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Cash Transfers and Employment among Syrian Refugees: Evidence from Rule-Based Eligibility in Türkiye

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Cash Transfers and Employment among Syrian Refugees: Evidence from Rule-Based Eligibility in Türkiye*

Abstract

We examine whether eligibility for a large-scale humanitarian cash transfer program affects employment outcomes among displaced populations. Exploiting deterministic demographic thresholds governing eligibility for the Emergency Social Safety Net (ESSN) in Türkiye, a nationwide unconditional cash transfer program established primarily to support Syrian refugees, we implement a local-randomization regression discontinuity design to estimate causal effects at the program's administrative eligibility frontier. We find no statistically significant discontinuities in employment probabilities for either women or men across multiple employment outcomes, including overall employment, wage employment, full-time employment, and nonfarm employment. Estimated effects are small, economically modest, and stable across specifications. Overall, the findings provide little evidence that sustained unconditional cash transfers under the ESSN generated economically meaningful labor supply disincentives at the eligibility margin.

JEL classification

C21, F22, H53, I38, J22

Keywords

unconditional cash transfers, refugees, labor supply, social assistance, regression discontinuity, rule-based eligibility

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1. Introduction

Unconditional cash transfers have become a central instrument of social protection and humanitarian response. By increasing non-labor income, they may affect labor supply through income effects, particularly along the extensive margin (Moffitt 2002). They may also relax liquidity constraints and reduce risk, potentially supporting labor market participation. A large empirical literature on cash transfers in low- and middle-income countries finds limited reductions in adult labor supply (Baird, McKenzie, and Özler 2018; Banerjee et al. 2017; Bastagli et al. 2019; Haushofer and Shapiro 2016, 2017). Far less is known about how sustained humanitarian transfers affect employment among displaced populations. Refugees operate in labor markets characterized by legal restrictions, informality, and limited job stability, which shape the margins through which income effects operate. Whether sustained humanitarian income support alters employment in such environments remains an open empirical question with direct implications for refugee and social protection policy.

This paper studies the employment effects of the Emergency Social Safety Net (ESSN) in Türkiye, a nationwide unconditional cash transfer program. Launched in 2016 and financed by the European Union, the ESSN provides regular monthly transfers to eligible households based on transparent demographic criteria (EC 2022a; IFRC 2019). The program was introduced in response to large-scale displacement following the onset of the Syrian Civil War, which led to the arrival of several million Syrians in Türkiye beginning in 2011. The program primarily serves Syrians under Temporary Protection.[1] At the time of the 2018 survey analyzed here, the ESSN served more than 1.67 million individuals and operated uniformly across the country without discretionary case-level adjustments. The program provided sustained monthly assistance intended to support basic consumption needs rather than replace labor income. The scale, sustained implementation, and transparent eligibility rules of the ESSN make it well suited to evaluate the employment effects of regular humanitarian income support. In what follows, we use the term "Syrian refugees" to refer to Syrians under Temporary Protection, who constitute the focus of the empirical analysis.

Eligibility is determined by a deterministic administrative rule defined over household demographic characteristics. Households that do not qualify under categorical vulnerability criteria become eligible if they meet one of two thresholds: at least four children under age 18 or a household dependency ratio of 1.5 or higher. These thresholds generate discrete changes in eligibility within a two-dimensional assignment space. We exploit this structure using a local-randomization regression discontinuity design, restricting attention to assignment cells adjacent to the eligibility frontier (Cattaneo, Idrobo, and Titiunik 2020; Cattaneo, Titiunik, and Vazquez-Bare 2017).

The analysis uses data from the Syrian Refugee Module in the 2018 Türkiye Demographic and Health Survey (TDHS), a survey designed to be representative of non-camp Syrian refugees within the constructed sampling frame, conducted while the eligibility rule operated without

[1] Syrians under Temporary Protection are individuals granted temporary protection status under Türkiye's Law on Foreigners and International Protection, a legal category distinct from formal refugee status, which provides legal stay, access to basic services, and regulated access to the labor market.

modification.[2] To our knowledge, this survey provides the only dataset with national coverage that combines detailed employment outcomes with the household demographic information required to reconstruct the program's eligibility rule. Within the prespecified local window, eligibility is treated as if randomly assigned under the local-randomization framework, allowing identification of causal effects at the eligibility margin. Our primary estimand is the intent-to-treat effect of eligibility, which captures the impact of the administrative targeting rule as implemented.

We find no statistically significant discontinuities in employment probabilities for either women or men. Within the local estimation window, eligibility is not associated with changes in overall employment, wage employment, full-time employment, or nonfarm employment. Point estimates are small, economically modest, and stable across specifications. Overall, the findings provide little evidence that regular unconditional transfers under the ESSN generated economically meaningful labor supply disincentives at the eligibility margin.

These findings provide causal evidence on the employment effects of sustained, rule-based humanitarian cash assistance in a middle-income refugee setting. Prior work documents heterogeneous employment responses across contexts and margins of adjustment (Altındağ and O'Connell 2023; Caria et al. 2024; Gupta et al. 2024; Salti et al. 2022). By examining a nationwide program with sustained monthly transfers and transparent assignment rules, this paper identifies employment responses at a clearly defined policy threshold.

The analysis focuses on labor market participation and selected measures of job composition. The data do not permit identification of finer adjustments in hours worked or earnings within employment. Formal employment is not analyzed within the regression discontinuity framework because its incidence is extremely low in the estimation window. In this setting, where employment is overwhelmingly informal, the primary margins of adjustment are participation and job type rather than transitions into formal employment.

The interpretation of these estimates is local. The regression discontinuity design identifies causal effects for households near the demographic thresholds that determine eligibility, and outcomes capture employment status at a point in time. The analysis does not capture longer-term trajectories in earnings, job stability, or occupational mobility, nor does it estimate general equilibrium effects operating through local labor demand (Egger et al. 2022). These limitations define the scope of interpretation rather than undermining the central finding. Within that scope, the results inform debates on how sustained income support shapes economic participation among Syrian refugees in middle-income host countries.

The remainder of the paper proceeds as follows. Section 2 reviews the literature on unconditional cash transfers and labor supply, with emphasis on refugee settings and prior evaluations of the ESSN. Section 3 describes the ESSN and the deterministic eligibility rule. Section 4 presents the data and variable construction. Section 5 outlines the local-randomization regression discontinuity framework and estimation procedures. Section 6 reports design validation, first-stage evidence, main effects, robustness, and power. Section 7 discusses interpretation. Section 8 concludes.

[2] Non-camp Syrian refugees refer to individuals residing in host communities rather than temporary accommodation centers (camps). At the end of 2018, approximately 4 percent of Syrian refugees in Türkiye resided in camps, with the remainder living in host communities across the country. Camp residents are not eligible for ESSN benefits.

2. Related Literature

2.1 Income Effects and Adult Labor Supply: Theory and Global Evidence

In standard labor supply models, unconditional cash transfers increase non-labor income and may reduce work effort through income effects, particularly along the extensive margin (Moffitt 2002). By raising reservation wages, transfers can lower labor force participation or hours worked when individuals can adjust labor supply. Transfers can also relax liquidity and credit constraints, enabling households to smooth consumption, reduce risk, or invest in productive activities. When search costs or capital constraints bind, transfers may instead increase economic activity rather than induce withdrawal from work. The net labor market effects of unconditional transfers are therefore theoretically ambiguous and depend on the margins of adjustment available in the institutional environment.

Empirical evidence from low- and middle-income countries provides a consistent benchmark.[3] Syntheses of randomized and quasi-experimental studies find little evidence of systematic reductions in adult labor supply following cash transfers (Banerjee et al. 2017; Baird, McKenzie, and Özler 2018). Large-scale randomized transfers in Kenya increased consumption and assets without reducing employment or hours worked (Haushofer and Shapiro 2016, 2017). Unconditional grants also compare favorably with alternative poverty programs without generating labor market distortions (McIntosh and Zeitlin 2022). A review of 165 studies reaches similar conclusions (Bastagli et al. 2019). Overall, the evidence does not indicate systematic reductions in employment following unconditional assistance.

Evidence also points to substantial heterogeneity in responses. Social pensions can affect labor force participation in extended households when participation margins are elastic (Ardington, Case, and Hosegood 2009). Transfers may also generate local multiplier effects and shift labor demand, complicating partial-equilibrium predictions (Egger et al. 2022). These patterns indicate that income effects operate through margins shaped by institutional conditions, labor demand, household structure, and access to complementary inputs. Labor supply responses are therefore context-dependent rather than structural constants.

2.2 Unconditional Cash Transfers in Refugee Settings

Refugee labor markets differ systematically from the citizen contexts in which most evidence on unconditional transfers has been generated. Refugees often face legal restrictions on formal employment, sectoral quotas, employer sponsorship requirements, and uncertainty regarding work authorization. Even where formal access exists, labor demand may be limited and informality widespread. Humanitarian transfers may also operate within eligibility systems that monitor

[3] Evidence from high-income settings provides a complementary benchmark. A large literature examines the labor supply effects of transfer programs and highlights potential work disincentives arising from income effects and implicit taxation of earnings (Moffitt 2002). Empirically however, the responses are often modest, reflecting optimization frictions and constraints on adjustment (Chetty 2012). Reviews of unconditional or near-unconditional income support programs similarly document mixed responses that vary with program design and context (Hoynes and Rothstein 2019).

earnings or employment status. In such settings, employment responses depend on the margins of adjustment available to households, including labor force participation, job search intensity, occupational choice, and self-employment.

Recent causal studies of unconditional cash transfers to refugees document heterogeneous employment responses across contexts. In Lebanon, multipurpose cash transfers reduced labor force participation at the eligibility threshold, reflecting a decline in participation without corresponding reductions in employment (Salti et al. 2022). In Uganda, a large one-time transfer had no statistically significant effect on overall employment despite increasing business activity, indicating no clear labor supply response (Gupta et al. 2024). In Jordan, a small unconditional grant increased job search intensity immediately and led to higher employment with a lag, consistent with the relaxation of liquidity constraints along the search margin (Caria et al. 2024). In contrast, while transfers in Lebanon improved welfare during receipt, they generated no sustained employment effects after program termination, indicating limited persistence (Altındağ and O'Connell 2023). These findings indicate that employment responses vary with institutional context, transfer design, and the margins of labor market adjustment available to beneficiaries.

