Unconditional cash-based assistance to the poor: What do at-scale programs achieve?

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Abstract

We study the effects of large, temporary income changes on a wide range of economic wellbeing indicators among Syrian refugees in Lebanon. Using a regression discontinuity design, we show that an unconditional cash transfer program (USD 2,100) and a food voucher program (USD 1,620) generate immediate, positive effects on consumption, child well-being, food security, and livelihood coping strategies. We find no evidence that any program effects persist even at six months after transfers end. Cash savings and the stock of durable goods increase while receiving assistance, but households liquidate and spend these assets during or soon after the beneficiary period.

Keywords: unconditional cash transfers, cash-based interventions, poverty, food vouchers, refugees, forced displacement, children, Lebanon.

JEL Classification: I38, I32, O12, D74

Disclaimer: The study was pre-specified with the Open Science Foundation on 18 December 2019, prior to data receipt. The pre-analysis plan included hypotheses, variable definitions, empirical specifications, and protocols for data cleaning and use, and can be found here. Details on our compliance with this plan can be found in the Appendix. As of October 2021, this paper has been superseded by "The short-lived effects of unconditional cash transfers to refugees," and will remain a working paper in perpetuity. The views expressed in this document are those of the authors and do not represent those of institutions involved or with which the authors are or may be associated.

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1 Introduction

Credit constraints are the cornerstone of poverty traps, precluding the poor from investing in productive assets and human capital despite high returns (Ravallion, 2012). The inability to meet daily basic needs and food insecurity contributes to a vicious cycle of poverty by diminishing adults' labor productivity and delaying children's mental and physical development (Dasgupta and Ray, 1986; Cunha and Heckman, 2007). Severe liquidity constraints, moreover, reduce households' ability to develop goal-oriented strategies or make long-term economic decisions to escape poverty (Mani *et al.*, 2013; Haushofer and Fehr, 2014).

More than 100 countries currently rely on unconditional cash transfer (UCT) programs to alleviate the self-perpetuating dynamics of monetary poverty (Bastagli *et al.*, 2016; Baird *et al.*, 2019). Existing literature suggests that these interventions increase consumption, reduce food insecurity, improve psychological well-being, increase educational investment, and lower child labor, teen fertility, and early marriage in the short run (Baird *et al.*, 2011, 2013; Blattman *et al.*, 2014; Hidrobo *et al.*, 2014; Haushofer and Shapiro, 2016; Kilburn *et al.*, 2018; Eyal and Burns, 2019; Angeles *et al.*, 2019; Pellerano *et al.*, 2020).

Whether such improvements sustain after transfers end is not clear. Studies that follow subjects up to five years post-intervention generally show evidence of persistent effects on income, consumption, asset stocks, and other measures of well-being (Araujo *et al.*, 2017; de Mel *et al.*, 2012; Haushofer and Shapiro, 2018; Baird *et al.*, 2019; Blattman *et al.*, 2019). Longer-term follow-ups provide a more mixed picture in which outcomes of recipients and non-recipients eventually converge depending on the context in which transfers are received. The horizon on which program effects persist depends on prevailing economic factors such as initial capital endowments and frictions in financial and labor markets. In the standard case of the S-shaped poverty trap, these factors determine the implicit asset threshold above which families leave the steady state of poverty to converge to a higher level of welfare (Blattman *et al.*, 2020; Balboni *et al.*, 2020) – implying that when constraints are sufficiently severe, the effects of even large asset transfers might be short-lived.

In this paper, we estimate the during- and after-program effects of two of at-scale cash-based interventions to poor refugee families living in non-camp settings throughout Lebanon. Using a threshold-based assignment rule that generates a discontinuity in program eligibility receipt across otherwise-comparable households, we show that a UCT program that distributes USD 2,100 per household over the course of a year yields immediate positive effects on expenditure, child well-being, and livelihoods. A cash-based value voucher for food purchases – additionally distributing USD 1,620 per year to the median-sized household of five – increases food expenditure, improves food security, and reduces livelihood coping strategies. The intent-to-treat and local average treatment effect sizes are economically large, indicating sizeable improvements in economic well-being.

