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Unexpected Work Incentives: How Cash Transfers Raise Refugee Men's Labor Supply

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Abstract

A central concern with social and humanitarian assistance is that unconditional cash transfers might discourage work among recipients. Refugees, however, differ from the chronically poor households in most cash-transfer studies. Displaced from once-stable livelihoods, many retain skills and labor-force attachment that transfers might help reactivate. We study this question using the largest humanitarian cash transfer scheme in the world, the Emergency Social Safety Net (ESSN) in Turkey, which for most of the past decade hosted more refugees than any other country. Drawing on a survey designed to represent Turkey's refugee population, we exploit the program's dependency-ratio eligibility rule in a regression discontinuity design to estimate the effect of cash transfers on refugee employment. Far from discouraging work, we find that receiving cash transfers increases the probability that any man in the household works by 13.2 percentage points and the share of working men by 19.3 percentage points. We find no evidence of an effect on refugee women's already-low labor supply. A design-based local-randomization analysis around the eligibility cutoff corroborates these responses with exact finite-sample inference, implying effects of 9.8 and 17.4 percentage points, respectively. Turning to mechanisms, we find that the transfers improve access to information, reduce reliance on harmful food-coping strategies, and raise enrollment in Turkish language courses. The transfers also raise spending on education. These findings suggest that cash transfers relax the liquidity and information constraints that refugees face in the labor market rather than inducing a shift toward leisure.

JEL classification

I38, J21, J22, J61, O15

Keywords

humanitarian assistance, forced displacement, unconditional cash transfers, social protection, work disincentives, regression discontinuity design, Turkey

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1 Introduction

Forced displacement now ranks among the world's most pressing global challenges. The number of refugees and internally displaced individuals (IDPs) has increased sharply since 2010, standing at 117.8 million people forcibly displaced at the end of 2025 (United Nations High Commissioner for Refugees, 2026). Displacement constitutes a major and often long-lasting disruption of adults' working lives: refugees lose assets and networks, face legal and language barriers to employment, and must rebuild their livelihoods in an unfamiliar labor market (Fiala, 2015; Betts et al., 2024; Schuettler and Do, 2025). Failing to integrate refugees economically leads to various adverse economic and political outcomes in host countries (Couttenier et al., 2019; Dustmann and Landersø, 2024; Fasani et al., 2021; Bousquet et al., 2025; Kırdar et al., 2022; Altındağ and Kaushal, 2021), and policy makers are increasingly interested in policies that improve their self-reliance (Erdoğan et al., 2021; Foged et al., 2024; Hoogeveen et al., 2025). Yet, the evidence remains thin on whether large-scale social assistance helps or hurts the ability of displaced individuals to support themselves, especially for the vast majority of refugees and IDPs who live in low- and middle-income countries (Maystadt et al., 2019). This article helps fill that gap by investigating the effects of social assistance on labor supply in the unique setting of Turkey, which for most of the past decade hosted more refugees than any other country and where the Emergency Social Safety Net (ESSN) constitutes the largest humanitarian cash transfer program for refugees globally. Nearly all of them live in cities and towns rather than camps, a rare setting in which unconditional assistance at scale can be studied among refugees living within host communities.

The concern that cash assistance discourages work is longstanding. While the fear that social assistance fosters "dependency" is frequently invoked in public debate, recent evidence, reviewed in Section 2, suggests that cash transfers have minimal or slightly positive impacts on labor supply (Banerjee et al., 2017; Crosta et al., 2024). However, whether these findings carry over to refugees is an open question (Baird et al., 2018). Indeed, refugees may not respond positively to transfers due to the specific barriers to labor force participation that they face. Because work opportunities available to them are frequently costly, informal, or precarious, cash transfers could have a negative impact on their labor supply. Finally, refugees tend to have pressing consumption needs and requests from their social networks, and their saving behavior responds to the prospect of return migration (Jakiela and Ozier, 2016; Dustmann and Görlach, 2016; Kırdar, 2012; Alrababa'h et al., 2023).

On the other hand, refugee families differ from the chronically poor households that this

literature has typically studied in a way that bears directly on the labor-supply question. The evidence that cash transfers do not reduce adult labor supply comes overwhelmingly from long-term low-income households, whose poverty reflects a persistent shortage of skills, assets, and opportunities. The poverty of refugees, by contrast, is largely the product of a sudden displacement shock rather than of chronic disadvantage (Fiala, 2015). Many Syrian refugees were middle-income earners with stable jobs before the conflict and retain occupational skills, work experience, and a strong attachment to the labor force. Consistent with this, despite formal barriers to employment, Syrian refugee men in Turkey work at rates close to those of native men, though overwhelmingly in informal, low-paid jobs (Demirci and Kırdar, 2023). For such a population, cash transfers can address the very constraints that limit their participation in high-quality economic opportunities by financing job search and enabling investment in productive activities. In doing so, they may help reactivate the occupational skills and labor-force attachment that refugees carry from their pre-displacement lives. For all of these reasons combined, the effect of unconditional transfers on the labor supply of refugee adults is theoretically ambiguous.

In this article, we measure the labor supply effects of the ESSN, a regular, unconditional cash transfer program for refugees rolled out in Turkey in 2016 and reaching 1.8 million individuals by 2021. The transfer is large relative to household resources: for the average six-member family, the 2018 benefit amounted to roughly 36 percent of monthly household consumption and 55 percent of average male labor income among Syrian refugees in Turkey (Aygün et al., 2024). We use data from the Comprehensive Vulnerability Monitoring Exercise (CVME), a World Food Programme (WFP) survey designed to represent Turkey's refugee population. To identify program impacts, we rely on one of the program's central eligibility rules: a family qualifies for ESSN transfers when its ratio of dependents to working-age adults (the dependency ratio) reaches 1.5. We build a regression discontinuity design (RDD) on this discrete running variable, comparing households on either side of the 1.5 threshold. Working within a fuzzy RDD framework, we test its assumptions extensively, and conduct a wide range of robustness checks, including a design-based local-randomization analysis around the eligibility cutoff.

The results show that, far from discouraging work, the ESSN substantially raises the labor supply of adult men. Specifically, receiving cash transfers increases the probability that any man in the household works by 13.2 percentage points and the probability that any adult works by 9.8 percentage points. The share of working men rises by 19.3 percentage points and the share of working adults by 13.5 percentage points. The effects on the total numbers of working men and of all adults are positive but imprecisely estimated, indicating

that the response is concentrated in whether at least one man works and in the share of men working rather than in large increases in worker counts. We find no evidence of an effect on female labor supply along any margin, in a context where refugee female labor participation is very low. The extensive-margin effect for men is the statistically strongest result, surviving wild-cluster bootstrap inference and multiple-testing corrections. Consistent with the RDD results, the design-based local-randomization analysis implies effects of 9.8 percentage points for any man working and 17.4 percentage points for the share of working men, and the randomization confidence intervals contain the parametric estimates.

We then present exploratory evidence of the mechanisms behind this positive labor-supply response. We find that the transfers improve connectivity and access to information: beneficiaries use their phones more frequently for communication and are more likely to consume news, which may reduce information frictions in the labor market. The program also sharply reduces reliance on harmful coping strategies. In addition, beneficiaries invest more in human capital and the future, increasing spending on education and enrolling more in Turkish language courses. We also find suggestive evidence of a personal well-being mechanism: we estimate large reductions in measures of psychological distress, but they are not robust to the design-based local-randomization analysis. We find no evidence of effects on physical health or social networks. Taken together, these patterns are consistent with cash transfers decreasing frictions in the labor market by relaxing the liquidity and information constraints that refugees face.

To the best of our knowledge, our study generates the first causal evidence on the labor-supply effects of a large-scale, sustained unconditional cash transfer program for refugees. As such, it contributes to the literature in several ways. First, it adds to the broad literature on social assistance, in particular the relationship between cash transfers and adult labor supply. Recent studies have debunked the hypothesis that social assistance creates dependency among poor beneficiaries, and showed no detectable impacts of cash transfers on adult labor outcomes (Banerjee et al., 2017; Baird et al., 2018). A recent meta-analysis shows a significant 3.4 percentage-point increase in labor supply generated by cash transfers (Crosta et al., 2024). Our study shows a much larger increase in labor supply of 13.2 percentage points, for a different type of population. Moreover, the mechanism at play is different from those identified in Sub-Saharan Africa and in other poor settings, where cash transfers help households invest in agricultural and non-agricultural income-generating activities (Handa et al., 2018; Fisher et al., 2017). Instead, our results suggest that in our setting (an upper-middle-income and OECD country), cash transfers support the integration of individuals in the labor market of the host country after a transition out of the labor market due to

displacement and to specific constraints related to their refugee situation.

Second, it contributes to the literature on forced displacement, and more specifically, on public policies aimed at supporting refugees. A sizable literature examines how the Syrian inflow affected the labor market outcomes of Turkish natives (Del Carpio and Wagner, 2015; Ceritoglu et al., 2017; Aksu et al., 2022; Aracı et al., 2022); much less is known about the economic lives of the refugees themselves. Despite strong policy interest in this issue, there is very little rigorous evidence on how to improve refugee self-sufficiency (Ott and Montgomery, 2015; Hoogeveen et al., 2025). Several studies have shown the effectiveness of job matching or skill-related interventions (including language training) for improving labor market integration (Foged et al., 2024; Heller and Mumma, 2023). In a context of rising anti-immigration sentiment (Rozo and Vargas, 2021), other studies have focused on the negative impact of cutting social assistance or implementing employment bans on labor market outcomes (Marbach et al., 2018; Fasani et al., 2021; Dustmann and Landersø, 2024), as well as on the benefits of granting forced migrants legal status (Bahar et al., 2021; Ibáñez et al., 2025). Given refugees' particular situation, Baird et al. (2018) suggest that the impact of cash transfers on this specific population's labor supply may be negative. We show, instead, that cash transfers provided to refugees can generate an increase in labor supply of a larger magnitude than what Crosta et al. (2024) find for the general population and Altındağ and O'Connell (2023) show for a small-scale, short-term cash transfer for refugees in Lebanon. Our article complements other studies of cash transfer support to refugees in Turkey, which showed important improvements in children's schooling, transitions out of child labor, and nutrition and growth (Aygün et al., 2024; Demirci et al., 2026), by showing positive impacts on adults in the labor market.

The remainder of the paper is organized as follows. Section 2 reviews the literature on cash transfers and adult labor supply, and Section 3 introduces the institutional background. Section 4 presents the data, and Section 5 describes the identification strategy and the estimation. Section 6 shows the main results, and Section 6.3 the alternative identification strategy. Section 7 concludes.

2 Cash transfers and adult labor supply

Basic economic theory predicts that unconditional cash transfers (UCTs) generate an income effect on time allocation: beneficiaries would substitute part of their income-earning time for leisure, reducing their labor supply (Becker, 1965). Related to this prediction, the

notion that social programs create a "dependency" among their recipients is pervasive and is sometimes instrumentalized by the press to criticize social protection policy (Lindert and Vincensini, 2010; Kalebe-Nyamongo and Marquette, 2014). A substantial empirical literature, however, has contributed to debunking this view. Using data from RCTs conducted in seven government-led programs in six middle-income countries in Latin America and Asia, Banerjee et al. (2017) find no evidence that cash transfers discourage work. In a broader narrative review, Baird et al. (2018) conclude that cash transfers generally do not affect adult labor supply, although they can generate trade-offs for specific populations. Similarly, Handa et al. (2018) show in seven African countries that cash transfer beneficiaries switch from less desirable casual wage labor to work in their own non-farm household enterprises. This is consistent with qualitative evidence from five African countries that reports a displacement from "subsistence" agricultural wage to higher-return on-farm activities (Fisher et al., 2017).

In a recent systematic review of 115 UCT studies, Crosta et al. (2024) find that among the 17 studies reporting labor-supply impacts, the overall effect is positive at both the extensive and the intensive margin. The average effect is relatively small, a 3.4 percentage-point increase at the median transfer amount, and is largely driven by two positive outliers from one program in Nigeria. The same meta-analysis also documents a larger shift from wage employment (-11.6 percentage points) toward non-wage employment (+14.7 percentage points). Country-specific studies support these findings, reporting null or positive effects of cash transfers on overall labor supply in high-income (Aizer et al., 2024), middle-income (Ardington et al., 2009; Rubio-Codina, 2010), and low-income settings (Covarrubias et al., 2012).

At the same time, the impacts of cash transfer programs on labor supply are heterogeneous, reflecting the various mechanisms at play. For instance, Gromadzki (2024) finds a medium-term negative effect of a universal cash transfer on the labor supply of low-socioeconomic-status households in Poland. In Brazil, Bolsa Familia increases the labor supply of the most disadvantaged groups but decreases formal earnings among urban men (Vidigal, 2026). This literature concerns settled, multi-dimensionally poor households. Instead, we test the dependency concern for displaced individuals with previous ties to the labor market, a setting where it is voiced most loudly.

Indeed, the pathways through which cash transfers can affect labor supply are multiple and context-dependent. Several studies in developing contexts emphasize the investment mechanism: as cash transfers alleviate credit and saving constraints, beneficiaries invest in productive activities for which they increase their labor supply (Covarrubias et al., 2012; Gertler et al., 2012; Stoeffler et al., 2020; Akresh et al., 2025). In these settings, cash transfers

can also operate as an insurance mechanism, allowing households to protect earnings and assets when shocks occur or to undertake higher-risk, higher-return activities ex ante; both mechanisms are likely to increase labor supply (Fisher et al., 2017; Macours et al., 2022; Premand and Stoeffler, 2022).

Another mechanism relates to better job matching, because program beneficiaries can be more active in their job search (overcoming liquidity constraints) or more patient (using the transfers as insurance). As a result, cash transfers conditioned on migration or job search have led to increases in job search, labor supply, and other outcomes (Bryan et al., 2014; Abebe et al., 2021), but it is not clear whether the same mechanism operates for unconditional transfers. For forcibly displaced individuals in particular, other potential mechanisms include better integration into the host society (Valli et al., 2019) and improved mental health (Zimmerman et al., 2021; McGuire et al., 2022; El-Enbaby et al., 2024). To our knowledge, however, none of the existing studies documents a clear pathway from cash transfers to labor market outcomes through these mediating factors. We address this gap from within a single design by estimating the effects of the same program, on the same population, on both labor supply and the mediating factors themselves.

We focus specifically on refugees, who may respond differently to cash transfer programs (Betts et al., 2024). They usually face additional barriers to labor force participation: their right to work is strongly constrained from an administrative standpoint, and their qualifications may not be officially recognized in the host country (Fasani et al., 2021). They are also likely to lack language skills, hold fewer assets, have access to different networks, and suffer from other adverse consequences of displacement, such as trauma and hopelessness (Ibáñez and Vélez, 2008; Habib et al., 2019; Moya, 2018; Moya and Carter, 2019). Foged et al. (2024) find that in Denmark, language training and strong initial labor markets are key determinants of refugees' long-run labor market integration, and that matching interventions have at least short-term effects. This suggests that factors such as language skills, job search and matching, and co-ethnic networks constrain the labor supply of refugees (Foged et al., 2024; Dagnelie et al., 2019). Baird et al. (2018) note that UCTs in refugee or humanitarian contexts are likely to reduce labor supply, given the costly or dangerous nature of the work opportunities generally available. In Lebanon, Altındağ and O'Connell (2023) study a cash transfer for refugees and document a shift from child to adult labor supply during the transfer period; the program's impacts, however, dissipate shortly after the transfers end. The evidence is scarce, as noted by Baird et al. (2018) and by a systematic review that remains inconclusive because of the lack of rigorous studies (Ott and Montgomery, 2015). Our article adds to this scarce evidence by evaluating a sustained program at national scale, comple-

menting the smaller-scale study of Altındağ and O'Connell (2023) as well as the companion evaluations of the ESSN, which focus on child outcomes (Aygün et al., 2024; Demirci et al., 2026).[1] In doing so, we contribute to the emerging literature that aims at identifying public policies that support refugee self-reliance and integration in host countries efficiently (Moya and Rozo, 2025).

3 Background: Refugees in Turkey and the ESSN program

3.1 Refugees in Turkey

The war that began in Syria in 2011 had pushed more than 5.5 million people beyond its borders as of mid-December 2020. Approximately 65% of them (3.6 million) of these refugees moved to Turkey, which kept its border open and is Syria's largest neighbor (UNHCR, 2020). Most arrived between 2013 and 2017. By the end of 2020, a mere 1.6% remained in camps, the rest having settled in cities and towns among host communities.

During the early years of Syrian displacement to Turkey, registration and camp administration were handled by Turkey's Disaster and Emergency Management Authority (AFAD). The *Temporary Protection Regime*, enacted in 2014, codified Syrian refugees' rights of access to public health care, public education, and social protection. It also created a Directorate General for Migration Management, now the Turkish Presidency of Migration Management (TPMM), to oversee registration and policy coordination. A European Union–Turkey agreement reached at the end of 2015 on financing and managing the so-called "refugee crisis" paved the way for the ESSN. The program was funded by the EU under the Facility for Refugees in Turkey (FRiT).

Syrian refugees differ markedly from natives in their demographic characteristics. Their households are larger, with about 6 members on average compared with 3.5 among Turkish families. They are also younger, less educated, and poorer. The Pre-Assistance Baseline

[1] Altındağ and O'Connell (2023) is closely related to our study. Both papers examine unconditional cash transfers to Syrian refugees in a neighboring host country. The Lebanese program, however, was small and temporary, and its effects dissipated once the transfers ended, whereas the ESSN is an ongoing program at national scale. Also, we document a larger increase in adult male employment together with the intermediate outcomes that plausibly support it. Closer still are the companion evaluations of the ESSN itself, which show that the program keeps refugee children in school and out of work (Aygün et al., 2024) and improves child growth and nutrition (Demirci et al., 2026). The present paper turns to the adults of the same households, while expanding the time period (fifth wave of CVME) and using alternative, robust identification strategies.

(PAB) survey, administered to ESSN applicants before any transfers were distributed, indicates the depth of this disadvantage: 76 percent of eligible refugee households lived below the poverty line and 24 percent were in food insecurity (i.e., reported unacceptable food consumption) (Cuevas et al., 2019).

Formal employment has remained largely out of reach for Syrian refugees. Until January 2016, they could not work legally except under special circumstances. Work permits became available thereafter, but take-up remained limited because of the conditions attached to them. Only 34,573 permits were issued to Syrians in 2018 (Memişoğlu, 2018; Ministry of Family, Labour and Social Services, 2019). As a consequence, most Syrian refugees earn their living in the informal sector, with no social security coverage. Refugee inflows have correspondingly expanded informal economic activity in host regions (Altındağ et al., 2020; Akgündüz et al., 2023). Using the nationally representative refugee sample of the 2018 Turkey Demographic and Health Survey (TDHS), Demirci and Kırdar (2023) show that paid-employment rates among Syrian refugee men (61.8%) approach those of native men (68.9%).[2] This paid-employment rate reaches its peak within a year of arrival, and holds steady afterwards. Paid-employment rates among refugee women, by contrast, are very low: they work at far lower rates (6%) than native women (22.2%), with little catch-up over time.

Refugees also differ from natives in the type of work they perform. They are overrepresented in wage work and underrepresented among the self-employed and employers. They take part-time jobs more frequently and cluster overwhelmingly in informal employment, facing an especially wide gap in access to formal jobs (see also Pinedo-Caro, 2020). In sum, refugee men tend to find work quickly but in poor-quality informal jobs, while women mostly stay out of the labor force. This gender pattern largely predates displacement. Female labor force participation in pre-conflict Syria, at 13 percent in 2010, was among the lowest in the world (World Bank, 2026). This context shapes our expectations for where the labor-supply effects of the ESSN are likely to appear.

Turkey also hosts refugees from other countries in the region. The ESSN and the CVME

[2]These rates are measured in 2018, while the ESSN was already in operation, and therefore they incorporate any effect of the program on beneficiary households. The embedded component equals the share of refugee men living in beneficiary households times the program's average effect on their individual employment. In the 2018 round of the CVME, about 38 percent of adult men in Syrian-headed refugee households lived in beneficiary households. This share is smaller than the roughly 48 percent of all refugees, because beneficiary households have high dependency ratios and thus relatively few working-age men. Our design identifies effects only for high-dependency households near the eligibility cutoff; but even if the program's average effect on all beneficiary men equaled our largest estimate there (19.3 percentage points for the share of working men), the counterfactual no-program employment rate would be at most about 7 percentage points lower, or roughly 55 percent. The qualitative pattern of high male informal employment and very low female employment is unchanged under any such adjustment.

cover all registered refugees, so these groups also enter our data. Non-Syrian households account for roughly one quarter of our estimation sample and consist mainly of Iraqis and, to a lesser extent, Afghans. Their legal situation differs from that of the Syrians. Most forcibly displaced individuals from Afghanistan or Iraq are unregistered in Turkey and therefore work informally. Those who are registered hold international protection status, rather than temporary protection while awaiting resettlement to a third country. In 2020, registered Afghans numbered 158,253 and registered Iraqis 281,074 (Turkish Statistical Institute, 2026), while the number of unregistered Afghans in Turkey is estimated to exceed one million (Karadağ and Sert, 2023). In Section 6.2.4, we show that our results are robust to restricting the sample to Syrian households.

3.2 The ESSN Program

The ESSN is an unconditional cash transfer scheme that provided monthly assistance to over 1.8 million refugees in Turkey as of February 2021 (International Federation of Red Cross and Red Crescent Societies, 2021). Launched nationwide in November 2016 with EU financing under the FRiT, it is the world's largest cash transfer program for refugees. The WFP supported the program's design, delivery, monitoring, and evaluation through its first two phases, until April 2020. From then on, the Turkish Red Crescent (TRC) operated it jointly with the International Federation of Red Cross and Red Crescent Societies (IFRC) and the Ministry of Family and Social Services. In August 2023, implementation was transferred to the Ministry, with the TRC continuing as a collaborating institution, and the ESSN was consolidated under the Social Cohesion Assistance Programme for Foreigners (Yabancılara Yönelik Sosyal Uyum Yardımları, Y-SUY). The new program maintained the eligibility and payment conditions established under the ESSN. The program thus continues to provide regular cash assistance to vulnerable refugee households using the same demographic criteria, marking a rare case of a humanitarian cash program being fully absorbed into the host country's social assistance system. Our study period falls entirely within the program's initial phases, before these institutional transitions.

Applying for the ESSN is open to registered refugees of any nationality. Application requires prior registration with the TPMM under temporary or international protection, together with a residential address on file with the local Civil Registry. Unregistered refugees cannot apply, although they are included in the CVME sampling frame. A household then qualifies by meeting any one of six demographic criteria: (1) a dependency ratio of at least 1.5, i.e., no fewer than three dependents per two able-bodied adults aged 18 to 59; (2) four or more

children; (3) a woman living alone; (4) an elderly household head (aged 60 and above); (5) a single parent; or (6) a member with a documented disability of 40% or more. The criteria were chosen as transparent proxies for poverty and vulnerability, reflecting the program's mandate for simple and transparent inclusion rules (Cuevas et al., 2019). Applications are taken at TRC Service Centers and at Social Assistance and Solidarity Foundation offices. Turkish authorities strictly enforced eligibility rules in all phases from initial registration through transfer disbursement and subsequent monitoring.[3]

In 2018, the benefit amount received was 120 TL per household member per month. For the average six-member Syrian family, this amounts to 720 TL per month, or roughly USD 149 at that year's average exchange rate of 4.83 TL/USD. Quarterly top-ups of 50 to 250 TL per person, decreasing in household size (250 TL for households of one to four members, 150 TL for five to eight, and 50 TL for nine or more), helped with lumpy expenses such as rent. Families with members who needed specialized care received an additional monthly payment. Benefits were loaded onto debit cards that beneficiaries could use for ATM withdrawals and purchases. Set against refugees' earnings, transfer amounts are substantial: a six-member family's 720 TL monthly transfer corresponded to about 55% of Syrian men's average monthly labor income and roughly 43% of Turkey's monthly minimum wage in 2017. These amounts place the ESSN at the high end of the cash-transfer literature, where benefits typically represent 10–25% of pre-program household consumption. They equaled roughly 20% for Mexico's *Progresa* (Hoddinott and Skoufias, 2004), 11% for Ghana's LEAP (Handa et al., 2013), and 12% for a multiyear program in rural Niger (Premand and Stoeffler, 2022). The program is also distinctive in its duration. Unlike many humanitarian transfers that last six months or less, the ESSN has provided continuous monthly support since 2016.

4 Data

4.1 Data Source, Sample Construction, and Outcomes

The data come from the Comprehensive Vulnerability Monitoring Exercise (CVME), a survey regularly conducted by WFP in Turkey from 2017 to 2020 to assess the vulnerability of the refugee population (World Food Programme, 2019). Its questionnaire covers the house-

[3]For instance, household composition was strictly verified by the administration, and eligibility was automatically adjusted based on current household member age and composition (see Section 5.3.1 for how this relates to our identification strategy). In addition, holding a valid work permit or owning registered assets in Turkey disqualified households, ensuring that the transfers reached the most vulnerable households.

hold's demographic composition and arrival profile, housing, income sources, consumption spending, food security and coping behavior, health indicators, and special needs. Importantly for our purposes, the CVME records the employment of adult household members, disaggregated by gender, as well as a rich set of variables that allow us to investigate the mechanisms through which cash transfers may affect labor supply.

We combine the third, fourth, and fifth waves of the CVME (CVME3 to CVME5). Each wave is a cross-sectional, representative survey of refugees in Turkey. Sampled households span ESSN beneficiaries, unsuccesful applicants, and non-applicants. Fieldwork took place from March to August 2018 for CVME3, from September to December 2018 for CVME4, and from November 2019 to February 2020 for CVME5. WFP staff conducted the interviews in person with support from the TRC. In total, 4,106 households were interviewed: 1,301 in CVME3, 1,380 in CVME4, and 1,425 in CVME5.

Our primary outcome variables capture adult labor supply. For men, women, and all adults, we construct (i) an extensive-margin indicator equal to one if any such member works ("any man works," "any woman works," and "any adult works"); (ii) the number of working members; and (iii) the share of members who work. As is standard in this setting, employment refers to market work in the 30 days preceding the survey. We report results for the population aged 18 and over.

To understand the mechanisms at play, we also examine a set of intermediate outcomes included in the CVME questionnaire. These include: *connectivity*—mobile-phone ownership and the frequency with which household members use their phones for messaging, voice calls, translation, news, and social media (in the 7 days preceding the survey); *food security and food expenditure*—the WFP's reduced Coping Strategies Index (rCSI) and several measures of food spending in adult-equivalent terms; *non-food expenditure*—spending on education, clothing, debt repayment, social events, agricultural inputs, savings, construction, and other essential and non-essential items; *adult health*—the share of (prime-age) men reported sick and household healthcare expenditure in the 6 months preceding the survey; *social cohesion*—whether households seek advice from Syrian or Turkish friends on daily tasks and on jobs or financial matters; *language and technical skills*—the share of men who speak Turkish, who attend Turkish language courses, and who attend technical or vocational courses; and *personal well-being*—a set of indicators for the frequency of mental-health-related problems in the two weeks preceding the survey. Some of these modules are available only in particular waves (for example, connectivity, the language and technical skills module, and personal well-being are only included in CVME4 and social cohesion in CVME5), as indicated in the notes to the corresponding tables.

Our main analysis restricts the sample to households that are *not* eligible for the ESSN through any criterion other than the dependency ratio. These households that qualify through one of the other criteria would receive transfers regardless of where they fall relative to the dependency-ratio cutoff. Retaining them would make the discontinuity in beneficiary status at the cutoff less sharp. Because this discontinuity identifies treatment effect, excluding households eligible through other criteria, as we do, makes the first stage estimation stronger (see section 5). Within this sample, our main specification further restricts the running variable to the interval $[0, 3]$, i.e., 1.5 units of the dependency ratio on either side of the cutoff. All of the descriptive statistics and results reported below are based on this estimation sample. We assess the robustness of these findings to alternative bandwidths and, in Appendix B, to using the full sample that retains the households eligible through the other criteria (Section 6.2.4).

4.2 Descriptive Statistics

Appendix Table A1 presents descriptive statistics for the main outcome variables from waves 3 to 5 of the CVME survey, restricted to the estimation sample. Panel A documents an important gender gap in adult labor supply: 79 percent of households report at least one working man, but only 7.5 percent report at least one working woman. Furthermore, the average share of adult men who work (70 percent) is more than ten times that of women (6 percent).

The remaining panels summarize the mechanism outcomes. Mobile phones are widespread (67 percent ownership) and are used mainly for interpersonal communication and social media. Task-specific uses such as reading the news (17 percent reported for "some days") and translation (12 percent) are less common (Panel B). In terms of livelihoods, the average household scores 14.4 on the 0–56 rCSI scale, indicating a frequent use of adverse coping strategies, although not at alarming levels compared to other humanitarian contexts. Food consumption is essentially market-based (Panel C). Non-food spending over the preceding six months is dominated by essential items, with education expenditure averaging 124 TL and savings 120 TL (Panel D). About 12 percent of households report a sick adult man (Panel E). Households rely heavily on Syrian networks for advice (78 percent) and far less on Turkish friends (29 percent) (Panel F). Among working-age men, 54 percent speak Turkish, but only 4.7 percent have received a Turkish-language course and 1.1 percent a technical or vocational course (Panel G).

Panel H reports the five personal well-being (PW) items, each measured on a 0–3 frequency

scale, and the aggregate PW-Score on a 0–15 scale. The average PW-Score is 6.4, indicating a relatively high level of mental health issues. The sample sizes in this panel (160 to 169 observations) are considerably smaller than those in the other panels, reflecting the fact that this module was administered to only a subsample of respondents.[4] Appendix Table A2 provides summary statistics for the control variables.

5 Identification Method and Estimation

We estimate the effect of ESSN transfers with two complementary regression discontinuity approaches. The two approaches rest on distinct identification assumptions. The first approach is parametric. It uses the full estimation window and fits separate linear trends in the dependency ratio on the two sides of the eligibility cutoff. It then estimates the effect of beneficiary status by two-stage least squares (Section 5.1). The second rests on local randomization and dispenses with trends altogether. It restricts the sample to narrow windows of mass points around the cutoff and treats households on the two sides as as-good-as-randomly assigned, using exact Fisherian inference (Section 5.2). Section 5.3 discusses the plausibility of the assumptions underlying the two designs and presents the corresponding validity checks.

5.1 The Parametric Design

To identify the effect of receiving cash transfers, we exploit the ESSN eligibility criteria in a regression discontinuity design. As noted above (Section 3), one of the ESSN's eligibility criteria is having a dependency ratio of 1.5 or above. Beneficiary status should therefore increase sharply at this value. As a consequence, other outcomes affected by cash transfers, such as adult employment, would potentially change discontinuously at the 1.5 threshold as well. We begin with the reduced-form effect of the dependency-ratio criterion, estimated

[4]The questions in this module were administered in private, and only a subset of the Wave 4 sample responded. Of the 704 households in our Wave 4 estimation sample, 169 answered at least one item and 160 answered all five. Appendix Table A12 shows that the response varies strongly with the region of residence. Specifically, the module was not fielded in three of the ten regions represented in Wave 4. Response rates also vary based on nationality and time of arrival. However, household composition, the dependency ratio, and beneficiary status are balanced across response status. Most importantly for our design, Appendix Table A13 shows that the response does not change discontinuously at the eligibility cutoff, and covariate balance at the cutoff is largely maintained within respondents. We therefore interpret the well-being estimates as internally valid for the areas in which the module was fielded, while noting that they rest on a small subsample.

from the sharp RDD specification

$$y_i = \beta_0 + \beta_1 T_i + \mathbb{I}(T_i = 0)\, f(x_i) + \mathbb{I}(T_i = 1)\, g(x_i) + X_i\Gamma + u_i, \tag{1}$$

where y_i is the outcome for household i. The treatment indicator T_i equals one when the dependency ratio is 1.5 or above and zero otherwise, while $\mathbb{I}(\cdot)$ is the indicator function. The functions $f(\cdot)$ and $g(\cdot)$ are the outcome trends on the two sides of the cutoff, which we take as linear functions estimated separately on each side in our benchmark specification. We discuss this choice in more detail later in this section. In Equation (1), x_i is the dependency ratio, X_i is the set of control variables, and u_i is the error term. The coefficient β_1 gives the reduced-form (intention-to-treat) effect of the dependency-ratio criterion on the outcome. The control vector X_i includes survey-wave dummies, household-head characteristics, such as nationality (Afghan, Iraqi, Syrian, and other), sex, education (illiterate, literate, primary, intermediate, secondary, technical, university), and occupation before arrival (management/highly skilled, skilled worker, unskilled worker, retired, other), as well as the time of arrival in Turkey and the region of residence (12 NUTS-1 regions).