Existing refugee-specific evidence does not indicate large or persistent reductions in employment following cash transfers, although estimated effects vary across settings and adjustment margins. Most studies focus on fragile or camp-based environments and evaluate short-run or one-time interventions. Evidence on sustained, rule-based humanitarian assistance in middle-income labor markets remains limited. Whether such programs generate discontinuous employment responses at eligibility thresholds therefore remains largely unexamined.

2.3 The ESSN in Türkiye

A growing literature evaluates the ESSN's welfare and human capital impacts using quasi-experimental methods. Robson et al. (2024) document significant reductions in multidimensional poverty using doubly robust inverse probability weighted regression adjustment applied to repeated cross-sectional survey data. Other studies exploit eligibility-based variation (regression discontinuity design) or selection-on-observables approaches to estimate impacts on a range of outcomes, including consumption, food security, indebtedness, coping strategies, school enrollment, and child labor (Aygün et al. 2024; Özler et al. 2021). Complementary program evaluations and monitoring reports document similar improvements in welfare outcomes (Maunder et al. 2020). This evidence indicates that the ESSN improved material welfare and human capital outcomes.

Despite this progress, employment responses remain unexamined. To our knowledge, no study of the ESSN analyzes employment outcomes using a rigorous causal identification strategy. ESSN eligibility is defined by transparent demographic criteria that generate discrete changes in transfer receipt along a well-defined administrative boundary. The program delivers sustained monthly transfers in a middle-income labor market characterized by widespread informality and substantial gender differences in labor force participation. These features motivate separate analysis of employment outcomes for women and men. This paper addresses this gap by exploiting the program's deterministic eligibility rule in a regression discontinuity design to estimate causal

effects at the eligibility frontier. We find no evidence of moderate or large discontinuous labor supply responses.

3. The Emergency Social Safety Net

3.1 Institutional Context and Scale

During the 2018 TDHS fieldwork period (November 2018–February 2019), the ESSN was Türkiye's primary humanitarian cash transfer program. Launched in November 2016, it provided regular, unconditional income support to Syrian refugees residing outside camps and operated nationwide across all 81 provinces. The program was established in response to large-scale displacement from Syria, and Syrian refugees accounted for the vast majority of beneficiaries. Other displaced populations, covered under the legally distinct International Protection framework, represented only a small share. At the time, the ESSN was the largest European Union–financed humanitarian assistance program globally.

Syrian refugees in Türkiye began arriving in large numbers following the onset of the Syrian conflict in 2011, with most arrivals preceding the 2018 survey period. Türkiye is a signatory to the 1951 Refugee Convention and its 1967 Protocol, although refugee status is subject to a geographic limitation; Syrians are instead granted Temporary Protection status, which provides a legal basis for residence and access to basic services. Within this framework, access to formal employment is regulated through a work permit system requiring employer sponsorship and subject to administrative and firm-level constraints, and formal employment remains limited in practice. As a result, a large share of refugees are engaged in informal work arrangements and are dispersed across host communities rather than concentrated in camps. By the end of 2018, approximately 4 percent resided in camps, while the remainder lived in host communities across the country. These features shape the margins through which income support programs such as the ESSN may affect labor supply.

The ESSN was implemented by the Turkish Red Crescent in coordination with the International Federation of Red Cross and Red Crescent Societies, in partnership with the Ministry of Family, Labour, and Social Services, and financed through the European Union's Facility for Refugees in Turkey (EC 2022a; IFRC 2018a). Program administration, enrollment, and benefit delivery were centralized, and eligibility rules were applied uniformly across provinces without enrollment caps, regional quotas, or discretionary case-level adjustments (IFRC 2019; TRC-IFRC 2019b).

Based on administrative data, 1.67 million individuals across 299,000 households received monthly ESSN support in December 2018, coinciding with the TDHS fieldwork period. Approximately 90–95 percent of beneficiaries were Syrian refugees. At the end of 2018, there were 3.62 million registered Syrian refugees in Türkiye, of whom 144,000 resided in camps and were therefore ineligible for ESSN benefits. We estimate that 43–46 percent of the non-camp Syrian refugee population were ESSN beneficiaries.

3.2 Transfer Design and Economic Magnitude

The ESSN provided regular, unconditional cash transfers through Türkiye's national social protection infrastructure via the Integrated Social Assistance Information System, the platform used to administer social assistance programs for Turkish citizens (IFRC 2018b; TRC-IFRC 2019a).

During the 2018 TDHS fieldwork period, beneficiaries received ₺120 per household member per month through the Kızılaykart, a prepaid debit card usable at automated teller machines and retail outlets nationwide. Households also received quarterly supplemental payments that increased with household size. A six-person household received ₺720 per month (about US$135), equivalent to roughly 45 percent of the statutory net minimum wage (₺1,603) in 2018. Transfers were unconditional and not tied to employment, job search, school attendance, or participation in activation programs.

3.3 Eligibility Rule and Assignment Mechanism

Eligibility for the ESSN was determined by a deterministic rule based on household demographic characteristics. A household qualified if it satisfied at least one of six criteria. Four captured categorical vulnerability: (i) single-parent households with dependents, (ii) elderly-headed households without working-age adults, (iii) the presence of a certified disability, or (iv) households with dependents under age 18 and no working-age adult.

Households not meeting these criteria were evaluated against two fixed demographic thresholds: (v) having four or more children under age 18, or (vi) a household dependency ratio of 1.5 or higher (EC 2022a; IFRC 2019; WFP 2018). The dependency ratio is defined as dependents divided by working-age adults, where dependents are individuals under age 18 or aged 60 or older, and working-age adults are those aged 18–59. Under program rules, certified disabled individuals are excluded from the denominator of working-age adults.

Administrative reporting indicates that these threshold criteria account for the majority of program enrollments (IFRC 2018a; WFP 2018). In practice, they define the primary margins of assignment and generate the discontinuities exploited in the regression discontinuity design. Because both variables are directly observable in the TDHS household roster, they can be constructed in the survey data and used as assignment variables in the empirical analysis.

Eligibility was computed automatically within the Integrated Social Assistance Information System, linked to Türkiye's civil registration database. Threshold rules were applied mechanically to recorded household composition, without discretionary overrides, case-by-case assessments, enrollment caps, or province-specific adjustments (TRC-IFRC 2019a). Assignment therefore depends solely on administratively recorded demographic characteristics and operates uniformly across provinces.

3.4 Administrative Implementation and Stability during the Survey Period

ESSN receipt was not conditioned on employment status. Continued eligibility, however, was subject to automated reassessment when formal income appeared in administrative records. Data sharing between the Social Security Institution and the Integrated Social Assistance Information System enabled detection of formally registered employment. Entry into formal employment therefore generated administrative records that could affect continued eligibility (EC 2022b; TRC-IFRC 2019b). Reassessment was triggered specifically by formally recorded income; informal employment, which is not captured in administrative systems, did not prompt reassessment. This feature creates a potential disincentive to formal employment but not to informal work.

Monitoring reports indicate that reassessments due to newly recorded formal income were infrequent during the study period, reflecting both the structure of the program and the low incidence of formal employment among Syrian refugees (EC 2022b; TRC-IFRC 2019a). Enrollment status exhibited high persistence throughout the survey window.

The eligibility criteria and demographic thresholds remained unchanged from the program's launch in 2016 through the 2018 TDHS fieldwork period (EC 2022a, 2022b; TRC-IFRC 2019b). Assignment was computed mechanically through a centralized administrative platform and applied uniformly across provinces. The targeting rule and benefit structure remained stable and nationally consistent during the survey period. Subsequent modifications to program parameters fall outside the scope of this analysis.

4. Data

4.1 Data Source and Survey Design

This study uses data from the 2018 Türkiye Demographic and Health Survey (TDHS), conducted by the Hacettepe University Institute of Population Studies (HUIPS). The TDHS is part of the Demographic and Health Surveys (DHS) Program and provides nationally representative data for the Turkish population, alongside a separate sample covering the Syrian population residing in Türkiye within the constructed sampling frame.

The Syrian refugee module was fielded between November 23, 2018, and February 12, 2019, alongside the main TDHS survey. It covers 15 provinces hosting the largest Syrian refugee populations, accounting for more than 70 percent of the registered refugee population. The design focuses primarily on non-camp households, with a smaller camp-based sample drawn from a separate stratum (HUIPS 2019a, 2019b).

The analysis restricts attention to the non-camp sample. Sampling follows a stratified two-stage cluster design. The sampling frame was constructed using registered Syrian population counts and limited to quarters (the smallest administrative units) with sufficiently large and dense Syrian populations. Primary sampling units are quarters, selected with probability proportional to size. In the second stage, field teams conducted in situ listing within each selected quarter, identifying 40 Syrian households and selecting 20 for interview. A total of 1,700 non-camp households were selected across 85 clusters, of which 1,593 completed interviews (a household response rate of

94.4 percent). Within these households, 2,125 women aged 15–49 were eligible for individual interviews, of whom 1,963 were successfully interviewed (92.4 percent) (HUIPS 2019a, 2019b).

4.2 Measurement of Program Participation, Employment Outcomes, and Assignment Variables

ESSN Participation

The household questionnaire asked members aged 12 and above whether they were receiving regular support, including retirement pensions, widow or orphan benefits, immigrant assistance, care payments, scholarships, or other transfers. We construct a household-level binary indicator equal to one if any household member reports receiving immigrant assistance and zero otherwise. The measure is based on self-reported receipt rather than administrative records.

During the 2018 survey period, the ESSN was the only nationwide program providing regular monthly cash transfers to refugees residing outside camps. Other refugee-focused programs were conditional, geographically limited, irregular, or delivered in kind rather than as predictable monthly transfers (EC 2022a; IFRC 2019; UNHCR 2019; UNICEF 2019; WFP 2018). Although the survey does not explicitly reference the ESSN, the immigrant assistance response is therefore interpreted as ESSN participation. In the absence of comparable alternative programs, systematic misclassification is unlikely. In our data, 47 percent of non-camp Syrian refugees report receipt of immigrant assistance, roughly consistent with our estimate of 43–46 percent ESSN coverage among non-camp Syrian refugees at the end of 2018 based on administrative data.

Employment Outcomes

Employment outcomes are constructed from two survey modules. For ever-married women aged 15–49, respondents report their own current employment status and job characteristics. Information on currently married men is obtained through proxy reporting by spouses, who report whether their husbands worked in the past seven days or, if not, within the past 12 months; if either condition is satisfied, job characteristics for the current or most recent job are recorded (HUIPS 2019a, 2019b). Employment measures therefore combine self-reports for women and proxy reports for men. For each job, the survey records occupation, sector, full-time or part-time status, work location, and registration with the social security system; earnings and hours worked are not collected.

