with policy advice and technical assistance throughout the project. Our awareness campaign materials and monitoring protocols have been refined through field testing and complement existing government programs. The permit support package is being developed and tested in close coordination with RRS field offices, ensuring the intervention can be integrated into existing administrative structures.

### **Treatment Assignment**

Our study employs a two-stage randomization design within each camp. First, we randomly select households from our eligible sample within each camp. Second, within each selected household, we randomly choose one eligible individual to receive the treatment offer. This two-stage approach ensures both statistical power and equitable distribution of opportunities across the refugee population. The randomization proceeds as follows:

1. Household-level randomization: We randomly assign households them to treatment or control groups.
2. Individual-level selection within households: Within treatment households, if multiple members are eligible (aged 18-65 and interested in work permits), we randomly select one individual to receive the support package.

The randomization occurs immediately following baseline data collection. For transparency and community understanding, we will work with RRS to ensure the selection process is perceived as fair and legitimate by all participants. We currently anticipate that this will imply a computer-based randomization at the household level and within household at the individual level.

After randomization, the individuals in the treatment group will receive the support package to apply for work permits or business licenses, while the control group will not receive this support but remains free to pursue these opportunities independently.

### **2.3 Compliers Definition**

We define compliers as individuals who obtain a permit if and only if offered the support package. Given two-sided noncompliance – where treatment group members can refuse the

support package and control group members can pursue permits independently – we identify four latent types: compliers (take up only when treated), always-takers (obtain permits regardless), never-takers (never obtain permits), and defiers (take up only when control). While we cannot identify individual types, under the exclusion restriction and monotonicity assumptions, IV estimates can identify the average treatment effect for compliers – those induced to obtain permits by our intervention. See below for our estimation strategy.

### **3 Data Collection**

The primary data sources for this study include an in-person baseline survey, an in-person endline survey approximately one year after baseline, and phone-based follow-up surveys between these two points. All data collection is implemented by an experienced professional survey firm with expertise in refugee contexts in Ethiopia.

The in-person baseline survey will take place from May to July 2025 across the four selected refugee camps. Between baseline and endline, we aim to implement high-frequency monitoring to capture real-time dynamics of labor market integration and permit utilization, primarily through phone surveys, where feasible, and focusing exclusively on treatment group participants. The in-person endline survey of both treatment and control households will be conducted approximately in October 2026, after the end of the monthly check-ins.

To supplement the individual and household surveys, we will attempt to conduct an establishment survey of local businesses in the selected areas, providing context on local labor market conditions and potential opportunities for refugee employment.

Given the distinct contexts across the four camps, we will analyze all data separately by camp location to account for heterogeneity in refugee experiences and local labor market conditions.

The survey firm will employ enumerators who speak relevant languages of the refugee populations, with comprehensive training on survey protocols, research ethics, and data quality assurance, including audio audits, back-checks, and daily data quality monitoring.

Before beginning data collection, we developed a comprehensive risk management protocol to ensure participant and enumerator safety during the data collection. This included an appropriate referral system, where participants with signs of severe mental illnesses are referred to an appropriate local provider for further assessment. Given the high levels of trauma in our sample, enumerators were trained in identifying problems and solutions to address any situation in which a particular respondent experiences distress.

## 4 Empirical Strategy

### 4.1 Empirical Specifications

Our primary analysis focuses on intent-to-treat (ITT) effects, which capture the causal impact of being offered the permit support package. However, since treatment group members may decline support and control group members may independently pursue permits, we also estimate local average treatment effects (LATE).

We estimate the intent-to-treat effect of our treatment on primary and secondary outcomes, compared to a control group. All ITT specifications take the following form:

$$Y_{ic} = \alpha + \beta \cdot T_{ic} + \gamma \cdot Y_{ic}^0 + \mathbf{X}_{ic}'\delta + \mu_c + \varepsilon_{ic} \quad (1)$$

where  $Y_{ic}$  denotes outcomes for individual  $i$  in camp  $c$ ,  $T_{ic}$  indicates random assignment to treatment,  $Y_{ic}^0$  represents baseline outcomes (when available),  $\mathbf{X}_{ic}$  includes any baseline controls including stratification variables, and  $\mu_c$  are camp fixed effects.