When measuring the same set of outcomes six months after programs end, however, families who previously received either of the yearlong assistance packages appear no different than otherwise similar non-beneficiary families. This result is due to recipients' rapid reversion to prior levels of consumption and well-being after the programs end. Investigating the possible mechanisms that mute the immediate economic gains from the programs so quickly, we first show evidence against myopic behavior. Beneficiaries exhibit no increase in the consumption of "temptation goods" (Evans and Popova, 2017), and they remove children from work and re-enroll them in school. The potential for persistent effects is evidenced by the fact that recipient households build savings in the form of cash and durable goods while receiving assistance. These are liquidated during or soon after the beneficiary period, however, quickly precluding the programs from having persistent effects via savings. Although we cannot directly observe why, savings are likely spent to cope with negative income shocks and provide basic needs (Karlan *et al.*, 2019). Given the severe financial and labor market constraints that refugees face, our findings are consistent with an environment in which large asset transfers do not yield sustained improvements in economic well-being. This implies that the asset threshold for a transition out of poverty is too high, in some contexts, for recipients to do anything but revert to counterfactual levels of welfare soon after programs end.

Our primary contribution is in estimating the during- and after-program effects of two at-scale unconditional cash-based interventions that provide large cash transfers to an extremely poor population, and in providing insight into why program effects do not persist even at such sizeable transfer values. This relates broadly to the literature that quantifies the effects of unconditional cash-based interventions on economic well-being in experimental or quasi-experimental settings (Aker *et al.*, 2016; Aker, 2017; Baird *et al.*, 2011, 2013, 2019; Blattman *et al.*, 2014, 2020; Haushofer and Shapiro, 2016, 2018; Haushofer *et al.*, 2020; Hidrobo *et al.*, 2014; Schwab, 2020),[1] as well as to a smaller literature that quantifies the post-program effects of large asset transfers (Haushofer and Shapiro, 2018; Baird *et al.*, 2019; Blattman *et al.*, 2020; Banerjee *et al.*, 2020a). We also contribute to a set of studies that empirically evaluate the effects of humanitarian aid programs (Hidrobo *et al.*, 2014; Aker, 2017; Lehmann and Masterson, 2020; Masterson and Lehmann, 2020; Quattrochi *et al.*, 2020).[2]

A unique feature of our study is that it occurs in a natural setting in which the United Nations (UN) agencies and their partners entirely design, direct, and implement the assistance programs in line with organizational goals and in consideration of the prevailing social, legal, and political context. Similarly structured and scaled programs operate worldwide within non-experimental settings to provide relief to the extreme poor and help cope with day-to-day vulnerability. The

[1]See Bastagli *et al.* (2016) for a review of the vast literature on both conditional and unconditional cash transfers.

[2]Donors and international NGOs have commissioned impact evaluation reports on humanitarian programs in Lebanon over the past several years using a variety of methods and samples. These include evaluations of the 2014 winterization cash program by Rescue International (Lehmann and Masterson, 2014), the No Lost Generation (schooling) Programme by UNICEF (De Hoop *et al.*, 2018), the multipurpose cash program by DFID and Lebanon Cash Consortium (Battistin, 2016), and the multipurpose cash program by ECHO, GFFO, NMFA, UK aid and CAMEALEON (Chaaban *et al.*, 2020).

programs we study achieve what they are designed to do, but despite their size, do not have lasting effects after the program ends.

Section 2 describes the context of the study, and Section 3 describes the data used in the analysis. We then discuss the empirical strategy, identification, and validity tests in Section 4. Our analytical approach was fully pre-specified with the Open Science Foundation prior to receipt of the data – including variable definitions, hypotheses, specifications, and data protocols.[3] Results from primary hypotheses are in Section 5. We investigate hypotheses regarding savings, asset stocks, and income shocks in Section 6. The final section concludes.