We construct five primary binary employment outcomes: overall employment, wage employment, full-time employment, nonfarm employment, and employment outside the home. We additionally construct indicators for public and formal employment, where public employment captures work in the public sector and formal employment identifies jobs registered with the social security system. These latter outcomes are reported descriptively but excluded from the regression discontinuity analysis due to extremely low incidence within the local estimation window.

Employment information for men is based on proxy reporting. While this may introduce measurement error, it follows standard DHS practice and is applied uniformly across households. To the extent that measurement error is unrelated to eligibility status, any resulting attenuation

would bias estimated effects toward zero rather than generate spurious discontinuities at the eligibility frontier.

Assignment Variables

The two household-level assignment variables are derived from the household roster. The number of children under age 18 is defined as the count of household members aged 0–17. The household dependency ratio is defined as the ratio of dependents to working-age adults, where dependents are individuals under age 18 or aged 60 or older, and working-age adults are those aged 18–59.

Under the administrative rule, certified disabled individuals are excluded from the denominator of working-age adults. Although the TDHS records disability status, it does not capture the administrative certification used for eligibility verification. The constructed dependency ratio therefore approximates the administrative rule using observed age structure and reported disability status. This measure relies solely on demographic information and is not derived from employment or income. Both assignment variables are discrete and predetermined.

4.3 Analytical Sample and Data Considerations

The analytical sample is constructed in three stages. First, the data are restricted to non-camp Syrian refugee households with at least one individual in the prime working-age range. Women are defined as ever-married individuals aged 25–49, consistent with the TDHS female questionnaire. Men are defined as currently married individuals aged 25–54, reflecting the age range for which spousal employment information is available. These definitions follow the structure of the survey modules rather than a standard labor market definition of working age.

Second, the sample is restricted to households with complete information on ESSN participation, assignment variables, and observable characteristics included as controls in the regression specifications, as well as to individuals with valid employment information. Observations with missing values are excluded. Item non-response is limited, and no imputation is applied. These restrictions are unrelated to eligibility status other than through missing information.

After applying these restrictions, the final analytical sample consists of 850 women and 1,016 men across 1,018 non-camp Syrian refugee households. The unit of analysis in the employment regressions is the individual, while assignment to eligibility is defined at the household level. The regression discontinuity analysis in sections 5 and 6 further restricts the sample to the local estimation window described in section 5.1. Under this restriction, the estimation sample consists of individuals in 418 households located in assignment cells adjacent to the eligibility frontier.

5. Empirical Strategy

5.1 Assignment and Identification

Institutional Assignment Rule

Eligibility for the ESSN is determined by a deterministic rule defined over two discrete household-level variables: the number of children under age 18 and the household dependency ratio. Let C_h denote the number of children under age 18 in household h, and let D_h denote the household dependency ratio. For households not qualifying under categorical vulnerability criteria, eligibility is granted if

$$C_h \geq 4 \text{ or } D_h \geq 1.5.$$

Define the eligibility indicator as

$$\text{Eligible}_h = \mathbf{1}\{C_h \geq 4 \text{ or } D_h \geq 1.5\},$$

where $\mathbf{1}\{\cdot\}$ denotes the indicator function.

Eligibility is computed mechanically through linked administrative records and applied uniformly nationwide without discretionary override. Assignment is determined entirely by recorded household demographic characteristics and does not depend on caseworker discretion or application behavior. Thus, eligibility status is fully determined by observed values of (C_h, D_h). The rule generates discontinuities in eligibility at fixed demographic thresholds within a two-dimensional discrete assignment space. Figure 1 displays the complete eligibility region implied by this rule across the grid of household composition values.

Identification Framework

Regression discontinuity designs identify causal effects by exploiting discontinuous changes in assignment induced by threshold rules (Hahn, Todd, and van der Klaauw 2001). In this setting, eligibility changes discontinuously along the eligibility frontier defined by $C_h = 4$ and $D_h = 1.5$. Because eligibility is determined solely by recorded household composition and applied without discretion, households located in assignment cells adjacent to this frontier differ discretely in eligibility status.

In principle, continuity-based regression discontinuity methods can be extended to multidimensional assignment frontiers by imposing smoothness of potential outcomes in each running variable and estimating local regressions near the boundary. Such approaches rely on dense support near the frontier and the ability to shrink bandwidths toward zero. In the present application, however, both assignment variables are discrete and exhibit limited support in the immediate neighborhood of the demographic thresholds. Moreover, the eligibility frontier is defined in two dimensions rather than by a single scalar running variable. Implementing a continuity-based estimator would therefore require projecting the assignment space onto an arbitrary distance metric from the frontier and imposing smoothness restrictions across two

dimensions. These modeling choices are not dictated by the institutional rule and would introduce functional form assumptions beyond the administrative structure governing assignment.

We therefore adopt the local-randomization regression discontinuity framework developed by Cattaneo, Titiunik, and Vazquez-Bare (2017) and Cattaneo, Idrobo, and Titiunik (2020). This framework is well suited to environments in which assignment variables are discrete and the relevant support near the frontier consists of a finite set of adjacent assignment cells. Identification is obtained by restricting attention to a prespecified local window W containing assignment cells immediately adjacent to the eligibility frontier and treating eligibility assignment as as-if randomized within that window.

Let $Y_{ih}(1)$ and $Y_{ih}(0)$ denote potential employment outcomes for individual i in household h under eligibility and non-eligibility, respectively. The observed outcome satisfies

$$Y_{ih} = \text{Eligible}_h \cdot Y_{ih}(1) + \big(1 - \text{Eligible}_h\big) \cdot Y_{ih}(0).$$

The identifying assumption under the local-randomization framework is

$$(Y_{ih}(1), Y_{ih}(0)) \perp \text{Eligible}_h \text{for} (C_h, D_h) \in W.$$

This assumption implies that households located in assignment cells immediately adjacent to the eligibility frontier are comparable in expectation with respect to potential outcomes. Because eligibility is determined mechanically from recorded demographic characteristics and applied uniformly nationwide, households in adjacent assignment cells near the demographic thresholds differ discretely in eligibility status but not systematically in other dimensions within the local window. In this framework, comparability pertains to both observed and unobserved characteristics; in practice, we assess its plausibility by examining balance in observable characteristics across eligibility status within the window. Under the local-randomization assumption, differences in observed outcomes within W can therefore be attributed to the discontinuous change in eligibility induced by the program rule. The local-randomization approach thus maps directly to the institutional structure of the ESSN assignment mechanism: deterministic eligibility based on discrete demographic measures within a finite two-dimensional assignment grid.

Estimand

The parameter of interest is the ITT effect of eligibility within the local window:

$$\tau_{\text{ITT}} = \mathbb{E}[Y_{ih}(1) - Y_{ih}(0) \mid (C_h, D_h) \in W].$$

This estimand corresponds to the finite-sample average causal effect of assignment to eligibility for households located near the eligibility frontier.

Eligibility is determined administratively, whereas receipt of the transfer is observed in survey data and may not coincide perfectly with eligibility status. The design therefore corresponds formally to a fuzzy regression discontinuity in which eligibility serves as an instrument for receipt.

However, eligibility constitutes the policy rule governing program access and is the relevant administrative margin for evaluating the effects of the targeting mechanism itself. The reduced-form effect of eligibility captures the causal impact of assignment under the rule as implemented. Instrumental-variable ratio estimates would rescale the reduced-form effect by the first-stage coefficient and thus yield a treatment-on-the-treated effect for compliers within the window, without introducing additional identifying variation (Lee and Lemieux 2010). Because such estimates are algebraic transformations of the same local comparison and are typically less precise, we focus on the ITT effect of eligibility as the primary estimand.

Definition of the Estimation Window

Because the assignment variables are discrete, we define the local estimation window directly in terms of specific assignment cells adjacent to the eligibility frontier. The window consists of the following eight cells in the two-dimensional assignment grid:

- $(C_h = 2,\ D_h = 1.0\text{–}1.4)$
- $(C_h = 2,\ D_h = 1.5\text{–}1.9)$
- $(C_h = 3,\ D_h = 0.5\text{–}0.9)$
- $(C_h = 3,\ D_h = 1.0\text{–}1.4)$
- $(C_h = 3,\ D_h = 1.5\text{–}1.9)$
- $(C_h = 4,\ D_h = 0.5\text{–}0.9)$
- $(C_h = 4,\ D_h = 1.0\text{–}1.4)$
- $(C_h = 4,\ D_h = 1.5\text{–}1.9)$

Under the deterministic rule $C_h \geq 4$ or $D_h \geq 1.5$, three of these cells are ineligible and five are eligible. All included cells lie immediately adjacent to the eligibility frontier defined by $C_h = 4$ and $D_h = 1.5$, preserving local support on both sides of the eligibility frontier in the two-dimensional assignment space.

This window represents the minimal contiguous set of assignment cells bordering the eligibility frontier that preserves identification under the local-randomization framework while maintaining sufficient observations for statistical inference. Figure 2 illustrates the geometry of the local window relative to the full assignment region. All estimation is restricted to observations satisfying $(C_h, D_h) \in W$.

5.2 Estimation, Inference, and Validation

Estimation Strategy

Within the local window W, we estimate the ITT effect using the linear specification

$$Y_{ih} = \alpha + \delta\,\text{Eligible}_h + X_{ih}'\gamma + \varepsilon_{ih},$$

where Y_{ih} denotes an employment outcome, Eligible_h is the eligibility indicator defined above, and X_{ih} includes observable covariates: age, age squared, years of education, indicators for year of arrival in Türkiye, and indicators for region of residence. All estimations are conducted

separately for women and men, in view of differences in survey measurement and substantially different baseline employment levels across genders. The coefficient δ identifies τ_{ITT} under the local-randomization assumption. Standard errors are clustered at the PSU level.

Under local randomization, identification relies solely on comparisons across eligibility status within the prespecified set of adjacent assignment cells comprising the local window and does not require functional form assumptions in the assignment variables. Covariates are included to improve precision and adjust for residual differences in observable characteristics within the window. Estimates without covariates yield similar point estimates, confirming that identification does not depend on covariate adjustment.

All estimations apply normalized survey sampling weights to preserve representativeness of the covered non-camp Syrian refugee population. Because identification arises from discrete comparisons within the local window, weighting does not affect the internal validity of the design but ensures that estimated effects correspond to population-relevant magnitudes.