Although eligibility is a deterministic function of household composition as recorded in the administrative system, beneficiary status in our data is not a deterministic function of the dependency ratio that we construct from the survey. Some households below the cutoff receive transfers while some above it do not (Figure 1). These patterns reflect discrepancies between the surveyed household and the assistance unit recorded in ISAS at the time eligibility was assessed, including members registered under a different household ID, differences in recorded ages or certified disability status, and changes in household composition between the administrative re-evaluation and the interview date, alongside potential misreporting of program receipt.[5] Because these discrepancies generate imperfect compliance on both sides

[5]Concretely, these discrepancies can arise through four channels. (i) *Household-unit mismatch:* the surveyed household need not coincide with the assistance unit registered in ISAS. For example, an adult son living with his parents may hold a separate household ID, so that transfers granted to one registered unit accrue to a surveyed household whose own dependency ratio lies below the cutoff. Conversely, a surveyed household split across several registered IDs may receive no transfers, although its pooled dependency ratio exceeds 1.5. (ii) *Component mismatch:* eligibility classifies members by their registered age and counts certified disabled members as dependents. Discrepancies between registered and reported ages (most consequentially around the age-18 and age-60 boundaries) or in disability certification status can place the administrative dependency ratio on the opposite side of the cutoff from the survey-based ratio. Disability certification is imperfectly observed in the survey, which records whether a member holds a disability health report but not the certified disability percentage, whereas the program requires an official report indicating a disability of at least 40 percent. (iii) *Timing:* the beneficiary status we observe refers to the survey date, so compositional changes that are invisible to the registry or that occur after the latest monthly administrative review leave eligibility resting on a composition that differs from the one we observe. (iv) *Misreported receipt:* although the survey question asks about the household's status in the ESSN by explicitly stating the program's name, respondents may conflate it with other assistance delivered through the same Kızılaykart

of the cutoff, we use a fuzzy RDD to recover the effect of *being a beneficiary*, estimated by two-stage least squares:

$$D_i = \alpha_0 + \alpha_1 T_i + \mathbb{I}(T_i = 0)\, k(x_i) + \mathbb{I}(T_i = 1)\, l(x_i) + X_i\Delta + \nu_i, \tag{2}$$

$$y_i = \gamma_0 + \gamma_1 \widehat{D}_i + \mathbb{I}(T_i = 0)\, m(x_i) + \mathbb{I}(T_i = 1)\, n(x_i) + X_i\Theta + e_i. \tag{3}$$

Equation (2) is the first stage, in which the beneficiary indicator D_i is regressed on the same variables as in Equation (1). We set $D_i = 1$ if the household reports beneficiary status in the survey's ESSN status question and zero otherwise. Households reporting that they have been found eligible but have not yet received payments (1.3 and 3.9 percent of below- and above-cutoff observations in our main estimation window, respectively) are coded as non-beneficiaries, and we verify that our results are robust to excluding them. Equation (3) replaces treatment assignment with the predicted beneficiary status $\widehat{D}_i$. The coefficient γ_1 identifies the local average treatment effect of receiving transfers for compliers near the cutoff.

The ESSN status question also permits a direct decomposition of this non-compliance (Appendix Table A4). Application to the program is widespread on both sides of the cutoff: 61 percent of below-cutoff households report having applied and been found ineligible, consistent with the administrative implementation of the dependency-ratio criterion. Among above-cutoff households, 13 percent report having applied and been found ineligible, reflecting the discrepancies discussed above. A further 14 percent report never applying. Almost four-fifths of these non-applicants cite registration-related reasons, such as not being registered with the DGMM or the civil registry, being registered in another province, or awaiting identity documents. Because application decisions reflect the eligibility criteria themselves (below-cutoff non-applicants predominantly report believing or being told they would not qualify), we do not condition the sample on application status.

Our identification is parametric. Given the discrete support of the running variable, only a limited number of mass points are available to pin down the trends on each side of the cutoff, so higher-order polynomials are weakly identified and render the estimated discontinuity sensitive to the fit at individual support points. Moreover, since the polynomial terms vary only across mass points, they absorb scarce cluster-level degrees of freedom. Our main

debit-card platform, most notably the Conditional Cash Transfer for Education (CCTE). Consistent with the operation of the child-independent channels, reported receipt is positive (5.3 percent) even among households with a dependency ratio of zero, for whom no child-related channel can operate, and rises to roughly one third at the support points just below the cutoff (34.5 percent, albeit among only 29 households; Figure 1). None of these channels threatens identification provided that the wedge between the survey-based and administrative measures does not change discontinuously at the cutoff; deliberate misdeclaration of household composition at registration, which could induce such a discontinuity, is addressed in Section 5.3.1.

analysis therefore fits separate linear trends on the two sides of the cutoff, in line with the recommendation of Gelman and Imbens (2019) to favor low-order polynomials. We begin with a wide interval covering nearly the entire support of the dependency ratio and progressively narrow the bandwidth around the cutoff: $[0, 6]$, $[0, 4.5]$, and $[0, 3]$. We use the symmetric $[0, 3]$ interval as our main specification. Narrowing the bandwidth limits reliance on the functional form. We also assess robustness to specifications that enter the numerator (the number of dependents) and the denominator (the number of prime-age adults) of the running variable separately in levels and in logarithms, following Demirci et al. (2026).

The discreteness of the running variable also shapes our inference. Because the fitted linear trends cannot be expected to pass exactly through the mean outcome at every mass point, our estimates embody an approximation error alongside the usual sampling error. Following Lee (2008), we treat this specification error as a random component shared by all households with the same value of the dependency ratio: if the linear trend over- or under-predicts the mean outcome at a given mass point, it does so for every household located at that point. Accordingly, we cluster standard errors on the dependency ratio so that our uncertainty statements reflect the quality of the trend approximation and not only sampling variation. Because the number of clusters equals the number of support points and is therefore modest (32 in our main estimation window and fewer in the narrower ones), we report wild-cluster bootstrap p-values (Cameron and Miller, 2015) alongside conventional clustered standard errors. Finally, Kolesár and Rothe (2018) caution that clustering on a discrete running variable can understate uncertainty when specification errors are large, sometimes performing worse than conventional heteroskedasticity-robust standard errors. We thus report heteroskedasticity-robust standard errors alongside the clustered ones. We do not implement their honest confidence intervals, which would guard against specification error in the fitted trends, because our local-randomization design serves the same purpose by dispensing with trends altogether.

5.2 The Discrete (Local-Randomization) Design

Our second identification approach takes the logic of narrowing the bandwidth to its limit and dispenses with trends altogether, treating the discreteness of the running variable as a design feature rather than an obstacle. The local-polynomial estimators with data-driven bandwidths, standard for continuous running variables, may not be well-suited to our setting. Local-polynomial estimators rely on the ability to shrink the estimation window arbitrarily close to the cutoff, which is difficult in our setting due to the discreteness of the dependency

ratio. Following Lee and Lemieux (2010), we therefore view nonparametric evidence as a complement to, rather than a substitute for, the parametric RD estimates. We implement the local-randomization RD framework of Cattaneo et al. (2015, 2020), which treats observations within a narrow window of dependency-ratio values around the cutoff as-if randomly assigned, without imposing any functional form on how the outcomes vary with the running variable.

The design remains fuzzy within each window. We therefore report the reduced-form (ITT) effect of crossing the eligibility cutoff and the first-stage effect on beneficiary status. We scale the former by the latter to obtain the implied local treatment effect of receiving the transfer.

We select windows manually at the observed mass points around the cutoff, guided by the within-window covariate-balance diagnostics reported in Appendix Table C1. Our preferred window is $[-0.5, 0]$ in the centered running variable, corresponding to dependency ratios between 1 and 1.5. This is the narrowest window that delivers a strong first stage and passes the covariate-balance check. Windows narrower than this are not informative in our setting. The only interior mass point just below the cutoff with a meaningful number of observations (a dependency ratio of 4/3) contains 20 households, 40 percent of which receive transfers despite falling below the cutoff. The resulting first stage is too weak for reliable inference. As a cross-check, we also report results for a wider window, $[-0.5, 0.5]$, covering dependency ratios between 1 and 2.

Inference is Fisherian and centers on the reduced-form effects. We report randomization p-values together with 95 percent confidence intervals obtained by inverting the randomization test under a constant treatment effect, both of which are exact in finite samples. The implied treatment effects are obtained by scaling each ITT by the first stage in that outcome's estimation sample. We report these as descriptive magnitudes without separate inferential statements, since large-sample p-values for the scaled estimates are unreliable at the module sample sizes. We assess the comparability assumption with within-window balance tests alongside our other validity checks below. Section 6.3 presents the results. Because the parametric and discrete designs rely on distinct assumptions (correct trend specification versus local randomization), their agreement reduces the concern that our conclusions are driven by either assumption.

5.3 Plausibility of Identification Assumptions

Both designs require that households be unable to sort themselves across the eligibility threshold. Sorting would violate the continuity assumption of the parametric design and the local-randomization assumption of the discrete design alike. This subsection evaluates that requirement from three perspectives: the institutional setting, the structure of refugee households, and direct empirical checks.

5.3.1 Institutional Context

One potential threat to our identification strategy would arise if households manipulated their composition to qualify for the program. The institutional setting leaves little room for such behavior. As described in Section 3, an ESSN application requires registration with the DGMM, which typically occurs upon arrival. During this process, officials verify background documents, take statements where needed, and rule on Temporary Protection Status. Approved refugees are issued a Temporary Protection Identification Card tied to a household ID. They must then record their residence address with the Directorate of Civil Registry before an ESSN application can be filed under that ID. Every application flows into the Integrated Social Assistance System (ISAS), the central platform running Turkey's social assistance programs. Its verification process leaves little scope for false declarations of household composition or for discretionary staff decisions.

Applicants found eligible are notified by text message to retrieve their ATM cards. They remain subject to unannounced home visits within the following year, during which the declared household composition is checked against the actual situation of the registered members. If the beneficiary's situation does not match ISAS records, ESSN assistance can be removed. Moreover, ISAS automatically re-evaluates eligibility every month. Changes in household circumstances, such as a child reaching age 18 or an adult acquiring a work permit, affect eligibility status and actual transfers. Taken together, the registration requirements, automated monthly re-evaluations, and the home visits make manipulation of eligibility status very unlikely.

Two distinct issues should be kept apart here. The institutional features above concern the household's ability to strategically misrepresent its composition to the administration, which is what the validity of the design requires ruling out: a household cannot place its *registered* composition on the eligible side of the cutoff at will. They do not imply that the composition we measure in the survey coincides with the registered assistance unit. Non-

strategic divergences between the two, arising from differences in the household definition, in recorded characteristics, and in timing, generate the imperfect compliance documented above, which our fuzzy design is built to accommodate. Such measurement wedges would threaten identification only if they changed discontinuously at the cutoff, a possibility that we examine directly below.

5.3.2 Household Structure

Another potential concern for our identification strategy would arise if household structures in our sample were uniform. If, for example, every household consisted of two parents, eligibility under the dependency-ratio criterion would coincide with having three or more children. In that scenario, we could not separate the effect of crossing the threshold from that of an additional child in a two-child family. The treatment indicator, the running variable, and the number of children would move together along a single family-size progression.

Substantial heterogeneity in refugee household structures breaks this mechanical overlap (Table 1). While households with two prime-age adults (non-disabled, aged 18 to 59) are the most common configuration, accounting for roughly 66% of the estimation sample, approximately 14% of households contain three prime-age adults, 9% contain four, and 4% contain only one. Consequently, each prime-age adult count from one to six appears on both sides of the eligibility threshold within our estimation window. The estimated discontinuity aggregates comparisons of households that cross the threshold at several different sizes. These include two-adult households with two versus three dependents, three-adult households with four versus five, four-adult households with five versus six, and so on. As such, identification does not rest on a single contrast between households with two and three children.

This heterogeneity is also what gives identifying content to the robustness checks in which the two components of the dependency ratio enter the specification as separate trends. The eligibility threshold corresponds to a different number of dependents for each count of prime-age adults. A third dependent, for example, makes a two-adult household eligible but leaves a three-adult household ineligible. Eligibility therefore depends on the interaction between the two counts. It cannot be absorbed by trends in the number of dependents and the number of prime-age adults entered separately. We motivate and present these checks in Section 6.2. Household-structure heterogeneity documented here also directly informs our local-randomization design (Section 5.2). Since the configurations that populate the two sides of the narrow window differ, the comparability of the two sides is examined with the within-window balance tests reported below.

5.3.3 Checks of the Identification Assumption

Since predetermined characteristics cannot respond to the treatment, a testable implication of both designs' assumptions (Sections 5.1 and 5.2) is that these characteristics should exhibit no jump at the cutoff. Table 2 indicates that only 6 out of the 37 covariates show a statistically significant difference at the 10 percent level. When we apply a stricter 5 percent threshold, only 3 remain significant. For comparison, roughly 3.7 and 1.9 rejections, respectively, would be expected by chance in the absence of any true discontinuity. However, counting rejections across a series of pairwise tests does not provide a definitive measure of overall balance. Kerwin et al. (2024) show that pairwise comparisons leave the overall verdict ambiguous, while standard omnibus tests over-reject the null. They recommend basing balance assessments on randomization inference instead. Our local-randomization design supplies the natural setting for this recommendation. Within the narrow window around the cutoff, assignment to the two sides of the threshold is taken as good as random, so balance can be assessed by randomization inference. We therefore complement the covariate-by-covariate comparison with a within-window balance check based on randomization inference (Appendix Table C1). This check also evaluates the comparability assumption of the discrete design, and we return to it in that context in Section 6.3.

In addition to general concerns with baseline covariates, a particular concern given our running variable is the potential manipulation of household composition. We find no evidence of such behavior. The estimates for the household-structure variables in Table 2, including household size, the number of children, and the number of dependents, are statistically insignificant.

Both designs also require that households cannot precisely manipulate the running variable to gain eligibility; the institutional context of Section 5.3.1 makes this highly unlikely, and we examine it empirically as well. Because our running variable is discrete, the density tests designed for continuous scores (such as that of Cattaneo et al., 2018) are not well suited to our setting. We therefore proceed in two complementary ways. First, we inspect the distribution of the dependency ratio directly (Appendix Figure A1). Although its discrete nature produces natural peaks at half-integer values (e.g., 0.5, 1.0, 1.5), there is no irregular spike at the 1.5 cutoff relative to the other values. This pattern is consistent with the absence of manipulation.

Second, because fertility is the only realistic margin through which a household could adjust its dependency ratio, we test directly for a fertility response at the cutoff. Strategic fertility would generate an excess of very young children among households just above the threshold.

Thus, it implies a positive discontinuity in the presence of infants. Appendix Table A3 reports RD estimates for the presence and the number of members under age two and for the pregnancy indicator, using the same specification as the covariate regressions above. None of the estimates supports strategic fertility behavior.[6]

As a final check on the design, we estimate placebo policy effects at artificial cutoff values. We estimate the treatment effects at four alternative thresholds: -0.75 points from the actual cutoff (using the sample on the left-hand side) and $+0.5$, $+1.0$, and $+1.5$ points from the actual cutoff (using the sample on the right-hand side). We find no statistically significant discontinuity in beneficiary status at any of these artificial thresholds (Table 3). Similarly, for labor supply outcomes, the estimates are generally statistically insignificant. While a few coefficients appear significant using standard clustering, none retains significance at the 5 percent level under the wild-cluster bootstrap, thereby supporting the conclusion that our main results are capturing a true policy effect rather than spurious local variation.

6 Results

We present the results in the order of the two designs. Section 6.1 reports the parametric estimates and Section 6.2 their robustness; Section 6.3 reports the results of the local-randomization design. Sections 6.4 and 6.5 then examine the heterogeneity of the employment response and the mechanisms behind it.

6.1 Main Results from the Parametric Design

Figure 1 illustrates the first stage in two complementary ways. Panel (b) presents the raw data: the fraction of beneficiary households at each value of the dependency ratio. The jump

[6]The point estimates for children under two are negative, i.e., they have the opposite sign of what manipulation would produce. A direct comparison of the two mass points adjacent to the cutoff shows no significant difference in any of the three measures (not reported), indicating that the negative estimates reflect trend extrapolation across the discrete support rather than a local jump. The pregnancy indicator is also negative. It cannot, however, serve as a manipulation channel, because an unborn child does not enter the dependency ratio. It could only indicate an intention to manipulate in the future. Moreover, had such anticipatory conceptions been occurring since the program's launch, the resulting births would appear as an excess of children under two on the eligible side of the cutoff; the null estimates for the under-two indicators rule this out. Since it is measured at the survey date, it partly reflects fertility behavior after program receipt rather than a predetermined characteristic. In addition, under the wild-cluster bootstrap none of the three discontinuities is statistically significant (column (4) of Appendix Table A3; $p \geq 0.224$). We therefore find no evidence of fertility responses aimed at obtaining ESSN benefits by a manipulation of the dependency-ratio criterion, consistent with the household-structure results above.

at the cutoff is immediate in the raw data, from 40 percent at the mass point just below the threshold to 77 percent at the threshold itself. Panel (a) presents the same relationship in smoothed form, binning the running variable and fitting linear trends on each side of the cutoff.[7] The two panels convey the same first-stage pattern. For the outcome and mechanism variables that follow, some of which are observed in a single survey wave and therefore in far smaller cells, the raw cell means become cluttered, while the smoothed format remains legible. All subsequent RD figures in the main text therefore use the format of Panel (a).

Figure 2 provides a first, descriptive look at the employment responses. The plots do not adjust for covariates, so they are suggestive rather than causal evidence. The estimates in Table 4 below account for the full set of controls. In the figure, the discontinuities at the cutoff are visible for the male and adult outcomes at the extensive margin and in the employed shares. In contrast, female employment profiles remain flat at low levels on both sides of the cutoff. Appendix Figure A2 shows the same outcomes using raw cell means, revealing identical patterns.[8]

Table 4 reports our main RDD estimates. Panel A presents the reduced-form (intention-to-treat) effect of satisfying the dependency-ratio criterion. Panel B reports the fuzzy 2SLS effect of being an ESSN beneficiary, which is our main estimate of interest. The first stage is strong. As shown in Column (1) of Panel A, crossing the eligibility cutoff (dependency ratio of 1.5) raises the probability of being a beneficiary by 53.0 percentage points.

As Panels (A) and (G) of Figure 2 suggested, the 2SLS estimates in Panel B show that receiving ESSN transfers substantially increases adult labor supply, an effect driven by the increase in the labor supply of men. Receiving transfers raises the probability that any adult in the household works by 9.8 percentage points (Column 4) and the probability that any man works by 13.2 percentage points (Column 2), while the corresponding effects for women are small and statistically indistinguishable from zero (Columns 3, 6, and 9). The effects on employment shares are larger. The share of men working rises by 19.3 percentage points (Column 8) and the share of adults working by 13.5 percentage points (Column 10).

[7] In Panel (b), each circle marks the share of beneficiary households at one dependency-ratio value, with the marker area proportional to the number of households at that value. Panel (a) is constructed with the `rdplot` command of Calonico, Cattaneo, and Titiunik (2015, 2017). The running variable is partitioned into three equal bins to the left of the cutoff and two to the right. The mean of the outcome is plotted for each bin with 90 percent heteroskedasticity-robust confidence intervals, and linear polynomials are fitted separately on each side using the underlying household-level data. Take-up is imperfect on both sides of the cutoff for the enrollment- and measurement-side reasons discussed in Section 5.1.

[8] In Appendix Figure A2, the point at the cutoff sits clearly above the left-side trend in Panels (a), (c), (g), and (i), i.e., any man working, any adult working, and the shares of men and of all adults working, while the female panels are flat.

The estimated effects on the number of working men (0.192, Column 5) and working adults (0.152, Column 7) are consistent in sign with the extensive-margin results but less precisely estimated, reflecting the greater dispersion of the count outcomes. Hence, the effect is concentrated on the extensive margin (whether at least one man works) and in the employed shares of men and of all adults.[9]

These magnitudes are large relative to the cash-transfer literature. The male extensive-margin response of 13.2 percentage points is roughly four times the 3.4 percentage-point median effect in the meta-analysis of Crosta et al. (2024). Part of the explanation is econometric. Our 2SLS estimates are local average treatment effects for complier households at the dependency-ratio cutoff. These are large, high-dependency households induced into the program by the eligibility rule, and their responses need not match those of the average beneficiary. Among control households near the cutoff, 23 percent have no working man; the estimated effect implies that the transfers move roughly half of these households into male employment. The response is thus large but concentrated precisely where slack exists.

The economic features of the program and of its population also support a strong response. The transfer amount is substantial, representing roughly 36 percent of household consumption and 55 percent of male labor income. These values place the ESSN at the high end of a literature where benefits typically represent 10 to 25 percent of pre-program consumption (Section 3). Furthermore, unlike humanitarian transfers lasting six months or less, the ESSN has provided regular monthly disbursements since 2016. A large and predictable income stream can finance job search and forward-looking investments that small or short-lived transfers cannot, consistent with the dissipation of effects after Lebanon's twelve-month program ended (Altındağ and O'Connell, 2023). Finally, as discussed in the introduction, displaced adults retain occupational skills and labor-force attachment that transfers can help reactivate, so the same transfer can generate a larger employment response among refugees than among the chronically poor.

We find no evidence of an effect on female labor supply. The coefficients on any woman working, the number of women working, and the share of women working are small, negative, and statistically insignificant (Columns 3, 6, and 9). The program thus raises the labor supply of adults in the household through increased male employment, without affecting female

[9]Our estimates are not comparable to the population employment rates reported in Section 3. They are local average treatment effects for high-dependency complier households near the eligibility cutoff, and the extensive-margin outcome is defined at the household level (whether any man works) rather than the individual level. Moreover, the population rates are measured with the ESSN in operation and thus already embed the program's effect on beneficiary households; adding our estimates to those rates would double-count the program's contribution.

workers. The absence of a female response, while men's responses are so strong, is consistent with which constraints cash assistance can relax and which it cannot. Transfers can ease the liquidity and information constraints that limit men's job search and employment. We examine these mechanisms directly in Section 6.5. They do not change the norms and childcare responsibilities that govern married women's participation, which are particularly binding in the high-dependency households around our eligibility cutoff. Also, for secondary earners, the income effect works against participation. This gender pattern mirrors the response to cash transfers among Syrian refugees in Lebanon, where men's employment rises while women's does not (Altındağ and O'Connell, 2023).

The reduced-form estimates in Panel A are consistent with the 2SLS estimates from Panel B. As expected, coefficients are smaller in magnitude. Satisfying the criterion raises the probability that any man works by 7.0 percentage points (Column 2) and the share of men working by 10.2 percentage points (Column 8), with a small and insignificant effect for women (-0.2 percentage points). Appendix Table B2 shows that these findings are qualitatively similar but less precise when estimated on the full sample, which includes households eligible via the other criteria.

6.2 Robustness of the Parametric Results

We conduct a series of robustness checks to examine the validity of our main results. We first use a range of alternative estimation bandwidths and functional forms for the running variable. Next, we test the sensitivity of our estimates to the set of controls and re-estimate the main results on alternative samples. We also report alternative standard errors together with corrections for multiple-hypothesis testing. We apply these checks to the nine employment outcomes. Corresponding safeguards for the mechanism outcomes are presented together with those results.

6.2.1 Alternative Bandwidths

We first assess whether the choice of bandwidth matters for the main findings. Appendix Table A5 re-estimates the baseline specification over progressively wider bandwidths, from the baseline (dependency ratios between 0 and 3) up to the full support of the dependency ratio. The estimates are highly robust in both statistical significance and coefficient magnitude. The effect on the probability that any man works ranges from 12.9 to 13.9 percentage points across bandwidths (13.2 percentage points in the baseline) and is significant at the 1

percent level throughout. Similarly, the effect on the probability that any adult works ranges from 9.8 to 10.7 percentage points. The share estimates are also stable in magnitude (17.3 to 19.3 percentage points for men), though less precisely estimated at the widest bandwidths. Count estimates remain positive and imprecise across all bandwidths, mirroring the baseline results.

6.2.2 Alternative Specifications of the Running Variable

We next test the sensitivity of our results to the functional form of the running variable. Table 5 presents 2SLS estimates using (i) single (not split) linear polynomials in the dependency ratio, (ii) quadratic polynomials in the dependency ratio, (iii) linear polynomials in the numerator (number of dependents) and denominator (number of prime-age adults) separately, and (iv) linear polynomials in the logarithms of the two counts, entered separately. For the last specification, we use $\log(1 + \cdot)$ because households with no dependents appear in the estimation window. The core findings are largely invariant to these alternative specifications. For instance, using a single linear polynomial, the estimated increase in the probability that a man works is 12.8 percentage points (Column 1), and using a quadratic polynomial, it is 12.6 percentage points. Similarly, the effects on the share of men working are 22.9 percentage points and 23.5 percentage points for the linear and quadratic specifications, respectively.

Our benchmark specification restricts household composition to affect the outcome only through the dependency ratio. It therefore places households with two dependents and one prime-age adult on the same trend as households with six dependents and three prime-age adults. If household size matters for the outcome beyond the ratio, the estimated discontinuity could partly reflect a gradual effect of family size rather than the crossing of the eligibility threshold. We address this concern using the specification proposed by Demirci et al. (2026). We replace the trends in the dependency ratio with separate trends in its two components, the number of dependents and the number of prime-age adults. We enter these components in levels and in logarithms while retaining the treatment indicator. Because the eligibility threshold corresponds to a different number of dependents for each count of prime-age adults (Section 5.3.2), the treatment indicator is not collinear with these component trends. Any smooth effect of an additional dependent or an additional adult is absorbed by the trends themselves, ensuring that any remaining discontinuity reflects crossing the eligibility threshold.

In the third specification, with separate linear trends in the number of dependents and the number of prime-age adults, the effect on the extensive margin for men is positive but

statistically insignificant (0.076). However, the effects on the number of working members remain significant: the number of working men increases by 0.187 and the number of working adults by 0.150. When the two components enter in logarithms, the results are similar, with the extensive-margin effect for men now larger and statistically significant. Specifically, the probability that a man works increases by 10.8 percentage points, while the number of working men increases by 0.205. Across all specifications, the baseline patterns hold. Labor supply increases for men and for adults overall. The estimates for women remain small and statistically insignificant.

6.2.3 Sensitivity to the Set of Controls

Our baseline specification conditions on a rich set of controls. Appendix Table A7 re-estimates the nine employment effects with no controls beyond the split linear trends. The estimates are similar in magnitude and follow the same pattern (any man working, 10.8 versus 13.2 percentage points; the share of working adults, 11.5 versus 13.5), indicating that identification comes from the discontinuity rather than from the covariates. Appendix Table A6 moves in the opposite direction and adds controls for the counts of men, women, and adults, matched to the outcome under study. We exclude them from the main specification because these counts are components of the denominator of the dependency ratio. Conditioning on elements of the running variable is difficult to reconcile with the design, even though the counts are balanced at the cutoff (Section 5.3.3). The composition-adjusted local-randomization estimates of Section 6.3.2 provide the design-consistent counterpart. With the counts added, the extensive-margin and share estimates remain essentially unchanged (any man working, 13.6 percentage points), while the counts of working men and of all adults sharpen (0.208 and 0.256, both significant). We interpret the latter as a mechanical consequence of conditioning on composition rather than as evidence in favor of these specifications.

6.2.4 Alternative Samples

Refugees of different nationalities face distinct legal rules governing labor-market access in Turkey (Section 3). To verify that our pooled estimates are not an artifact of combining groups for which the program may operate differently, we re-estimate the main specification on Syrian households. Syrians constitute 78 percent of the estimation sample and face a uniform work-permit regime. Appendix Table A8 shows that these estimates match baseline results closely. Being a beneficiary raises the probability that any man works by 13.8 per-

centage points and the probability that any adult works by 11.3 percentage points, while the effects on women's employment remain small and statistically indistinguishable from zero. The remaining 454 households pool refugees of several nationalities and legal statuses. This subsample is too small and heterogeneous for subgroup estimates to be informative, so we do not report separate results for it.

Appendix B widens the sample beyond our main sample restriction by reporting estimates on the full sample, which includes the 1,586 households eligible for the ESSN through criteria other than the dependency ratio (Table B2). Because these households receive transfers on either side of the cutoff, their beneficiary status barely responds to it. Retaining them dilutes the first stage from 0.530 to 0.377 ($F = 23.5$). The weaker first stage reduces precision but does not change the qualitative pattern of results, as no full-sample estimate is statistically distinguishable from its main-sample counterpart. The effects on employment shares remain nearly identical to baseline estimates, yielding increases of 0.203 for men and 0.139 for adults. Both are significant at the 5 percent level by clustered standard errors, with wild-bootstrap p-values of 0.112 and 0.115, respectively. Extensive-margin estimates become too imprecise to be informative. Precision improves exactly when households eligible through alternative criteria are excluded, providing empirical support for the sample restriction described in Section 4.

6.2.5 Alternative Standard Errors

Because the running variable is discrete and we cluster on it, the number of clusters is moderate. The dependency ratio takes 32 distinct values in the estimation sample, and these 32 clusters underlie the clustered and bootstrap inference throughout the paper. As Kolesár and Rothe (2018) note, standard errors clustered on a discrete running variable can have poorer coverage than heteroskedasticity-robust (Eicker–Huber–White, EHW) standard errors when the number of clusters is small. Alongside the wild-cluster bootstrap p-values reported throughout (Cameron and Miller, 2015), Appendix Table A9 therefore reports the main reduced form and 2SLS employment estimates with EHW standard errors.

Overall, the statistical conclusions are unchanged. The extensive-margin and share effects for men and for all adults remain significant under EHW, while estimates for women remain statistically indistinguishable from zero. The comparison between EHW and clustered standard errors differs across outcomes. EHW standard errors are larger than the clustered ones for the extensive-margin outcomes, but markedly smaller for the counts and shares. In particular, the estimates for the numbers of working men and of all adults, which are

imprecise under clustering, attain significance under EHW. This precision gain arises under the variance estimator that presumes the parametric specification is correct. By contrast, clustering follows the specification-error reasoning of Lee (2008). We therefore retain the more conservative clustered and wild-bootstrap inference as our benchmark and continue to describe the count effects as imprecisely estimated.

As noted in Section 5, we do not implement the honest confidence intervals of Kolesár and Rothe (2018), which guard against misspecification of the fitted trends. The local-randomization design in Section 6.3 serves that purpose in our setting, as it dispenses with trends altogether. With the running variable concentrated on a few support points at the threshold, it also formalizes the near-cutoff comparison that an honest-optimal estimator approximates.

6.2.6 Multiple-Hypothesis Testing

To account for testing across multiple outcomes, we guard against false discoveries by reporting family-wise error-rate-adjusted p-values using the step-down procedure of Romano and Wolf (2005), together with false-discovery-rate-adjusted q-values (both the sharpened q-values of Anderson (2008) and the procedure of Benjamini and Hochberg (1995)) within each outcome family. Table 11 reports these corrections for the main employment outcomes, conducted within each three-outcome block: whether any member works, the number working, and the share working. We apply the same domain-wise convention to the mechanism outcomes in Section 6.5.[10] As a stricter robustness check, we also computed every correction treating all nine outcomes as a single family and summarize those results below.

The extensive-margin effect for men emerges as the result that survives the corrections. Its false-discovery-rate q-values are below 0.001 under both the Anderson and the Benjamini–Hochberg procedures, regardless of the family definition (including under the alternative partition of the nine outcomes into demographic groups). Its Romano–Wolf p-value within the extensive-margin block is 0.052. When the familywise error rate is instead controlled across all nine outcomes at once, no estimate clears the Romano–Wolf threshold; the adjusted

[10] A family should collect the tests that bear on a single substantive claim, following the convention established by Anderson (2008), who groups outcomes into domains and applies corrections within each domain. Our three blocks address three distinct economic questions: whether anyone in the household works, how many members work, and what share of the household works. An incorrect inference on one margin implies a fundamentally different conclusion than a false positive on another. This approach also maintains internal consistency across the paper, since the mechanism corrections in Section 6.5 follow the same domain convention. Carried to its conclusion, pooling would place every outcome in the paper into a single family, a practice rarely used in empirical work.

p-value for any man working is 0.208.[11] The remaining positive estimates, including the share outcomes, do not retain statistical significance once the corrections are applied, while the estimates for women are null across all procedures.