2 Cash transfers to Syrian refugees in Lebanon

The United Nations (UN) Office for the Coordination of Humanitarian Affairs estimates that 1.43% of the global population needs urgent humanitarian assistance and protection – the majority of whom have been forcibly displaced by conflict (GHO 2019). As of 2018, the joint effort of donors have culminated in $22 billion dollars of global funding to alleviate various humanitarian crises around world – most notably in conflict-affected regions in and around the periphery of Somalia, South Sudan, Sudan, and Syria. More than half of humanitarian aid is distributed through UN-led humanitarian response plans (HRPs), which aim to reach more than 100 million people globally (GHO 2019). Despite the scale of international aid effort, little is known about the impact of assistance programs on the welfare of recipients due to lack of data, challenges in establishing credible empirical research designs, the tension between humanitarian principles and randomization-based studies, and the volatility of the rapidly changing environments in which humanitarian organizations operate (Quattrochi *et al.*, 2020).

There are more than 19 million refugees globally under the mandate of the UNHCR, of which 5.5 million were displaced since 2011 due to the ongoing Syrian Civil War (United Nations High Commissioner for Refugees, 2018a). Lebanon alone hosts over 1.5 million of these refugees, making it the country with the highest per capita refugee population share globally. In this context, the UNHCR has been the leading actor in the registration of refugees since the beginning of the Syrian conflict; currently, UNHCR, WFP, and other national and international NGOs provide cash based assistance to refugee families in Lebanon.

We quantify the effect of the two largest cash-based programs in Lebanon, which are jointly administered by the UNHCR and WFP. Multipurpose cash assistance (from hereon referred to as "multipurpose cash") provides a fixed amount of USD175 per month to eligible families through an ATM card, allowing the owner to withdraw cash from ATMs across the country. This program has supported roughly 55,000 to 60,000 families in recent years. The second program is WFP's "food e-card", which provides value voucher assistance to over 120,000 Syrian refugee families annually. This program similarly places a monthly cash balance on an ATM card, which beneficiaries redeem

[3] Details on our compliance with this plan can be found in Appendix Section A and the pre-analaysis plan can be accessed here.

at WFP-contracted shops for specific goods. This program is allocated at the household level, and gives eligible families USD27 per person per month. All unconditional cash beneficiaries also receive the USD27 per person per month e-card assistance as well. Our research design thus helps to recover the effect of unconditional cash among those already receiving the food value voucher, and the effect of a food value voucher relative to entirely unassisted households.