Inference Procedures

Inference is conducted using wild cluster bootstrap procedures with 500 replications, clustering at the PSU level (Cameron, Gelbach, and Miller 2008). Clustering at the PSU level is consistent with the two-stage sampling design of the TDHS and accounts for intra-cluster correlation in outcomes. The wild cluster bootstrap is used as the primary procedure because it improves finite-sample performance when the number of clusters is moderate and conventional cluster-robust variance estimators may over-reject.

For robustness, we implement alternative inference procedures that address distinct finite-sample concerns. We report conventional cluster-robust standard errors for comparability with standard asymptotic practice. We also report heteroskedasticity-consistent variance estimators using the HC3 correction (MacKinnon and White 1985), which mitigates leverage-related bias but does not account for clustering, and jackknife-based standard errors (Efron and Tibshirani 1993) as an additional resampling-based estimator.

We further report randomization-inference *p*-values based on permutation tests implemented within the local window, following the local-randomization regression discontinuity framework of Cattaneo, Idrobo, and Titiunik (2020) and Cattaneo, Titiunik, and Vazquez-Bare (2017). These procedures reassign eligibility labels across households while preserving the number of eligible and ineligible observations. They provide finite-sample exact inference under the sharp null hypothesis and do not rely on large-sample approximations. Together, these approaches allow us to assess the robustness of statistical conclusions to alternative inference methods.

Design Validity and Diagnostic Checks

To assess the plausibility of the local-randomization assumption, we implement three diagnostic checks. First, we examine the density of the assignment variables around the eligibility frontier. Because the running variables are discrete, formal density tests designed for continuous assignment variables, such as McCrary (2008), are not applicable. Instead, we assess potential

manipulation by inspecting assignment-cell frequencies near the frontier and evaluating whether excess mass appears in cells adjacent to the threshold. Because eligibility is computed mechanically from administratively recorded household composition and applied without discretionary override, systematic manipulation would require deliberate misreporting or alteration of registered demographic information.

Second, we estimate the first-stage relationship between eligibility and reported ESSN receipt. Although the reduced-form ITT estimand is defined with respect to eligibility, a discontinuous increase in receipt at the frontier confirms that eligibility generates meaningful variation in program exposure within the local window.

Third, we examine balance in observable characteristics within the local window. Under the local-randomization framework, these characteristics should not differ systematically across eligibility status among households near the frontier. Systematic imbalance would undermine the as-if random assignment assumption underlying identification.

Robustness

We conduct three robustness exercises. First, we assess the sensitivity of statistical inference to alternative variance estimators. Although treatment is assigned at the household level, outcomes are analyzed separately for women and men, and the number of same-sex observations per household in each estimation sample is limited. Within-household dependence is therefore unlikely to be a first-order concern, and clustering at the PSU level remains appropriate given the survey design. That said, second, we examine the sensitivity of inference to clustering at the household level. Third, we assess robustness to functional form by estimating binomial probit models and reporting marginal effects, providing a nonlinear specification check relative to the linear probability model while preserving the same local-window comparison.

6. Results

6.1 Design Validity

Sample Support within the Estimation Window

We begin by examining sample support within the local estimation window. Table 1 reports the number of Syrian refugee households in each assignment cell of the two-dimensional eligibility grid. Under the window shown in figure 2, which includes three ineligible and five eligible cells adjacent to the eligibility frontier, the sample comprises 418 households, of which 230 are assigned to eligibility and 188 to non-eligibility.

All assignment cells within the window contain positive counts, with support on both sides of the eligibility thresholds. Treated and control households therefore originate from contiguous regions of the assignment space bordering the eligibility frontier along both dimensions, consistent with the local-randomization design.

Density of Assignment Variables

We assess whether the assignment variables exhibit sorting at the eligibility frontier. Because both running variables are discrete, formal density tests designed for continuous assignment variables, such as McCrary (2008), are not applicable. Instead, we examine assignment-cell frequencies near the eligibility frontier.

Figure 3 presents histograms of the number of children under age 18 and the household dependency ratio. The distributions show no excess mass or bunching in cells adjacent to the eligibility thresholds, and counts are comparable on either side. The underlying demographic characteristics—household size and age structure—are slow-moving and recorded through administrative systems linked to civil registration. Systematic sorting would therefore require deliberate misreporting or alteration of registered demographic information. These patterns provide no evidence of manipulation at the eligibility frontier.

First-Stage Relationship

We verify that eligibility induces a discontinuous change in program receipt. Table 2 reports ordinary least squares (OLS) estimates of ESSN receipt on the eligibility indicator. In the full assignment region, eligibility increases reported receipt by 59.9 percentage points. Within the local estimation window, the increase is 46.5 percentage points. Both estimates are statistically significant at the 1 percent level.

Figure 4 plots participation probabilities across the assignment grid. The heatmap reveals a clear discontinuity at the eligibility thresholds, consistent with the regression estimates. Receipt probabilities are uniformly low in ineligible cells and increase sharply in eligible cells.

The strength of the first-stage relationship varies with distance from the eligibility frontier. In cells far from the threshold, eligibility and receipt correspond closely. Near the frontier, however, receipt does not approach one among eligible households, and some ineligible households report participation. This attenuation reflects measurement error in survey-based data. Both eligibility status and the underlying assignment variables are measured with reporting and sampling error, which has limited consequences far from the threshold but blurs the discontinuity locally.

Institutional frictions—such as processing delays, updating lags, or discrepancies between registry status and survey reporting—may also contribute to this attenuation. Regardless of its source, the resulting fuzziness is concentrated near the eligibility frontier, as expected when a deterministic rule is observed with imperfect measurement.

The smaller coefficient within the local window therefore reflects reduced correspondence between measured eligibility and receipt near the threshold rather than a breakdown of the administrative rule. Even within the estimation window, eligibility increases participation by nearly one-half of households, providing economically meaningful variation in program exposure and supporting a strong first stage for the regression discontinuity design.

Covariate Balance within the Estimation Window

A central implication of the local-randomization assumption is that observable characteristics are balanced across eligibility status within the estimation window. We therefore assess balance at the eligibility frontier.

Table 3 reports differences in observable characteristics across eligibility status, separately for the full assignment region and the local estimation window, and for women and men. In the full assignment region, several characteristics differ systematically, reflecting the mechanical relationship between household composition and the eligibility rule.

Restricting attention to the local estimation window substantially attenuates these differences. Estimated differences in age, years of education, year-of-arrival indicators, and region of residence are small in magnitude for both women and men. Although a limited number of coefficients remain statistically significant at conventional levels, there is no systematic pattern of imbalance across characteristics or gender-specific samples. The number of statistically significant differences is consistent with sampling variation given the number of tests conducted and is not accompanied by economically meaningful differences. Including covariates in the outcome regressions does not materially affect the estimated treatment effects.

The reduction in imbalance from the full assignment region to the local estimation window is consistent with the local-randomization assumption that households near the eligibility frontier are comparable in expectation.

6.2 Main Effects

Baseline Employment

We begin by describing baseline employment patterns in the full assignment region and within the local estimation window. Table 4 reports descriptive statistics separately for women and men, distinguishing between the overall assignment region and the local window around the eligibility frontier.

For women, employment is low in both samples, at 8.5 percent in the overall assignment region and 7.9 percent in the local window. Wage employment is 6.2 percent overall and 5.8 percent in the window. Full-time employment declines from 5.5 to 4.3 percent, while nonfarm employment remains similar (6.0 percent overall and 6.4 percent in the window). Employment outside the home decreases from 5.8 to 4.6 percent. Public employment is rare (0.4 percent overall and 0.6 percent in the window), and formal employment is negligible. Restricting attention to the local window does not materially alter baseline labor market attachment among women.

For men, employment rates are substantially higher. Employment rises from 67.7 percent in the overall assignment region to 73.1 percent in the local window. Wage employment increases from 59.2 to 63.0 percent, full-time employment from 63.0 to 68.6 percent, and nonfarm employment from 63.6 to 70.1 percent. Public employment remains low (1.7 percent overall and 1.2 percent in the window), and formal employment is extremely limited (0.6 percent overall and 0.2 percent in

the window). Even within the local window, male employment remains substantially higher than female employment, underscoring the pronounced gender gap in labor force participation.

These patterns establish baseline levels for interpreting the ITT estimates and confirm that the local estimation window preserves both gender differences and meaningful variation in employment outcomes.

Main ITT Effects

We now turn to the causal effect of assignment to eligibility on employment outcomes within the local estimation window. Table 5 reports ITT estimates of the effect of ESSN eligibility on employment outcomes within the local window, presented separately for women and men.[4] For each outcome, the first column reports estimates without covariates, and the second column includes observable characteristics as controls.[5]

For women, eligibility is not associated with statistically significant changes in employment outcomes. In the specification with controls, the estimated effect on overall employment is −2.7 percentage points (standard error 0.035), relative to a baseline employment rate of 7.9 percent in the local window. The corresponding estimate without controls is −1.3 percentage points (standard error 0.032). Estimates for wage employment (−2.0 percentage points with controls), full-time employment (+3.1 percentage points), nonfarm employment (−5.1 percentage points), and employment outside the home (+1.0 percentage point) are similarly modest in magnitude and statistically indistinguishable from zero. Across outcomes, estimates remain modest and stable across specifications with and without covariates.

For men, the effects are likewise small and statistically indistinguishable from zero. In the specification with controls, eligibility is associated with a −1.7 percentage-point change in overall employment (standard error 0.049), relative to a baseline employment rate of 73.1 percent in the local window. The corresponding estimate without controls is −2.3 percentage points (standard error 0.049). Estimates for wage employment (−1.5 percentage points), full-time employment

[4] We present results separately for women and men rather than pooling the samples. Although pooling would increase statistical power, it would combine groups with markedly different baseline employment levels and labor market attachment, implying distinct margins of adjustment. In addition, the samples differ in both measurement and construction: employment outcomes for women are self-reported, whereas those for men are reported by spouses, and the male sample is restricted to currently married individuals. Pooling would therefore average effects across groups that are not directly comparable and could obscure differences in how treatment effects relate to baseline outcomes. Estimating effects separately allows for a clearer interpretation of employment responses at the eligibility margin for each group. As a robustness check, pooled specifications allowing for gender-specific effects yield estimates that are small in magnitude and statistically indistinguishable from zero, consistent with the main results.