For the share outcomes, the wild-cluster bootstrap is the conservative outlier among our inference methods. The EHW p-values in Appendix Table A9 are 0.002 and below 0.001 for the shares of working men and of all adults. Similarly, the local-randomization design of Section 6.3 delivers both effects with exact Fisherian p-values below 0.001 in the preferred window (values that survive the multiple-testing corrections are reported in Appendix Table C7). Consequently, we view the share results as corroborated by the design-based analysis rather than by the bootstrap.

6.3 Results from the Discrete (Local-Randomization) Design

We now present the results of the local-randomization design of Section 5.2. Our preferred window covers dependency ratios between 1 and 1.5. This subsection reports the preferred-window estimates for the employment outcomes (Table 9). Section 6.5 discusses the mechanism estimates: the multi-wave blocks, which retain sufficient mass within the window (Table 10), and the single-wave modules, whose within-window samples are substantially thinner (Appendix Table C2). Appendix C collects the remaining exhibits: the window-selection diagnostics, the wider-window estimates, and the composition-adjusted estimates discussed in Section 6.3.2.

6.3.1 Employment Outcomes

Table 9 shows a strong first stage in the preferred window. Crossing the cutoff increases the probability of being a beneficiary by 63 percentage points. The employment effects are concentrated among men and on extensive-margin outcomes. Crossing the cutoff increases the probability that any man works by 6.2 percentage points and the probability that any adult works by 5.2 percentage points. Scaling by the first stage implies local treatment effects of 9.8 and 8.2 percentage points, respectively. The effects on employment shares are also positive and statistically significant. The share of men working increases by 10.9 percentage

[11] This figure overstates the multiplicity burden. A Bonferroni correction across all nine outcomes applied to the wild-bootstrap p-value for any man working (below 0.001) yields an adjusted p-value below 0.01. The Romano–Wolf value instead reflects the heavy tails of the cluster-resampling scheme, driven by the imprecisely estimated count equations with only 32 clusters; the limited number of clusters is why we rely on wild-cluster bootstrap inference throughout.

points in reduced form, implying a treatment effect of 17.4 percentage points. Similarly, the share of adults working increases by 5.3 percentage points, implying a treatment effect of 8.4 percentage points.

We find no evidence of an increase in women's employment. The coefficients on any woman working, the number of women working, and the share of women working are small, negative, and statistically insignificant. The estimates for the numbers of working men and of all adults are also insignificant. These results suggest that the response is mainly reflected in whether at least one man works and in the fraction of men and of all adults employed, rather than in a large increase in the total number of workers. The randomization confidence intervals also encompass the parametric results. Dividing the intervals for any man working and for the share of men working by the corresponding first stages yields implied-effect ranges of $[0.023, 0.173]$ and $[0.097, 0.250]$, which include the 2SLS estimates of 0.132 and 0.193.

Appendix Table C7 applies the within-block multiple-testing structure of Table 11 to the Fisherian p-values. We use Holm's step-down procedure for familywise control together with the two false-discovery-rate procedures. Since Holm's adjustment remains valid under arbitrary dependence, it preserves the finite-sample validity of the randomization tests. The discrete results remain robust to these corrections. The share effects remain below 0.001 across all procedures, including exact familywise control. The effect on any man working stands at 0.036–0.037 across all three, while the effect on any adult working ranges between 0.037 and 0.062. The local-randomization design therefore confirms the share results under multiplicity corrections precisely where the parametric wild-bootstrap inference is most conservative.

Panel A of Appendix Table C3 provides a cross-check in the wider $[-0.5, 0.5]$ window. The first stage remains strong at 57.4 percentage points. Effects on the shares of men and of all adults working remain positive and statistically significant. The reduced-form effects are 6.3 and 3.5 percentage points, implying treatment effects of 11.0 and 6.0 percentage points. The estimates for any man working and any adult working retain positive signs but are less precisely estimated than in the preferred window. This loss of precision occurs because the mass points added above the cutoff have substantially lower beneficiary rates, which dilutes the treatment contrast. Overall, the local-randomization design corroborates the main parametric findings under much weaker functional-form assumptions. Beneficiary status increases male and adult employment at the cutoff, with no evidence of an effect on women's employment.

6.3.2 Composition-Adjusted Estimates

A remaining concern with the window estimates is household composition. Because the running variable is a ratio, households on the two sides of the cutoff differ not only in their eligibility but also in the number of adults and dependents they contain. In the preferred window, this difference is substantial. 99.4 percent of treated households have exactly two prime-age adults, whereas 24 percent of control households have three or more. Part of the raw window contrast could therefore reflect household size and structure rather than the transfer.

To address this concern, Appendix Tables C5 and C6 report composition-adjusted estimates. The adjustment mirrors the count-controlled parametric specification. We regress each outcome on the treatment indicator and dummies for the relevant household counts, and we conduct inference by re-estimating this adjusted regression within each permutation draw. In this window, the adjustment has a transparent interpretation. The adjustment effectively compares beneficiary households with two adults and three dependents to two-adult control households with two dependents, isolating families that differ by a single child across the eligibility line.

The adjusted extensive-margin estimates are smaller than both the covariate-free window estimates and the parametric magnitudes. Specifically, the adjusted ITT for any man working is 0.040 ($p = 0.080$; confidence interval $[-0.005, 0.085]$), with an implied LATE of 6.1 percent. This interval still covers the parametric reduced-form estimates (0.070–0.072); however, once scaled by the adjusted first stage, its upper bound of 0.130 falls just short of the parametric 2SLS estimates of 0.132–0.136. The count estimates instead rise under the adjustment (0.044 versus 0.024 for men; 0.043 versus 0.007 for adults), mirroring the mechanical sharpening that the parametric count controls produce. Even so, the scaled intervals for the number of working men, $[-0.009, 0.145]$, and the number of working adults, $[-0.020, 0.145]$, exclude the count-controlled parametric magnitudes of 0.208 and 0.256. Hence, we do not rely on the count-controlled parametric magnitudes in our interpretation.

The adjustment also qualifies the results for employment shares. The pooled window effect on the share of men working (0.109) exceeds the within-cell contrast (0.042) because control households with more adults have mechanically lower shares. We leave the share rows unadjusted to mirror the parametric convention; they should be read with this caveat in mind.

6.4 Heterogeneity

Table 8 explores how the labor-supply response varies across households, splitting the estimation sample by the education of the household head (Panel A) and by the household's time since arrival in Turkey (Panel B). Because these splits reduce both the number of observations and the number of clusters, we report wild-cluster bootstrap p-values alongside the clustered standard errors and read the estimates as suggestive. We do not formally test the equality of effects across subgroups.

The response is concentrated among the most disadvantaged households. Among households whose head is illiterate or literate without a degree, being a beneficiary raises the probability that any man works by 23.5 percentage points, the probability that any adult works by 20.6 percentage points, and the share of men working by 27.5 percentage points. These estimates are significant at the 5 percent level with clustered standard errors but lose precision under the wild-cluster bootstrap (p-values of 0.080 to 0.149). This is also the only subgroup in which women respond: the share of women working rises by 6.5 percentage points, significant at the 10 percent level under both procedures. Among households whose head has primary to technical education, the estimates are much smaller (5.6 percentage points for any man working); only the share of adults working is significant with clustered standard errors (9.8 percentage points), and it does not survive the bootstrap. The university-educated subsample is too small (154 observations spread over 16 clusters) to be informative.

This concentration of the response among the least-educated households aligns with the mechanisms of Section 6.5: the constraints that the transfers appear to relax may be more binding most where resources are scarcest. This pattern echoes Aygün et al. (2024), who document that the beneficial effects of the ESSN program on child outcomes are concentrated among the most vulnerable groups.

The response is likewise concentrated among households who have been in Turkey longer. For households that first arrived in Turkey more than three years before the survey, being a beneficiary raises the probability that any man works by 21.9 percentage points (wild-bootstrap $p = 0.006$) and the share of adults working by 18.1 percentage points ($p = 0.027$). The estimate for the share of men working is also large at 30.4 percentage points, though less precise under the bootstrap ($p = 0.126$). For households that arrived within the previous three years, all estimates are small and statistically indistinguishable from zero. These patterns suggest that cash transfers are most effective once refugees have accumulated the baseline local knowledge, language exposure, and networks needed to translate relaxed liquidity and information constraints into employment. Recent arrivals appear to lack these

complementary inputs, consistent with the connectivity and language-training mechanisms documented below.

6.5 Mechanisms

We next examine potential mechanisms behind the positive employment response. We estimate the effects of the program on a rich set of intermediate outcomes. The resulting estimates identify the causal effects of the transfers on the intermediate outcomes themselves, but they do not show how much each outcome contributes to the employment response. We therefore interpret the results below as evidence on the plausibility of candidate explanations rather than as a quantification of each mechanism's contribution. Figure 3 previews the estimates. It displays the RD plots for the nine mechanism outcomes that are statistically significant under clustered inference, with personal well-being represented by the aggregated PW-Score. The corresponding plots for the complete outcome sets appear by mechanism family, in Appendix Figures A3 through A9. Appendix Figures A10 through A16 present the same outcomes in raw cell means.[12]

6.5.1 Parametric Estimates by Mechanism

Panel A of Table 6 presents the 2SLS estimates of the effect of ESSN cash transfers on mobile phone ownership and use. The estimates are consistent with the discontinuities visible in Figure 3. We find no evidence of an effect on the extensive margin of connectivity. The coefficient on mobile phone ownership is small, negative, and statistically insignificant, suggesting that the program does not operate through buying new devices. In contrast, we find positive effects on the intensive margin of use, particularly for communication. The probability of using messaging services on some days increases by 12.2 percentage points. The likelihood of making voice calls also rises, by 7.1 percentage points for use on some days and by 11.8 percentage points for everyday use. The transfers also improve access to information. The probability of checking the news on some days increases by 14.6 percentage points, while the probability of checking the news every day rises by 12.5 percentage points. We find no evidence of effects on the use of translation applications or social media.

The news result is corroborated by the local-randomization design. Within the preferred window, crossing the cutoff raises the probability of consuming news by 9.1 percentage points

[12]The profiles for the outcomes omitted from Figure 3 remain flat at the cutoff, consistent with the null estimates in Tables 6 and 7. In particular, the health and social cohesion indicators show no discontinuities.

(randomization $p = 0.030$). This estimate implies a treatment effect of 14.1 percentage points, closely matching the parametric estimate (Appendix Table C2, Panel A). Overall, while cash transfers do not increase phone ownership, they facilitate more frequent communication and access to news. Improved connectivity may reduce information frictions in the labor market, enabling beneficiaries (particularly men) to find and maintain employment.

Panel B of Table 6 presents the effects on food consumption and coping strategies. The transfers significantly reduce reliance on harmful coping mechanisms, as measured by the WFP's reduced Coping Strategies Index (rCSI). The rCSI is constructed from the frequency of five severity-weighted behaviors in the past week: relying on lower-quality food, borrowing food, reducing the number of meals, limiting portion sizes, and restricting consumption by adults in favor of children. Receiving cash transfers lowers this index by 2.21 points, indicating lower reliance on these distress behaviors. The food expenditure estimates, by contrast, are statistically insignificant, though the coefficient for annual adult-equivalent spending is positive (approximately 209 TL). The evidence on increased food spending is thus suggestive at most, while the evidence on reduction in distress coping is considerably stronger.

We next consider whether the cash transfers facilitate investment in productive inputs and human capital. Panel C of Table 6 reports the 2SLS estimates for expenditures on non-food items. Aggregate non-food expenditure increases by approximately 310 TL, with the composition of this increase pointing more toward future-oriented investments than immediate consumption. In particular, receiving transfers increases expenditure on education by 118 TL and savings by roughly 145 TL. We also estimate a small but statistically significant increase in spending on agricultural inputs (11 TL), whereas we find no evidence of changes in spending on clothing, social events, or construction. The point estimate for debt repayment is negative (-59 TL) but statistically insignificant.

Next, we examine whether the transfers affect physical health or social networks, two potential mechanisms for the labor supply response. Panels A and B of Table 7 present the 2SLS estimates for adult health outcomes and social cohesion measures, respectively. We find no evidence that the cash transfers improve physical health among men. The effects on the share of men reported sick, including those aged 18–59, and on healthcare expenditure are small and statistically insignificant. The social cohesion results are also null; the program does not increase the likelihood that beneficiaries seek advice from either Syrian or Turkish friends regarding daily tasks or employment opportunities. Hence, these results provide no support in our setting for two mechanisms often emphasized for displaced populations: improved physical health and stronger social networks.

We also explore whether the cash transfers facilitate labor market integration through skill acquisition. Panel C of Table 7 presents the 2SLS estimates for language proficiency and course participation among men aged 18–59. Receiving transfers increases the share of men attending Turkish language courses by 4.1 percentage points. However, this increased enrollment does not yet translate into higher self-reported proficiency: the coefficient on the share of men speaking Turkish is negative and statistically insignificant. We likewise find no evidence of an effect on participation in technical or vocational courses. These results suggest that while the program encourages investment in language skills, this mechanism may take time to materialize into improved fluency or broader workforce skills.

Finally, we investigate the impact of the program on the personal well-being of beneficiaries. Financial scarcity can impose a significant cognitive burden, potentially impeding decision-making and job search. Mental-health needs are also salient in this population: depressive and post-traumatic stress symptoms are widespread among Syrian refugees in Turkey, and most of those affected receive no treatment (Fuhr et al., 2020). Panel D of Table 7 presents the 2SLS estimates for a set of personal well-being indicators, where higher values indicate a higher frequency of negative feelings. The parametric estimates point to sizable reductions in psychological distress. The aggregate Personal Well-being Score (PW-Score) decreases by 5.60 points on a scale of 0 to 15. The individual components also move in the same direction: beneficiaries report less frequently having "no interest in doing things" (-1.33 points), "feeling down or hopeless" (-0.92 points), "having poor appetite" (-1.28 points), and "being unable to stop worrying" (-1.50 points). All four estimates are statistically significant at the 1% level.

Several features of the data, however, caution against a causal reading of this magnitude. A reduction of 5.6 points exceeds one standard deviation of the outcome, approaches its sample mean, and would far surpass typical cash-transfer effects on mental health (McGuire et al., 2022). The local-randomization estimates in the preferred window are also small and statistically insignificant for the PW-Score and for every component (Appendix Table C2, Panel D). Scaling the randomization confidence interval for the PW-Score by the first stage yields an implied range of $[-4.20, 2.64]$, which excludes an effect as large as the parametric estimate; no such divergence arises for the employment outcomes. The cell means in Appendix Figure A16 explain the discrepancy. With only 160 observations, the fitted trends left of the cutoff are steep and reach the threshold well above the treated-side means. The implied negative jump therefore owes more to extrapolated trends than to the cell means adjacent to the cutoff. In addition, the module was administered to a selected subsample. Respondents differ from non-respondents mainly by region, but response is unrelated to ben-

eficiary status and shows no discontinuity at the cutoff (Appendix Tables A12 and A13). We therefore treat the well-being mechanism as suggestive at most; our interpretation of the employment results does not rest on it.

In sum, the mechanisms analysis provides the strongest support for improved connectivity and access to information, reduced reliance on harmful coping strategies, and investment in human capital. We find no corresponding evidence for changes in physical health or social networks. The evidence on personal well-being is suggestive at most. These mechanisms, combined with the increased investment in language training and savings, are consistent with cash transfers enabling beneficiary men to overcome liquidity and information constraints and engage more actively in the labor market.

6.5.2 Robustness of the Mechanism Estimates

Table 10 reports the local-randomization estimates for the mechanism blocks available across multiple survey waves. The education result corroborates the parametric finding. Education spending rises by 79.1 TL in reduced form (randomization $p < 0.001$; confidence interval $[44.5, 113.3]$). This estimate implies a treatment effect of 125.6 TL, compared to the parametric estimate of 118.2 TL. For the rCSI, the local-randomization point estimate is near zero and imprecise. Scaled by the first stage, it spans $[-2.77, 3.33]$ and includes the parametric estimate of -2.21. The two designs cannot be statistically distinguished for this outcome. The remaining expenditure and health estimates are imprecise nulls whose intervals include their parametric counterparts, with two exceptions. The agriculture effect agrees qualitatively across the two designs: it is positive and statistically significant in both (randomization $p = 0.040$), though the magnitude in the window is much smaller. For food purchases, both designs deliver statistical nulls, though the randomization interval lies above the negative parametric point estimate. Panels B–D of Appendix Table C3 show that these conclusions are unchanged in the wider window.

Estimates for the single-wave modules are reported in Appendix Table C2, with corresponding wider-window estimates in Appendix Table C4. We draw on them where they inform the parametric results above. The local-randomization design supports the news-consumption effect but not the large parametric well-being estimates. We accordingly treat the latter as suggestive at most.

The main mechanism results remain robust to the composition adjustment described in Section 6.3.2 (Table C5). The education effect strengthens (ITT of 93.4 TL; confidence

interval $[58.9, 127.5]$), and the news-consumption effect survives ($p = 0.049$; implied effect of 0.111). The rCSI estimate moves toward the parametric sign but remains a statistical null. Its scaled interval, $[-3.23, 2.47]$, still includes the parametric estimate of -2.21. Finally, the scaled interval for the PW-Score, $[-3.24, 2.88]$, continues to exclude an effect of the parametric magnitude.

The mechanism estimates are also re-estimated on the full sample of Appendix B (Tables B3 and B4). As with the employment outcomes (Section 6.2.4), retaining these households dilutes the first stage and reduces precision. None of the full-sample estimates differ statistically from their main-sample counterparts. The reduction in reliance on food-related coping strategies is almost unchanged at -2.17, compared with -2.21 in the main sample. The remaining mechanism estimates become too imprecise to be informative.

The personal well-being module requires a separate note. In the full sample, its first stage falls into the weak-instrument range, with F statistics between 3.3 and 5.0 (Panel D of Appendix Table B4). We retain the Panel D 2SLS estimates for completeness but do not draw inferences from them. Appendix Table B5 instead reports the reduced-form effects of crossing the eligibility cutoff, which remain valid regardless of first-stage strength. The reduced-form estimates are negative for all six outcomes, and the PW-Score falls by 2.8 points at the cutoff. All six are significant under both clustered and wild-bootstrap inference. The nervousness outcome is the least precisely estimated, with a bootstrap p-value of 0.064, as in the main sample. These reduced forms rely on the same split-trend specification and small subsample as the parametric estimates. The same functional-form concerns therefore apply, and we continue to treat the well-being evidence as suggestive.

The mechanism estimates presented in this section span some forty outcomes, several of which are observed in only one survey wave with correspondingly small samples, so the scope for false discoveries is larger here than for the employment outcomes. As such, we apply the corrections described in Section 6.2.6 within domain families, substituting the deterministic Holm step-down for the resampled familywise procedure as it is not computationally stable across these families. The table notes provide further details. These corrections, reported in Appendix Tables A10 and A11, sort mechanism estimates into three groups. The reductions in food-related coping strategies, the increases in news consumption, and the rise in Turkish language-course enrollment remain significant after all corrections (Holm-adjusted p-values of 0.042 or below and q-values of 0.045 or below). The education, aggregate non-food expenditure, and voice-call (some days) effects sit at the boundary (q-values around 0.10). The education result, however, is additionally supported by the local-randomization estimate (Fisherian p-value below 0.01). The savings and everyday voice-call effects, significant by

conventional and bootstrap inference, do not survive the corrections and lack design-based counterparts. Hence, we treat them as suggestive.

7 Conclusion

The economic self-reliance of refugees has become a central issue in the design of support programs. Refugees' welfare rests on being able to earn a living, whether they eventually stay in the host country or return home. A long-standing policy concern is that cash assistance may undermine this objective by discouraging work. Public debates often invoke this "dependency" concern to argue against social assistance for refugees, yet the evidence for this population remains scarce. We investigate this issue in the context of the ESSN, the largest humanitarian cash transfer program in the world. Exploiting program eligibility rules in a regression discontinuity design, we evaluate the causal impact of cash transfers on the labor supply of adult refugees.

We find that the transfers substantially increase men's labor supply without affecting women's. Receiving transfers increases the probability that any man works by 13.2 percentage points. The share of working men rises by 19.3 percentage points, while the effects on the total numbers of working men and of all adults are positive but imprecise. The corresponding effects for women are small and statistically insignificant. The extensive-margin effect for men is robust to a series of checks, including wild-cluster bootstrap inference, multiple-testing corrections, and alternative bandwidths and specifications. A design-based local-randomization analysis around the eligibility cutoff corroborates these results with exact finite-sample inference: the implied effects are 9.8 percentage points for any man working and 17.4 percentage points for the share of working men, and the randomization confidence intervals contain the parametric estimates.

Our investigation of the mechanisms at play points towards the specific constraints faced by displaced households. We find no evidence that the transfers improve physical health or increase refugees' social networks. Instead, the transfers increase the frequency of phone use to access the news, reduce reliance on harmful food-coping strategies, and increase enrollment in Turkish language courses. In addition, the transfers increase spending on education, an effect independently supported by the local-randomization design. The parametric estimates also indicate large reductions in psychological distress, but these gains do not survive the design-based analysis. Hence, we treat the evidence on the personal well-being mechanism as suggestive at most. Taken together, these patterns are consistent with the transfers relaxing

the liquidity and information constraints that may limit job search and employment among displaced men.

These findings contribute to the growing body of evidence refuting the premise that cash assistance discourages work and extend this literature to a forced-displacement setting. This extension is highly relevant because evidence on cash assistance for refugees is scarce. Distinct characteristics of the refugee population and the institutional setting mean that they face peculiar constraints, so findings from other populations need not carry over. Prior research suggests that a priori expectations for refugees are, if anything, negative (Baird et al., 2018). We show that, rather than inducing a so-called "dependency," ESSN transfers appear to have supported the economic integration of refugee men, at least in the labor market. Together with companion evidence that the ESSN keeps refugee children in school and out of work (Aygün et al., 2024), our results suggest a reallocation of work within refugee households from children to adults. At the same time, the absence of response among women indicates that cash alone does not overcome the additional barriers to women's employment in this context.

Several caveats apply. Our estimates identify a local average treatment effect for households near the dependency-ratio cutoff. Although we conduct extensive specification tests, our results rely on a discrete running variable. The parametric design requires functional-form assumptions, while the local-randomization design instead requires exchangeability within a narrow window around the cutoff. Regarding the mechanisms at play, we identify the effects of the program on intermediate outcomes, but we cannot determine how much each contributes to the employment response. Finally, some of these results are only suggestive, as they rely on specific waves or sub-samples.

Nonetheless, the results have direct implications for the design of refugee assistance. They suggest that well-targeted unconditional cash transfers can be delivered to refugees at scale without deterring adult labor supply and, in this setting, increasing it. This evidence strengthens the case for cash-based assistance as a tool for both protection and economic inclusion and suggests that there need not be a trade-off between immediate humanitarian objectives and long-term social and economic inclusion of refugees. These results are especially relevant across low- and middle-income host countries, where 68 percent of the world's refugees live (United Nations High Commissioner for Refugees, 2026) and informal employment is often an important source of income. They also suggest that predictable cash assistance may complement policies that expand refugees' access to formal employment in high-income countries.

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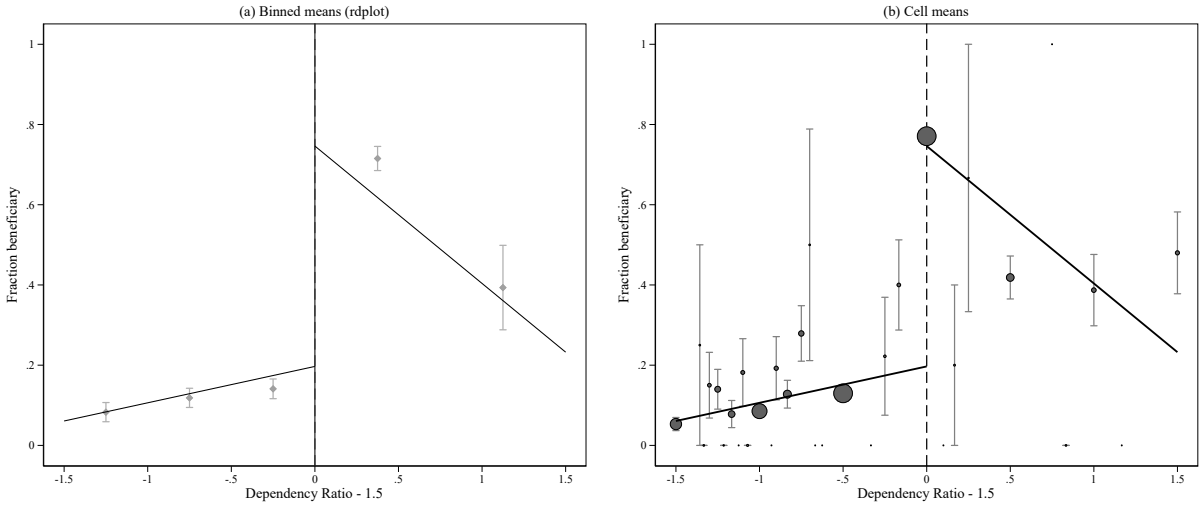
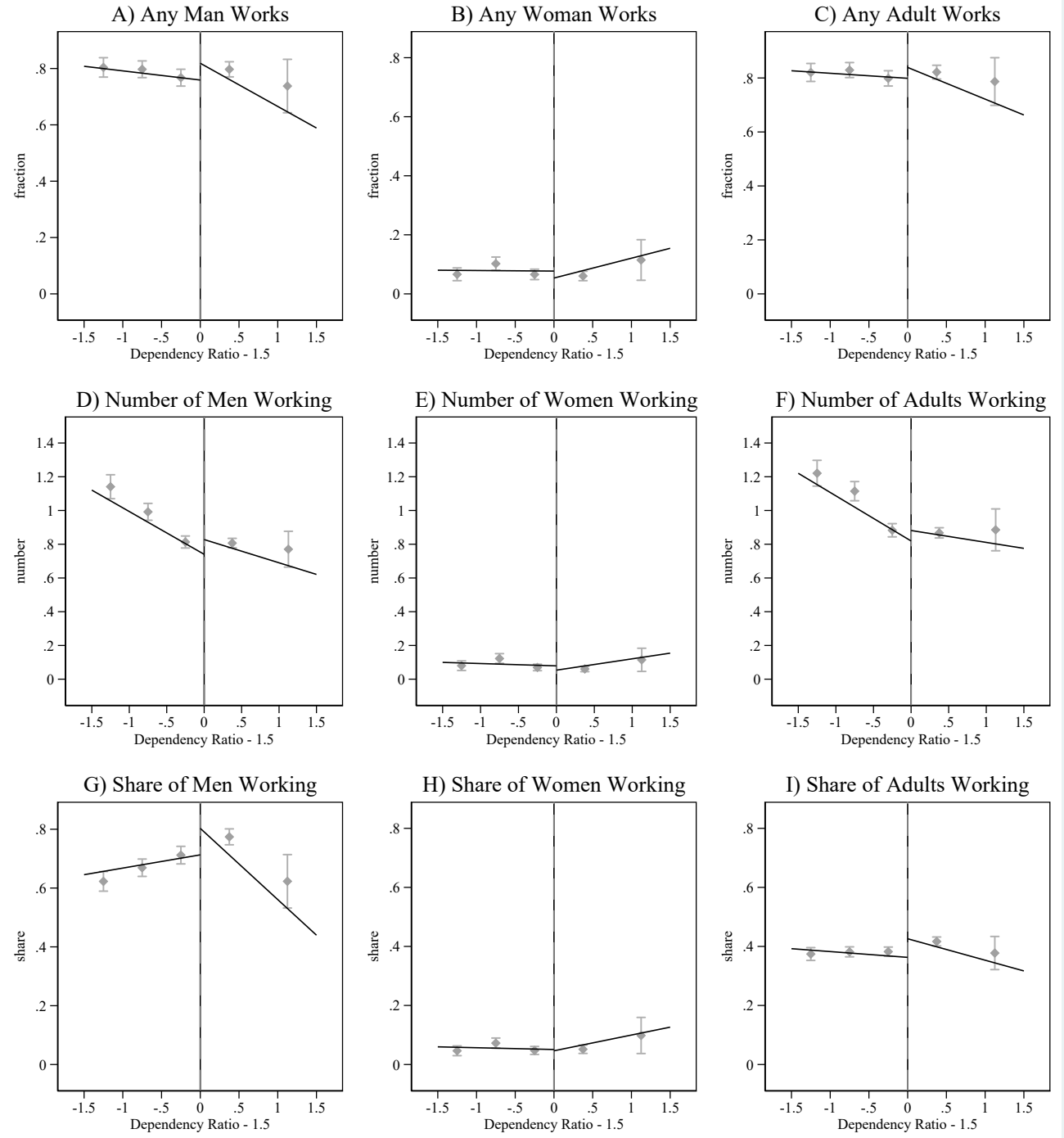
Figures

Figure 1: First stage: Fraction Receiving ESSN by Dependency-ratio Value



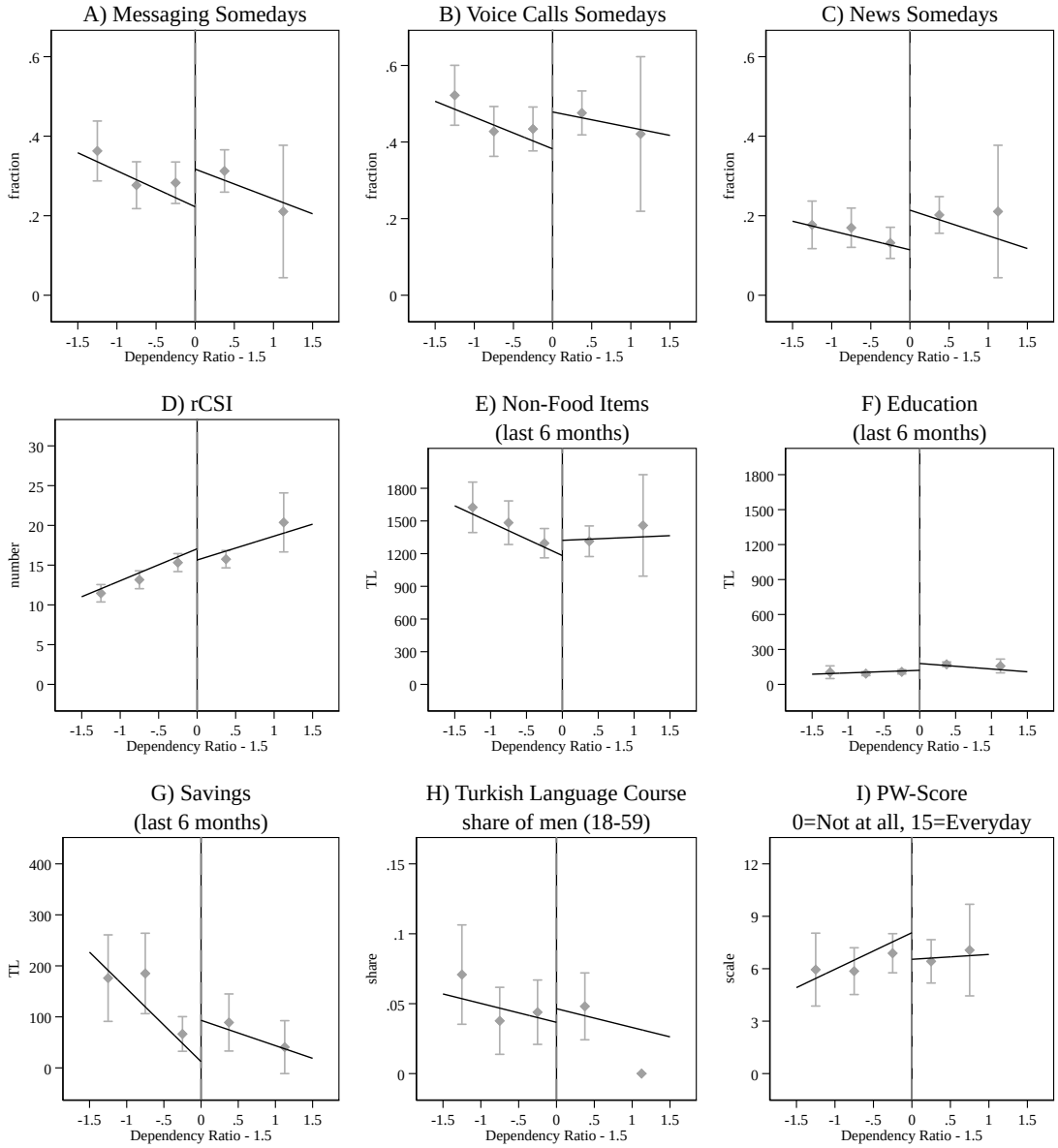
Notes: Each panel plots an adult-employment outcome against the running variable (the dependency ratio, centered at the 1.5 cutoff) for the sample that excludes households eligible for the ESSN program via criteria other than the dependency ratio, restricted to dependency ratios in $[0, 3]$. Figures are constructed using the `rdplot` command of Calonico, Cattaneo, and Titiunik (2015, 2017). To account for the uneven distribution of the running variable, it is partitioned into three equal bins on the left and two equal bins on the right of the cutoff, and the mean of the outcome variable is plotted for each bin. Bin-specific 90 percent confidence intervals, robust to heteroskedasticity, are shown as error bars. Linear local polynomial regressions are fitted separately on each side of the cutoff using the original individual-level data to depict pre- and post-threshold trends.