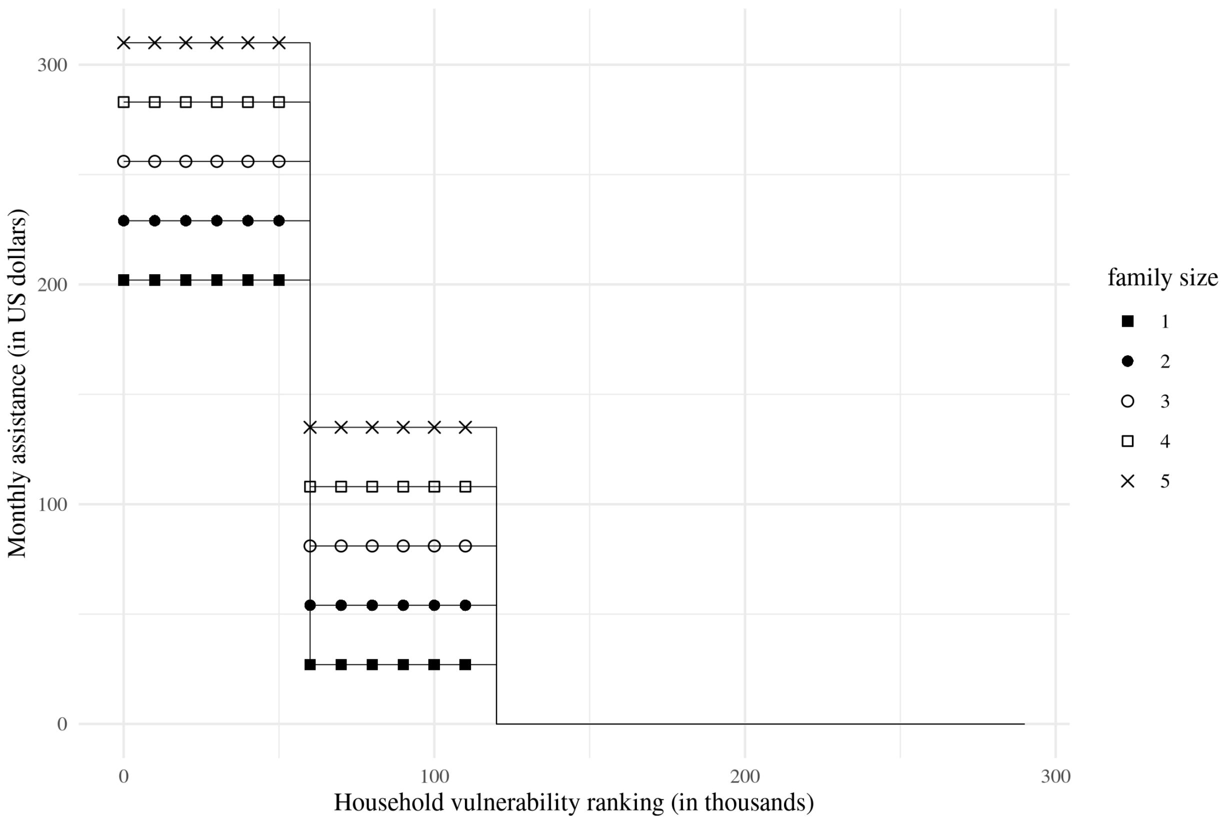
Since neither poverty measurement nor income data exist for the full refugee population, aid agencies use a regression-based proxy means test to target assistance programs. Since 2016, the econometric targeting model has used data from a nationally representative[4] household survey called the Vulnerability Assessment of Syrian Refugees (VASyR) to determine the predictors of per capita expenditure. These predictors are then used to generate a continuous measure of predicted expenditure per capita, *i.e.*, "targeting score", for each of the households in the administrative data held by UNHCR-Lebanon. The targeting score thus ranks the full population of households in order of priority for assistance programs, and this ranking is used to assign eligibility across modalities of assistance for the upcoming program cycle. The programs assign eligibility starting with the household with the lowest predicted expenditure per capita, and continues up through to the highest predicted expenditure household that the respective program's annual budget can support. This eligibility assignment mechanism generates the sharp discontinuity in assignment probability that we use to identify program effects, as is further detailed in Section 4.

Figure 1 provides a schematic of a hypothetical program cycle in which the multipurpose cash program has the budget to reach the lowest-scoring 60,000 families and the food e-card program reaches the lowest scoring 120,000 households. In this scenario, a family of five individuals, for example, would receive USD310 ($175+5\times27$) from both programs if their proxy-means test score placed them in the first 60,000 households; if they ranked above 60,000 and below 120,000 they would receive USD135 (5×27), and no assistance if they ranked above 120,000 (see Figure 1).[5]

[4]The term "nationally representative" refers to representation of the Syrian refugee population in Lebanon.

[5]These figures are for illustrative purposes only, but generally reflect total and relative program sizes in recent years. In some years, humanitarian agencies applied regional "quotas" intended to geographically disperse aid across the country. We explain the regional quotas and our incorporation of this feature in our empirical approach in Section 4.