[5] Appendix tables A.1 and A.2 report naïve OLS associations between ESSN receipt and employment outcomes and between ESSN eligibility and employment outcomes, respectively, in the overall assignment region. These estimates are descriptive and do not have a causal interpretation. Because the ESSN targets households with higher dependency burdens and other indicators of vulnerability, cross-sectional comparisons conflate program exposure with underlying differences in labor market attachment. Such correlations are often cited in policy discussions as evidence that humanitarian transfers reduce refugee labor supply. The regression discontinuity design in the main analysis addresses this selection problem by exploiting the deterministic eligibility rule to identify quasi-experimental variation at the eligibility frontier.

(+0.2 percentage points), and nonfarm employment (−3.7 percentage points) are similarly modest in magnitude and statistically indistinguishable from zero.

Overall, the estimated effects for both women and men are economically modest and stable across specifications. The results provide little evidence of economically meaningful discontinuities in employment outcomes at the eligibility frontier.

Because eligibility does not perfectly determine receipt within the local window, the design corresponds to a fuzzy regression discontinuity. Using eligibility as an instrument for reported ESSN receipt yields treatment-on-the-treated estimates that are mechanical transformations of the same local comparison, scaled by the first-stage coefficient reported in table 2. These instrumental-variable estimates are similar in sign and proportional in magnitude to the ITT effects and remain small in magnitude across outcomes. We therefore focus on the reduced-form effects of eligibility, which correspond directly to the administrative targeting rule and constitute the relevant policy estimand.

Robustness

We examine the sensitivity of the main estimates to alternative inference procedures and functional form assumptions. Appendix table A.3 reports p-values under conventional cluster-robust standard errors, HC3 corrections, jackknife variance estimation, and randomization inference. Across all employment outcomes for women and men, the conclusions are unchanged across inference procedures.

Appendix table A.4 reports estimates using wild cluster bootstrap standard errors clustered at the household level. The estimates are similar in magnitude and sign to those obtained under the baseline specification with clustering at the primary sampling unit level. While the estimate for nonfarm employment among women in the specification with controls becomes marginally significant at the 10 percent level under household-level clustering, the overall pattern of results remains unchanged, and estimates continue to be small in magnitude and not systematically different across specifications.

Appendix table A.5 presents marginal effects from binomial probit specifications estimated on the same local-window sample. These estimates are similar in magnitude and sign to the linear probability model estimates. The findings are therefore robust to alternative inference procedures and nonlinear functional form specifications.

6.3 Statistical Power

To assess the magnitudes of employment changes for which the design is informative, we compute minimum detectable effects (MDEs) for overall employment within the local estimation window under 80 percent power. Results are reported for two-sided tests at the 10 and 5 percent significance levels. The calculations use the number of individuals assigned to eligibility and non-eligibility within the window, together with the observed variance of overall employment among control-group individuals, with moments computed using survey weights.

For women, the estimation sample includes 201 eligible and 126 ineligible observations. The control-group mean for overall employment is 0.087 and the standard deviation is 0.283. Under these parameters, the design can detect absolute changes of approximately 7.9 percentage points at the 10 percent level and 8.9 percentage points at the 5 percent level. Relative to the baseline employment rate of 7.9 percent in the local window (table 4), effects of this magnitude would represent very large proportional changes in employment.

For men, the estimation sample includes 236 eligible and 191 ineligible observations. The control-group mean for overall employment is 0.744 and the standard deviation is 0.438. Under these parameters, the design can detect absolute changes of approximately 10.4 percentage points at the 10 percent level and 11.7 percentage points at the 5 percent level. Relative to a baseline employment rate of 73.1 percent in the local window, this corresponds to changes equivalent to roughly 14–16 percent of baseline employment.

The confidence intervals reported in table 5 are broadly consistent with these calculations. For both women and men, the estimates are inconsistent with large employment declines at the eligibility margin, although smaller negative effects remain within the range of statistical uncertainty.

Overall, the power calculations indicate that the design is informative about economically meaningful changes in employment at the eligibility frontier. The absence of statistically significant effects in section 6.2 is therefore difficult to reconcile with large labor supply disincentives, while remaining consistent with smaller effects within the bounds of the data.

Identification relies on comparisons within a narrowly defined set of assignment cells bordering the eligibility frontier to preserve the local-randomization assumption. Expanding the window or partitioning the assignment space would weaken the credibility of the identification strategy or introduce additional modeling assumptions.[6] The reported power calculations reflect the tradeoff between internal validity and statistical precision inherent in the design.

7. Discussion

This paper exploits a deterministic, rule-based targeting system to estimate the causal effect of ESSN eligibility on employment outcomes among non-camp Syrian refugees in Türkiye. Using a local-randomization regression discontinuity design defined around discrete demographic thresholds, we find no statistically significant discontinuities in employment probabilities for either women or men. Estimated effects are small, economically modest, and stable across

[6] In principle, treatment effects may vary along different segments of the eligibility frontier in the two-dimensional assignment space. Because eligibility is granted when either $C_h \geq 4$ or $D_h \geq 1.5$, one can conceptually partition the frontier into (i) a "dependency-ratio" segment that crosses $D_h = 1.5$ for households with $C_h < 4$, and (ii) a "children-threshold" segment that crosses $C_h = 4$ for households with $D_h < 1.5$, as well as the neighborhood of the corner where the frontier changes direction. Estimating segment-specific effects would require restricting the local window to the corresponding subset of adjacent assignment cells and conducting inference separately for each subset. In our setting, such partitioning would substantially reduce the effective sample sizes and the number of clusters available for inference in each segment, sharply lowering statistical power and weakening the reliability of finite-sample inference procedures. We therefore estimate a single ITT effect within the local window, while recognizing that the design does not provide sufficient precision for credibly distinguishing effects across frontier segments.

specifications. Interpreted at this administrative eligibility margin, the results provide little evidence that assignment to regular, unconditional cash transfers under the ESSN generated economically meaningful labor supply disincentives.

This finding can be interpreted in light of standard labor supply theory and the broader cash transfer literature. In canonical models, unconditional transfers increase non-labor income and may reduce labor supply through income effects, particularly along the extensive margin (Moffitt 2002). Transfers may also relax liquidity constraints and reduce risk, potentially offsetting such effects depending on context. Consistent with this ambiguity, empirical syntheses across low- and middle-income countries generally find limited evidence of large labor supply reductions following unconditional transfers (Baird, McKenzie, and Özler 2018; Banerjee et al. 2017; Bastagli et al. 2019). The absence of discontinuities in the ESSN setting is consistent with this broader evidence while providing causal estimates at a clearly defined administrative margin.

The structure of the labor market in which Syrian refugees participate is central to interpreting these results. Formal employment constitutes only a small share of employment among Syrian refugees in the local window. Formal hiring in Türkiye entails compliance and administrative requirements that raise costs and reduce flexibility relative to informal arrangements. These include social security registration and payroll contributions, compliance with minimum wage and labor regulations, occupational health and safety obligations, and documentation requirements. For Syrian refugees, formal employment may also require work permits and may be subject to firm-level quotas. For workers, formal employment may be less attractive when net earnings are reduced by employee contributions, when jobs are available primarily through short-duration or informal arrangements, or when long-term benefits are discounted due to uncertainty or limited perceived portability. Because most employment near the eligibility threshold occurs in informal arrangements that are not directly monitored by the program's eligibility system, the most relevant margin of adjustment is labor force participation rather than formal registration. In this setting, the absence of large employment responses is consistent with adjustment occurring along margins that are only imperfectly captured by formal employment measures.

Gender differences in baseline employment further shape interpretation. Employment rates among Syrian women near the eligibility threshold are substantially lower than those among Syrian men, indicating markedly different baseline attachment to the labor market. These differences are consistent with a combination of constraints and labor market conditions that vary systematically by gender in refugee settings, including caregiving responsibilities, mobility and safety constraints, sectoral and occupational segregation, differences in prior work experience and language proficiency, job search networks, and employer demand. Legal and administrative barriers may also interact with these factors in gender-specific ways. While this study does not identify the specific mechanisms underlying the gender gap, the large difference in initial employment levels implies that the scope for discrete changes at the eligibility margin may differ between women and men. The absence of economically meaningful discontinuities for either group should therefore be interpreted as reduced-form evidence of employment responses in a setting characterized by sharply different baseline labor market attachment across genders.

Relative to the refugee-specific evidence base, the ESSN provides a setting to examine employment responses to sustained humanitarian assistance at a clearly defined eligibility margin.

Much of the existing evidence in refugee contexts evaluates smaller-scale or shorter-term interventions and documents effects that vary across institutional environments and margins of adjustment (Altındağ and O'Connell 2023; Caria et al. 2024; Gupta et al. 2024). This analysis contributes by leveraging quasi-experimental variation generated by transparent demographic eligibility rules in a large-scale national program. By focusing on a sustained, nationwide intervention with rule-based assignment, the paper complements evidence based on smaller-scale or temporary programs. The results are consistent with prior findings that large labor supply reductions are uncommon in evaluations of unconditional transfers, while remaining specific to the institutional context studied here.

The interpretation of the estimates is inherently local. The regression discontinuity design identifies causal effects for households near the demographic thresholds defining eligibility (Cattaneo, Idrobo, and Titiunik 2020). Employment is measured at a single point in time, and the analysis does not capture longer-term trajectories in earnings, job stability, or occupational mobility. Because households may have been eligible for extended periods prior to observation, the estimates reflect steady-state differences rather than short-run adjustments following program entry. The analysis also does not capture general equilibrium effects operating through local labor demand (Egger et al. 2022). These limitations define the scope of interpretation but do not alter the central finding that economically meaningful labor supply disincentives are absent at the eligibility margin.

8. Conclusion

This paper examines whether eligibility for a large, rule-based humanitarian cash transfer program affects employment outcomes among Syrian refugees in Türkiye. Exploiting deterministic demographic thresholds governing eligibility for the ESSN, we implement a local-randomization regression discontinuity design to estimate causal effects at the eligibility cutoff. We find no statistically significant discontinuities in employment probabilities for either women or men. Estimated effects are small, economically modest, and stable across specifications, providing little evidence that regular unconditional transfers under the ESSN generated economically meaningful labor supply disincentives at the eligibility margin.

These results provide causal evidence on employment responses to sustained humanitarian cash assistance at a clearly defined administrative margin in a middle-income host country. The findings indicate that regular, rule-based income support does not generate large labor supply disincentives at this margin. The interpretation is local to households near the demographic eligibility thresholds and to the labor market conditions prevailing in Türkiye during the study period.