Figure 2: RDD Graphs for Employment Outcomes



Notes: The figure plots the employment outcomes against the running variable (the dependency ratio, centered at the 1.5 cutoff) for the sample that excludes households eligible for the ESSN program via criteria other than the dependency ratio. The plots are constructed using the `rdplot` command of Calonico, Cattaneo, and Titiunik (2015, 2017): to account for the uneven distribution of the running variable, it is partitioned into three equal bins on the left and two equal bins on the right of the cutoff, the mean is plotted for each bin, and bin-specific 90 percent confidence intervals, robust to heteroskedasticity, are shown as error bars. Linear local polynomial regressions are fitted separately on each side of the cutoff using the original individual-level data to depict pre- and post-threshold trends.

Figure 3: RDD Graphs for Selected Mechanism Outcomes



Notes: Each panel plots a mechanism outcome against the running variable (the dependency ratio, centered at the 1.5 cutoff) for the sample that excludes households eligible for the ESSN program via criteria other than the dependency ratio, restricted to dependency ratios in $[0, 3]$. The nine outcomes shown are those with statistically significant 2SLS estimates under clustered inference in Tables 6 and 7; personal well-being is represented by the grouped PW-Score, and the complete outcome sets appear in Appendix Figures A3 through A9. Figures are constructed using the `rdplot` command of Calonico, Cattaneo, and Titiunik (2015, 2017). To account for the uneven distribution of the running variable, it is partitioned into three equal bins on the left and two equal bins on the right of the cutoff, and the mean of the outcome variable is plotted for each bin. Bin-specific 90 percent confidence intervals, robust to heteroskedasticity, are shown as error bars. Linear local polynomial regressions are fitted separately on each side of the cutoff using the original individual-level data. Panels A–C and H draw on the Wave 4 module ($N = 704$), Panels D–G on the pooled sample ($N = 2{,}078$), and Panel I on the Wave 4 well-being module ($N = 160$).

Tables

Table 1: Family Composition

	Number of Dependents													
# prime adults	0	1	2	3	4	5	6	7	8	9	10	11	12+	Total
1	12	23	**26**	**15**										76
2	115	246	392	**512**	**57**	**30**	**9**							1361
3	33	51	93	90	20	**4**	**2**	**4**	**1**					298
4	15	49	66	43	5	9	3	3	1	1			**1**	196
5	9	20	22	26	4	5			**1**					87
6	3	4	13	14	1	1	1	1			**1**		**1**	40
7		4	3	6	1									14
8			1	1	2			1						5
9	1													1
Total	188	397	616	707	107	54	20	12	4	3	1	3	1	2,078

Notes: The sample excludes households eligible for the ESSN via criteria other than the dependency ratio and is restricted to dependency ratios between 0 and 3. The number of prime adults refers to individuals aged 18 to 59 who are not disabled, while the number of dependents includes all other family members. Cells are shown in bold when the dependency ratio is 1.5 or higher.

Table 2: Covariate Balance

	RD Effect	p-value
Household Composition		
Household Size	-0.569	0.554
Number of Prime Age Adults not Disabled	-0.077	0.892
Number of Males Aged 18–59	0.087	0.764
Number of Females Aged 18–59	-0.167	0.555
Number of Dependents	-0.488	0.237
Number of Children	-0.162	0.126
Number of Elderly	-0.356	0.283
Household Head Characteristics		
Female	0.005	0.670
Education		
Primary School	0.012	0.535
Intermediate	0.005	0.660
Secondary	0.002	0.932
Technical	-0.017	0.132
University	-0.004	0.829
Occupation before Arrival		
Management/highly skilled	0.014	0.113
Skilled worker	0.021	0.063
Unskilled worker	-0.014	0.335
Retirement salary	-0.004	0.641
Other	-0.018	0.315
Nationality		
Afghan	-0.043	0.000
Iraqi	0.023	0.190
Syrian	0.020	0.114
Time Since Arrival in Turkey		
Less than 6 months	-0.009	0.475
6 months – 1 year	0.000	0.962
1 year – 3 years	-0.010	0.666
More than 3 years	0.023	0.417
Before the conflict started	-0.004	0.350
Region		
West Marmara Region	0.017	0.141
Aegean Region	-0.022	0.150
East Marmara Region	-0.003	0.889
West Anatolia Region	0.037	0.059
Mediterranean Region	-0.027	0.282
Central Anatolia Region	-0.015	0.099
West Black Sea Region	-0.009	0.262
East Black Sea Region	-0.009	0.008
Northeast Anatolia Region	-0.004	0.687
Central East Anatolia Region	0.008	0.356
Southeast Anatolia Region	0.045	0.006

Notes: A bandwidth of 1.5 is taken on either side of the cutoff; in other words, the dependency ratio ranges between 0 and 3. The sample excludes households eligible for the ESSN via criteria other than the dependency ratio; the sample size is 2,078. The dependent variables are given in the first column. All regressions include a control for the policy dummy and split linear polynomials around the cutoff in dependency ratio. Clustering is done at the level of dependency ratio. Statistically significant: *** 1 percent level; ** 5 percent level; * 10 percent level.

Table 3: Continuity-based Analysis with Alternative Cutoffs

	(2)	(3)	(4)	(5)
	Sample: Left-Hand Side of Actual Cutoff	Sample: Right-Hand Side of Actual Cutoff		
	Location of Alternative Cutoff relative to Actual Cutoff			
	-0.75	0.5	1	1.5
Beneficiary Status	0.037	-0.025	0.165	0.191
	[0.087]	[0.043]	[0.109]	[0.166]
Bootstrapped p-value	0.722	0.622	0.267	0.404
Any Man Works	0.025	0.187**	-0.060	-0.123
	[0.026]	[0.082]	[0.161]	[0.119]
Bootstrapped p-value	0.511	0.316	0.741	0.400
Any Woman Works	-0.049***	0.042	0.099	0.037
	[0.015]	[0.057]	[0.069]	[0.160]
Bootstrapped p-value	0.146	0.532	0.222	0.825
Any Adult Works	0.001	0.186**	-0.012	-0.124
	[0.017]	[0.068]	[0.147]	[0.129]
Bootstrapped p-value	0.953	0.229	0.955	0.494
Number Men Working	0.062	0.211**	-0.057	-0.163
	[0.193]	[0.087]	[0.180]	[0.129]
Bootstrapped p-value	0.807	0.310	0.771	0.312
Number Women Working	-0.072***	0.051	0.118	0.088
	[0.021]	[0.063]	[0.084]	[0.204]
Bootstrapped p-value	0.090	0.484	0.175	0.624
Number Adult Working	-0.009	0.261***	0.061	-0.074
	[0.196]	[0.078]	[0.172]	[0.242]
Bootstrapped p-value	0.964	0.132	0.754	0.809
Share Men Aged 18+ Working	0.093	0.130*	0.045	-0.076
	[0.086]	[0.070]	[0.173]	[0.140]
Bootstrapped p-value	0.469	0.441	0.838	0.641
Share Women Aged 18+ Working	-0.029**	0.039	0.065	-0.001
	[0.013]	[0.043]	[0.048]	[0.106]
Bootstrapped p-value	0.239	0.425	0.276	0.988
Share Adult Aged 18+ Working	0.037	0.081**	0.073	-0.056
	[0.049]	[0.036]	[0.075]	[0.090]
Bootstrapped p-value	0.592	0.216	0.509	0.646
Share Men Aged 18–59 Working	0.060	0.182*	-0.041	-0.095
	[0.068]	[0.087]	[0.168]	[0.127]
Bootstrapped p-value	0.528	0.396	0.748	0.522
Share Women Aged 18–59 Working	-0.030**	0.048	0.092	0.031
	[0.013]	[0.052]	[0.059]	[0.142]
Bootstrapped p-value	0.237	0.416	0.160	0.777
Share Adult Aged 18–59 Working	0.033	0.054	0.031	-0.126
	[0.041]	[0.055]	[0.079]	[0.099]
Bootstrapped p-value	0.578	0.452	0.769	0.405
Observations	1,406	709	709	709

Notes: The sample is restricted to the left of the actual cutoff in column (2) and to the right of the actual cutoff in columns (3) to (5). On the right-hand side of the cutoff, all values of the dependency ratio are taken. Hence, the cutoff is at DR=0.75 in column (2), DR=2 in column (3), DR=2.5 in column (4), and DR=3.0 in column (5) — where DR stands for dependency ratio. Each cell comes from a separate regression. All regressions include a control for the policy dummy, split linear polynomials around the cutoff in the running variable (dependency ratio), and several control variables. These controls include a dummy variable for ESSN program eligibility via any of the five other eligibility criteria other than that on the dependency ratio, dummies for household head's sex, education, occupation before arrival, nationality, time of arrival, and 12 NUTS-1 level regions. Clustering is done at the level of dependency ratio. Statistically significant: *** 1 percent level; ** 5 percent level; * 10 percent level.

Table 4: Reduced-Form and 2SLS RDD Estimates for Adult Employment Outcomes

	(1) Beneficiary	(2) Any Man Work	(3) Any Woman Work	(4) Any Adult Work	(5) Number Man Work	(6) Number Woman Work	(7) Number Adult Work	(8) Share Man Work	(9) Share Woman Work	(10) Share Adult Work
Panel A: Reduced Form (Intention-to-Treat)										
Dependency Ratio Criterion	0.530***	0.070***	-0.021	0.052**	0.102	-0.021	0.080	0.102**	-0.002	0.071***
	[0.040]	[0.021]	[0.022]	[0.022]	[0.111]	[0.029]	[0.137]	[0.049]	[0.012]	[0.024]
Bootstrapped p-value	0.019	0.235	0.636	0.387	0.805	0.698	0.849	0.204	0.858	0.121
Panel B: 2SLS (Effect of Being an ESSN Beneficiary)										
ESSN		0.132***	-0.039	0.098***	0.192	-0.041	0.152	0.193**	-0.004	0.135***
		[0.032]	[0.042]	[0.037]	[0.198]	[0.054]	[0.247]	[0.097]	[0.021]	[0.049]
Bootstrapped p-value		0.000	0.552	0.105	0.588	0.582	0.662	0.238	0.849	0.079
F statistics		178.40	178.40	178.40	178.40	178.40	178.40	178.40	178.40	178.40
Mean of Dep. Var.		0.789	0.075	0.816	0.910	0.083	0.993	0.702	0.056	0.391

Notes: The data come from the Comprehensive Vulnerability Monitoring Exercise (CVME) Turkey, rounds 3-5. Panel A reports reduced-form (intention-to-treat) estimates of the dependency-ratio criterion; column (1) is the first-stage effect on beneficiary status. Panel B reports 2SLS estimates of the effect of being an ESSN beneficiary, instrumented by the dependency-ratio criterion; the F statistic is the first-stage F. The sample excludes households eligible for the ESSN via criteria other than the dependency ratio and is restricted to dependency ratios between 0 and 3. All regressions include controls for dummies for the household head's sex, education, and occupation before arrival, nationality, time of arrival, and 12 NUTS-1 level regions, survey wave, and split-linear trends of the running variable (dependency ratio) on both sides of the cutoff. The sample has 2,078 observations. Clustering is done at the level of the dependency ratio.

Table 5: Adult Employment Outcomes — 2SLS Estimates with Alternative Specifications

	(1) Any Man Work	(2) Any Woman Work	(3) Any Adult Work	(4) Number Man Work	(5) Number Woman Work	(6) Number Adult Work	(7) Share Man Work	(8) Share Woman Work	(9) Share Adult Work
Single (Not Split) Linear Polynomials in Dependency Ratio									
ESSN	0.128***	-0.054	0.093***	0.111	-0.059	0.052	0.229**	-0.021	0.121***
	[0.026]	[0.042]	[0.029]	[0.195]	[0.052]	[0.243]	[0.098]	[0.030]	[0.035]
Bootstrapped p-value	0.000	0.589	0.000	0.831	0.633	0.949	0.460	0.685	0.026
F statistics	38.93	38.93	38.93	38.93	38.93	38.93	38.93	38.93	38.93
Single (Not Split) Quadratic Polynomials in Dependency Ratio									
ESSN	0.126***	-0.057*	0.092***	0.092	-0.062	0.030	0.235***	-0.025	0.116***
	[0.025]	[0.034]	[0.026]	[0.144]	[0.042]	[0.181]	[0.079]	[0.020]	[0.035]
Bootstrapped p-value	0.000	0.451	0.001	0.748	0.531	0.936	0.108	0.453	0.054
F statistics	137.20	137.20	137.20	137.20	137.20	137.20	137.20	137.20	137.20
Linear Polynomials in Number of Dependents and Number of Prime Adults									
ESSN	0.076	-0.038	0.061	0.187***	-0.037	0.150**	0.127*	-0.014	0.078***
	[0.046]	[0.028]	[0.043]	[0.052]	[0.030]	[0.065]	[0.075]	[0.026]	[0.022]
Bootstrapped p-value	0.269	0.411	0.260	0.000	0.391	0.005	0.390	0.724	0.000
F statistics	40.19	40.19	40.19	40.19	40.19	40.19	40.19	40.19	40.19
Linear Polynomials in Log Number of Dependents and Log Number of Prime Adults									
ESSN	0.108***	-0.023	0.086***	0.205***	-0.027	0.178*	0.141*	0.003	0.078***
	[0.029]	[0.035]	[0.029]	[0.065]	[0.039]	[0.093]	[0.084]	[0.027]	[0.022]
Bootstrapped p-value	0.004	0.674	0.001	0.000	0.643	0.060	0.383	0.929	0.000
F statistics	44.78	44.78	44.78	44.78	44.78	44.78	44.78	44.78	44.78
Mean of Dep. Var.	0.789	0.075	0.816	0.910	0.083	0.993	0.702	0.056	0.391

Notes: The data come from the Comprehensive Vulnerability Monitoring Exercise (CVME) Turkey, rounds 3-5. The sample excludes households eligible for the ESSN via criteria other than the dependency ratio and is restricted to dependency ratios between 0 and 3. All regressions include controls for dummies for the household head's sex, education, and occupation before arrival, nationality, time of arrival, and 12 NUTS-1 level regions, survey wave, and split-linear trends of the running variable (dependency ratio) on both sides of the cutoff. The sample has 2,078 observations. Clustering is done at the level of the dependency ratio.

Table 6: The Effect of ESSN Cash Transfers on Connectivity, Food Security, and Non-Food Expenditure

Outcome	ESSN coefficient	SE	Bootstrapped p-value	First-stage F	Mean of dep. var.	Obs.
Panel A: Connectivity (CVME Wave 4)						
Owns mobile phone	-0.048	[0.047]	0.452	346.90	0.673	704
Messaging	0.122**	[0.054]	0.129	346.90	0.301	704
Messaging every day	0.066	[0.045]	0.301	346.90	0.251	704
Voice calls	0.071**	[0.036]	0.028	346.90	0.459	704
Voice calls every day	0.118**	[0.049]	0.064	346.90	0.402	704
Translation	-0.010	[0.042]	0.882	346.90	0.119	704
Translation every day	-0.027	[0.034]	0.504	346.90	0.055	704
News	0.146***	[0.044]	0.003	346.90	0.170	704
News every day	0.125***	[0.043]	0.002	346.90	0.136	704
Social media	0.010	[0.052]	0.860	346.90	0.322	704
Social media every day	0.004	[0.057]	0.959	346.90	0.294	704
Panel B: Food Security and Food Expenditure (CVME Waves 3–5)						
rCSI	-2.211**	[0.867]	0.000	178.40	14.406	2,078
Purchased food expenditure, past 7 days (TL)	-27.872	[49.336]	0.767	178.40	205.683	2,078
Non-purchased food expenditure, past 7 days (TL)	-2.222	[1.745]	0.259	178.40	6.835	2,078
Adult-equivalent food expenditure, past year (TL)	208.648	[213.490]	0.385	178.40	4588.258	2,078
Panel C: Non-Food Expenditure, last 6 Months, TL (CVME Waves 3–5)						
Education	118.157***	[41.962]	0.022	178.40	124.213	2,078
Clothes and shoes	55.839	[44.782]	0.255	178.40	273.115	2,078
Debt repayment	-59.002	[77.745]	0.611	178.40	288.072	2,078
Social events and celebrations	-39.549	[76.065]	0.671	178.40	105.117	2,078
Agricultural inputs	11.098**	[4.784]	0.277	178.40	1.346	2,078
Savings	144.796**	[58.118]	0.049	178.40	119.826	2,078
Construction	-54.705	[34.598]	0.167	178.40	84.607	2,078
Essential non-food items	79.743	[155.425]	0.571	178.40	587.052	2,078
Non-food items	310.409**	[132.684]	0.017	178.40	1408.507	2,078

Notes: The data come from the CVME Turkey; Panel A uses Wave 4 and Panels B and C use Waves 3–5. The sample excludes households eligible for the ESSN via criteria other than the dependency ratio and is restricted to dependency ratios between 0 and 3. Each row reports a separate 2SLS regression of the outcome on beneficiary status, instrumented by the dependency-ratio criterion; the columns report the coefficient, its standard error (clustered at the level of the dependency ratio), the wild-cluster bootstrap p-value (9,999 replications), the first-stage F statistic, the mean of the dependent variable, and the number of observations. All regressions include controls for dummies for the household head's sex, education and occupation before arrival, nationality, time of arrival, and 12 NUTS-1 level regions, survey-wave fixed effects where the outcome is observed in more than one wave, and split-linear trends of the running variable (dependency ratio) on both sides of the cutoff. For the phone-use outcomes in Panel A, unmarked rows refer to use on some days in the past 7 days and "every day" rows to daily use. Annual food expenditure in Panel B is expressed in adult-equivalent terms because the running variable is defined by household composition, which makes per-capita normalizations mechanically sensitive to the dependency ratio. In Panel C, essential non-food items are the sum of education, clothing, social events, and construction; non-food items additionally include health, debt, agricultural inputs, and savings.

Table 7: The Effect of ESSN Cash Transfers on Adult Health, Social Cohesion, Language and Technical Skills, and Personal Well-being

Outcome	ESSN coefficient	SE	Bootstrapped p-value	First-stage F	Mean of dep. var.	Obs.
Panel A: Adult Health and Healthcare Expenditure (CVME Waves 4–5)						
Share of men sick	0.009	[0.039]	0.857	158.20	0.122	1,422
Share of men aged 18–59 sick	0.056	[0.036]	0.210	158.20	0.110	1,422
Healthcare expenditure, past 6 months (TL)	133.775	[89.182]	0.329	178.40	412.212	2,078
Panel B: Social Cohesion (CVME Wave 5)						
Advice from Syrian friends: getting things done	-0.003	[0.105]	0.982	63.78	0.779	718
Advice from Syrian friends: jobs/financial matters	0.075	[0.092]	0.477	63.78	0.759	718
Advice from Turkish friends: getting things done	-0.004	[0.064]	0.958	63.78	0.290	718
Advice from Turkish friends: jobs/financial matters	-0.053	[0.064]	0.482	63.78	0.260	718
Panel C: Language and Technical Skills (CVME Waves 4–5)						
Share of adult men speaking Turkish	-0.069	[0.079]	0.662	158.20	0.537	1,422
Share of men aged 18–59 in language training	0.041**	[0.018]	0.014	346.90	0.047	704
Share of men aged 18–59 in technical training	0.012	[0.022]	0.638	346.90	0.011	704
Panel D: Personal Well-being (CVME Wave 4; 0 = not at all, 3 = every day)						
No interest in doing things	-1.327***	[0.254]	0.000	68.74	1.283	166
Felt down or hopeless	-0.924***	[0.268]	0.000	73.74	1.396	164
Poor appetite or craving for more food	-1.284***	[0.290]	0.000	77.95	1.225	169
Felt nervous, anxious, on edge	-0.758*	[0.432]	0.403	73.22	1.359	167
Cannot stop worrying	-1.504***	[0.243]	0.000	80.94	1.234	167
PW-Score (0–15)	-5.604***	[1.232]	0.000	65.31	6.425	160

Notes: The data come from the CVME Turkey; Panel A uses Waves 4–5 except healthcare expenditure (Waves 3–5), Panel B uses Wave 5, Panel C uses Waves 4–5 for the Turkish-speaking outcome and Wave 4 for the course outcomes, and Panel D uses Wave 4. Panel D outcomes are measured on a 0–3 frequency scale (0 = not at all, 3 = every day), with the PW-Score summing the five items onto a 0–15 scale; these outcomes were collected in private for a subsample of households (see Appendix Tables A12 and A13). The sample excludes households eligible for the ESSN via criteria other than the dependency ratio and is restricted to dependency ratios between 0 and 3. Each row reports a separate 2SLS regression of the outcome on beneficiary status, instrumented by the dependency-ratio criterion; the columns report the coefficient, its standard error (clustered at the level of the dependency ratio), the wild-cluster bootstrap p-value (9,999 replications), the first-stage F statistic, the mean of the dependent variable, and the number of observations. All regressions include controls for dummies for the household head's sex, education and occupation before arrival, nationality, time of arrival, and 12 NUTS-1 level regions, survey-wave fixed effects where the outcome is observed in more than one wave, and split-linear trends of the running variable (dependency ratio) on both sides of the cutoff.

Table 8: Heterogeneity in the Effect of ESSN Cash Transfers on Adult Employment by Household Characteristics

	(1) Any Man Work	(2) Any Woman Work	(3) Any Adult Work	(4) Number Man Work	(5) Number Woman Work	(6) Number Adult Work	(7) Share Man Work	(8) Share Woman Work	(9) Share Adult Work	(10) Nbr of Obs.
A) Education of Household Head										
Illiterate or Literate with no degree	0.235**	0.009	0.206***	0.280	0.002	0.282	0.275***	0.065*	0.203***	559
	[0.091]	[0.053]	[0.072]	[0.244]	[0.073]	[0.277]	[0.092]	[0.039]	[0.051]	
Bootstrapped p-value	0.149	0.884	0.109	0.510	0.979	0.624	0.080	0.060	0.035	
F statistics	237.10	237.10	237.10	237.10	237.10	237.10	237.10	237.10	237.10	
Primary/Middle/High/Technical	0.056	-0.060	0.027	0.131	-0.062	0.069	0.138	-0.030	0.098**	1,351
	[0.040]	[0.049]	[0.045]	[0.215]	[0.056]	[0.264]	[0.107]	[0.028]	[0.049]	
Bootstrapped p-value	0.323	0.475	0.635	0.688	0.507	0.826	0.394	0.512	0.130	
F statistics	130.60	130.60	130.60	130.60	130.60	130.60	130.60	130.60	130.60	
University	0.127	-0.126	0.081	0.143	-0.126	0.017	0.095	-0.105	0.072	154
	[0.091]	[0.151]	[0.079]	[0.134]	[0.151]	[0.198]	[0.184]	[0.122]	[0.070]	
Bootstrapped p-value	0.160	0.580	0.273	0.159	0.580	0.950	0.730	0.549	0.367	
F statistics	56.51	56.51	56.51	56.51	56.51	56.51	56.51	56.51	56.51	
B) Time of Arrival in Turkey										
First arrival < 3 years	0.019	-0.018	0.019	0.125	-0.030	0.095	0.042	0.006	0.069	908
	[0.040]	[0.050]	[0.044]	[0.135]	[0.058]	[0.188]	[0.101]	[0.024]	[0.064]	
Bootstrapped p-value	0.641	0.810	0.705	0.541	0.722	0.723	0.794	0.858	0.543	
F statistics	100.60	100.60	100.60	100.60	100.60	100.60	100.60	100.60	100.60	
First arrival > 3 years	0.219***	-0.050	0.164***	0.235	-0.043	0.192	0.304***	-0.010	0.181***	1,170
	[0.050]	[0.036]	[0.051]	[0.224]	[0.048]	[0.263]	[0.107]	[0.018]	[0.049]	
Bootstrapped p-value	0.006	0.390	0.067	0.575	0.555	0.673	0.126	0.632	0.027	
F statistics	274.00	274.00	274.00	274.00	274.00	274.00	274.00	274.00	274.00	

Notes: The data come from the Comprehensive Vulnerability Monitoring Exercise (CVME) Turkey, rounds 3-5. The sample excludes households eligible for the ESSN via criteria other than the dependency ratio and is restricted to dependency ratios between 0 and 3. All regressions include the same set of controls for dummies for the household head's sex, education (drops in Panel A), and occupation before arrival, nationality, time of arrival (drops in Panel B), 12 NUTS-1 level regions, and survey wave, as well as split-linear trends of the running variable (dependency ratio) on both sides of the cutoff. Clustering is done at the level of the dependency ratio. The number of clusters (dependency-ratio mass points) is 23, 28, and 16 for the three education groups of Panel A (illiterate or literate with no degree; primary through technical; university) and 26 and 28 for the two arrival groups of Panel B (within and beyond three years), respectively.

Table 9: Local-Randomization RD Estimates for Employment Outcomes Around the Eligibility Cutoff

Outcome	ITT	Fisherian p-value	95% randomization confidence interval	First stage	Implied LATE
Any man works	0.062**	0.012	[0.015, 0.109]	0.630	0.098
Any woman works	-0.014	0.359	[-0.042, 0.015]	0.630	-0.021
Any adult works	0.052**	0.031	[0.006, 0.098]	0.630	0.082
Number men work	0.024	0.415	[-0.031, 0.077]	0.630	0.038
Number women work	-0.017	0.272	[-0.047, 0.012]	0.630	-0.027
Number adults work	0.007	0.849	[-0.052, 0.066]	0.630	0.010
Share men work	0.109***	0	[0.061, 0.157]	0.630	0.174
Share women work	-0.002	0.900	[-0.025, 0.023]	0.630	-0.002
Share adults work	0.053***	0	[0.028, 0.079]	0.630	0.084

Notes: The table reports local-randomization regression discontinuity estimates comparing households just above and just below the eligibility cutoff, where the running variable is the dependency ratio centered at the cutoff. Estimates are obtained using the `rdrandinf` command following Cattaneo et al. (2020). The estimation window is $[-0.5, 0]$ in the centered running variable (dependency ratios between 1 and 1.5), the narrowest window with a strong first stage that passes the within-window covariate-balance check. The ITT columns report reduced-form effects of crossing the eligibility threshold; inference is Fisherian: p-values and 95 percent confidence intervals come from the randomization distribution of the difference in means, the latter by inverting the randomization test under a constant treatment effect, with 5,000 replications. The first-stage column reports the effect of crossing the threshold on beneficiary status in each outcome's estimation sample, and the implied-LATE column scales the ITT by that first stage; the implied LATEs are descriptive, and we attach no separate inferential statements to them. p-values below 0.001 are reported as 0. Statistical significance (ITT): * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Local-Randomization RD Estimates Around the Eligibility Cutoff: Food Security, Non-Food Expenditure, and Adult Health

Outcome	ITT	Fisherian p-value	95% randomization confidence interval	First stage	Implied LATE
Panel A: Food Security and Food Expenditure (CVME Waves 3–5)					
rCSI	0.172	0.861	[-1.744, 2.094]	0.630	0.274
Food purchases, past 7 days	8.3	0.215	[-5.0, 21.7]	0.630	13.2
Food non-purchases, past 7 days	-1.0	0.533	[-3.9, 2.0]	0.630	-1.5
Food consumption, past year, PAE	-2.5	0.987	[-303.1, 298.3]	0.630	-3.9
Panel B: Non-Food Expenditure, last 6 Months, TL (CVME Waves 3–5)					
Education	79.1***	0	[44.5, 113.3]	0.630	125.6
Clothing	13.2	0.624	[-39.0, 65.3]	0.630	21.0
Debt repayment	-25.2	0.640	[-130.5, 80.1]	0.630	-40.0
Social expenditures	-10.8	0.877	[-104.9, 83.1]	0.630	-17.2
Agriculture	0.65**	0.040	[0.01, 1.28]	0.630	1.03
Savings	25.6	0.582	[-57.5, 108.8]	0.630	40.6
Construction	-13.4	0.526	[-52.5, 25.6]	0.630	-21.3
Non-food expenditures, past 6 months (robust)	68.1	0.325	[-63.2, 199.4]	0.630	108.1
Non-food expenditures, past 6 months	35.9	0.769	[-213.4, 288.8]	0.630	57.1
Panel C: Adult Health and Healthcare Expenditure (CVME Waves 4–5)					
Share of men sick	-0.012	0.651	[-0.058, 0.035]	0.637	-0.019
Share of men aged 18–59 sick	0.008	0.762	[-0.036, 0.051]	0.637	0.012
Health expenditures	-36.3	0.589	[-166.7, 93.2]	0.637	-56.9

Notes: The table reports local-randomization regression discontinuity estimates comparing households just above and just below the eligibility cutoff, where the running variable is the dependency ratio centered at the cutoff. Estimates are obtained using the `rdrandinf` command following Cattaneo et al. (2020). The estimation window is $[-0.5, 0]$ in the centered running variable (dependency ratios between 1 and 1.5), the narrowest window with a strong first stage that passes the within-window covariate-balance check. The ITT columns report reduced-form effects of crossing the eligibility threshold; inference is Fisherian: p-values and 95 percent confidence intervals come from the randomization distribution of the difference in means, the latter by inverting the randomization test under a constant treatment effect, with 5,000 replications. The first-stage column reports the effect of crossing the threshold on beneficiary status in each outcome's estimation sample, and the implied-LATE column scales the ITT by that first stage; the implied LATEs are descriptive, and we attach no separate inferential statements to them. p-values below 0.001 are reported as 0. Statistical significance (ITT): * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Multiple-Hypothesis Testing for Main Labor-Supply Outcomes

Outcome	Bootstrapped p-value	Romano–Wolf p-value	Anderson q-value	Benjamini–Hochberg q-value
Panel A: Any member works				
Any man works	0.000	0.052	0.000	0.000
Any woman works	0.552	0.389	0.226	0.552
Any adult works	0.105	0.162	0.118	0.158
Panel B: Number of members working				
Number of men working	0.588	0.510	1.000	0.662
Number of women working	0.582	0.604	1.000	0.662
Number of adults working	0.662	0.604	1.000	0.662
Panel C: Share of members working				
Share of men working	0.238	0.388	0.313	0.357
Share of women working	0.849	0.819	0.556	0.849
Share of adults working	0.079	0.284	0.312	0.238

Notes: Each row reports inference for the 2SLS (fuzzy RD) effect of ESSN beneficiary status on the indicated outcome, estimated on the main sample of households within a dependency-ratio bandwidth of ± 1.5 of the cutoff, excluding households that qualify for ESSN through criteria other than the dependency ratio. Bootstrapped p-values are wild-cluster-bootstrap p-values, clustered on the running variable, from the main specification, with 9,999 replications; they coincide with the bootstrapped p-values in Table 4. Romano–Wolf p-values control the familywise error rate through studentized resampling with 5,000 replications. Anderson q-values are Anderson (2008) sharpened two-stage FDR q-values, and Benjamini–Hochberg q-values follow Benjamini and Hochberg (1995); both are calculated from the bootstrapped p-values. Multiple-testing adjustments are conducted separately within each panel, mirroring the domain-wise convention of Tables A10 and A11; corrections treating all nine outcomes as a single family are summarized in the text.