Given two-sided noncompliance in our setting, we also LATE for compliers – individuals induced to obtain permits by our intervention. We instrument actual permit receipt with random assignment to the support package and estimate:

First stage:

$$D_{ic} = \pi + \rho \cdot T_{ic} + \tau \cdot D_{ic} + \mathbf{X}_{ic}'\phi + \mu_c + v_{ic} \quad (2)$$

Second stage:

$$Y_{ic} = \alpha' + \beta' \cdot \hat{D}_{ic} + \gamma' \cdot Y_{ic}^0 + \mathbf{X}_{ic}'\delta' + \mu_c + \varepsilon'_{ic} \quad (3)$$

where  $D_{ic}$  indicates successful permit/license acquisition and  $\hat{D}_{ic}$  represents predicted values from the first stage. Under standard IV assumptions,  $\beta'$  identifies the causal effect of work authorization for compliers.

We estimate both ITT and LATE specifications in two ways: (i) Pooled across camps, including all observations with camp fixed effects  $\mu_c$  to estimate the average treatment effect across all camps. (ii) Separately by camp, estimating the equations for each camp  $c$  independently and dropping  $\mu_c$ , yielding camp-specific treatment effects.

### 4.2 Estimation of Heterogeneous Treatment Effects

Our research design and sample size allow us to examine heterogeneous treatment effects across several policy-relevant subgroups (listed in Section 6 below).

To estimate heterogeneous treatment effects, we will employ a regression framework with interaction terms between treatment assignment and our pre-specified heterogeneity

dimensions. For each heterogeneity dimension  $H_i$ , we will estimate:

$$Y_{ic} = \alpha + \beta \cdot T_{ic} + \gamma \cdot H_{ic} + \delta \cdot (T_{ic} \times H_{ic}) + \lambda \cdot Y_{ic}^0 + \mathbf{X}_{ic}'\theta + \mu_c + \varepsilon_{ic} \quad (4)$$

where  $H_{ic}$  represents the heterogeneity dimension of interest, and  $\delta$  captures the differential treatment effect for individuals with characteristic  $H_{ic}$ . All other notation follows the primary specification. As with our main specification, we estimate this both pooled (with camp fixed effects) and separately by camp.

### 4.3 Multiple Hypothesis Testing

Following [Benjamini, Krieger, and Yekutieli \(2006\)](#), we will use false discovery rate corrections to account for multiple hypothesis testing across our primary and secondary outcome variables. Therefore, for each hypothesis test, we will report two values:

1. The usual  $p$ -value from a Wald test;
2. False discovery Rate  $q$ -values, taken across outcome groups.

We will do FDR corrections separately for each outcome category and each outcome group within this category, reflecting our belief that results in each domain are of separate interest.

### 4.4 Covariate Selection

We first present all specifications controlling for our stratification variables, gender and prime treatment. To increase the precision of our estimates and soak up any imbalance across treatment groups, we also present a second set of results including baseline covariates. Following [Belloni, Chernozhukov, and Hansen \(2014\)](#), we adopt the "post-double-selection" method for selecting regressors, including first-order interaction and quadratic terms. We will correct for FDR within each method, but not across.

### 4.5 Testing Balance across Treatment and Control Groups

To verify the integrity of our randomization, we test for balance on baseline characteristics by estimating separately for each camp:

$$B_{ic} = \alpha + \beta \cdot T_{ic} + \mathbf{X}_{ic}'\delta + \varepsilon_{ic} \quad (5)$$

where  $B_{ic}$  represents each baseline characteristic for individual  $i$  in camp  $c$ ,  $T_{ic}$  indicates treatment assignment,  $\mathbf{X}_{ic}$  includes stratification variables, and  $\varepsilon_{ic}$  is the error term. We



estimate this equation separately for each of the four camps to ensure randomization integrity within each implementation site.

For each baseline variable in each camp, we test  $H_0 : \beta = 0$  using robust standard errors. We report both individual p-values and the F-statistic from a joint test of balance across all baseline variables within each camp. Given multiple testing across numerous baseline characteristics, we expect some rejections by chance and focus on the joint test and magnitude of imbalances rather than individual significance levels.

#### **4.6 Testing for Differential Attrition**

To test for differential attrition, we will create a dummy variable for whether the individual's interview is missing in follow-up surveys and regress this dummy on the treatment dummies. If and only if we find significant differential attrition by treatment status, we will report ? bounds. To adjust for attrition, we will also report the main tables with inverse probability weighting.

#### **4.7 Robustness: Social Desirability and Rule Orientation**

A potential concern with the collection of self-reported data is social desirability bias. In our specific context, social desirability bias may affect reporting of economic activity. Respondents in the treatment group may feel inclined to report improved secondary outcomes to our survey enumerators – i.e. there may be experimenter demand effects. To formally test for this concern, we follow [Dhar, Jain, and Jayachandran \(2022\)](#) in using the Marlowe–Crowne Social Desirability Scale ([Crowne and Marlowe, 1960](#)), which measures the individual propensity for social desirability bias. Like [Dhar et al. \(2022\)](#), we will present the interaction of the Marlowe–Crowne scale with the treatment status as a robustness check for selected outcome variables of interest.