The results have direct implications for policy design. Concerns that cash transfers discourage work remain central in policy debates, yet the evidence here indicates no large disincentive effects in this setting. This does not imply that transfers are neutral in all contexts or along all margins, but it suggests that employment reductions are unlikely to be the primary constraint on economic participation among Syrian refugees in Türkiye. Policies that reduce or restrict income support in response to anticipated disincentives may therefore do little to improve labor market outcomes in similar settings.

Instead, policy efforts may be more effectively directed toward addressing barriers to labor market participation and job access, including information frictions, mobility constraints, weak job matching, and regulatory barriers to formal employment. Where concerns about potential disincentives persist, they may be addressed through program design features such as gradual phase-out schedules or earnings-compatible benefit structures, alongside complementary interventions that support employment transitions, including job search assistance, demand-driven training, and measures that facilitate formalization.

The results also underscore the importance of gender-specific constraints. Low female employment rates point to barriers related to caregiving responsibilities, safety, and job suitability, while male employment patterns reflect constraints in job quality and stability. Improving labor market integration, rather than reducing transfers, is therefore likely to be more important for strengthening economic participation among Syrian refugees.

Continued evaluation of sustained income support programs remains important, particularly with respect to longer-term labor market trajectories, job quality, and economic integration. Future research should examine how such programs interact with labor demand, job matching, and formalization, and whether complementary interventions can strengthen pathways into stable and productive employment. Such evidence can help inform refugee and social protection policy in contexts where income support operates alongside constrained and segmented labor markets.

Data Availability Statement

The primary data used in this study were drawn from the 2018 Türkiye Demographic and Health Survey Syrian Refugee Module, which is part of the Demographic and Health Surveys (DHS) Program. These data are publicly accessible subject to registration and approval through the DHS Program website (https://dhsprogram.com/). The authors do not have permission to redistribute the underlying microdata.

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Table 1. Distribution of Valid Households across the Assignment Grid, Overall Assignment Region

Assignment variable 1: Number of children in the household	*Assignment variable 2*: Household dependency ratio										
	0–0.4	0.5–0.9	1.0–1.4	1.5–1.9	2.0–2.4	2.5–2.9	3.0–3.4	3.5–3.9	4.0–4.4	4.5+	All
0	44	0	0	0	0	0	0	0	0	0	44
1	36	68	4	4	0	0	0	0	0	0	112
2	13	54	125	15	2	0	0	0	0	0	209
3	4	34	29	150	7	3	0	0	0	0	227
4	0	15	41	9	112	10	0	0	0	1	188
5	0	4	15	19	4	64	6	0	0	0	112
6	0	1	12	9	8	0	36	2	0	0	68
7	0	2	8	6	4	1	0	10	3	0	34
8	0	0	0	4	0	0	1	0	7	0	12
9	0	0	0	0	0	1	2	0	0	1	4
10	0	0	0	1	2	1	1	0	0	1	6
11	0	0	0	0	0	0	0	0	0	0	0
12+	0	0	0	0	2	0	0	0	0	0	2
All	97	178	234	217	141	80	46	12	10	3	1,018

Notes: Entries report the number of valid non-camp Syrian refugee households with members aged 25–54 in each cell of the two-dimensional assignment grid defined by the number of children under age 18 and the household dependency ratio. The eligibility thresholds are $C_h = 4$ children and $D_h = 1.5$ for the dependency ratio. The local estimation window used in the regression discontinuity analysis consists of eight contiguous assignment cells adjacent to this eligibility frontier, comprising three ineligible cells and five eligible cells.

Table 2. First-Stage Discontinuity in ESSN Receipt

	Overall assignment region	Local estimation window
	(1)	(2)
Eligibility indicator	0.599***	0.465***
	(0.028)	(0.045)
Constant	0.190***	0.261***
	(0.020)	(0.031)
Observations	1,018	418
R^2	0.348	0.214

Notes: Estimates are from OLS regressions of reported ESSN receipt on the eligibility indicator. The dependent variable is an indicator for reported ESSN receipt. The overall assignment region includes all valid non-camp Syrian refugee households with members aged 25–54. The local estimation window restricts the sample to assignment cells adjacent to the eligibility frontier. Wild cluster bootstrap standard errors, based on 500 replications and clustered at the primary sampling unit level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 3. Covariate Balance across the Eligibility Threshold

Covariate	Women				Men			
	Overall assignment region		Local estimation window		Overall assignment region		Local estimation window	
	Mean	Discontinuity	Mean	Discontinuity	Mean	Discontinuity	Mean	Discontinuity
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age	33.927	–1.488***	33.127	–1.184	36.02	3.100***	35.104	2.220***
	(6.646)	(0.515)	(7.078)	(0.884)	(7.591)	(0.442)	(7.566)	(0.684)
Years of education	6.225	–0.960***	7.089	–0.192	6.972	–0.678**	7.078	–0.007
	(4.088)	(0.346)	(4.142)	(0.505)	(4.023)	(0.271)	(4.027)	(0.447)
Year of arrival in Türkiye (indicators)								
2011 or 2012	0.132	–0.002	0.123	–0.021	0.135	0.014	0.110	–0.009
	(0.339)	(0.030)	(0.328)	(0.042)	(0.342)	(0.022)	(0.314)	(0.029)
2013	0.269	0.065*	0.263	0.066	0.274	0.065**	0.279	0.097**
	(0.444)	(0.036)	(0.441)	(0.053)	(0.446)	(0.033)	(0.449)	(0.048)
2014	0.233	–0.064**	0.229	–0.092*	0.239	–0.047*	0.267	–0.114***
	(0.423)	(0.028)	(0.421)	(0.048)	(0.427)	(0.026)	(0.443)	(0.043)
2015	0.191	–0.001	0.223	0.028	0.176	0.007	0.199	0.048
	(0.393)	(0.030)	(0.417)	(0.043)	(0.381)	(0.025)	(0.400)	(0.036)
2016	0.111	0.011	0.107	0.007	0.109	–0.036*	0.096	–0.025
	(0.314)	(0.024)	(0.310)	(0.036)	(0.312)	(0.019)	(0.295)	(0.027)
2017 or 2018	0.065	–0.008	0.055	0.012	0.066	–0.004	0.049	0.004
	(0.246)	(0.017)	(0.228)	(0.026)	(0.248)	(0.015)	(0.217)	(0.019)
Region of residence (indicators)								
İstanbul	0.167	–0.096***	0.190	–0.117***	0.185	–0.089***	0.197	–0.060
	(0.373)	(0.031)	(0.393)	(0.041)	(0.388)	(0.028)	(0.398)	(0.037)
Mediterranean	0.366	–0.048	0.367	–0.022	0.361	–0.045	0.379	–0.033
	(0.482)	(0.043)	(0.483)	(0.054)	(0.481)	(0.035)	(0.486)	(0.046)
Southeast Anatolia	0.356	0.117***	0.345	0.162***	0.332	0.144***	0.311	0.147***
	(0.479)	(0.040)	(0.476)	(0.052)	(0.471)	(0.033)	(0.463)	(0.048)
Other	0.111	0.027	0.099	–0.022	0.122	–0.009	0.113	–0.054
	(0.315)	(0.024)	(0.299)	(0.035)	(0.327)	(0.026)	(0.317)	(0.037)

Table 3. Covariate Balance across the Eligibility Threshold

Covariate	Women				Men			
	Overall assignment region		Local estimation window		Overall assignment region		Local estimation window	
	Mean	Discontinuity	Mean	Discontinuity	Mean	Discontinuity	Mean	Discontinuity
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Observations	850	850	327	327	1,016	1016	427	427

Notes: Means are reported in columns (1), (3), (5), and (7), with standard deviations in parentheses. Columns (2), (4), (6), and (8) report estimated discontinuities at the eligibility frontier from OLS regressions of each covariate on the eligibility indicator. Wild cluster bootstrap standard errors, based on 500 replications and clustered at the primary sampling unit level, are reported in parentheses. The overall assignment region includes all valid non-camp Syrian refugee households with members aged 25–54. The local estimation window restricts the sample to assignment cells adjacent to the eligibility frontier. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 4. Descriptive Statistics: Employment Outcomes

Variable	Women		Men	
	Overall assignment region	Local estimation window	Overall assignment region	Local estimation window
	(1)	(2)	(3)	(4)
Overall employment	0.085	0.079	0.677	0.731
	(0.278)	(0.271)	(0.468)	(0.444)
Wage employment	0.062	0.058	0.592	0.630
	(0.242)	(0.234)	(0.492)	(0.483)
Full-time employment	0.055	0.043	0.630	0.686
	(0.229)	(0.203)	(0.483)	(0.464)
Nonfarm employment	0.060	0.064	0.636	0.701
	(0.238)	(0.245)	(0.481)	(0.459)
Employment outside home	0.058	0.046	--	--
	(0.233)	(0.209)		
Public employment	0.004	0.006	0.017	0.012
	(0.059)	(0.078)	(0.128)	(0.108)
Formal employment	0.001	--	0.006	0.002
	(0.035)		(0.077)	(0.048)