Appendix A: Additional Figures and Tables

Additional Figures

Figure A1: Histogram of the Running Variable

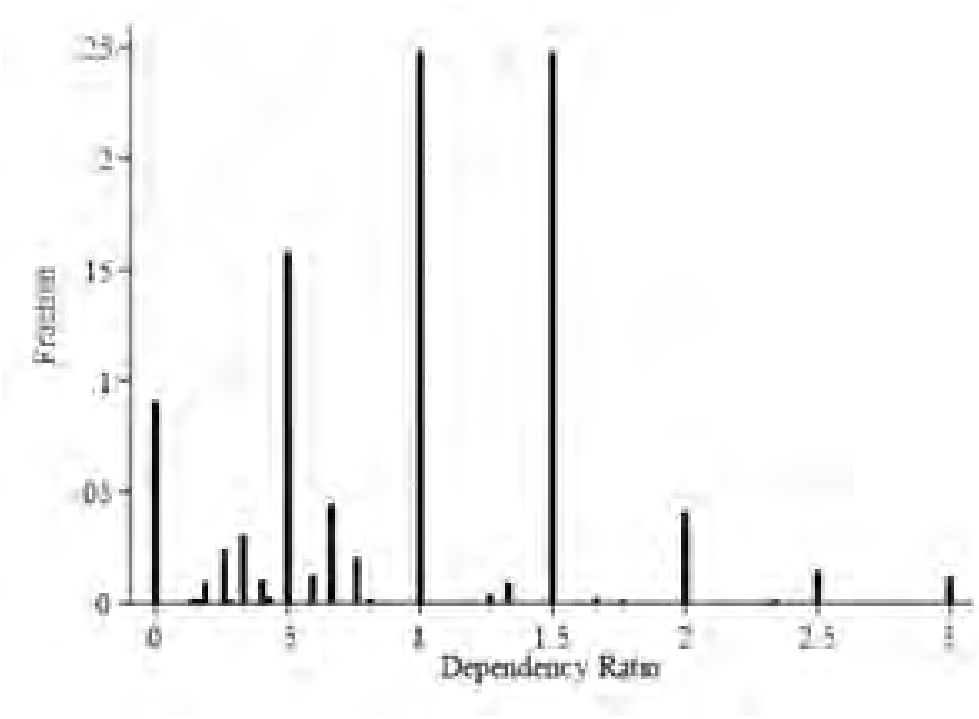
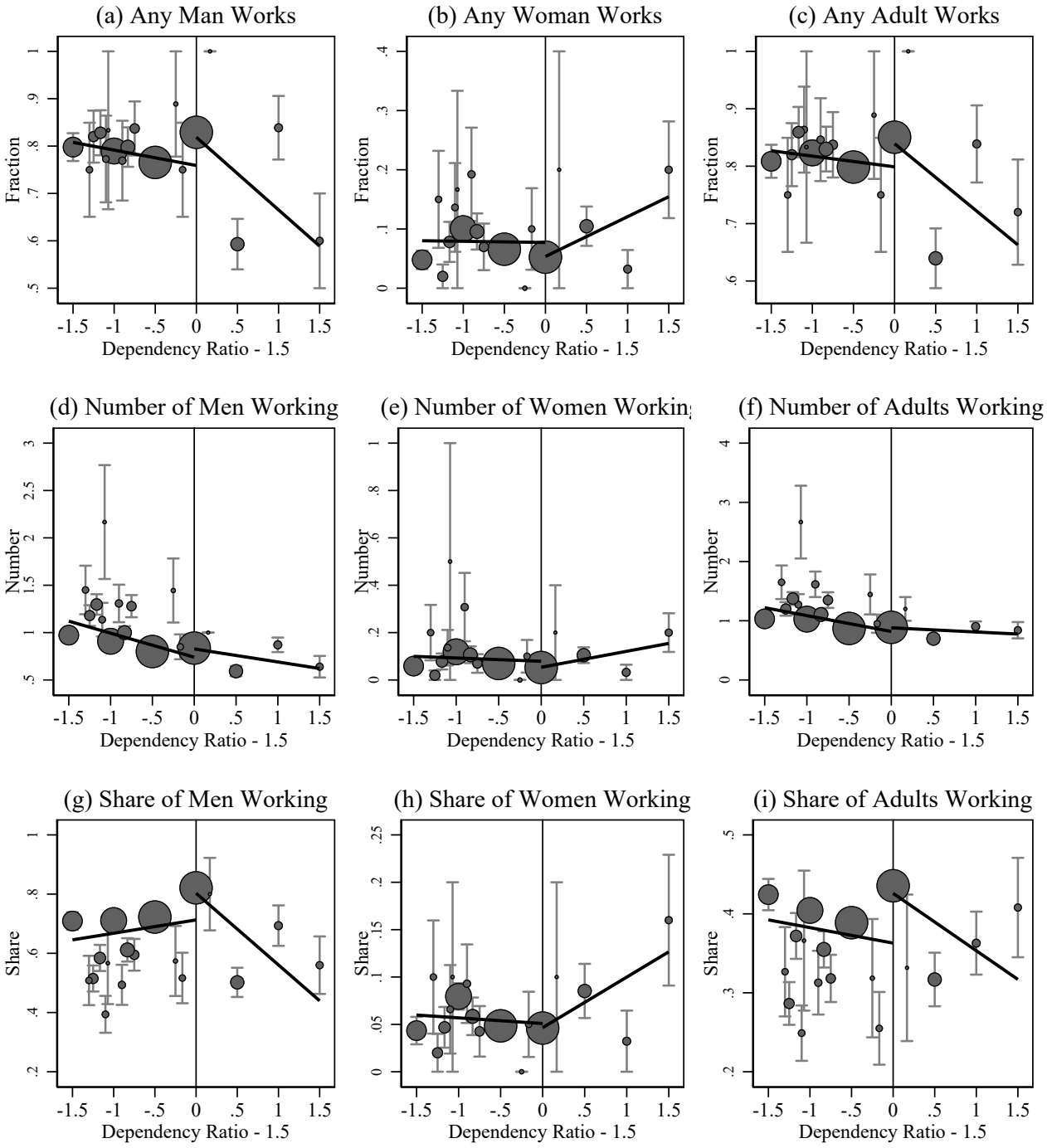
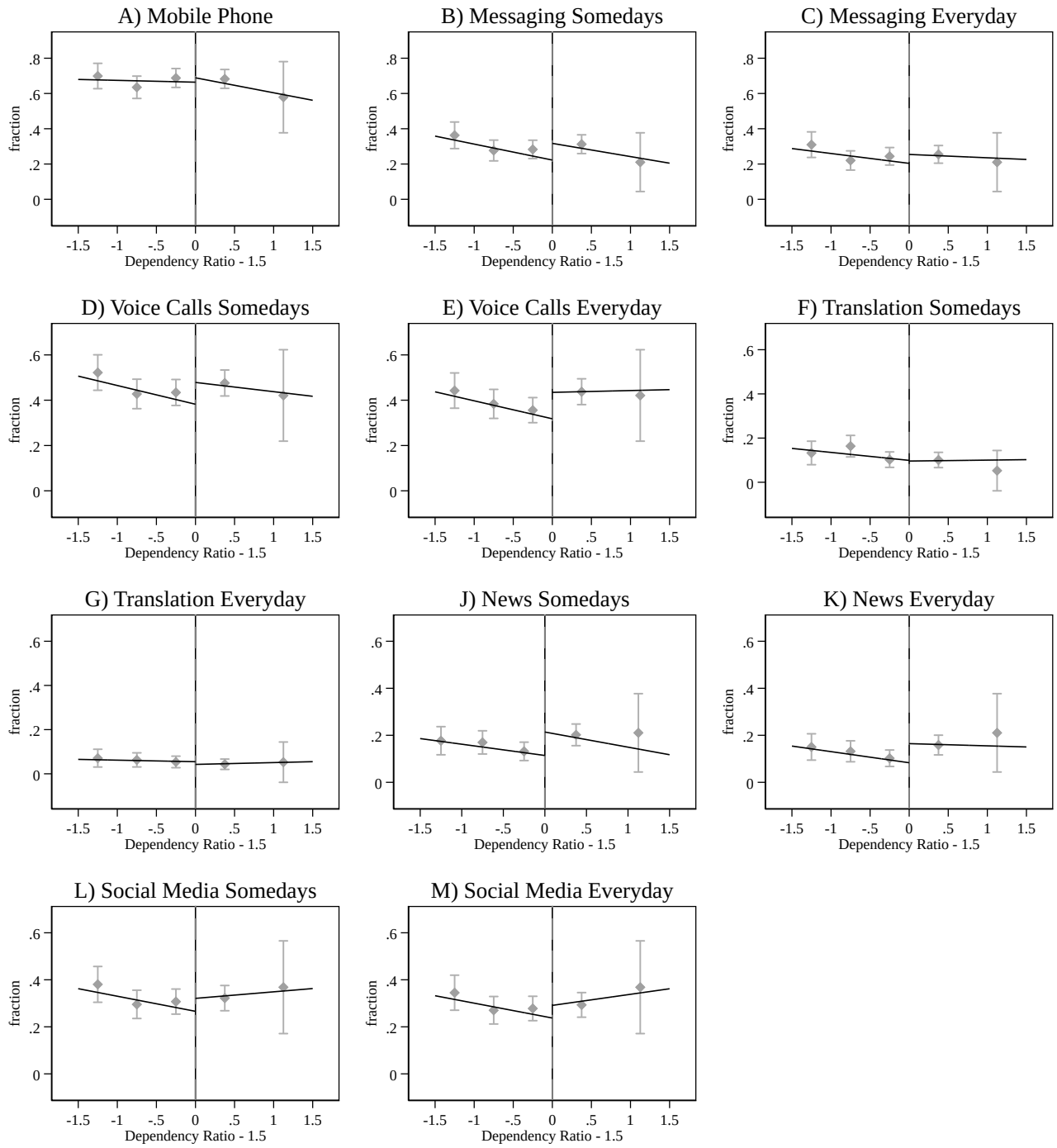


Figure A2: Cell Means for Employment Outcomes by Dependency-ratio Value



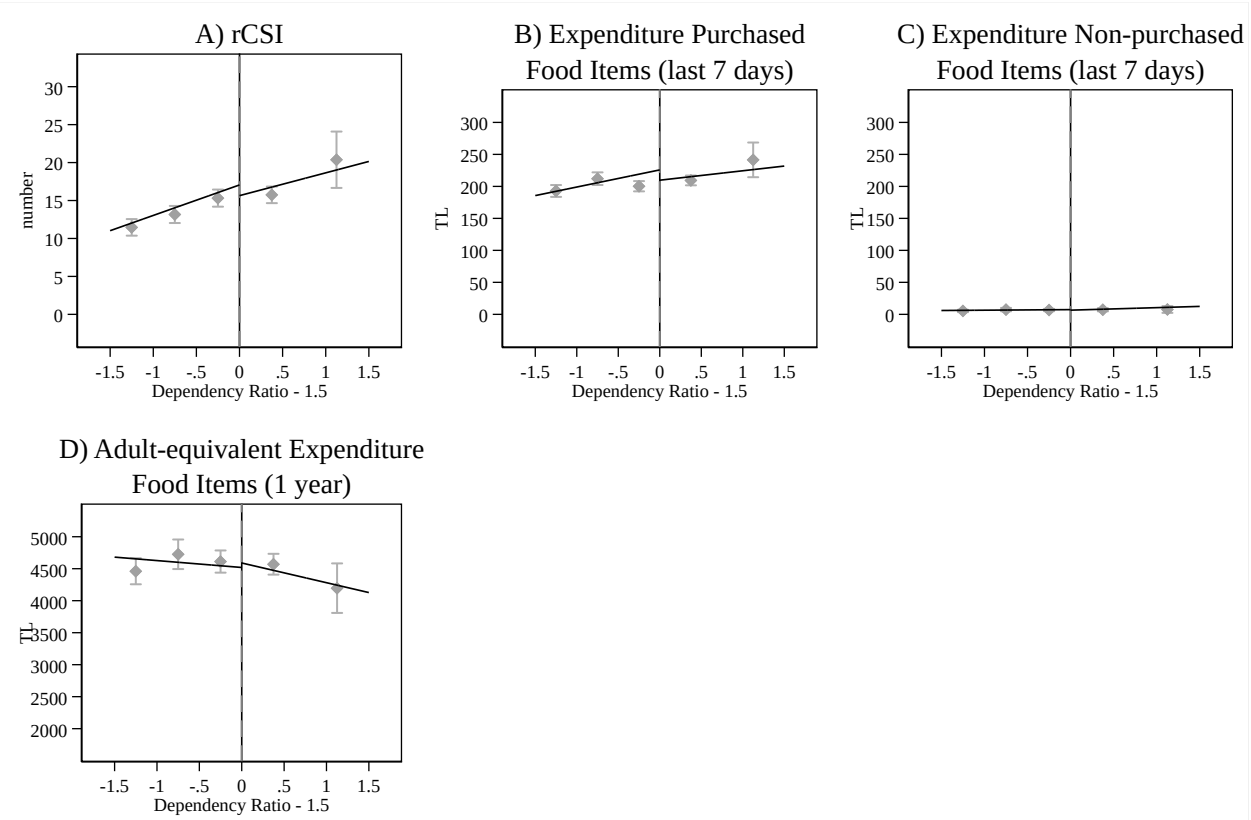
Notes: Each panel plots an adult-employment outcome against the running variable (the dependency ratio, centered at the 1.5 cutoff) for the sample that excludes households eligible for the ESSN program via criteria other than the dependency ratio, restricted to dependency ratios in $[0, 3]$. Because the running variable is discrete, each of its distinct values defines a natural cell; each marker is the mean of the outcome in that cell, and marker area is proportional to the number of observations in the cell. Cells containing fewer than five observations are not plotted. Error bars are ± 1 standard error of the cell mean. The solid lines are linear regressions fitted separately on each side of the cutoff using the original individual-level data (descriptive; no controls), drawn out to the cutoff to depict pre- and post-threshold trends.

Figure A3: RDD Graphs for Connectivity Outcomes



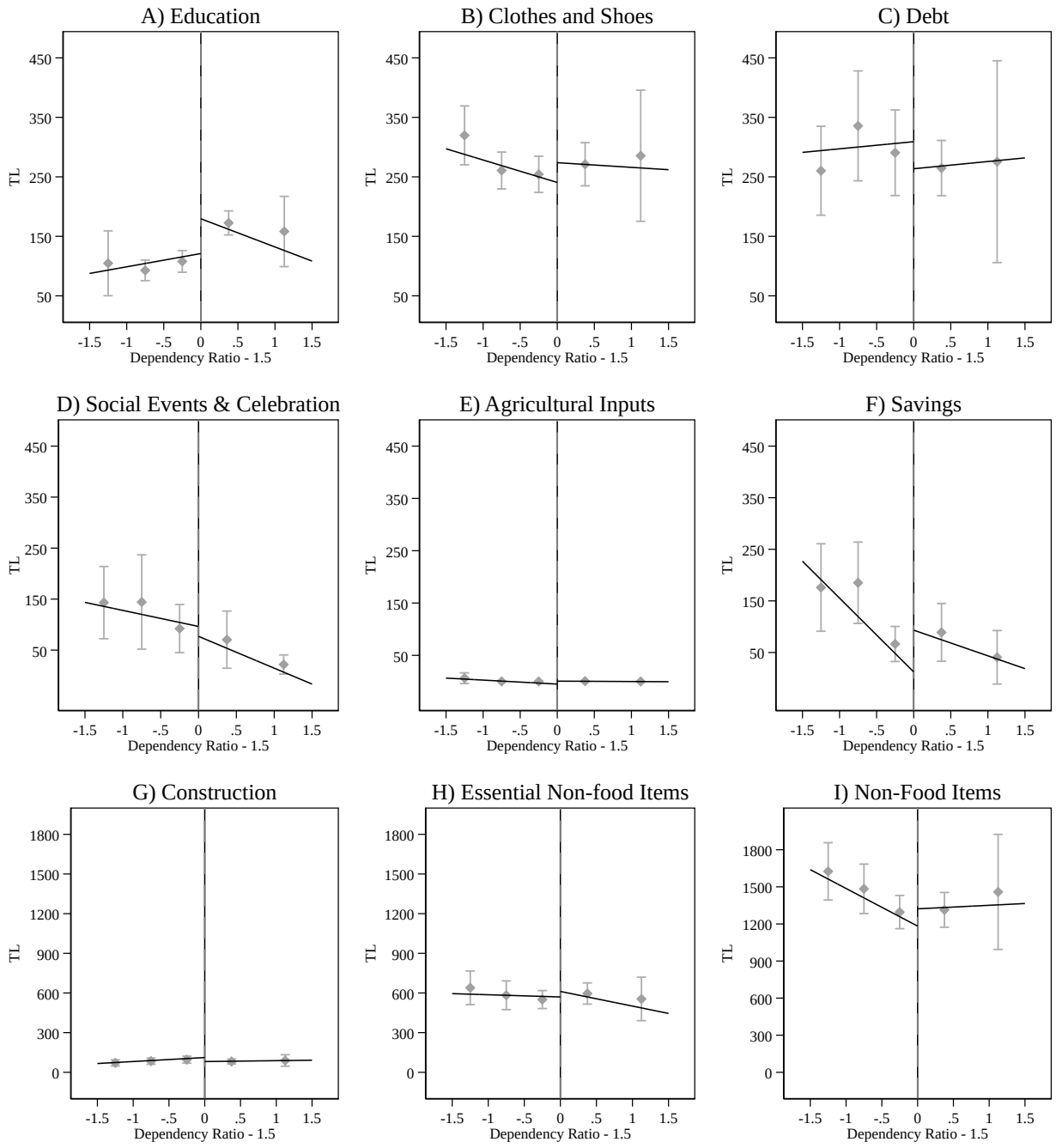
Notes: The figure plots the fraction of connectivity variables against the running variable (the dependency ratio, centered at the 1.5 cutoff) for the sample that excludes households eligible for the ESSN program via criteria other than the dependency ratio. All figures are constructed using the `rdplot` command of Calonico, Cattaneo, and Titiunik (2015, 2017): to account for the uneven distribution of the running variable, it is partitioned into three equal bins on the left and two equal bins on the right of the cutoff, the mean is plotted for each bin, and bin-specific 90 percent confidence intervals, robust to heteroskedasticity, are shown as error bars. Linear local polynomial regressions are fitted separately on each side of the cutoff using the original individual-level data to depict pre- and post-threshold trends.

Figure A4: RDD Graphs for Food Security and Expenditure Outcomes



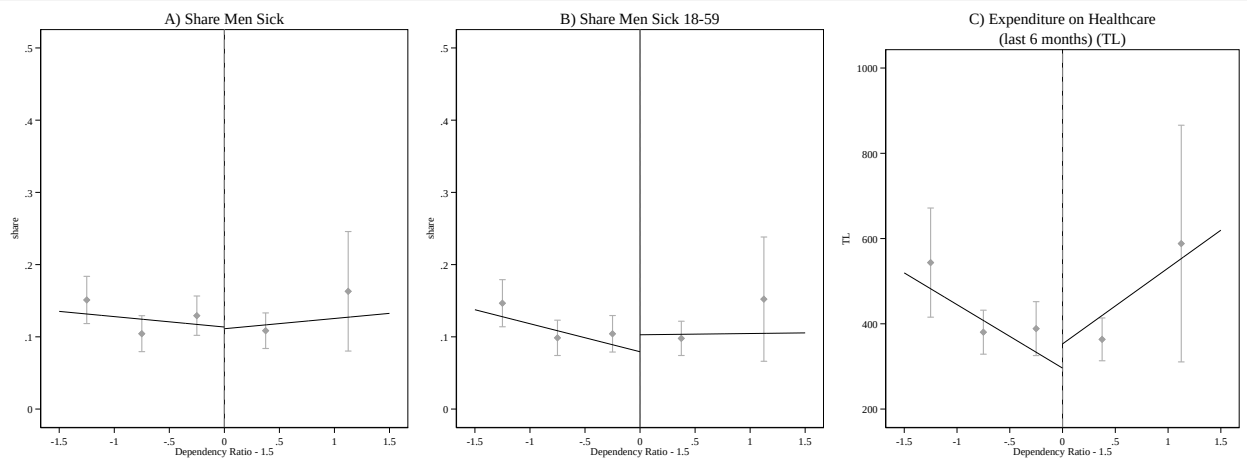
Notes: The figure plots the variable means against the running variable (the dependency ratio, centered at the 1.5 cutoff) for the sample that excludes households eligible for the ESSN program via criteria other than the dependency ratio. All figures are constructed using the `rdplot` command of Calonico, Cattaneo, and Titiunik (2015, 2017): to account for the uneven distribution of the running variable, it is partitioned into three equal bins on the left and two equal bins on the right of the cutoff, the mean is plotted for each bin, and bin-specific 90 percent confidence intervals, robust to heteroskedasticity, are shown as error bars. Linear local polynomial regressions are fitted separately on each side of the cutoff using the original individual-level data to depict pre- and post-threshold trends.

Figure A5: RDD Graphs for Expenditure Outcomes



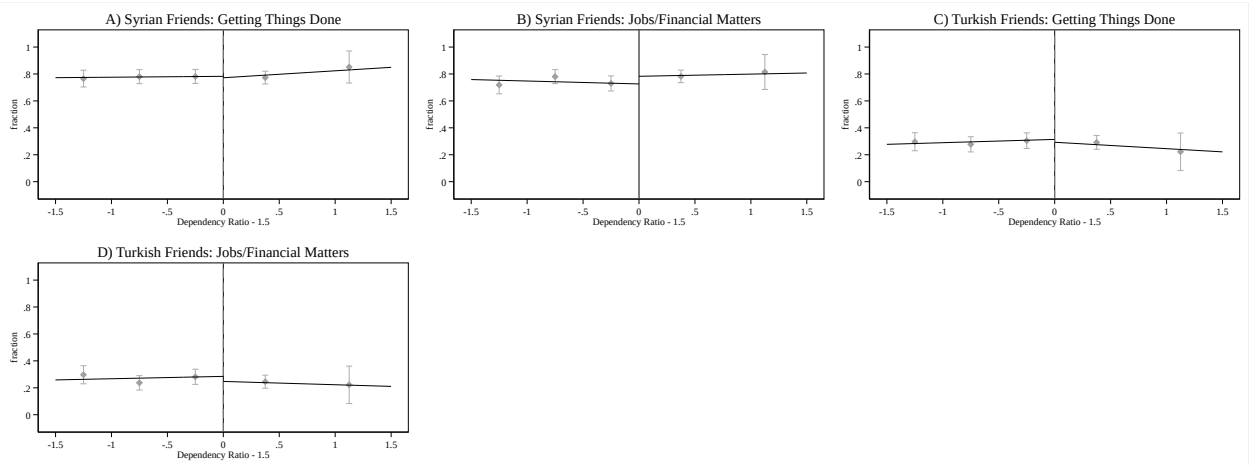
Notes: The figure plots the variable means against the running variable (the dependency ratio, centered at the 1.5 cutoff) for the sample that excludes households eligible for the ESSN program via criteria other than the dependency ratio. All figures are constructed using the `rdplot` command of Calonico, Cattaneo, and Titiunik (2015, 2017): to account for the uneven distribution of the running variable, it is partitioned into three equal bins on the left and two equal bins on the right of the cutoff, the mean is plotted for each bin, and bin-specific 90 percent confidence intervals, robust to heteroskedasticity, are shown as error bars. Linear local polynomial regressions are fitted separately on each side of the cutoff using the original individual-level data to depict pre- and post-threshold trends.

Figure A6: RDD Graphs for Adult Health and Household Health Expenditure Outcomes



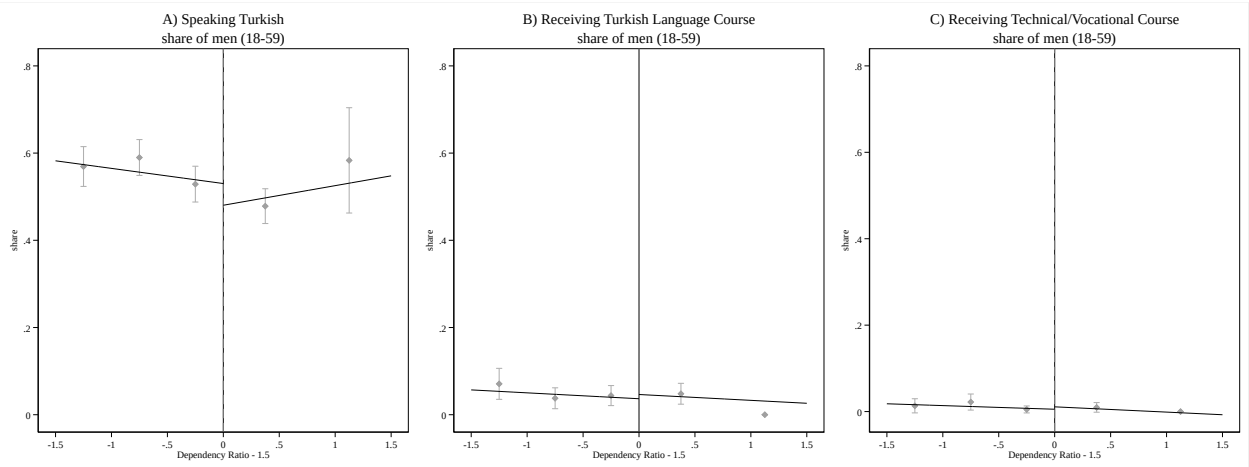
Notes: The figure plots the shares and health expenditure variable mean against the running variable (the dependency ratio, centered at the 1.5 cutoff) for the sample that excludes households eligible for the ESSN program via criteria other than the dependency ratio. All figures are constructed using the `rdplot` command of Calonico, Cattaneo, and Titiunik (2015, 2017): to account for the uneven distribution of the running variable, it is partitioned into three equal bins on the left and two equal bins on the right of the cutoff, the mean is plotted for each bin, and bin-specific 90 percent confidence intervals, robust to heteroskedasticity, are shown as error bars. Linear local polynomial regressions are fitted separately on each side of the cutoff using the original individual-level data to depict pre- and post-threshold trends.

Figure A7: RDD Graphs for Social Cohesion Outcomes



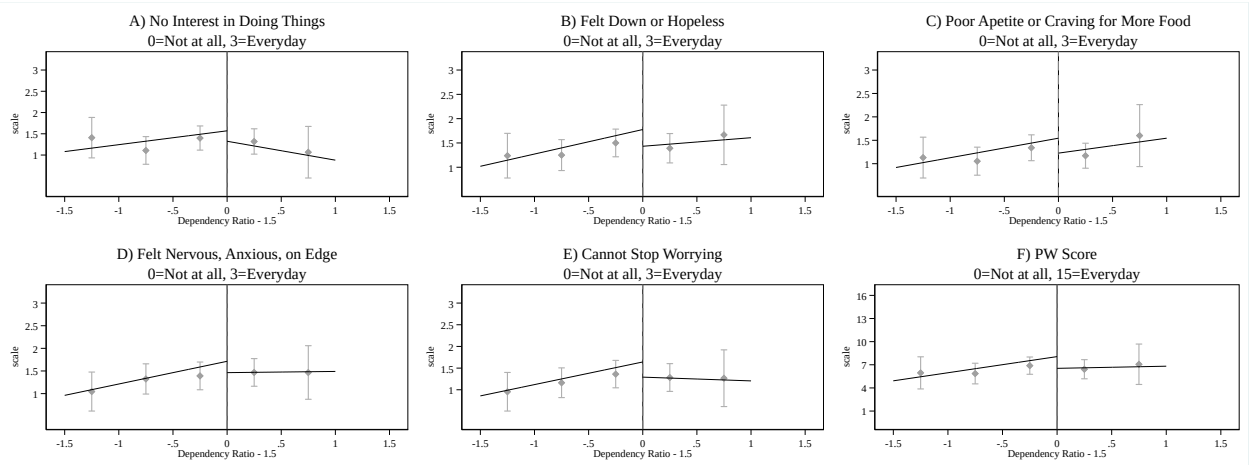
Notes: The figure plots the shares against the running variable (the dependency ratio, centered at the 1.5 cutoff) for the sample that excludes households eligible for the ESSN program via criteria other than the dependency ratio. All figures are constructed using the `rdplot` command of Calonico, Cattaneo, and Titiunik (2015, 2017): to account for the uneven distribution of the running variable, it is partitioned into three equal bins on the left and two equal bins on the right of the cutoff, the mean is plotted for each bin, and bin-specific 90 percent confidence intervals, robust to heteroskedasticity, are shown as error bars. Linear local polynomial regressions are fitted separately on each side of the cutoff using the original individual-level data to depict pre- and post-threshold trends.

Figure A8: RDD Graphs for Language and Technical Skills Outcomes



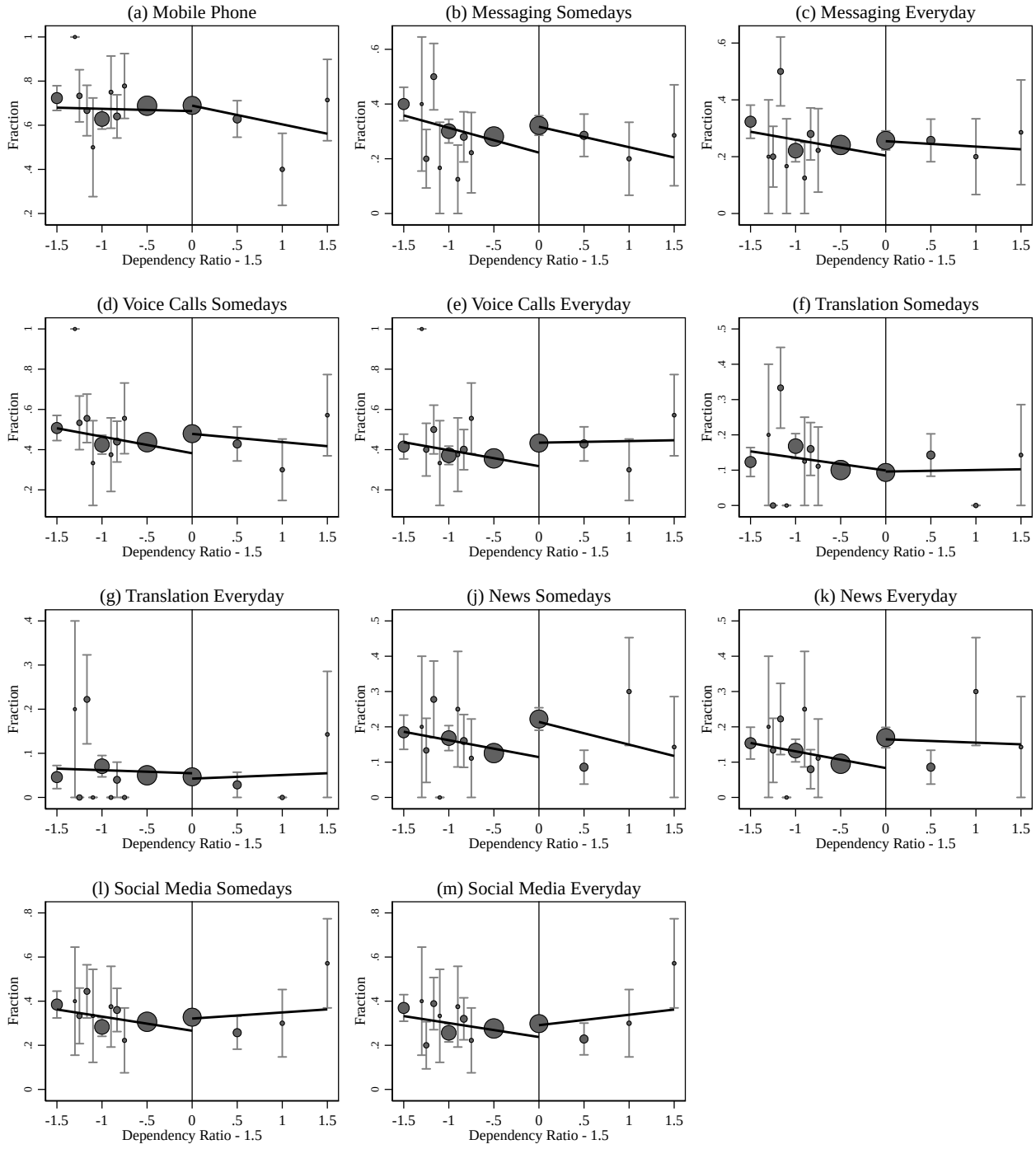
Notes: The figure plots the variable means against the running variable (the dependency ratio, centered at the 1.5 cutoff) for the sample that excludes households eligible for the ESSN program via criteria other than the dependency ratio. All figures are constructed using the `rdplot` command of Calonico, Cattaneo, and Titiunik (2015, 2017): to account for the uneven distribution of the running variable, it is partitioned into three equal bins on the left and two equal bins on the right of the cutoff, the mean is plotted for each bin, and bin-specific 90 percent confidence intervals, robust to heteroskedasticity, are shown as error bars. Linear local polynomial regressions are fitted separately on each side of the cutoff using the original individual-level data to depict pre- and post-threshold trends.