Second, irrespective of treatment status, respondents may see engagement in economic activities without a formal work permit as a rule violation in the local context. Respondents that are more concerned about such rule violations may thus under-report their engagement in economic activities. We do not hypothesize that such potential under-reporting will differ by treatment status. In other words, it should not affect the validity of our experimental results. Assessing the extent of potential under-reporting will still be important to draw appropriate policy conclusions in this context. To do so, we measure the individual propensity to think about rules in a rule-oriented manner vs. in a manner that recognizes exceptions using an abbreviated version of the Rule Orientation scale ([Fine, Van Rooij, Feldman, Shalvi, Scheper, Leib, and Cauffman, 2016](#)). We will report and discuss potential differences in relevant outcomes by the individual score on this scale as a robustness check.

## 5 Hypotheses

We organize our hypotheses into two main categories: individual-level effects and household-level effects. For each group of outcomes within these categories (I.1, I.2, I.3, I.4, and HH.1), we will create standardized indices following [Kling, Liebman, and Katz \(2007\)](#) to reduce multiple testing concerns while maintaining statistical power. We will not construct indices for our analysis of individual- and household-level mechanisms outlined below.

### 5.1 Individual-Level Hypotheses

#### I.1 Labor Market Integration

We hypothesize that the treatment will increase efforts to find and take up economic opportunities and thus ultimately improving refugees' labor market outcomes along both extensive and intensive margins.

- **Increased labor force participation (extensive margin):** Based on economic activities including (self- and wage) employment status, number of income-generating activities
- **Increased labor supply and earnings (intensive margin):** Measured through hours worked and earnings from economic activities.
- **Increased formality of economic activities:** Measured through possession of documentation for economic activities (written contract for paid work and business registration for self-employment).

#### I.2 Individual Economic Outcomes

We expect improved labor market access to enhance individual economic welfare:

- **Higher income:** Measured through self-reported individual monthly income, with separate accounting for wage employment, self-employment.
- **Increased consumption and improved food security:** Measured through various individual-level consumption expenditure aggregates (food, non-food essentials, durables, services) and self-reported food security.
- **Enhanced savings behavior:** Measured through frequency of savings.

### I.3 Mental Health and Subjective Well-being

Legal documentation and formal economic opportunities may improve well-being:

- **Improved mental health:** Measured through standardized scales for depression symptoms (PHQ-8).
- **Improved subjective well-being:** Measured through life satisfaction (Cantril's ladder for today and 5 years from now) and aspirations.

### I.4 Perceived Safety and Trust

Legal documentation and formal economic opportunities may improve perceived safety in the host community:

- **Increased safety perceptions and trust:** Measured through questions on the perceived safety and trust in the host community.
- **Reduced harassment and discrimination:** Measured through frequency of needing to make informal payments to authorities and experiences of discrimination.

### Mechanisms: Search for Economic Activities

The treatment may affect actions and behaviors to find economic opportunities or start a business, measured by questions that ask for search intensity, search channels, and efforts and actions to start self-employment.

- Treated individuals may intensify efforts to look for work.
- Effects may vary by baseline employment status: Treatment may lead individuals without work to expand their search, while individuals with work may deepen investment in current activities and thus reduce search.
- Effects may vary by baseline employment type, for those that have work: Treatment effects may be bigger for individuals with less formal baseline work.
- The right to work may increase reservation wages and "reservation profits" as individuals can afford to be more selective.
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- Effects may vary by baseline employment type, for those that have work: Treatment effects may be bigger for individuals with less formal baseline work.
- The right to work may increase reservation wages and “reservation profits” as individuals can afford to be more selective.

## 5.2 Household-Level Hypotheses

### HH.1 Household Economic Outcomes

When a household member is treated, we expect effects on overall household economic outcomes:

- **Increased consumption and improved food security:** Measured through household-level consumption expenditure aggregates and food security indicators.
- **Enhanced asset accumulation:** Measured through household asset indices and housing quality indicators.

### Mechanisms: Intra-Household Dynamics

When a household member is treated, we expect the intra-household allocation of economic activities to change. We will explore several margins, including:

- **Market vs. home production:** The treated household member will increase work in formal employment/business, while other household members compensate by increasing unpaid household work
- **Formal-informal substitution:** The treated household member will increase work in formal employment/business, while other household members will reduce participation in informal or low-return income-generating activities (cash-for-work programs, unpaid apprenticeships).