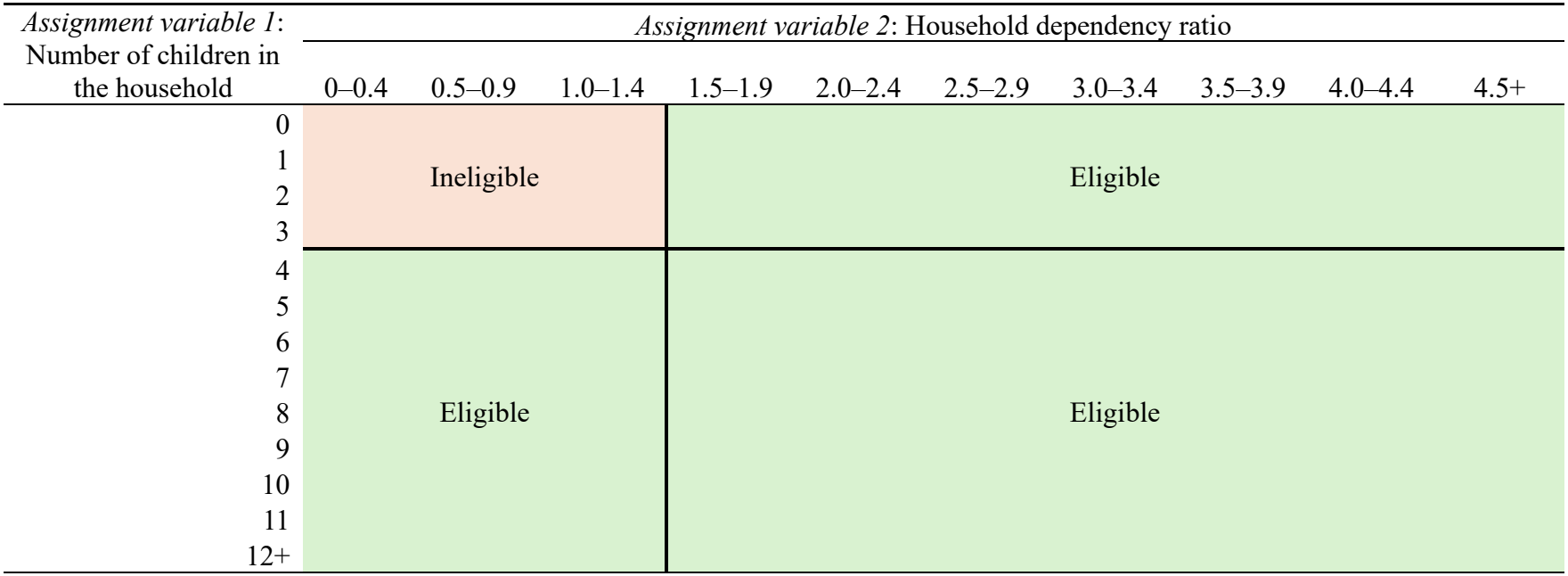
Notes: Entries report sample means. Standard deviations are reported in parentheses. The overall assignment region includes all valid non-camp Syrian refugee households with members aged 25–54. The local estimation window restricts the sample to assignment cells adjacent to the eligibility frontier. Employment outside the home is not reported for men because the survey instrument does not measure this outcome comparably across genders. Formal employment is not reported for women within the local window because incidence is negligible.

Table 5. Intent-to-Treat Effects on Employment Outcomes, Local Estimation Window

	Overall employment		Wage employment		Full-time employment		Nonfarm employment		Employment outside home	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Women										
ITT	–0.013	–0.027	–0.009	–0.020	0.031	0.031	–0.037	–0.051	0.010	0.010
	(0.032)	(0.035)	(0.028)	(0.031)	(0.021)	(0.021)	(0.030)	(0.032)	(0.024)	(0.026)
Constant	0.087***	–0.490	0.063***	–0.102	0.024*	–0.290	0.087***	–0.339	0.040**	0.100
	(0.025)	(0.392)	(0.021)	(0.372)	(0.014)	(0.347)	(0.025)	(0.309)	(0.017)	(0.322)
Controls	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*
Observations	327	327	327	327	327	327	327	327	327	327
R^2	0.001	0.027	0.000	0.020	0.006	0.025	0.006	0.064	0.001	0.020
B. Men										
ITT	–0.023	–0.017	–0.026	–0.015	–0.019	0.002	–0.040	–0.037		
	(0.049)	(0.049)	(0.049)	(0.048)	(0.051)	(0.050)	(0.051)	(0.048)		
Constant	0.744***	–0.926	0.644***	–0.284	0.697***	–0.632	0.723***	–1.240**		
	(0.034)	(0.620)	(0.038)	(0.631)	(0.036)	(0.658)	(0.036)	(0.603)		
Controls	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*		
Observations	427	427	427	427	427	427	427	427		
R^2	0.001	0.121	0.001	0.130	0.000	0.113	0.002	0.137		

Notes: Entries report intent-to-treat (*ITT*) estimates of the effect of eligibility on employment outcomes within the local estimation window. Estimates are from OLS regressions of each employment indicator on the eligibility indicator within the local window. "Controls" indicates inclusion of observable covariates: age, age squared, years of education, indicators for year of arrival in Türkiye (omitted category: 2013), and indicators for region of residence (omitted category: İstanbul). Wild cluster bootstrap standard errors, based on 500 replications and clustered at the primary sampling unit level, are reported in parentheses. Employment outside the home is not reported for men because the survey does not measure this outcome comparably across genders. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Figure 1. Eligibility Status across the Assignment Grid, Overall Assignment Region



Notes: The figure displays the assignment cells comprising the local estimation window around the eligibility frontier for the ESSN. Rows correspond to the number of children under age 18, and columns correspond to the household dependency ratio. Eligibility is determined by the deterministic administrative rule $C_h \geq 4$ or $D_h \geq 1.5$. Within the local estimation window, three cells are ineligible under the rule, defined by $C_h < 4$ and $D_h < 1.5$, and five cells are eligible. The discontinuous change in eligibility status along the frontier formed by these thresholds provides the basis for the local-randomization regression discontinuity design.

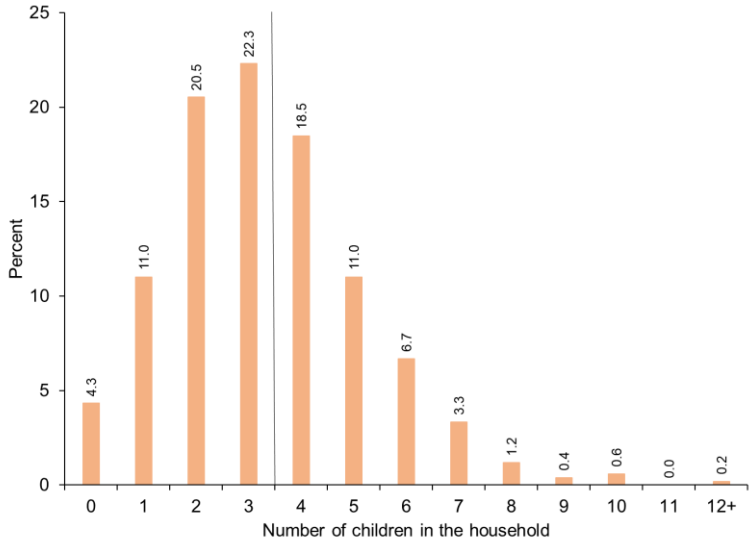
Figure 2. Eligibility Status within the Local Estimation Window

Assignment variable 1: Number of children in the household	*Assignment variable 2*: Household dependency ratio									
	0–0.4	0.5–0.9	1.0–1.4	1.5–1.9	2.0–2.4	2.5–2.9	3.0–3.4	3.5–3.9	4.0–4.4	4.5+
0										
1										
2			Ineligible	Eligible						
3		Ineligible	Ineligible	Eligible						
4		Eligible	Eligible	Eligible						
5										
6										
7										
8										
9										
10										
11										
12+										

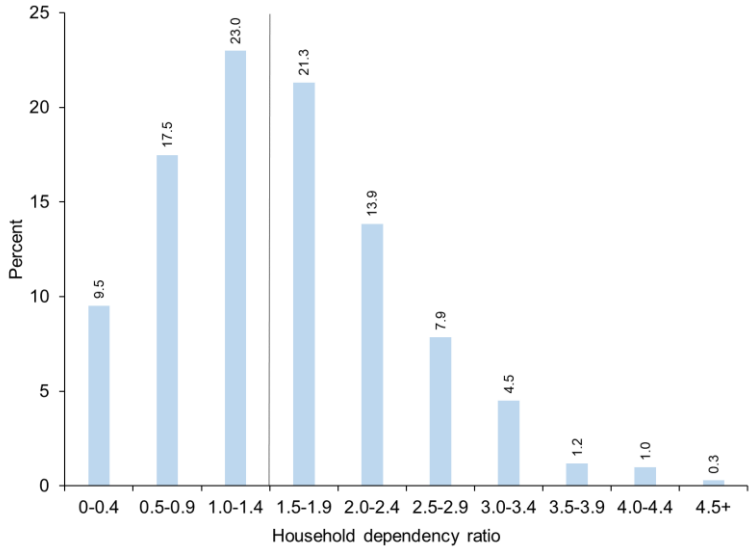
Notes: The figure displays the assignment cells comprising the local estimation window around the eligibility frontier for the ESSN. Rows correspond to the number of children under age 18, and columns correspond to the household dependency ratio. Eligibility is determined by the deterministic administrative rule $C_h \geq 4$n or $D_h \geq 1.5$. Within the local estimation window, three cells are ineligible under the rule, defined by $C_h < 4$ and $D_h < 1.5$, and five cells are eligible. The discontinuous change in eligibility status along this frontier forms the basis of the local-randomization regression discontinuity design.

Figure 3. Distribution of Assignment Variables in the Overall Assignment Region

A. Number of children in the household (assignment variable 1)



B. Household dependency ratio (assignment variable 2)



Notes: The histograms display the empirical distribution of the two assignment variables used to determine eligibility for the ESSN: the number of children under age 18 and the household dependency ratio. Vertical lines indicate the demographic eligibility thresholds $C_h = 4$ and $D_h = 1.5$. The sample comprises valid non-camp Syrian refugee households with members aged 25–54 in the full assignment region prior to restricting the sample to the local estimation window.

Figure 4. Probability of ESSN Receipt across the Assignment Grid, Overall Assignment Region

Assignment variable 1: Number of children in the household	*Assignment variable 2*: Household dependency ratio										
	0–0.4	0.5–0.9	1.0–1.4	1.5–1.9	2.0–2.4	2.5–2.9	3.0–3.4	3.5–3.9	4.0–4.4	4.5+	All
0	0.11										0.11
1	0.11	0.07	0.25	0.25							0.10
2	0.08	0.22	0.18	0.60	0.00						0.21
3	0.25	0.38	0.48	0.78	0.86	1.00					0.68
4		0.47	0.68	0.67	0.89	0.90				0.00	0.80
5		0.50	0.67	0.73	1.00	0.91	1.00				0.84
6		1.00	0.83	0.67	0.88		0.81	1.00			0.81
7		0.00	0.88	0.83	0.75	0.00		0.90	0.67		0.76
8				1.00			0.00		0.71		0.75
9						1.00	1.00			1.00	1.00
10				0.00	0.50	1.00	1.00			1.00	0.67
11											
12+					0.50						0.50
All	0.11	0.22	0.39	0.75	0.87	0.90	0.83	0.92	0.70	0.67	0.55

Notes: Entries report the weighted mean of reported ESSN receipt within each assignment cell in the overall assignment region. The eligibility thresholds are $C_h = 4$ and $D_h = 1.5$ for the household dependency ratio. Cells satisfying $C_h \geq 4$ or $D_h \geq 1.5$ are eligible under the administrative rule. The shading gradient reflects the magnitude of the estimated receipt probability, with darker shading indicating higher probabilities. The sample includes valid non-camp Syrian refugee households with members aged 25–54.