Figure A9: RDD Graphs for Personal Well-being Outcomes



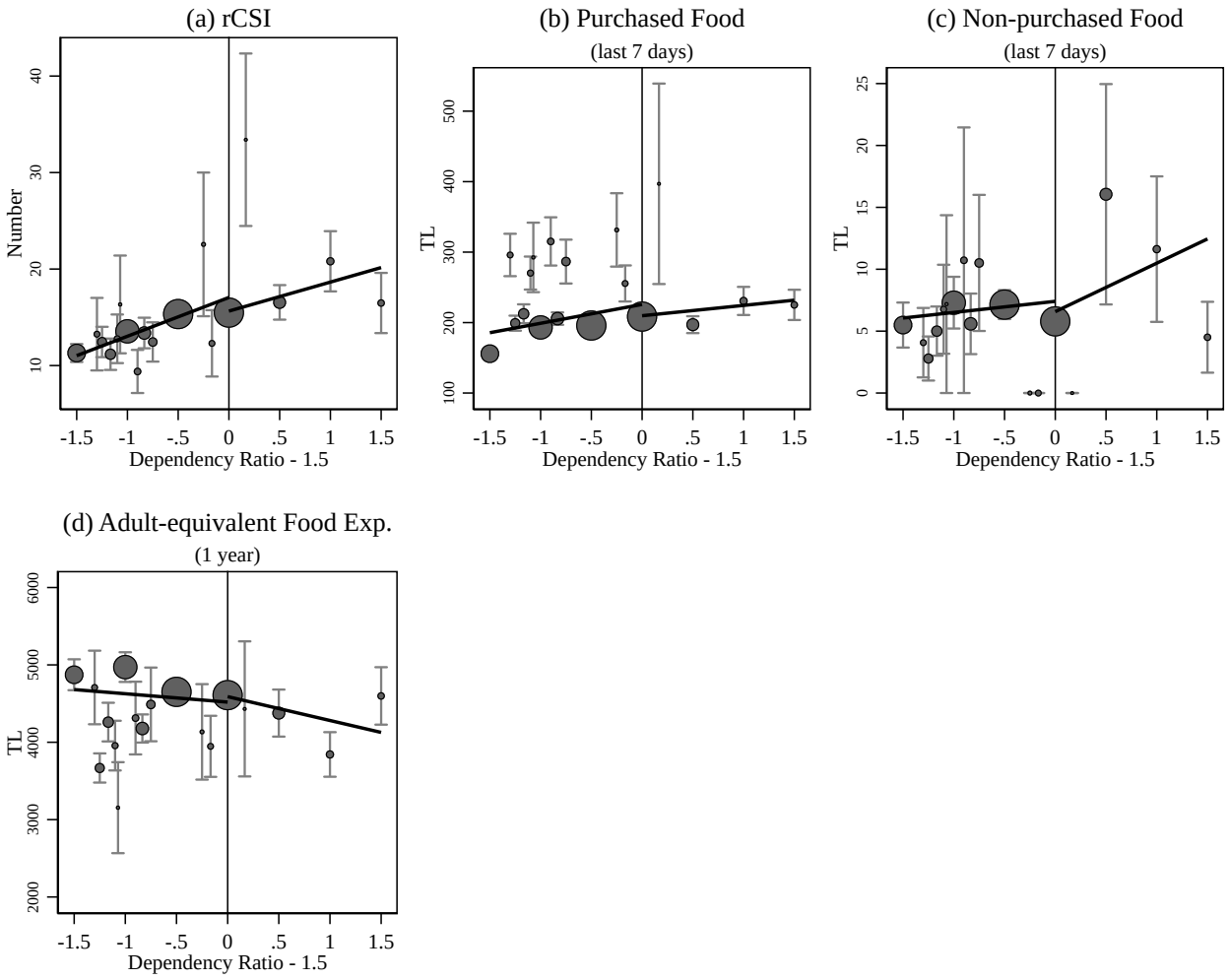
Notes: The figure plots the variable means against the running variable (the dependency ratio, centered at the 1.5 cutoff) for the sample that excludes households eligible for the ESSN program via criteria other than the dependency ratio. All figures are constructed using the `rdplot` command of Calonico, Cattaneo, and Titiunik (2015, 2017): to account for the uneven distribution of the running variable, it is partitioned into three equal bins on the left and two equal bins on the right of the cutoff, the mean is plotted for each bin, and bin-specific 90 percent confidence intervals, robust to heteroskedasticity, are shown as error bars. Linear local polynomial regressions are fitted separately on each side of the cutoff using the original individual-level data to depict pre- and post-threshold trends.

Figure A10: Cell Means for Connectivity Outcomes by Dependency-ratio Value



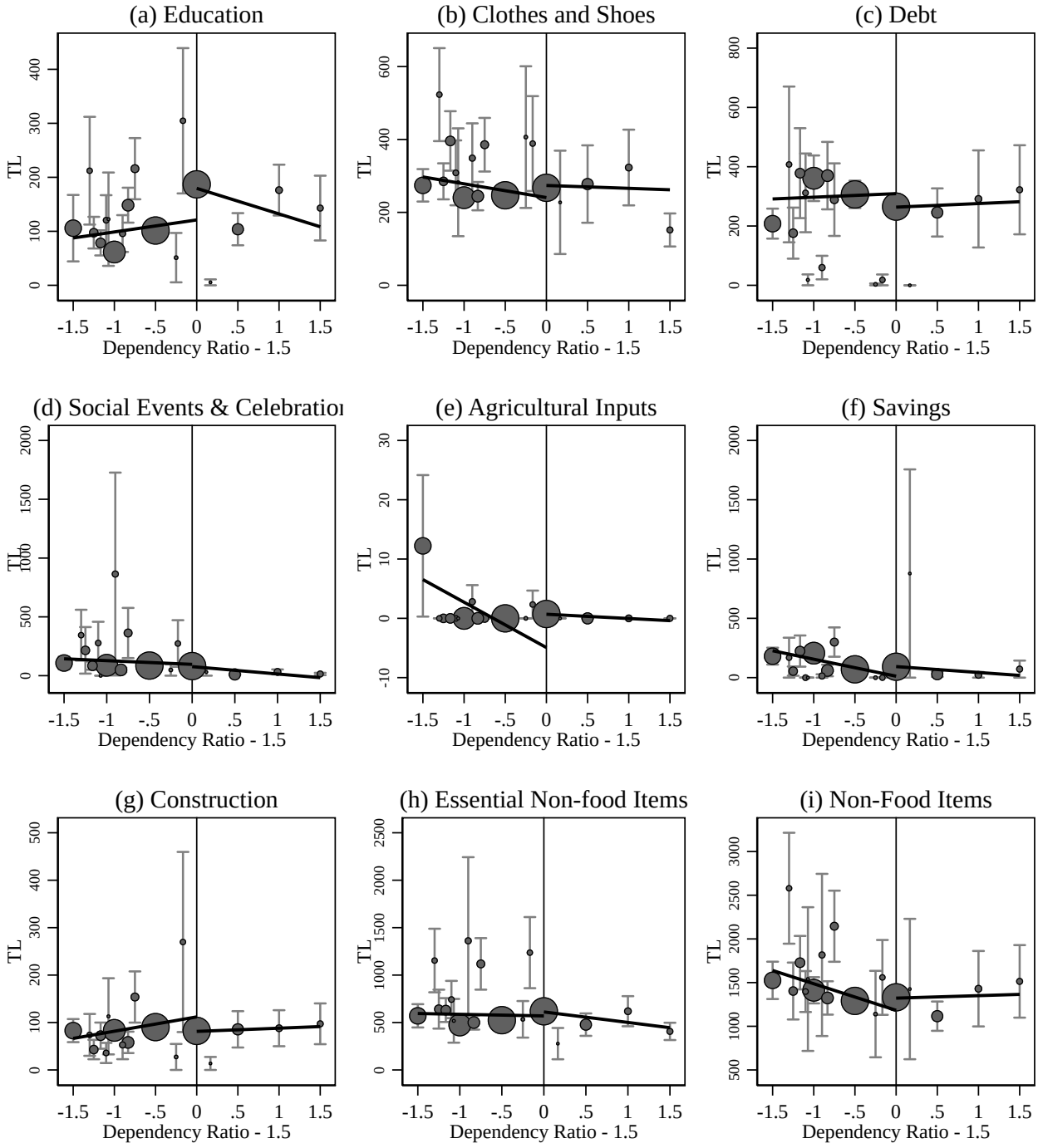
Notes: Each panel plots a connectivity outcome against the running variable (the dependency ratio, centered at the 1.5 cutoff) for the sample that excludes households eligible for the ESSN program via criteria other than the dependency ratio, restricted to dependency ratios in $[0, 3]$. Because the running variable is discrete, each of its distinct values defines a natural cell; each marker is the mean of the outcome in that cell, and marker area is proportional to the number of observations in the cell. Cells containing fewer than five observations are not plotted. Error bars are ± 1 standard error of the cell mean. The solid lines are linear regressions fitted separately on each side of the cutoff using the original individual-level data (descriptive; no controls), drawn out to the cutoff to depict pre- and post-threshold trends.

Figure A11: Cell Means for Food Security and Expenditure Outcomes by Dependency-ratio Value



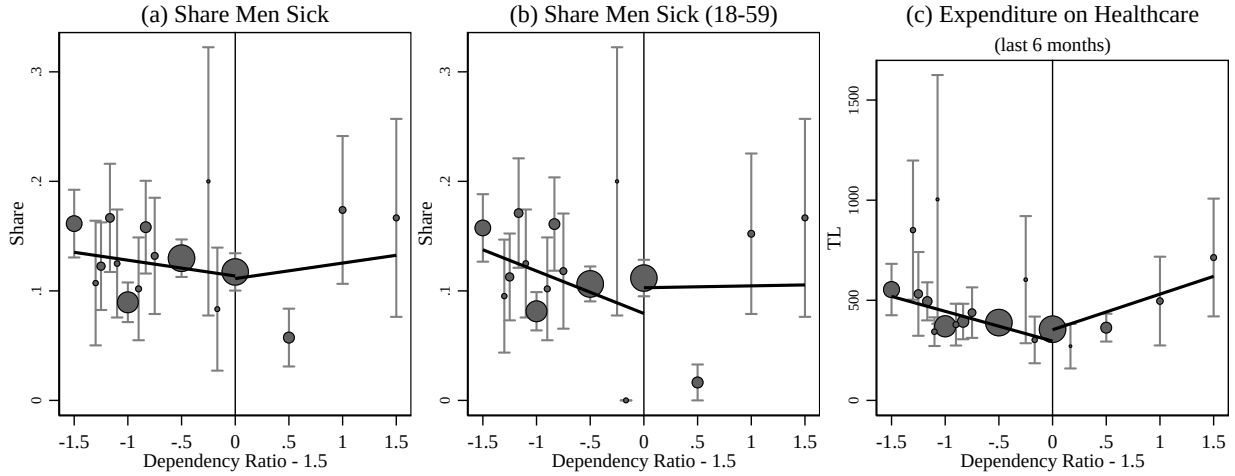
Notes: Each panel plots an outcome against the running variable (the dependency ratio, centered at the 1.5 cutoff) for the sample that excludes households eligible for the ESSN program via criteria other than the dependency ratio, restricted to dependency ratios in $[0, 3]$. Because the running variable is discrete, each of its distinct values defines a natural cell; each marker is the mean of the outcome in that cell, and marker area is proportional to the number of observations in the cell. Cells containing fewer than five observations are not plotted. Error bars are ± 1 standard error of the cell mean. The solid lines are linear regressions fitted separately on each side of the cutoff using the original individual-level data (descriptive; no controls), drawn out to the cutoff to depict pre- and post-threshold trends.

Figure A12: Cell Means for Expenditure Outcomes by Dependency-ratio Value



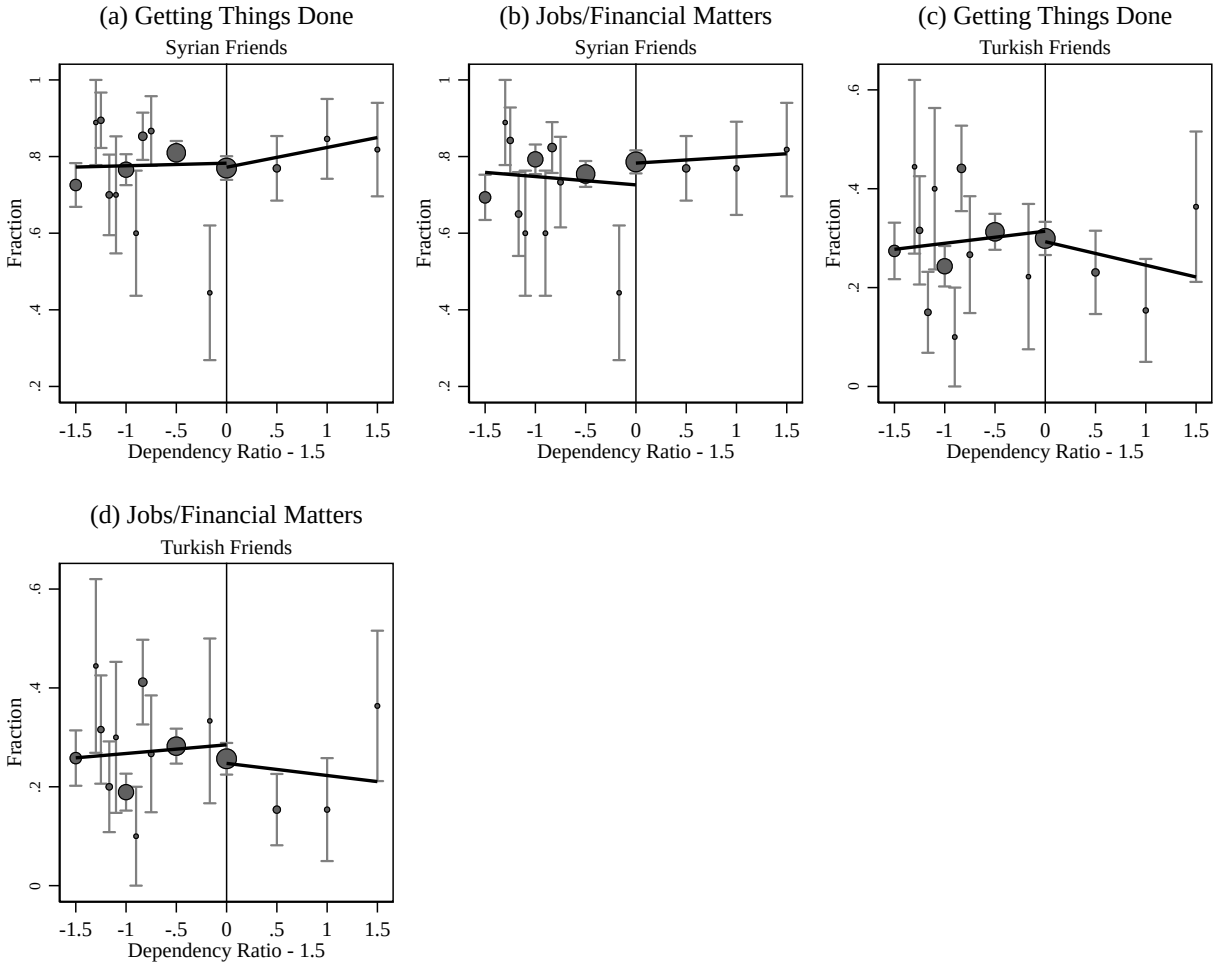
Notes: Each panel plots an outcome against the running variable (the dependency ratio, centered at the 1.5 cutoff) for the sample that excludes households eligible for the ESSN program via criteria other than the dependency ratio, restricted to dependency ratios in $[0, 3]$. Because the running variable is discrete, each of its distinct values defines a natural cell; each marker is the mean of the outcome in that cell, and marker area is proportional to the number of observations in the cell. Cells containing fewer than five observations are not plotted. Error bars are ± 1 standard error of the cell mean. The solid lines are linear regressions fitted separately on each side of the cutoff using the original individual-level data (descriptive; no controls), drawn out to the cutoff to depict pre- and post-threshold trends.

Figure A13: Cell Means for Adult Health and Household Health Expenditure Outcomes by Dependency-ratio Value



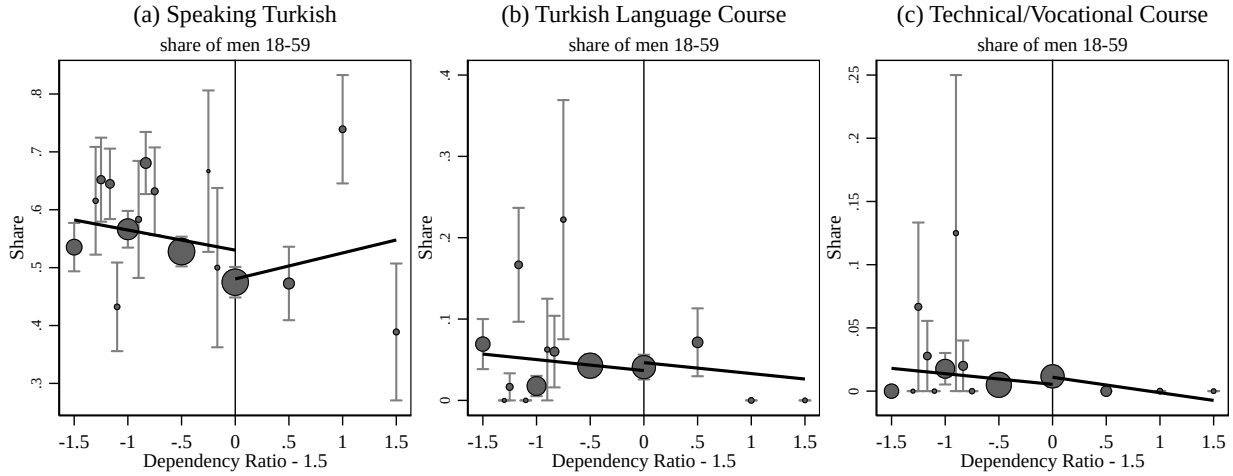
Notes: Each panel plots an outcome against the running variable (the dependency ratio, centered at the 1.5 cutoff) for the sample that excludes households eligible for the ESSN program via criteria other than the dependency ratio, restricted to dependency ratios in $[0, 3]$. Because the running variable is discrete, each of its distinct values defines a natural cell; each marker is the mean of the outcome in that cell, and marker area is proportional to the number of observations in the cell. Cells containing fewer than five observations are not plotted. Error bars are ± 1 standard error of the cell mean. The solid lines are linear regressions fitted separately on each side of the cutoff using the original individual-level data (descriptive; no controls), drawn out to the cutoff to depict pre- and post-threshold trends.

Figure A14: Cell Means for Social Cohesion Outcomes by Dependency-ratio Value



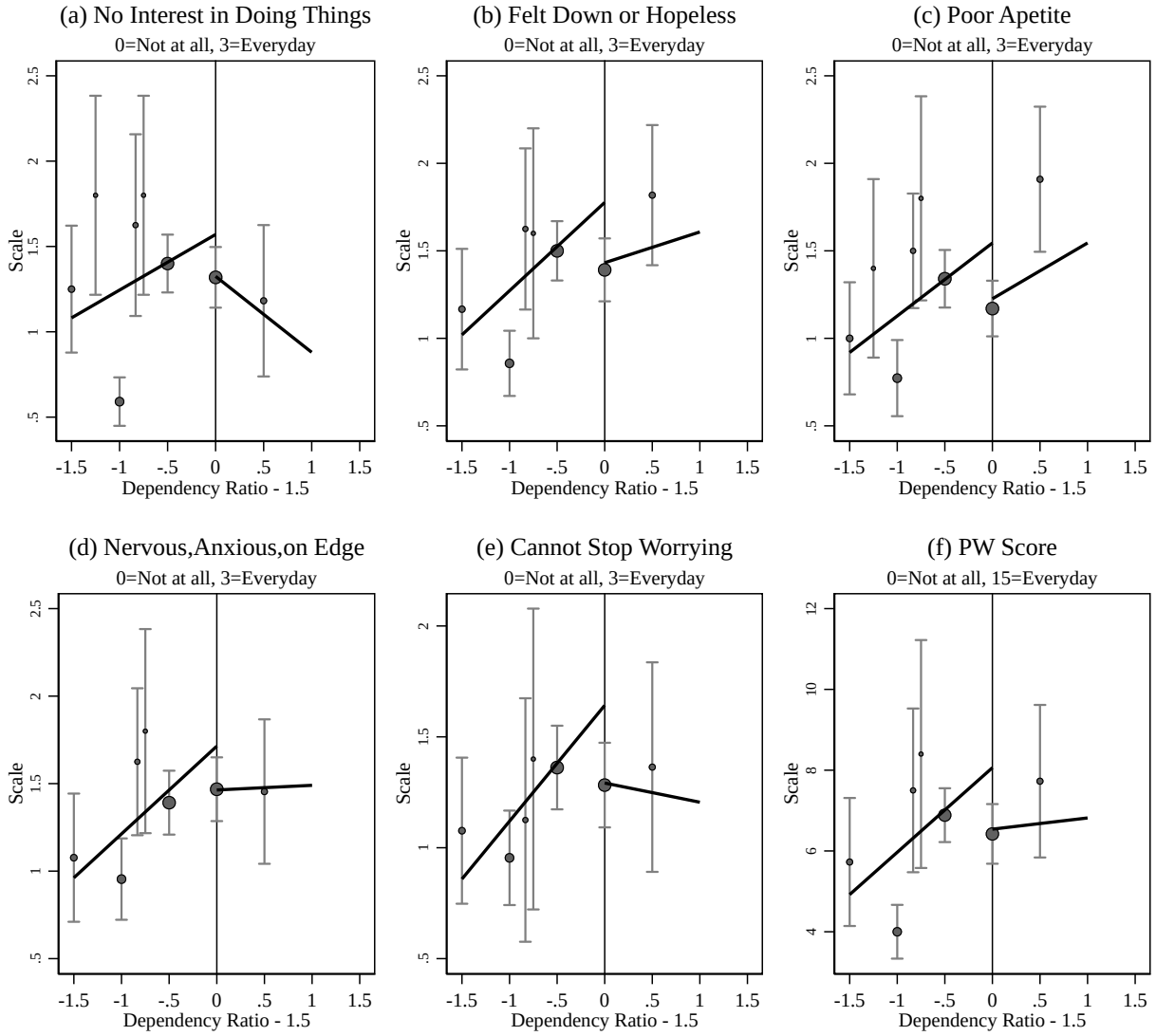
Notes: Each panel plots an outcome against the running variable (the dependency ratio, centered at the 1.5 cutoff) for the sample that excludes households eligible for the ESSN program via criteria other than the dependency ratio, restricted to dependency ratios in $[0, 3]$. Because the running variable is discrete, each of its distinct values defines a natural cell; each marker is the mean of the outcome in that cell, and marker area is proportional to the number of observations in the cell. Cells containing fewer than five observations are not plotted. Error bars are ± 1 standard error of the cell mean. The solid lines are linear regressions fitted separately on each side of the cutoff using the original individual-level data (descriptive; no controls), drawn out to the cutoff to depict pre- and post-threshold trends.

Figure A15: Cell Means for Language and Technical Skills Outcomes by Dependency-ratio Value



Notes: Each panel plots an outcome against the running variable (the dependency ratio, centered at the 1.5 cutoff) for the sample that excludes households eligible for the ESSN program via criteria other than the dependency ratio, restricted to dependency ratios in $[0, 3]$. Because the running variable is discrete, each of its distinct values defines a natural cell; each marker is the mean of the outcome in that cell, and marker area is proportional to the number of observations in the cell. Cells containing fewer than five observations are not plotted. Error bars are ± 1 standard error of the cell mean. The solid lines are linear regressions fitted separately on each side of the cutoff using the original individual-level data (descriptive; no controls), drawn out to the cutoff to depict pre- and post-threshold trends.

Figure A16: Cell Means for Personal Well-being Outcomes by Dependency-ratio Value



Notes: Each panel plots an outcome against the running variable (the dependency ratio, centered at the 1.5 cutoff) for the sample that excludes households eligible for the ESSN program via criteria other than the dependency ratio, restricted to dependency ratios in $[0, 3]$. Because the running variable is discrete, each of its distinct values defines a natural cell; each marker is the mean of the outcome in that cell, and marker area is proportional to the number of observations in the cell. Cells containing fewer than five observations are not plotted. Error bars are ± 1 standard error of the cell mean. The solid lines are linear regressions fitted separately on each side of the cutoff using the original individual-level data (descriptive; no controls), drawn out to the cutoff to depict pre- and post-threshold trends.

Additional Tables

Table A1: Descriptive Statistics

	Mean	S.D.	Min.	Max.	Obs.
Panel A: Adult Employment					
Any Man Work	0.789	0.408	0	1	2,078
Any Woman Work	0.075	0.263	0	1	2,078
Any Adult Work	0.816	0.387	0	1	2,078
Number Man Work	0.910	0.607	0	4	2,078
Number Woman Work	0.083	0.307	0	3	2,078
Number Adult Work	0.993	0.670	0	5	2,078
Share Man Work	0.702	0.409	0	1	2,078
Share Woman Work	0.056	0.211	0	1	2,078
Share Adult Work	0.391	0.232	0	1	2,078
Panel B: Connectivity					
Mobile Phone	0.673	0.469	0	1	704
Messaging Somedays	0.301	0.459	0	1	704
Messaging Everyday	0.251	0.434	0	1	704
Voice Calls Somedays	0.459	0.499	0	1	704
Voice Calls Everyday	0.402	0.491	0	1	704
Translation Somedays	0.119	0.324	0	1	704
Translation Everyday	0.055	0.229	0	1	704
News Somedays	0.170	0.376	0	1	704
News Everyday	0.136	0.343	0	1	704
Social Media Somedays	0.322	0.468	0	1	704
Social Media Everyday	0.294	0.456	0	1	704
Panel C: Food Security and Expenditure					
rCSI	14.406	15.536	0	56	2,078
Expenditure on Purchased Food Items (last 7 days)	205.683	118.598	0	1391.654	2,078
Expenditure on Non-purchased Food Items (last 7 days)	6.835	31.820	0	715.619	2,078
Adult-equivalent Expenditure on Food Items (1 year)	4588.258	2597.695	0	35664.250	2,078
Panel D: Household Consumption (last 6 months)					
Education Expenditure	124.213	360.161	0	7779.368	2,078
Clothes and Shoes Expenditure	273.115	492.710	0	8922.932	2,078
Debt Repayment	288.072	968.607	0	18973.140	2,078
Social Events and Celebration	105.117	904.309	0	22426.820	2,078
Agricultural Inputs Expenditure	1.346	49.396	0	2242.924	2,078
Savings	119.826	843.231	0	17941.450	2,078
Construction Expenditure	84.607	312.624	0	5338.782	2,078
Essential Non-food Items	587.052	1255.461	0	23234.180	2,078
Non-Food Items	1408.507	2325.504	0	33640.220	2,078
Panel E: Adult Health and Health Expenditure					
Share Men Sick	0.122	0.305	0	1	1,422
Share Men Sick (18-59)	0.110	0.295	0	1	1,422
Expenditure on Healthcare (last 6 months)	412.212	963.940	0	15362.860	2,078
Panel F: Social Cohesion					
Syrian Friends – Advice about Getting Things Done	0.779	0.416	0	1	718

Continued on next page

	Mean	S.D.	Min.	Max.	Obs.
Syrian Friends – Advice about Jobs/Financial Matters	0.759	0.428	0	1	718
Turkish Friends – Advice about Getting Things Done	0.290	0.454	0	1	718
Turkish Friends – Advice about Jobs/Financial Matters	0.260	0.439	0	1	718
Panel G: Language and Technical Skills					
Share Men (18-59) Speaking Turkish	0.537	0.474	0	1	1,422
Share Men (18-59) Receiving Turkish Language Course	0.047	0.201	0	1	704
Share Men (18-59) Receiving Technical/Vocational Course	0.011	0.103	0	1	704
Panel H: Personal Well-being (0=Not at all, 3=Everyday)					
No Interest in Doing Things	1.283	1.200	0	3	166
Felt Down or Hopeless	1.396	1.186	0	3	164
Poor Appetite or Craving for More Food	1.225	1.148	0	3	169
Felt Nervous, Anxious, on Edge	1.359	1.223	0	3	167
Cannot Stop Worrying	1.234	1.275	0	3	167
PW-Score (0=Not at all, 15=Everyday)	6.425	4.862	0	15	160

Notes: The data come from the Comprehensive Vulnerability Monitoring Exercise (CVME) Turkey, waves 3–5. The sample is restricted to observations within a bandwidth of 1.5 on either side of the cutoff (i.e., dependency ratios between 0 and 3), and excludes households eligible for the ESSN via criteria other than the dependency ratio. Panel B variables are drawn from CVME4; Panel E from Waves 4 and 5; Panel F from Wave 5; Panel G from Waves 4 and 5; and Panel H from Wave 4. For the expenditure variables in Panels C–E, a CPI deflator is applied to account for price differences over time. The annual expenditure variable in Panel C is censored at the 99th percentile (values above the 99th percentile are set equal to the 99th percentile value).

Table A2: Descriptive Statistics — Control Variables

	Mean	S.D.	Observations
Survey Wave			
Wave 3	0.316	0.465	2,078
Wave 4	0.339	0.473	2,078
Wave 5	0.346	0.476	2,078
Household Head's Gender			
Male	0.873	0.333	2,078
Female	0.127	0.333	2,078
Household Head's Education			
Missing	0.007	0.082	2,078
Illiterate	0.168	0.374	2,078
Literate	0.101	0.301	2,078
Primary School	0.299	0.458	2,078
Middle School or High School	0.351	0.477	2,078
University	0.074	0.262	2,078
Household Head's Occupation Before Arrival			
Management/Highly Skilled	0.055	0.229	2,078
Skilled Worker	0.103	0.304	2,078
Unskilled Worker	0.755	0.430	2,078
Retired	0.006	0.076	2,078
Other	0.051	0.220	2,078
Not Employed	0.030	0.172	2,078
Household Head's Nationality			
Afghan	0.051	0.221	2,078
Iraqi	0.155	0.362	2,078
Syrian	0.782	0.413	2,078
Other	0.012	0.107	2,078
Time since Arrival in Turkey			
Less than 6 Months	0.036	0.187	2,078
6 Months–1 Year	0.049	0.216	2,078
1–3 Years	0.352	0.478	2,078
More than 3 Years	0.552	0.497	2,078
Before the Conflict Started	0.011	0.102	2,078
Region			
Istanbul	0.069	0.254	2,078
West Marmara	0.041	0.199	2,078
Aegean	0.112	0.316	2,078
East Marmara	0.115	0.319	2,078
West Anatolia	0.108	0.310	2,078
Mediterranean	0.157	0.364	2,078
Central Anatolia	0.127	0.333	2,078
West Black Sea	0.071	0.257	2,078
East Black Sea	0.006	0.079	2,078
Northeast Anatolia	0.023	0.149	2,078
Central East Anatolia	0.032	0.175	2,078
Southeast Anatolia	0.139	0.346	2,078

Notes: The data come from the Comprehensive Vulnerability Monitoring Exercise (CVME) Turkey, Waves 3–5. The sample is restricted to observations within a bandwidth of 1.5 on either side of the cutoff (i.e., dependency ratios between 0 and 3), and excludes households eligible for the ESSN via criteria other than the dependency ratio.

Table A3: Fertility Manipulation Test

	(1) RD Effect	(2) Cluster SE	(3) p-value	(4) Wild bootstrap p-value	(5) Mean below cutoff
Any member under age two	−0.124*	[0.073]	0.098	0.332	0.273
Number of members under age two	−0.165*	[0.091]	0.078	0.224	0.306
Any pregnant or lactating member	−0.099**	[0.034]	0.007	0.235	0.211
Observations			2,078		

Notes: The data come from the Comprehensive Vulnerability Monitoring Exercise (CVME) Turkey, waves 3–5. The sample excludes households eligible for the ESSN via criteria other than the dependency ratio and is restricted to dependency ratios between 0 and 3. The CVME records household members in age brackets, the youngest of which is under age two; the pregnancy indicator equals one if any household member is pregnant or lactating at the survey date. Each row reports a separate regression of the indicated variable on the treatment dummy and split linear polynomials on both sides of the cutoff in the running variable (dependency ratio), with no additional controls, mirroring the covariate-balance regressions in Table 2. Strategic fertility aimed at crossing the eligibility threshold would generate a positive discontinuity in the under-two measures. Standard errors, clustered at the level of the dependency ratio, are in brackets; column (4) reports wild-cluster bootstrap p-values. Statistically significant: *** 1 percent level; ** 5 percent level; * 10 percent level.