## 6 Heterogeneous Treatment Effects

Our research design and sample size allow us to examine heterogeneous treatment effects across several policy-relevant dimensions. In addition to the heterogeneity dimensions below, we will analyze impacts by camp location, as implementation contexts and local economic opportunities vary substantially across the four sites (Bambasi, Sherkole, Tsore, and Sheder).

## **H.1 Gender**

We hypothesize significant gender-based heterogeneity in treatment effects, particularly in how cultural norms and household responsibilities influence the ability to leverage work permits. Our baseline data collection includes detailed modules on individual and household-level constraints that may affect women's labor force participation differently from men's. We will examine how gender interacts with the intervention.

## **H.2 Baseline Mental Health and Trauma**

Building on our previous work in [Ashraf, Bryan, Delfino, Holmes, Iacovone, Meyer, and Pople \(2025\)](#) showing strong links between trauma and economic outcomes among refugees in Ethiopia, we will analyze how baseline mental health status moderates program impacts. Using baseline measures of symptoms of post-traumatic stress disorder (PTSD) based on the DSM-5 criteria and current depression (using the Patient Health Questionnaire 8), we will examine the relationship between baseline mental health and economic opportunity access.

## **H.3 Skills-Market Match**

If we can implement the establishment survey as planned, we will use our detailed skills assessment data and information from the establishment survey on local labor demand to examine whether permits are more effective when refugees' capabilities align with host community labor market needs. We aim to construct measures of skills-market match that may consider educational background and literacy/numeracy levels, previous work experience and sector-specific skills, language skills, local demand for specific occupations and skills.

## **H.4 Household Position**

Whether the permit recipient is the household head versus another household member may affect treatment effects. We hypothesize that household heads may have greater autonomy in utilizing permits, while non-head recipients might face additional constraints or enable different household adaptation strategies. We will compare outcomes when the household head receives the permit vs. another adult household member receives the permit. We will also look at gender of the household head.

## 7 Attenuating Factors to Treatment Effects

### 7.1 Spatial Constraints to Work

Given the relative remoteness of the camps in our sample to urban areas and thus economic opportunities in the host community, refugees may face two major barriers to work – a lack of legal documentation and spatial remoteness. Our intervention only alleviates the first constraint, thus potentially attenuating the effectiveness of the treatment. We acknowledge that our treatment effects may be smaller than one would anticipate in other contexts, namely ones where refugees are already living in urban areas.

We aim to account for the role of commuting and remoteness in framing the treatment effect sizes we find. In order to assess individuals' preferences for commuting to job opportunities, we implement a discrete choice experiment at baseline where respondents make choices between two job profiles with varying wages and commuting distances. This experiment will allow us to compute camp-level willingness to pay for shorter commuting times and a corresponding elasticity between wages and commuting.

In addition to commuting times, we also consider the requirement of relocation for a given economic opportunity. For example, we present hypothetical jobs that require relocation to an urban area for varying durations of time. This allows us to elicit willingness to relocate for relative to commuting for jobs. Figure 1 presents an example of the discrete choice survey experiment, where respondents are asked to choose between a pair of jobs.

The screenshot shows a survey interface for a discrete choice experiment. At the top, it says "Group selected where: Isberg - C3" and "et, prefer job (required)". Below this, it says "ROUND 1" and "Please take a look at the following two job pairs carefully. Both jobs have 8 working hours with a working schedule from 8:30 AM to 5:30 PM. The commute time refers to a one-way commute." The main part of the screen is divided into two columns for Job A and Job B. Job A has a yellow header and Job B has an orange header. Both jobs have a monthly earnings of 3750 Bir and a commuting time of 4 hours - Commute. Job B also has a note: "7 Permanently - Relocation (permanent move to a new city)". On the right side, there is a list of preference options: "1. Strongly prefer Job A", "2. Prefer Job A", "3. Prefer Job B", and "4. Strongly prefer Job B".

Job A	Job B
Monthly Earnings 3750 Bir	Monthly Earnings 5000 Bir
Commute Time (One Way) 4 Hours - Commute	Commute Time (One Way) 7 Permanently - Relocation (permanent move to a new city)

Figure 1: Example of Discrete Choice Survey Experiment

We also plan to use these survey experiments to motivate our second phase of this research project, which will consider relocation in conjunction with the right to work to alleviate both of these constraints to refugees' economic integration at once.

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