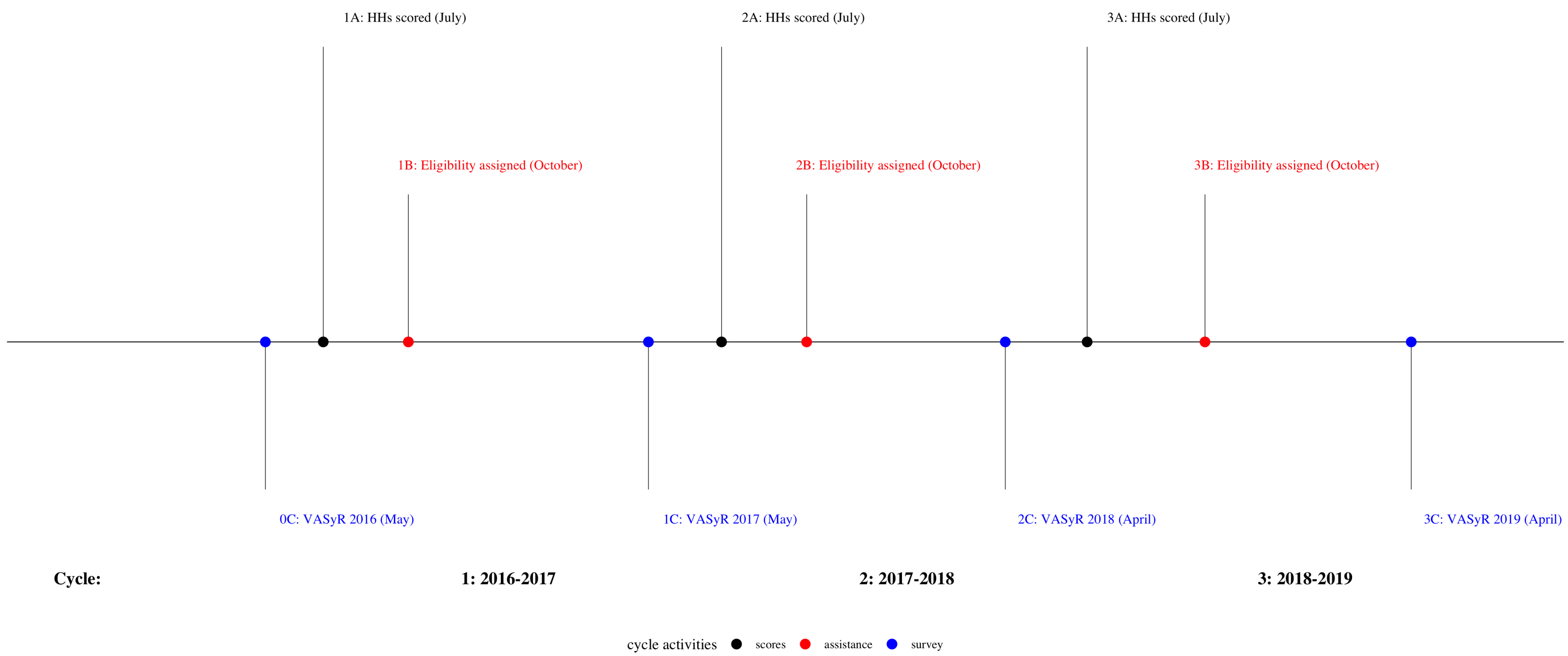
Figure 1: Program assignment schedule: assistance rates by household size and targeting score rank



Note: Graph depicts assistance levels by household size across the targeting score rank, under a hypothetical situation in which the multipurpose cash program reaches 60,000 households jointly and the food e-card program reaches 120,000 households.

Program cycles are annual and synchronous; beneficiary lists are regenerated every year based on a budget and using the proxy-means test ranking system described above. Figure 2 shows the timeline of the annual program cycles over the period we study. Importantly, the annual beneficiary assignment and start of the assistance cycle occurs each November, and nationally representative cross-sectional survey data are collected each year in April.

Figure 2: Timeline of cycle activities and data observation



Note: Graph depicts timing of assistance cycle activities and data collection. From June to August, the targeting model is developed, targeting scores are assigned to "active" households, and beneficiary status for the upcoming cycle is determined. In November, the new assistance cycle begins. Households ranked sufficiently poor to be covered by either program either begin or continue receiving benefits, and those ranked in a way that implies they are beyond the program capacity are discontinued or remain excluded. In April, annual representative survey data collection takes place. Items are indexed by annual program rounds (0-3) and activities are indexed by A-C, corresponding to the household scoring (providing the running variable), assignment of assistance, and survey data collection (providing data on outcomes), respectively.