Table A4: ESSN Status by Side of the Eligibility Cutoff

	$[0, 3]$ window		$[1.25, 1.75]$ window	
	DR < 1.5	DR ≥ 1.5	DR < 1.5	DR ≥ 1.5
Panel A: ESSN status (percent)				
Beneficiary	11.8	68.6	34.5	76.3
Applied, found ineligible	61.3	13.1	44.8	11.6
Non-applicant	25.6	14.4	20.7	8.2
Pending (eligible, not yet paid)	1.3	3.9	0.0	3.8
Observations	1,406	672	29	524
Panel B: Reason for non-application, among non-applicants (percent)				
Registration-related	31.7	78.3	—	—
Believed or told not eligible	55.9	13.4	—	—
Lack of information about the ESSN	10.5	6.2	—	—
Did not need assistance	1.7	2.1	—	—
Other	0.3	0.0	—	—
Observations	360	97	—	—

Notes: Column percentages of household-wave observations in the estimation sample, which excludes households satisfying any of the five non–dependency-ratio criteria; percentages are unweighted. Panel A is based on the survey question on the household's current ESSN status. "Applied, found ineligible" reflects the reported outcome of the household's application, which may include rejections for reasons unrelated to the eligibility criteria, most notably incomplete registration. Panel B is based on the follow-up question asked of non-applicants and is reported for the $[0, 3]$ window; "registration-related" combines not being registered with the DGMM, not being registered with the civil registry (Nüfus), being registered in another province, and waiting for identity documents; "believed or told not eligible" combines the household's own assessment and being told so by the SASF office; "lack of information" combines not knowing about the program and not understanding how to apply.

Table A5: Adult Employment Outcomes — 2SLS Estimates with Alternative Bandwidths

	(1) Any Man Work	(2) Any Woman Work	(3) Any Adult Work	(4) Number Man Work	(5) Number Woman Work	(6) Number Adult Work	(7) Share Man Work	(8) Share Woman Work	(9) Share Adult Work
Panel A: Full Bandwidth (All Dependency-Ratio Values)									
ESSN	0.129***	-0.026	0.098**	0.198	-0.028	0.170	0.173	0.007	0.137***
	[0.040]	[0.047]	[0.043]	[0.210]	[0.060]	[0.263]	[0.109]	[0.027]	[0.052]
Bootstrapped p-value	0.004	0.757	0.090	0.591	0.769	0.651	0.312	0.867	0.071
F statistics	70.02	70.02	70.02	70.02	70.02	70.02	70.02	70.02	70.02
Panel B: BW [0-6]									
ESSN	0.139***	-0.029	0.106**	0.206	-0.031	0.175	0.185*	0.005	0.140***
	[0.037]	[0.046]	[0.042]	[0.207]	[0.058]	[0.259]	[0.104]	[0.026]	[0.052]
Bootstrapped p-value	0.000	0.702	0.041	0.579	0.717	0.643	0.254	0.896	0.056
F statistics	87.26	87.26	87.26	87.26	87.26	87.26	87.26	87.26	87.26
Panel C: BW [0-4.5]									
ESSN	0.137***	-0.031	0.107***	0.202	-0.033	0.169	0.190*	0.004	0.141***
	[0.035]	[0.045]	[0.040]	[0.203]	[0.057]	[0.255]	[0.101]	[0.025]	[0.051]
Bootstrapped p-value	0.000	0.679	0.047	0.581	0.686	0.647	0.235	0.917	0.052
F statistics	106.70	106.70	106.70	106.70	106.70	106.70	106.70	106.70	106.70
Panel D: BW [0-3]									
ESSN	0.132***	-0.039	0.098***	0.192	-0.041	0.152	0.193**	-0.004	0.135***
	[0.032]	[0.042]	[0.037]	[0.198]	[0.054]	[0.247]	[0.097]	[0.021]	[0.049]
Bootstrapped p-value	0.000	0.552	0.105	0.588	0.582	0.662	0.238	0.849	0.079
F statistics	178.40	178.40	178.40	178.40	178.40	178.40	178.40	178.40	178.40

Notes: The data come from the Comprehensive Vulnerability Monitoring Exercise (CVME) Turkey, rounds 3-5. The sample excludes households eligible for the ESSN via criteria other than the dependency ratio and is restricted to the dependency-ratio range indicated for each panel. All regressions include controls for dummies for the household head's sex, education, and occupation before arrival, nationality, time of arrival, and 12 NUTS-1 level regions, survey wave, and split-linear trends of the running variable (dependency ratio) on both sides of the cutoff. The sample has 2,115 observations in Panel A, 2,114 observations in Panel B, 2,104 observations in Panel C, and 2,078 observations in Panel D. Clustering is done at the level of the dependency ratio.

Table A6: Reduced-Form and 2SLS RDD Estimates for Adult Employment Outcomes-Controls for Number of Men, Women & Adults

	(1) Beneficiary	(2) Any Man Work	(3) Any Woman Work	(4) Any Adult Work	(5) Number Man Work	(6) Number Woman Work	(7) Number Adult Work	(8) Share Man Work	(9) Share Woman Work	(10) Share Adult Work
Panel A: Reduced Form (Intention-to-Treat)										
Dependency Ratio Criterion	0.530***	0.072***	-0.008	0.056***	0.110***	-0.001	0.138***	0.102**	-0.002	0.071***
	[0.040]	[0.020]	[0.012]	[0.016]	[0.033]	[0.014]	[0.032]	[0.049]	[0.012]	[0.024]
Bootstrapped p-value	0.019	0.111	0.532	0.121	0.122	0.939	0.093	0.204	0.858	0.121
Panel B: 2SLS (Effect of Being an ESSN Beneficiary)										
ESSN		0.136***	-0.014	0.105***	0.208***	-0.002	0.256***	0.193**	-0.004	0.135***
		[0.037]	[0.022]	[0.027]	[0.062]	[0.025]	[0.057]	[0.097]	[0.021]	[0.049]
Bootstrapped p-value		0.000	0.533	0.000	0.000	0.936	0.000	0.238	0.849	0.079
F statistics		211.00	244.40	243.20	211.00	244.40	243.20	178.40	178.40	178.40
Mean of Dep. Var.		0.789	0.075	0.816	0.910	0.083	0.993	0.702	0.056	0.391

Notes: The data come from the Comprehensive Vulnerability Monitoring Exercise (CVME) Turkey, rounds 3-5. Panel A reports reduced-form (intention-to-treat) estimates of the dependency-ratio criterion; column (1) is the first-stage effect on beneficiary status. Panel B reports 2SLS estimates of the effect of being an ESSN beneficiary, instrumented by the dependency-ratio criterion; the F statistic is the first-stage F. The sample excludes households eligible for the ESSN via criteria other than the dependency ratio and is restricted to dependency ratios between 0 and 3. All regressions include controls for dummies for the household head's sex, education, and occupation before arrival, nationality, time of arrival, and 12 NUTS-1 level regions, survey wave, and split-linear trends of the running variable (dependency ratio) on both sides of the cutoff. The number of men (columns 2 and 5), women (columns 3 and 6), and adults (columns 4 and 7) in the household is also controlled. The sample has 2,078 observations. Clustering is done at the level of the dependency ratio.

Table A7: Adult Employment Outcomes — 2SLS Estimates with No Controls

	(1) Any Man Work	(2) Any Woman Work	(3) Any Adult Work	(4) Number Man Work	(5) Number Woman Work	(6) Number Adult Work	(7) Share Man Work	(8) Share Woman Work	(9) Share Adult Work
ESSN	0.108***	-0.043	0.073**	0.159	-0.046	0.113	0.164	-0.008	0.115**
	[0.032]	[0.041]	[0.036]	[0.195]	[0.053]	[0.243]	[0.110]	[0.019]	[0.057]
Bootstrapped p-value	0.024	0.516	0.188	0.625	0.549	0.718	0.377	0.694	0.233
F statistics	155.60	155.60	155.60	155.60	155.60	155.60	155.60	155.60	155.60

Notes: The data come from the Comprehensive Vulnerability Monitoring Exercise (CVME) Turkey, rounds 3-5. The sample excludes households eligible for the ESSN via criteria other than the dependency ratio and is restricted to dependency ratios between 0 and 3. All regressions include controls for split-linear trends of the running variable (dependency ratio) on both sides of the cutoff. The sample has 2,078 observations. Clustering is done at the level of the dependency ratio.

Table A8: Adult Employment Outcomes — 2SLS Estimates, Syrian Households Only

	(1) Any Man Work	(2) Any Woman Work	(3) Any Adult Work	(4) Number Man Work	(5) Number Woman Work	(6) Number Adult Work	(7) Share Man Work	(8) Share Woman Work	(9) Share Adult Work
ESSN	0.138***	-0.031	0.113***	0.153	-0.031	0.122	0.216*	0.008	0.145***
	[0.024]	[0.053]	[0.025]	[0.191]	[0.066]	[0.250]	[0.116]	[0.030]	[0.051]
Bootstrapped p-value	0.000	0.710	0.000	0.684	0.744	0.774	0.319	0.832	0.087
F statistics	195.10	195.10	195.10	195.10	195.10	195.10	195.10	195.10	195.10

Notes: The data come from the Comprehensive Vulnerability Monitoring Exercise (CVME) Turkey, rounds 3-5. The sample is restricted to households with a Syrian household head, excludes households eligible for the ESSN via criteria other than the dependency ratio, and is restricted to dependency ratios between 0 and 3; it has 1,624 observations and 28 clusters. All regressions include controls for dummies for the household head's sex, education, and occupation before arrival, time of arrival, and 12 NUTS-1 level regions, survey wave, and split-linear trends of the running variable (dependency ratio) on both sides of the cutoff; the nationality dummies drop in this sample. Clustering is done at the level of the dependency ratio.

Table A9: Adult Employment Outcomes — Estimates with Heteroskedasticity-Robust (EHW) Standard Errors

	(1) Beneficiary	(2) Any Man Work	(3) Any Woman Work	(4) Any Adult Work	(5) Number Man Work	(6) Number Woman Work	(7) Number Adult Work	(8) Share Man Work	(9) Share Woman Work	(10) Share Adult Work
Panel A: Reduced Form (Intention-to-Treat)										
Dependency Ratio Criterion	0.530***	0.070**	-0.021	0.052*	0.102**	-0.021	0.080*	0.102***	-0.002	0.071***
Clustered SE	[0.040]	[0.021]	[0.022]	[0.022]	[0.111]	[0.029]	[0.137]	[0.049]	[0.012]	[0.024]
EHW SE	(0.030)	(0.031)	(0.019)	(0.030)	(0.043)	(0.021)	(0.047)	(0.032)	(0.015)	(0.018)
EHW p-value	0.000	0.025	0.284	0.087	0.018	0.312	0.090	0.001	0.883	0.000
Panel B: 2SLS (Effect of Being an ESSN Beneficiary)										
ESSN		0.132**	-0.039	0.098*	0.192**	-0.041	0.152*	0.193***	-0.004	0.135***
Clustered SE		[0.032]	[0.042]	[0.037]	[0.198]	[0.054]	[0.247]	[0.097]	[0.021]	[0.049]
EHW SE		(0.059)	(0.036)	(0.057)	(0.082)	(0.040)	(0.090)	(0.061)	(0.029)	(0.035)
EHW p-value		0.027	0.283	0.091	0.020	0.312	0.093	0.002	0.883	0.000

Notes: The data come from the Comprehensive Vulnerability Monitoring Exercise (CVME) Turkey, rounds 3-5. Coefficients and the sample are identical to Table 4; the table isolates the comparison of variance estimators recommended by Kolesár and Rothe (2018) for regression discontinuity designs with a discrete running variable. Clustered standard errors (in brackets, carried over from Table 4) cluster on the running variable, which takes 32 distinct values in the estimation sample; EHW standard errors (in parentheses) are heteroskedasticity-robust without clustering. Panel A reports reduced-form estimates, with the first-stage effect on beneficiary status in column (1); Panel B reports 2SLS estimates. All regressions include the full baseline control set and split-linear trends of the running variable on both sides of the cutoff; the sample has 2,078 observations. Unlike in Table 4, asterisks in this table follow the EHW p-values: *** 1 percent level; ** 5 percent level; * 10 percent level.

Table A10: Multiple Hypothesis Testing: Connectivity and Expenditure Outcomes

	Bootstrapped p-value	Holm p-value	Anderson q-value	Benjamini–Hochberg q-value
Panel A: Connectivity Outcomes				
Mobile Phone	0.452	1.000	0.607	0.692
Messaging Somedays	0.129	0.903	0.221	0.284
Messaging Everyday	0.301	1.000	0.431	0.552
Voice Calls Somedays	0.028	0.252	0.093	0.104
Voice Calls Everyday	0.064	0.512	0.146	0.175
Translation Somedays	0.882	1.000	1.000	0.959
Translation Everyday	0.504	1.000	0.607	0.692
News Somedays	0.003	0.030	0.018	0.018
News Everyday	0.002	0.022	0.018	0.018
Social Media Somedays	0.860	1.000	1.000	0.959
Social Media Everyday	0.959	1.000	1.000	0.959
Panel B: Food Security and Food Expenditure Outcomes				
rCSI	0.000	0.000	0.000	0.000
Expenditure on Purchased Food Items (7 days)	0.767	0.777	1.000	0.767
Expenditure on Non-purchased Food Items (7 days)	0.259	0.777	0.625	0.513
Adult-equivalent Food Expenditure (1 year)	0.385	0.777	0.625	0.513
Panel C: Non-Food Expenditure Outcomes				
Education	0.022	0.176	0.112	0.101
Clothes and Shoes	0.255	1.000	0.383	0.415
Debt Repayment	0.611	1.000	0.604	0.671
Social Events and Celebration	0.671	1.000	0.604	0.671
Agricultural Inputs	0.277	1.000	0.383	0.415
Savings	0.049	0.343	0.129	0.147
Construction	0.167	1.000	0.335	0.376
Essential Non-food Items	0.571	1.000	0.604	0.671
Non-food Items	0.017	0.153	0.112	0.101

Notes: Each row reports the significance of the 2SLS (fuzzy RD) effect of ESSN beneficiary status on the indicated outcome. Estimates use the main RD sample, comprising households within a dependency-ratio bandwidth of ± 1.5 of the cutoff and excluding households that qualify for ESSN through criteria other than the dependency ratio. Panel A is restricted to wave 4; Panels B and C use waves 3–5. Bootstrapped p-values are wild-cluster-bootstrap Anderson–Rubin p-values, clustered on the running variable, from the main specification. Holm-adjusted p-values control the familywise error rate through the Holm step-down procedure; the adjustment is valid under arbitrary dependence, requires no resampling, and is calculated from the bootstrapped p-values. Anderson q-values are Anderson (2008) sharpened two-stage FDR q-values, and Benjamini–Hochberg q-values follow Benjamini and Hochberg (1995); both are calculated from the bootstrapped p-values. Multiple-testing corrections are conducted separately within each panel. We report the Holm adjustment rather than resampled Romano–Wolf p-values because the studentized cluster-resampling algorithm failed to run for most of these outcome families in Stata; for the families in which it did run, it was more conservative than the Bonferroni bound it is designed to refine, reflecting the heavy tails of the cluster-resampling scheme in this design (see the corresponding discussion for the employment outcomes in Section 6.2).

Table A11: Multiple Hypothesis Testing: Health, Social Cohesion, Language and Technical Skills, and Personal Well-being Outcomes

	Bootstrapped p-value	Holm p-value	Anderson q-value	Benjamini–Hochberg q-value
Panel A: Health Outcomes				
Share Men Sick	0.857	0.857	0.975	0.857
Share Men Sick (18–59)	0.210	0.630	0.975	0.494
Healthcare Expenditure (last 6 months)	0.329	0.658	0.975	0.494
Panel B: Social Cohesion Outcomes				
Syrian Friends – Getting Things Done	0.982	1.000	1.000	0.982
Syrian Friends – Jobs/Financial Matters	0.477	1.000	1.000	0.963
Turkish Friends – Getting Things Done	0.958	1.000	1.000	0.982
Turkish Friends – Jobs/Financial Matters	0.482	1.000	1.000	0.963
Panel C: Language and Technical Skills Outcomes				
Share Men (18–59) Speaking Turkish	0.662	1.000	0.790	0.662
Share Men (18–59) Receiving Turkish Language Course	0.014	0.042	0.045	0.043
Share Men (18–59) Receiving Technical/Vocational Course	0.638	1.000	0.790	0.662
Panel D: Personal Well-being Outcomes				
No Interest in Doing Things	0.000	0.000	0.000	0.000
Felt Down or Hopeless	0.000	0.000	0.000	0.000
Poor Appetite or Craving for More Food	0.000	0.000	0.000	0.000
Felt Nervous, Anxious, or on Edge	0.403	0.403	0.072	0.403
Cannot Stop Worrying	0.000	0.000	0.000	0.000
PW-Score (0=Not at all, 15=Everyday)	0.000	0.000	0.000	0.000

Notes: Each row reports the significance of the 2SLS (fuzzy RD) effect of ESSN beneficiary status on the indicated outcome. Estimates use the main RD sample, comprising households within a dependency-ratio bandwidth of ± 1.5 of the cutoff and excluding households that qualify for ESSN through criteria other than the dependency ratio. Panel A uses waves 4–5, except for healthcare expenditure; Panel B is restricted to wave 5; Panel C uses waves 4–5; and Panel D is restricted to wave 4. Bootstrapped p-values are wild-cluster-bootstrap Anderson–Rubin p-values, clustered on the running variable, from the main specification. Holm-adjusted p-values control the familywise error rate through the Holm step-down procedure; the adjustment is valid under arbitrary dependence, requires no resampling, and is calculated from the bootstrapped p-values. Anderson q-values are Anderson (2008) sharpened two-stage FDR q-values, and Benjamini–Hochberg q-values follow Benjamini and Hochberg (1995); both are calculated from the bootstrapped p-values. Multiple-testing corrections are conducted separately within each panel. We report the Holm adjustment rather than resampled Romano–Wolf p-values because the studentized cluster-resampling algorithm failed to run for most of these outcome families in Stata; for the families in which it did run, it was more conservative than the Bonferroni bound it is designed to refine, reflecting the heavy tails of the cluster-resampling scheme in this design (see the corresponding discussion for the employment outcomes in Section 6.2).

Table A12: Selection into the Personal Well-being Module: Respondents vs. Non-respondents

	Respondents	Non-respondents	Difference	p-value
Treatment and Running Variable				
Dependency ratio	1.026	0.964	+0.062	0.235
Treated (dependency ratio ≥ 1.5)	0.367	0.308	+0.058	0.167
Beneficiary	0.290	0.258	+0.032	0.422
Household Composition				
Household size	4.799	4.559	+0.240	0.118
Number of prime-age adults	2.503	2.424	+0.079	0.401
Number of males aged 18–59	1.320	1.305	+0.015	0.817
Number of females aged 18–59	1.337	1.262	+0.076	0.229
Number of dependents	2.308	2.135	+0.173	0.121
Number of children	2.154	1.993	+0.161	0.144
Number of elderly	0.154	0.138	+0.016	0.703
Household Head Characteristics				
Female	0.107	0.153	-0.047	0.101
Education: Illiterate	0.207	0.172	+0.035	0.320
Education: Literate	0.077	0.067	+0.010	0.679
Education: Primary school	0.231	0.286	-0.055	0.147
Education: Middle or high school	0.414	0.355	+0.059	0.173
Education: University	0.065	0.095	-0.030	0.187
Occupation: Management/highly skilled	0.036	0.077	-0.041**	0.025
Occupation: Skilled worker	0.059	0.079	-0.019	0.372
Occupation: Unskilled worker	0.799	0.744	+0.055	0.131
Occupation: Retired	0.006	0.007	-0.002	0.824
Occupation: Other	0.041	0.050	-0.009	0.617
Occupation: Not employed	0.059	0.043	+0.016	0.424
Nationality				
Afghan	0.012	0.080	-0.069***	0.000
Iraqi	0.065	0.189	-0.124***	0.000
Syrian	0.917	0.716	+0.201***	0.000
Other	0.006	0.015	-0.009	0.254
Time Since Arrival in Turkey				
Less than 6 months	0.012	0.049	-0.037***	0.003
6 months–1 year	0.012	0.050	-0.039***	0.002
1 year–3 years	0.290	0.260	+0.030	0.450
More than 3 years	0.680	0.621	+0.060	0.151
Before the conflict started	0.006	0.021	-0.015*	0.087
Region				
West Marmara	0.130	0.026	+0.104***	0.000
Aegean	0.065	0.179	-0.114***	0.000
East Marmara	0.065	0.065	-0.000	0.988
West Anatolia	0.260	0.032	+0.229***	0.000
Mediterranean	0.254	0.133	+0.122***	0.001
Central Anatolia	0.000	0.144	-0.144***	0.000
West Black Sea	0.000	0.181	-0.181***	0.000
Northeast Anatolia	0.000	0.071	-0.071***	0.000
Central East Anatolia	0.053	0.056	-0.003	0.888
Southeast Anatolia	0.172	0.112	+0.059*	0.066
Joint test, all covariates: $F(33, 670)$		11.83	($p < 0.001$)	
Joint test, excluding regions: $F(24, 679)$		4.11	($p < 0.001$)	

Notes: The sample is Wave 4 of the CVME, restricted to the main estimation sample (dependency ratios between 0 and 3, excluding households eligible for the ESSN via criteria other than the dependency ratio); 169 of 704 households responded to at least one item of the personal well-being module, which was administered in private. The table reports means for respondents and non-respondents, their difference, and the p-value from a two-sample t-test with unequal variances. The joint tests report the heteroskedasticity-robust F statistic from a linear probability model of response on the covariates listed; household size is omitted from the joint tests because it equals the sum of prime-age adults and dependents. Statistically significant: *** 1 percent level; ** 5 percent level; * 10 percent level.

Table A13: Selection into the Personal Well-being Module and the Eligibility Cutoff

Specification	RD effect on response	SE	p-value	N
No controls	0.034	[0.022]	0.138	704
Baseline controls	-0.017	[0.018]	0.343	704
Fielded regions only, no controls	0.010	[0.030]	0.733	492

Notes: The dependent variable is an indicator for responding to at least one item of the personal well-being module. The sample is Wave 4 of the CVME, restricted to the main estimation sample. Each row reports the coefficient on the dependency-ratio criterion from a regression that includes split linear trends of the running variable on both sides of the cutoff; the second row adds the baseline controls (household head's sex, education, occupation before arrival, nationality, time of arrival, and NUTS-1 regions). The third row restricts the sample to the seven regions in which the module was fielded. Standard errors clustered at the level of the dependency ratio in brackets. Raw response rates are 0.273 above and 0.224 below the cutoff. Statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix B: Results for the Full Sample

This appendix reports results for the full sample, which adds to the main estimation sample the households that are eligible for the ESSN through criteria other than the dependency ratio. Table B1 reports descriptive statistics for the outcome variables in this sample, and Table B2 reports the corresponding reduced-form and 2SLS estimates for the adult employment outcomes, mirroring Table 4 in the main text. Tables B3 and B4 repeat the mechanism estimates of Tables 6 and 7 on the full sample. Because the first stage for the personal well-being module falls into the weak-instrument range in this sample, Table B5 additionally reports the reduced-form estimates for those outcomes.

Table B1: Descriptive Statistics — Full Sample

	Mean	S.D.	Min.	Max.	Obs.
Panel A: Adult Employment					
Any Man Work	0.759	0.428	0	1	3,664
Any Woman Work	0.081	0.273	0	1	3,664
Any Adult Work	0.791	0.407	0	1	3,664
Number Man Work	0.893	0.646	0	5	3,664
Number Woman Work	0.091	0.327	0	3	3,664
Number Adult Work	0.984	0.714	0	6	3,664
Share Man Work	0.667	0.422	0	1	3,664
Share Woman Work	0.062	0.223	0	1	3,664
Share Adult Work	0.372	0.241	0	1	3,664
Panel B: Connectivity					
Mobile Phone	0.682	0.466	0	1	1,253
Messaging Somedays	0.296	0.457	0	1	1,253
Messaging Everyday	0.243	0.429	0	1	1,253
Voice Calls Somedays	0.486	0.500	0	1	1,253
Voice Calls Everyday	0.411	0.492	0	1	1,253
Translation Somedays	0.121	0.326	0	1	1,253
Translation Everyday	0.053	0.223	0	1	1,253
News Somedays	0.167	0.373	0	1	1,253
News Everyday	0.131	0.337	0	1	1,253
Social Media Somedays	0.326	0.469	0	1	1,253
Social Media Everyday	0.294	0.456	0	1	1,253
Panel C: Food Security and Expenditure					
rCSI	14.523	15.618	0	56	3,664
Expenditure on Purchased Food Items (last 7 days)	228.383	136.630	0	1,391.654	3,664
Expenditure on Non-purchased Food Items (last 7 days)	7.766	36.228	0	820.973	3,664
Adult-equivalent Expenditure on Food Items (1 year)	4,467.315	2,447.794	0	35,664.25	3,664
Panel D: Household Consumption (last 6 months)					
Education Expenditure	196.490	450.268	0	7,779.368	3,664
Clothes and Shoes Expenditure	320.123	533.616	0	8,922.932	3,664
Debt Repayment	305.662	972.961	0	18,973.14	3,664
Social Events and Celebration	93.308	787.263	0	22,426.82	3,664
Agricultural Inputs Expenditure	1.269	38.859	0	2,242.924	3,664
Savings	118.373	1,086.741	0	44,287.51	3,664

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	Mean	S.D.	Min.	Max.	Obs.
Construction Expenditure	82.030	300.853	0	5,511.29	3,664
Essential Non-food Items	691.951	1,225.103	0	23,234.18	3,664
Non-Food Items	1,577.797	2,519.518	0	47,073.93	3,664
Panel E: Adult Health and Health Expenditure					
Share Men Sick	0.128	0.310	0	1	2,504
Share Men Sick (18-59)	0.117	0.302	0	1	2,504
Expenditure on Healthcare (last 6 months)	460.542	1,172.827	0	27,600.42	3,664
Panel F: Social Cohesion					
Syrian Friends – Advice about Getting Things Done	0.793	0.405	0	1	1,251
Syrian Friends – Advice about Jobs/Financial Matters	0.772	0.420	0	1	1,251
Turkish Friends – Advice about Getting Things Done	0.308	0.462	0	1	1,251
Turkish Friends – Advice about Jobs/Financial Matters	0.271	0.445	0	1	1,251
Panel G: Language and Technical Skills					
Share Men (18-59) Speaking Turkish	0.524	0.471	0	1	2,504
Share Men (18-59) Receiving Turkish Language Course	0.056	0.217	0	1	1,253
Share Men (18-59) Receiving Technical/Vocational Course	0.011	0.099	0	1	1,253
Panel H: Personal Well-being (0=Not at all, 3=Everyday)					
No Interest in Doing Things	1.275	1.166	0	3	306
Felt Down or Hopeless	1.482	1.179	0	3	303
Poor Appetite or Craving for More Food	1.168	1.141	0	3	309
Felt Nervous, Anxious, on Edge	1.355	1.213	0	3	307
Cannot Stop Worrying	1.307	1.269	0	3	306
PW-Score (0=Not at all, 15=Everyday)	6.497	4.780	0	15	294

Notes: The data come from the Comprehensive Vulnerability Monitoring Exercise (CVME) Turkey, waves 3–5. Unlike the main estimation sample, the sample retains households eligible for the ESSN via criteria other than the dependency ratio; it is restricted to observations within a bandwidth of 1.5 on either side of the cutoff (i.e., dependency ratios between 0 and 3). Panel B variables are drawn from CVME4; Panel E from Waves 4 and 5; Panel F from Wave 5; Panel G from Waves 4 and 5; and Panel H from Wave 4. For the expenditure variables in Panels C–E, a CPI deflator is applied to account for price differences over time. The annual expenditure variable in Panel C is censored at the 99th percentile.

Table B2: Reduced-Form and 2SLS RDD Estimates for Adult Employment Outcomes — Full Sample

	(1) Beneficiary	(2) Any Man Work	(3) Any Woman Work	(4) Any Adult Work	(5) Number Man Work	(6) Number Woman Work	(7) Number Adult Work	(8) Share Man Work	(9) Share Woman Work	(10) Share Adult Work
Panel A: Reduced Form (Intention-to-Treat)										
Dependency Ratio Criterion	0.377***	0.016	-0.026	0.006	-0.061	-0.037	-0.097	0.077**	-0.008	0.052**
	[0.078]	[0.041]	[0.022]	[0.037]	[0.142]	[0.032]	[0.165]	[0.033]	[0.013]	[0.022]
Bootstrapped p-value	0.017	0.852	0.366	0.934	0.794	0.403	0.716	0.156	0.600	0.162
Panel B: 2SLS (Effect of Being an ESSN Beneficiary)										
ESSN		0.042	-0.068	0.016	-0.161	-0.097	-0.258	0.203**	-0.022	0.139**
		[0.100]	[0.067]	[0.094]	[0.393]	[0.098]	[0.470]	[0.096]	[0.036]	[0.063]
Bootstrapped p-value		0.831	0.500	0.928	0.830	0.514	0.805	0.112	0.548	0.115
F statistics		23.50	23.50	23.50	23.50	23.50	23.50	23.50	23.50	23.50
Mean of Dep. Var.		0.759	0.081	0.791	0.893	0.091	0.984	0.667	0.062	0.372

Notes: The data come from the Comprehensive Vulnerability Monitoring Exercise (CVME) Turkey, rounds 3-5. Panel A reports reduced-form (intention-to-treat) estimates of the dependency-ratio criterion; column (1) is the first-stage effect on beneficiary status. Panel B reports 2SLS estimates of the effect of being an ESSN beneficiary, instrumented by the dependency-ratio criterion; the F statistic is the first-stage F. Unlike the main estimation sample, the sample retains households eligible for the ESSN via criteria other than the dependency ratio; it is restricted to dependency ratios between 0 and 3. All regressions include a dummy for ESSN eligibility via any of the five criteria other than the dependency ratio, controls for dummies for the household head's sex, education, and occupation before arrival, nationality, time of arrival, and 12 NUTS-1 level regions, survey wave, and split-linear trends of the running variable (dependency ratio) on both sides of the cutoff. Clustering is done at the level of the dependency ratio.