Appendix A

Table A.1. Naïve OLS Estimates of Employment Effects Using Actual Receipt, Overall Assignment Region

	Overall employment		Wage employment		Full-time employment		Nonfarm employment		Employment outside home	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Women										
Actual receipt	–0.059***	–0.060***	–0.044**	–0.043**	–0.035**	–0.034**	–0.047***	–0.044***	–0.056***	–0.052***
	(0.019)	(0.019)	(0.017)	(0.017)	(0.015)	(0.014)	(0.015)	(0.016)	(0.016)	(0.015)
Constant	0.122***	–0.221	0.090***	0.088	0.077***	–0.152	0.090***	–0.269	0.093***	0.048
	(0.018)	(0.319)	(0.016)	(0.260)	(0.014)	(0.250)	(0.014)	(0.220)	(0.016)	(0.244)
Controls	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*
Observations	850	850	850	850	850	850	850	850	850	850
R^2	0.010	0.024	0.008	0.021	0.005	0.030	0.009	0.050	0.013	0.031
B. Men										
Actual receipt	–0.068**	–0.034	–0.085***	–0.048	–0.073***	–0.033	–0.078**	–0.056*		
	(0.029)	(0.030)	(0.030)	(0.030)	(0.028)	(0.029)	(0.032)	(0.030)		
Constant	0.715***	–0.213	0.640***	0.196	0.671***	–0.257	0.680***	–0.453		
	(0.027)	(0.394)	(0.027)	(0.390)	(0.028)	(0.397)	(0.032)	(0.394)		
Controls	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*		
Observations	1,016	1,016	1,016	1,016	1,016	1,016	1,016	1,016		
R^2	0.005	0.136	0.007	0.131	0.006	0.132	0.006	0.145		

Notes: Entries report naïve OLS estimates of the association between actual ESSN receipt and employment outcomes in the overall assignment region. These estimates are descriptive correlations and do not have a causal interpretation. "Controls" indicates inclusion of observable covariates: age, age squared, years of education, indicators for year of arrival in Türkiye (omitted category: 2013), and indicators for region of residence (omitted category: İstanbul). Wild cluster bootstrap standard errors, based on 500 replications and clustered at the primary sampling unit level, are reported in parentheses. Employment outside the home is not reported for men because the survey does not measure this outcome comparably across genders. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.2. Naïve Intent-to-Treat Estimates Using Eligibility, Overall Assignment Region

	Overall employment		Wage employment		Full-time employment		Nonfarm employment		Employment outside home	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Women										
Eligibility indicator	–0.076***	–0.072***	–0.055***	–0.052***	–0.027*	–0.018	–0.069***	–0.059***	–0.056***	–0.047***
	(0.021)	(0.021)	(0.018)	(0.020)	(0.016)	(0.016)	(0.018)	(0.018)	(0.017)	(0.018)
Constant	0.136***	–0.277	0.100***	0.047	0.074***	–0.135	0.107***	–0.322	0.096***	0.034
	(0.020)	(0.327)	(0.018)	(0.271)	(0.015)	(0.253)	(0.017)	(0.225)	(0.018)	(0.253)
Controls	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*
Observations	850	850	850	850	850	850	850	850	850	850
R^2	0.016	0.027	0.011	0.023	0.003	0.027	0.018	0.054	0.013	0.028
B. Men										
Eligibility indicator	–0.070**	–0.053	–0.078**	–0.052	–0.073**	–0.049	–0.088**	–0.085***		
	(0.033)	(0.033)	(0.034)	(0.033)	(0.032)	(0.033)	(0.036)	(0.032)		
Constant	0.720***	–0.289	0.639***	0.156	0.675***	–0.325	0.690***	–0.575		
	(0.027)	(0.391)	(0.030)	(0.396)	(0.029)	(0.403)	(0.033)	(0.384)		
Controls	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*		
Observations	1,016	1,016	1,016	1,016	1,016	1,016	1,016	1,016		
R^2	0.005	0.137	0.006	0.131	0.005	0.133	0.008	0.148		

Notes: Entries report naïve intent-to-treat estimates from OLS regressions of each employment outcome on the eligibility indicator in the overall assignment region. These estimates are descriptive and do not have a causal interpretation. "Controls" indicates inclusion of observable covariates: age, age squared, years of education, indicators for year of arrival in Türkiye (omitted category: 2013), and indicators for region of residence (omitted category: İstanbul). Wild cluster bootstrap standard errors, based on 500 replications and clustered at the primary sampling unit level, are reported in parentheses. Employment outside the home is not reported for men because the survey does not measure this outcome comparably across genders. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.3. Sensitivity of Inference to Alternative Variance Estimators, Intent-to-Treat Effects, Local Estimation Window

	Overall employment		Wage employment		Full-time employment		Nonfarm employment		Employment outside home	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Women										
ITT	–0.013	–0.027	–0.009	–0.020	0.031	0.031	–0.037	–0.051	0.010	0.010
Clustered (PSU)	0.693	0.441	0.753	0.522	0.144	0.154	0.215	0.108	0.670	0.709
Heteroskedasticity-robust	0.688	0.427	0.748	0.516	0.143	0.146	0.205	0.101	0.664	0.697
HC3 correction	0.689	0.436	0.749	0.524	0.144	0.157	0.207	0.107	0.665	0.704
Jackknife	0.688	0.435	0.748	0.523	0.144	0.156	0.207	0.106	0.665	0.704
Randomization inference	0.850	0.410	0.800	0.470	0.230	0.180	0.190	0.070	0.790	0.750
Controls	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*
B. Men										
ITT	–0.023	–0.017	–0.026	–0.015	–0.019	0.002	–0.040	–0.037		
Clustered (PSU)	0.639	0.727	0.606	0.761	0.718	0.962	0.426	0.437		
Heteroskedasticity-robust	0.590	0.691	0.586	0.760	0.681	0.959	0.363	0.397		
HC3 correction	0.591	0.697	0.587	0.763	0.681	0.960	0.364	0.405		
Jackknife	0.590	0.696	0.586	0.763	0.681	0.959	0.364	0.405		
Randomization inference	0.670	0.670	0.620	0.730	0.780	0.950	0.440	0.410		
Controls	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*		

Notes: Entries report intent-to-treat (*ITT*) estimates from OLS regressions of employment outcomes on the eligibility indicator within the local estimation window. The table reports *p*-values corresponding to alternative variance estimators: standard errors clustered at the primary sampling unit (PSU) level; heteroskedasticity-robust standard errors; HC3-corrected standard errors; jackknife resampling standard errors; and randomization inference based on permutation tests within the local window. "Controls" indicates inclusion of observable covariates: age, age squared, years of education, indicators for year of arrival in Türkiye (omitted category: 2013), and indicators for region of residence (omitted category: İstanbul). Employment outside the home is not reported for men because the survey does not measure this outcome comparably across genders.

Table A.4. Intent-to-Treat Effects on Employment Outcomes, Local Estimation Window, Household-Level Clustering

	Overall employment		Wage employment		Full-time employment		Nonfarm employment		Employment outside home	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Women										
ITT	–0.013	–0.027	–0.009	–0.020	0.031	0.031	–0.037	–0.051*	0.010	0.010
	(0.032)	(0.034)	(0.028)	(0.031)	(0.022)	(0.022)	(0.029)	(0.031)	(0.024)	(0.027)
Constant	0.087***	–0.490	0.063***	–0.102	0.024*	–0.290	0.087***	–0.339	0.040**	0.100
	(0.025)	(0.427)	(0.022)	(0.379)	(0.014)	(0.361)	(0.025)	(0.348)	(0.017)	(0.336)
Controls	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*
Observations	327	327	327	327	327	327	327	327	327	327
R^2	0.001	0.027	0.000	0.020	0.006	0.025	0.006	0.064	0.001	0.020
B. Men										
ITT	–0.023	–0.017	–0.026	–0.015	–0.019	0.002	–0.040	–0.037		
	(0.043)	(0.044)	(0.048)	(0.048)	(0.046)	(0.046)	(0.045)	(0.044)		
Constant	0.744***	–0.926*	0.644***	–0.284	0.697***	–0.632	0.723***	–1.240**		
	(0.031)	(0.522)	(0.035)	(0.543)	(0.033)	(0.556)	(0.032)	(0.534)		
Controls	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*		
Observations	427	427	427	427	427	427	427	427		
R^2	0.001	0.121	0.001	0.130	0.000	0.113	0.002	0.137		

Notes: Entries report intent-to-treat (*ITT)* estimates of the effect of eligibility on employment outcomes within the local estimation window. Estimates are from OLS regressions of each employment indicator on the eligibility indicator within the local window. "Controls" indicates inclusion of observable covariates: age, age squared, years of education, indicators for year of arrival in Türkiye (omitted category: 2013), and indicators for region of residence (omitted category: İstanbul). Wild cluster bootstrap standard errors, based on 500 replications and clustered at the household level, are reported in parentheses. Employment outside the home is not reported for men because the survey does not measure this outcome comparably across genders. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.5. Sensitivity to Binomial Probit Specification, Marginal Effects, Local Estimation Window

	Overall employment		Wage employment		Full-time employment		Nonfarm employment		Employment outside home	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Women										
ITT	–0.013	–0.021	–0.009	–0.015	0.031	0.029	–0.037	–0.052	0.010	0.013
	(0.032)	(0.030)	(0.027)	(0.025)	(0.021)	(0.018)	(0.030)	(0.033)	(0.024)	(0.021)
Controls	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*
Observations	327	327	327	327	327	327	327	269	327	327
B. Men										
ITT	–0.023	–0.020	–0.026	–0.019	–0.019	0.002	–0.040	–0.045		
	(0.049)	(0.051)	(0.049)	(0.052)	(0.051)	(0.053)	(0.051)	(0.050)		
Controls	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*		
Observations	427	427	427	427	427	427	427	427		

Notes: Entries report marginal effects of the eligibility indicator from binomial probit regressions estimated within the local estimation window. Marginal effects are evaluated at sample means. "Controls" indicates inclusion of observable covariates: age, age squared, indicators for year of arrival in Türkiye (omitted category: 2013), indicators for region of residence (omitted category: İstanbul), and indicators for educational attainment (omitted category: primary education completed). Cluster-robust standard errors, clustered at the primary sampling unit level, are reported in parentheses. Employment outside the home is not reported for men because the survey does not measure this outcome comparably across genders. Observations in panel A, column (8) are lower due to collinearity among control variables in that specification. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.