Table B3: The Effect of ESSN Cash Transfers on Connectivity, Food Security, and Non-Food Expenditure — Full Sample

Outcome	ESSN coefficient	SE	Bootstrapped p-value	First-stage F	Mean of dep. var.	Obs.
Panel A: Connectivity (CVME Wave 4)						
Owns mobile phone	-0.140*	[0.072]	0.074	30.17	0.682	1,253
Messaging	0.046	[0.082]	0.673	30.17	0.296	1,253
Messaging every day	0.002	[0.070]	0.977	30.17	0.243	1,253
Voice calls	0.067	[0.076]	0.404	30.17	0.486	1,253
Voice calls every day	0.117*	[0.070]	0.291	30.17	0.411	1,253
Translation	-0.055	[0.081]	0.601	30.17	0.121	1,253
Translation every day	-0.022	[0.042]	0.619	30.17	0.053	1,253
News	0.098	[0.081]	0.488	30.17	0.167	1,253
News every day	0.065	[0.086]	0.652	30.17	0.131	1,253
Social media	-0.048	[0.070]	0.549	30.17	0.326	1,253
Social media every day	-0.031	[0.069]	0.706	30.17	0.294	1,253
Panel B: Food Security and Food Expenditure (CVME Waves 3–5)						
rCSI	-2.166**	[1.089]	0.128	23.50	14.523	3,664
Purchased food expenditure, past 7 days (TL)	-54.175	[75.013]	0.660	23.50	228.383	3,664
Non-purchased food expenditure, past 7 days (TL)	-5.567	[6.727]	0.453	23.50	7.766	3,664
Adult-equivalent food expenditure, past year (TL)	802.005*	[466.991]	0.078	23.50	4,467.315	3,664
Panel C: Non-Food Expenditure, last 6 Months, TL (CVME Waves 3–5)						
Education	64.554	[91.541]	0.628	23.50	196.490	3,664
Clothes and shoes	-6.587	[132.675]	0.973	23.50	320.123	3,664
Debt repayment	45.718	[134.270]	0.775	23.50	305.662	3,664
Social events and celebrations	-148.617	[170.858]	0.554	23.50	93.308	3,664
Agricultural inputs	9.319	[5.758]	0.463	23.50	1.269	3,664
Savings	271.669	[182.219]	0.138	23.50	118.373	3,664
Construction	-111.847	[81.672]	0.313	23.50	82.030	3,664
Essential non-food items	-202.497	[419.408]	0.743	23.50	691.951	3,664
Non-food items	64.958	[501.875]	0.917	23.50	1,577.797	3,664

Notes: The data come from the CVME Turkey; Panel A uses Wave 4 and Panels B and C use Waves 3–5. Unlike the main estimation sample, the sample retains households eligible for the ESSN via criteria other than the dependency ratio; it is restricted to dependency ratios between 0 and 3. Each row reports a separate 2SLS regression of the outcome on beneficiary status, instrumented by the dependency-ratio criterion; the columns report the coefficient, its standard error (clustered at the level of the dependency ratio), the wild-cluster bootstrap p-value, the first-stage F statistic, the mean of the dependent variable, and the number of observations. All regressions include a dummy for ESSN eligibility via any of the five criteria other than the dependency ratio, controls for dummies for the household head's sex, education, and occupation before arrival, nationality, time of arrival, and 12 NUTS-1 level regions, survey wave, and split-linear trends of the running variable (dependency ratio) on both sides of the cutoff. For the phone-use outcomes in Panel A, unmarked rows refer to use on some days in the past 7 days and "every day" rows to daily use. Annual food expenditure in Panel B is expressed in adult-equivalent terms because the running variable is defined by household composition, which makes per-capita normalizations mechanically sensitive to the dependency ratio. In Panel C, essential non-food items are the sum of education, clothing, social events, and construction; non-food items additionally include health, debt, agricultural inputs, and savings.

Table B4: The Effect of ESSN Cash Transfers on Adult Health, Social Cohesion, Language and Technical Skills, and Personal Well-being — Full Sample

Outcome	ESSN coefficient	SE	Bootstrapped p-value	First-stage F	Mean of dep. var.	Obs.
Panel A: Adult Health and Healthcare Expenditure (CVME Waves 4–5)						
Share of men sick	0.013	[0.043]	0.797	24.70	0.128	2,504
Share of men aged 18–59 sick	0.057*	[0.035]	0.121	24.70	0.117	2,504
Healthcare expenditure, past 6 months (TL)	-59.251	[191.968]	0.796	23.50	460.542	3,664
Panel B: Social Cohesion (CVME Wave 5)						
Advice from Syrian friends: getting things done	-0.152	[0.127]	0.365	16.59	0.793	1,251
Advice from Syrian friends: jobs/financial matters	-0.050	[0.119]	0.739	16.59	0.772	1,251
Advice from Turkish friends: getting things done	-0.008	[0.070]	0.909	16.59	0.308	1,251
Advice from Turkish friends: jobs/financial matters	-0.058	[0.068]	0.314	16.59	0.271	1,251
Panel C: Language and Technical Skills (CVME Waves 4–5)						
Share of adult men speaking Turkish	-0.069	[0.106]	0.673	24.70	0.524	2,504
Share of men aged 18–59 in language training	0.055	[0.035]	0.073	30.17	0.056	1,253
Share of men aged 18–59 in technical training	0.017	[0.023]	0.655	30.17	0.011	1,253
Panel D: Personal Well-being (CVME Wave 4; 0 = not at all, 3 = every day)						
No interest in doing things	-1.901*	[1.044]	0.000	3.85	1.275	306
Felt down or hopeless	-2.022	[1.241]	0.000	4.34	1.482	303
Poor appetite or craving for more food	-2.656	[1.539]	0.000	4.25	1.168	309
Felt nervous, anxious, on edge	-2.129	[1.687]	0.101	4.99	1.355	307
Cannot stop worrying	-3.155**	[1.412]	0.000	4.79	1.307	306
PW-Score (0–15)	-12.236	[8.035]	0.000	3.31	6.497	294

Notes: The data come from the CVME Turkey; Panel A uses Waves 4–5 except healthcare expenditure (Waves 3–5), Panel B uses Wave 5, Panel C uses Waves 4–5 for the Turkish-speaking outcome and Wave 4 for the course outcomes, and Panel D uses Wave 4. Panel D outcomes are measured on a 0–3 frequency scale (0 = not at all, 3 = every day), with the PW-Score summing the five items onto a 0–15 scale; these outcomes were collected in private for a subsample of households (see Appendix Tables A12 and A13). Unlike the main estimation sample, the sample retains households eligible for the ESSN via criteria other than the dependency ratio; it is restricted to dependency ratios between 0 and 3. Each row reports a separate 2SLS regression of the outcome on beneficiary status, instrumented by the dependency-ratio criterion; the columns report the coefficient, its standard error (clustered at the level of the dependency ratio), the wild-cluster bootstrap p-value, the first-stage F statistic, the mean of the dependent variable, and the number of observations. All regressions include a dummy for ESSN eligibility via any of the five criteria other than the dependency ratio, controls for dummies for the household head's sex, education, and occupation before arrival, nationality, time of arrival, and 12 NUTS-1 level regions, survey wave, and split-linear trends of the running variable (dependency ratio) on both sides of the cutoff. The first-stage F statistics in Panel D fall into the weak-instrument range; we therefore do not draw inferences from the Panel D 2SLS point estimates. The bootstrapped p-values in this table are computed from the Anderson–Rubin statistic and remain valid under weak instruments; Table B5 reports the corresponding reduced-form estimates for the Panel D outcomes.

Table B5: Reduced-Form Estimates for the Personal Well-being Outcomes — Full Sample

Outcome	Dependency-ratio criterion	SE	Bootstrapped p-value	Mean of dep. var.	Obs.
No interest in doing things	-0.430***	[0.135]	0.024	1.275	306
Felt down or hopeless	-0.471***	[0.129]	0.015	1.482	303
Poor appetite or craving for more food	-0.616***	[0.156]	0.011	1.168	309
Felt nervous, anxious, on edge	-0.523**	[0.228]	0.064	1.355	307
Cannot stop worrying	-0.771***	[0.142]	0.012	1.307	306
PW-Score (0–15)	-2.778***	[0.631]	0.006	6.497	294

Notes: The data come from the CVME Turkey, Wave 4. The sample is that of Panel D of Table B4: unlike the main estimation sample, it retains households eligible for the ESSN via criteria other than the dependency ratio, and it is restricted to dependency ratios between 0 and 3. Each row reports a separate reduced-form (intention-to-treat) regression of the outcome on the dependency-ratio criterion; the columns report the coefficient, its standard error (clustered at the level of the dependency ratio), the wild-cluster bootstrap p-value (9,999 replications), the mean of the dependent variable, and the number of observations. Outcomes are measured on a 0–3 frequency scale (0 = not at all, 3 = every day); the PW-Score sums the five items onto a 0–15 scale. All regressions include a dummy for ESSN eligibility via any of the five criteria other than the dependency ratio, controls for dummies for the household head's sex, education, and occupation before arrival, nationality, time of arrival, and 12 NUTS-1 level regions, and split-linear trends of the running variable (dependency ratio) on both sides of the cutoff. We report these estimates because the first stage for this module falls into the weak-instrument range in the full sample (F between 3.3 and 5.0 in Panel D of Table B4), which makes the 2SLS estimates unreliable, whereas the reduced form remains valid regardless of first-stage strength.

Appendix C: Local-Randomization Estimates

This appendix collects the supplementary exhibits for the local-randomization design of Section 6.3: the within-window covariate-balance table underlying the window selection, the estimates for the single-wave modules in the preferred window, the alternative-window estimates, and the composition-adjusted estimates. The main-text exhibits for this design are Tables 9 and 10.

A note on the scope of the covariate-balance table is in order. Because the running variable is a ratio of household composition, the counts that enter it differ across the window boundary by construction: in the preferred window, the control side is dominated by households with two adults and two dependents, whereas nearly all treated households have two adults and three dependents, so treated households contain roughly one more dependent than control households by the definition of the design. Testing balance on the number of dependents, children, or prime-age adults within the window would therefore amount to testing balance on the score itself, and these variables are excluded from Table C1; their balance in the parametric design, where the split polynomials absorb the composition gradient, is assessed in Table 2. For the same reason, the fertility manipulation test of Section 5.3.3 is conducted in the parametric framework only: within the window, the presence of a very young child cannot be separated from the marginal dependent that defines treatment itself. The balance exhibit reports diagnostics for three candidate windows, including the narrowest candidate ($[-0.167, 0]$, the mass point at a dependency ratio of $4/3$), which we exclude from estimation for the weak first stage discussed in Section 5.2; estimates are reported for $[-0.5, 0]$ and $[-0.5, 0.5]$.

Table C1: Covariate Balance Test — Discrete Regression Discontinuity

	[−0.167, 0.000]		(Main) [−0.500, 0.000]		[−0.500, +0.500]	
Covariate	Diff.	p	Diff.	p	Diff.	p
Household Head						
Female household head	+0.003	[1.000]	−0.009	[0.696]	−0.004	[0.924]
Household Head's Education						
University	−0.129	[0.282]	+0.001	[1.000]	+0.026	[0.274]
Technical	+0.078	[0.396]	−0.025	[0.224]	−0.029	[0.112]
Secondary	+0.009	[1.000]	+0.055*	[0.068]	+0.035	[0.226]
Primary	−0.028	[1.000]	−0.026	[0.412]	−0.028	[0.312]
Intermediate	+0.062	[0.580]	−0.006	[0.746]	−0.007	[0.706]
Household Head's Occupation (origin country)						
Management / highly skilled	+0.000	[1.000]	+0.005	[0.840]	+0.002	[0.998]
Skilled worker	+0.059	[0.698]	+0.015	[0.504]	+0.012	[0.612]
Unskilled worker	−0.014	[1.000]	+0.023	[0.434]	+0.022	[0.432]
Retirement salary	+0.000	[1.000]	−0.004	[0.552]	−0.004	[0.434]
Other	+0.025	[1.000]	−0.035***	[0.008]	−0.026*	[0.056]
Nationality of Household Head						
Afghan	−0.015	[1.000]	−0.033**	[0.028]	−0.027*	[0.072]
Iraqi	−0.072	[0.464]	−0.006	[0.872]	+0.001	[1.000]
Syrian	+0.077	[0.490]	+0.045*	[0.074]	+0.033	[0.192]
Time of First Arrival						
Less than 6 months ago	+0.021	[1.000]	−0.015	[0.230]	−0.001	[1.000]
6 months – 1 year ago	−0.048	[0.574]	+0.005	[0.802]	+0.011	[0.458]
1 – 3 years	+0.059	[0.788]	−0.007	[0.852]	−0.027	[0.368]
3 – 6 years	+0.009	[1.000]	+0.023	[0.506]	+0.021	[0.484]
Before the conflict in Syria	−0.042	[0.328]	−0.005	[0.626]	−0.005	[0.634]
Region of Residence (NUTS-1)						
Region 2	−0.001	[1.000]	+0.008	[0.608]	+0.012	[0.422]
Region 3	−0.101	[0.276]	−0.020	[0.380]	−0.024	[0.196]
Region 4	+0.033	[0.986]	−0.012	[0.584]	−0.005	[0.836]
Region 5	+0.126	[0.176]	+0.040*	[0.050]	+0.035*	[0.086]
Region 6	−0.137	[0.202]	−0.018	[0.458]	−0.012	[0.640]
Region 7	−0.018	[1.000]	−0.002	[1.000]	−0.011	[0.662]
Region 8	+0.012	[1.000]	−0.013	[0.448]	−0.013	[0.464]
Region 9	+0.004	[1.000]	−0.005	[0.516]	−0.006	[0.372]
Region 10	+0.017	[1.000]	−0.010	[0.360]	−0.008	[0.484]
Region 11	+0.037	[0.932]	+0.008	[0.602]	+0.008	[0.558]
Region 12	+0.007	[1.000]	+0.024	[0.314]	+0.024	[0.264]
Observations (control / treated)	20	515	546	515	546	610

Notes: Each row reports the difference in means between treated (dependency ratio ≥ 1.5) and control households within each local-randomization window, obtained using the `rdrandinf` command following Cattaneo et al. (2020). Windows are defined at mass points of the running variable (dependency ratio relative to the cutoff). The window $[-0.500, 0.000]$ is the preferred specification; $[-0.167, 0.000]$ is the narrowest candidate and $[-0.500, +0.500]$ the widest. The cutoff mass is assigned to the treated side. Components of the running variable (numbers of dependents and prime-age adults and their constituents) are excluded, as they are mechanically discontinuous at the cutoff. p-values in brackets are Fisherian randomization-based p-values (1,000 replications, seed = 50). Statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C2: Local-Randomization RD Estimates Around the Eligibility Cutoff: Single-Wave Modules

Outcome	ITT	Fisherian p-value	95% randomization confidence interval	First stage	Implied LATE
Panel A: Connectivity (CVME Wave 4)					
Owns mobile phone	0.002	1.000	[-0.093, 0.098]	0.640	0.004
Messaging	0.039	0.424	[-0.054, 0.132]	0.640	0.060
Messaging every day	0.013	0.807	[-0.074, 0.102]	0.640	0.021
Voice calls	0.045	0.406	[-0.055, 0.146]	0.640	0.071
Voice calls every day	0.077	0.143	[-0.022, 0.178]	0.640	0.120
Translation	-0.009	0.857	[-0.071, 0.053]	0.640	-0.014
Translation every day	-0.007	0.831	[-0.051, 0.037]	0.640	-0.011
News	0.091**	0.030	[0.011, 0.167]	0.640	0.141
News every day	0.067*	0.066	[0.000, 0.137]	0.640	0.105
Social media	0.020	0.731	[-0.073, 0.115]	0.640	0.032
Social media every day	0.020	0.726	[-0.071, 0.113]	0.640	0.032
Panel B: Social Cohesion (CVME Wave 5)					
Syrian friends for things	-0.012	0.804	[-0.098, 0.075]	0.622	-0.019
Syrian friends for jobs	0.056	0.217	[-0.033, 0.145]	0.622	0.090
Turkish friends for things	-0.005	1.000	[-0.102, 0.091]	0.622	-0.008
Turkish friends for jobs	-0.025	0.624	[-0.115, 0.066]	0.622	-0.040
Panel C: Language and Technical Skills (CVME Waves 4–5)					
Share of adult men speaking Turkish	-0.054	0.135	[-0.124, 0.016]	0.637	-0.085
Share of men aged 18–59 in language training	-0.003	0.897	[-0.043, 0.037]	0.640	-0.005
Share of men aged 18–59 in technical training	0.007	0.596	[-0.011, 0.024]	0.640	0.011
Panel D: Personal Well-being (CVME Wave 4)					
Little interest or pleasure in doing things	-0.081	0.785	[-0.572, 0.415]	0.549	-0.147
Feeling down, depressed, or hopeless	-0.109	0.717	[-0.600, 0.381]	0.565	-0.192
Poor appetite or overeating	-0.170	0.513	[-0.632, 0.300]	0.553	-0.308
Feeling nervous, anxious, or on edge	0.077	0.807	[-0.444, 0.609]	0.551	0.139
Not able to stop or control worrying	-0.079	0.812	[-0.619, 0.450]	0.567	-0.139
Well-being index	-0.464	0.640	[-2.421, 1.522]	0.576	-0.806

Notes: The table reports local-randomization regression discontinuity estimates comparing households just above and just below the eligibility cutoff, where the running variable is the dependency ratio centered at the cutoff. Estimates are obtained using the `rdrandinf` command following Cattaneo et al. (2020). The estimation window is $[-0.5, 0]$ in the centered running variable (dependency ratios between 1 and 1.5), the narrowest window with a strong first stage that passes the within-window covariate-balance check. The ITT columns report reduced-form effects of crossing the eligibility threshold; inference is Fisherian: p-values and 95 percent confidence intervals come from the randomization distribution of the difference in means, the latter by inverting the randomization test under a constant treatment effect, with 5,000 replications. The first-stage column reports the effect of crossing the threshold on beneficiary status in each outcome's estimation sample, and the implied-LATE column scales the ITT by that first stage; the implied LATEs are descriptive, and we attach no separate inferential statements to them. p-values below 0.001 are reported as 0. Statistical significance (ITT): * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C3: Local-Randomization RD Estimates, Alternative Window: Employment and Multi-Wave Mechanism Outcomes

Outcome	ITT	Fisherian p-value	95% randomization confidence interval	First stage	Implied LATE
Panel A: Employment (CVME Waves 3–5)					
Any man works	0.029	0.259	[-0.018, 0.078]	0.574	0.051
Any woman works	-0.005	0.727	[-0.034, 0.024]	0.574	-0.009
Any adult works	0.023	0.324	[-0.021, 0.068]	0.574	0.040
Number men work	-0.007	0.856	[-0.062, 0.048]	0.574	-0.012
Number women work	-0.009	0.579	[-0.039, 0.021]	0.574	-0.016
Number adults work	-0.016	0.639	[-0.074, 0.043]	0.574	-0.027
Share men work	0.063**	0.010	[0.015, 0.110]	0.574	0.110
Share women work	0.004	0.735	[-0.019, 0.028]	0.574	0.007
Share adults work	0.035***	0.009	[0.009, 0.060]	0.574	0.060
Panel B: Food Security and Food Expenditure (CVME Waves 3–5)					
rCSI	0.431	0.653	[-1.441, 2.286]	0.574	0.751
Food purchases, past 7 days	9.1	0.179	[-4.0, 22.2]	0.574	15.8
Food non-purchases, past 7 days	0.5	0.823	[-3.3, 4.2]	0.574	0.8
Food consumption, past year, PAE	-41.9	0.775	[-334.0, 245.8]	0.574	-73.1
Panel C: Non-Food Expenditure, last 6 Months, TL (CVME Waves 3–5)					
Education	64.7***	0	[32.1, 97.7]	0.574	112.7
Clothing	16.9	0.566	[-40.6, 74.4]	0.574	29.4
Debt repayment	-25.7	0.615	[-126.3, 75.1]	0.574	-44.9
Social expenditures	-21.7	0.684	[-108.4, 64.7]	0.574	-37.8
Agriculture	0.53*	0.080	[-0.04, 1.10]	0.574	0.93
Savings	22.5	0.596	[-53.0, 98.2]	0.574	39.2
Construction	-13.9	0.484	[-51.6, 23.6]	0.574	-24.3
Non-food expenditures, past 6 months (robust)	45.9	0.492	[-77.9, 170.5]	0.574	80.0
Non-food expenditures, past 6 months	18.0	0.881	[-210.4, 245.6]	0.574	31.3
Panel D: Adult Health and Healthcare Expenditure (CVME Waves 4–5)					
Share of men sick	-0.021	0.358	[-0.064, 0.022]	0.576	-0.036
Share of men aged 18–59 sick	-0.006	0.766	[-0.048, 0.035]	0.576	-0.011
Health expenditures	-31.5	0.628	[-156.9, 92.9]	0.576	-54.7

Notes: The table reports local-randomization regression discontinuity estimates comparing households just above and just below the eligibility cutoff, where the running variable is the dependency ratio centered at the cutoff. Estimates are obtained using the `rdrandinf` command following Cattaneo et al. (2020). The window takes the interval $[-0.5, 0.5]$ in the centered running variable (dependency ratios between 1 and 2), widening the preferred $[-0.5, 0]$ window of Tables 9 and 10 to include the mass points above the cutoff. The ITT columns report reduced-form effects of crossing the eligibility threshold; inference is Fisherian: p-values and 95 percent confidence intervals come from the randomization distribution of the difference in means, the latter by inverting the randomization test under a constant treatment effect, with 5,000 replications. The first-stage column reports the effect of crossing the threshold on beneficiary status in each outcome's estimation sample, and the implied-LATE column scales the ITT by that first stage; the implied LATEs are descriptive, and we attach no separate inferential statements to them. p-values below 0.001 are reported as 0. Statistical significance (ITT): * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C4: Local-Randomization RD Estimates, Alternative Window: Single-Wave Modules

Outcome	ITT	Fisherian p-value	95% randomization confidence interval	First stage	Implied LATE
Panel A: Connectivity (CVME Wave 4)					
Owns mobile phone	-0.005	0.913	[-0.097, 0.087]	0.580	-0.009
Messaging	0.030	0.529	[-0.059, 0.118]	0.580	0.051
Messaging every day	0.011	0.824	[-0.076, 0.098]	0.580	0.019
Voice calls	0.042	0.432	[-0.058, 0.141]	0.580	0.072
Voice calls every day	0.081	0.112	[-0.012, 0.177]	0.580	0.140
Translation	-0.001	1.000	[-0.060, 0.058]	0.580	-0.003
Translation every day	-0.010	0.666	[-0.052, 0.030]	0.580	-0.018
News	0.070*	0.064	[-0.000, 0.141]	0.580	0.121
News every day	0.056	0.111	[-0.010, 0.122]	0.580	0.097
Social media	0.015	0.755	[-0.076, 0.105]	0.580	0.026
Social media every day	0.015	0.748	[-0.072, 0.103]	0.580	0.026
Panel B: Social Cohesion (CVME Wave 5)					
Syrian friends for things	-0.008	0.905	[-0.092, 0.075]	0.561	-0.015
Syrian friends for jobs	0.053	0.245	[-0.032, 0.140]	0.561	0.094
Turkish friends for things	-0.013	0.830	[-0.106, 0.080]	0.561	-0.023
Turkish friends for jobs	-0.036	0.486	[-0.124, 0.051]	0.561	-0.065
Panel C: Language and Technical Skills (CVME Waves 4–5)					
Share of adult men speaking Turkish	-0.050	0.153	[-0.119, 0.019]	0.576	-0.088
Share of men aged 18–59 in language training	0.004	0.903	[-0.035, 0.043]	0.580	0.007
Share of men aged 18–59 in technical training	0.005	1.000	[-0.010, 0.020]	0.580	0.008
Panel D: Personal Well-being (CVME Wave 4)					
Little interest or pleasure in doing things	-0.107	0.681	[-0.573, 0.369]	0.532	-0.201
Feeling down, depressed, or hopeless	-0.026	0.934	[-0.500, 0.440]	0.545	-0.048
Poor appetite or overeating	-0.030	0.927	[-0.480, 0.417]	0.536	-0.056
Feeling nervous, anxious, or on edge	0.074	0.817	[-0.435, 0.577]	0.534	0.139
Not able to stop or control worrying	-0.063	0.817	[-0.576, 0.456]	0.546	-0.116
Well-being index	-0.208	0.834	[-2.143, 1.731]	0.552	-0.376

Notes: The table reports local-randomization regression discontinuity estimates comparing households just above and just below the eligibility cutoff, where the running variable is the dependency ratio centered at the cutoff. Estimates are obtained using the `rdrandinf` command following Cattaneo et al. (2020). The window takes the interval $[-0.5, 0.5]$ in the centered running variable (dependency ratios between 1 and 2), widening the preferred $[-0.5, 0]$ window of Table C2 to include the mass points above the cutoff. The ITT columns report reduced-form effects of crossing the eligibility threshold; inference is Fisherian: p-values and 95 percent confidence intervals come from the randomization distribution of the difference in means, the latter by inverting the randomization test under a constant treatment effect, with 5,000 replications. The first-stage column reports the effect of crossing the threshold on beneficiary status in each outcome's estimation sample, and the implied-LATE column scales the ITT by that first stage; the implied LATEs are descriptive, and we attach no separate inferential statements to them. p-values below 0.001 are reported as 0. Statistical significance (ITT): * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C5: Composition-Adjusted Local-Randomization Estimates, Preferred Window: Employment and Multi-Wave Mechanism Outcomes

Outcome	ITT	Fisherian p-value	95% randomization confidence interval	First stage	Implied LATE
Panel A: Employment (CVME Waves 3–5)					
Any man works	0.040*	0.080	[-0.005, 0.085]	0.656	0.061
Any woman works	0.010	0.495	[-0.018, 0.037]	0.640	0.015
Any adult works	0.024	0.285	[-0.020, 0.070]	0.680	0.036
Number men work	0.044*	0.084	[-0.006, 0.095]	0.656	0.068
Number women work	0.008	0.580	[-0.021, 0.037]	0.640	0.013
Number adults work	0.043	0.137	[-0.013, 0.099]	0.680	0.063
Share men work†	0.109***	0	[0.062, 0.156]	0.630	0.174
Share women work†	-0.002	0.908	[-0.025, 0.022]	0.630	-0.002
Share adults work†	0.053***	0	[0.028, 0.078]	0.630	0.084
Panel B: Food Security and Food Expenditure (CVME Waves 3–5)					
rCSI	-0.260	0.792	[-2.197, 1.677]	0.680	-0.383
Food purchases, past 7 days	21.2***	0.002	[8.1, 34.2]	0.680	31.2
Food non-purchases, past 7 days	0.6	0.717	[-2.4, 3.5]	0.680	0.8
Food consumption, past year, PAE	-113.3	0.447	[-401.2, 176.1]	0.680	-166.7
Panel C: Non-Food Expenditure, last 6 Months, TL (CVME Waves 3–5)					
Education	93.4***	0	[58.9, 127.5]	0.680	137.3
Clothing	43.6*	0.095	[-7.4, 94.6]	0.680	64.1
Debt repayment	-35.0	0.512	[-139.9, 70.3]	0.680	-51.5
Social expenditures	27.7	0.595	[-66.1, 121.0]	0.680	40.7
Agriculture	0.731**	0.011	[0.113, 1.348]	0.680	1.075
Savings	33.4	0.476	[-50.3, 117.0]	0.680	49.1
Construction	1.2	0.953	[-38.3, 40.5]	0.680	1.8
Non-food expenditures, past 6 months (robust)	165.8**	0.011	[35.2, 296.5]	0.680	244.0
Non-food expenditures, past 6 months	170.4	0.179	[-79.3, 418.8]	0.680	250.7
Panel D: Adult Health and Healthcare Expenditure (CVME Waves 4–5)					
Share of men sick	-0.003	0.890	[-0.050, 0.044]	0.705	-0.004
Share of men aged 18–59 sick	0.001	0.945	[-0.043, 0.046]	0.705	0.002
Health expenditures	15.0	0.833	[-112.8, 142.8]	0.705	21.3

Notes: The table reports composition-adjusted local-randomization estimates in the preferred window $[-0.5, 0]$ (dependency ratios between 1 and 1.5). Because the running variable is a ratio of household composition, the window straddles composition cells by construction; following the local-randomization framework of Cattaneo et al. (2020), the estimates adjust for the channel through which the score operates within the window. Regressions include dummies for the relevant household counts: the number of men aged 18–59 (men employment outcomes), the number of women aged 18–59 (women employment outcomes), or the number of prime-age adults (adult employment outcomes and all mechanism outcomes, which are household-level module measurements); a score-polynomial adjustment is infeasible because the treated side of the window is a single mass point. Outcomes marked † (shares) are not composition-adjusted, mirroring the parametric count-controlled specification. Inference re-estimates the adjusted regression within each of 5,000 fixed-margins permutation draws; 95 percent confidence intervals invert the randomization test under a constant treatment effect. The first-stage column applies the same adjustment to beneficiary status, and the implied-LATE column scales the ITT by that first stage; implied LATEs are descriptive. p-values below 0.001 are reported as 0. Statistical significance (ITT): * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C6: Composition-Adjusted Local-Randomization Estimates, Preferred Window: Single-Wave Modules

Outcome	ITT	Fisherian p-value	95% randomization confidence interval	First stage	Implied LATE
Panel A: Connectivity (CVME Wave 4)					
Owns mobile phone	-0.045	0.360	[-0.141, 0.050]	0.715	-0.062
Messaging	0.002	0.968	[-0.092, 0.096]	0.715	0.003
Messaging every day	-0.022	0.637	[-0.110, 0.068]	0.715	-0.030
Voice calls	0.010	0.849	[-0.093, 0.112]	0.715	0.014
Voice calls every day	0.038	0.445	[-0.061, 0.137]	0.715	0.053
Translation	-0.022	0.479	[-0.084, 0.040]	0.715	-0.031
Translation every day	-0.008	0.700	[-0.053, 0.038]	0.715	-0.011
News	0.079**	0.049	[0.001, 0.158]	0.715	0.111
News every day	0.068*	0.054	[-0.001, 0.137]	0.715	0.094
Social media	-0.013	0.799	[-0.108, 0.082]	0.715	-0.018
Social media every day	-0.001	0.979	[-0.094, 0.092]	0.715	-0.002
Panel B: Social Cohesion (CVME Wave 5)					
Syrian friends for things	-0.008	0.852	[-0.094, 0.078]	0.687	-0.012
Syrian friends for jobs	0.070	0.123	[-0.019, 0.158]	0.687	0.102
Turkish friends for things	-0.003	0.977	[-0.098, 0.090]	0.687	-0.005
Turkish friends for jobs	-0.015	0.759	[-0.106, 0.078]	0.687	-0.021
Panel C: Language and Technical Skills (CVME Waves 4–5)					
Share of adult men speaking Turkish	-0.086**	0.015	[-0.158, -0.016]	0.705	-0.122
Share of men aged 18–59 in language training	0.000	1.000	[-0.041, 0.042]	0.715	0.000
Share of men aged 18–59 in technical training	0.005	0.600	[-0.013, 0.023]	0.715	0.007
Panel D: Personal Well-being (CVME Wave 4)					
Little interest or pleasure in doing things	-0.036	0.891	[-0.522, 0.466]	0.606	-0.059
Feeling down, depressed, or hopeless	-0.093	0.704	[-0.591, 0.401]	0.620	-0.149
Poor appetite or overeating	0.014	0.944	[-0.424, 0.455]	0.607	0.023
Feeling nervous, anxious, or on edge	0.210	0.420	[-0.301, 0.732]	0.606	0.347
Not able to stop or control worrying	-0.030	0.919	[-0.565, 0.505]	0.621	-0.048
Well-being index	-0.111	0.915	[-2.054, 1.822]	0.633	-0.175

Notes: The table reports composition-adjusted local-randomization estimates in the preferred window $[-0.5, 0]$ (dependency ratios between 1 and 1.5). Because the running variable is a ratio of household composition, the window straddles composition cells by construction; following the local-randomization framework of Cattaneo et al. (2020), the estimates adjust for the channel through which the score operates within the window. Regressions include dummies for the number of prime-age adults: the mechanism outcomes in this table, including the men-specific ones, are household-level module measurements and receive the adult-count adjustment; a score-polynomial adjustment is infeasible because the treated side of the window is a single mass point. Inference re-estimates the adjusted regression within each of 5,000 fixed-margins permutation draws; 95 percent confidence intervals invert the randomization test under a constant treatment effect. The first-stage column applies the same adjustment to beneficiary status, and the implied-LATE column scales the ITT by that first stage; implied LATEs are descriptive. p-values below 0.001 are reported as 0. Statistical significance (ITT): * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C7: Multiple-Hypothesis Testing for the Local-Randomization Estimates, Preferred Window

Outcome	Fisherian p-value	Holm p-value	Anderson q-value	Benjamini–Hochberg q-value
Panel A: Any member works				
Any man works	0.012	0.036	0.037	0.036
Any woman works	0.359	0.359	0.136	0.359
Any adult works	0.031	0.062	0.037	0.046
Panel B: Number of members working				
Number of men working	0.415	0.830	1.000	0.622
Number of women working	0.272	0.816	1.000	0.622
Number of adults working	0.849	0.849	1.000	0.849
Panel C: Share of members working				
Share of men working	0	0	0	0
Share of women working	0.900	0.900	0.429	0.900
Share of adults working	0	0	0	0

Notes: The table applies multiple-testing corrections to the Fisherian randomization p-values of Table 9 (preferred window, 5,000 permutations), with the family structure mirroring Table 11: adjustments are conducted separately for the three outcomes in each panel. Holm-adjusted p-values control the familywise error rate exactly under arbitrary dependence and require no additional resampling, so the correction inherits the finite-sample validity of the underlying randomization tests; a Westfall–Young step-down computed within the same permutations could only tighten these values. Anderson q-values are Anderson (2008) sharpened two-stage FDR q-values, and Benjamini–Hochberg q-values follow Benjamini and Hochberg (1995); both are calculated from the Fisherian p-values. p-values below 0.001 are reported as 0.