3 Data

To implement the research design borne of the discontinuity in assignment probability based on program capacity, we combine three pieces of information about each household in the survey data: (i) the annual targeting scores that determined assistance eligibility between 2016 and 2019, (ii) information on the type, amount, and duration of assistance a family received, and (iii) outcomes measured during the program cycle and after the program ends.[6] Next, we describe in further detail the three data sets, which we are able to link using a permanent unique family identifier.[7]

3.1 Targeting scores

The targeting score is a continuous measure of predicted expenditure per capita that comes from an annually calibrated proxy means test. The scores serve exclusively to determine the set of program beneficiaries for the following year in accordance with the available budget. At the beginning of each assistance cycle, households are ranked and designated a beneficiary status and amount through strict cutoff values.[8] We obtained records that include the targeting score history of all the households who were interviewed in any of the representative survey rounds (described below) between 2016 and 2019. For example, if the household outcome data come from a 2018 survey, we obtained the targeting scores in 2016, 2017, and 2018, or all the years during which the family was enrolled with UNHCR-Lebanon.[9]

3.2 Assistance data

The Refugee Assistance Information System (RAIS) maintains the household- and individual-level record of humanitarian assistance allocated to the refugee population in Lebanon. The data include information on all refugee families who received assistance from any of the major international organizations or their partners, with records linked by unique individual and household/case identifiers. In the first step of the analysis, we link assistance records to targeting scores in order to detect the implicit assignment threshold for the transfer programs in each year. In Section 4, we use the detected thresholds, scores, and the type, amount, and period of assistance receipt by household in each annual cycle to confirm the rank-based assignment mechanism.

[6] Falsification tests additionally use outcome measures collected prior to the assignment of eligibility status.

[7] Refugees typically register with international organizations in the host country as family units; when registered together, these groups form what is known as a "case." We use the unique case identifiers that are assigned to refugee families upon their registration with the UNHCR to link the various datasets. On rare occasion, multiple cases live within the same residential unit or a single household may be represented across several cases.

[8] Altindag *et al.* (2021) provides detail on the econometric targeting approach used in Lebanon in recent years.

[9] Enrollment with the UNHCR is akin to registration, including new arrivals into the assistance system and providing them with legal status and proof of identity. It also allows them to avail various types of humanitarian aid and health care, and is a requirement for resettlement in other countries. Refugees thus have strong incentives to make themselves known to UNHCR in the host country. Households that are known to have left the country permanently are considered "not active" and are not included in the set of cases considered as potentially eligible for assistance.

3.3 Survey data

The Vulnerability Assessment of Syrian Refugees in Lebanon (VASyR) constitutes our sample for the main analysis and provides data on outcomes. Since 2013, the VASyR survey has collected detailed information on refugee families' demographic background, expenditures, economic well-being, and poverty coping strategies. The survey design is similar to a household expenditure survey and/or living standards surveys administered in various developing country settings, with additional modules specific to the forced displacement context.[10] The sample size varies annually, but has typically comprised 4,000 to 5,000 households (corresponding to between 20,000 to 25,000 individuals) across Lebanon. The survey is administered over the course of several weeks spanning April and May of each year, and is a nationally representative sample of the refugee population in Lebanon. For our analysis, we obtained four rounds of these data, from 2016 to 2019.

4 Empirical Design

4.1 Sharp Regression Discontinuity Design (RDD)

The regression discontinuity design (RDD) we use for identification relies on quasi-random assignment of cash assistance around the strict thresholds in the targeting score imposed by implementing agencies when allocating beneficiaries. As shown in Figure 2, households receive a new targeting score in July of each year, cash-based assistance from both programs begins in November, and the first outcomes are collected in VASyR surveys the following April/May, approximately six months after the first transfer and while recipients are still receiving monthly transfers. The subsequent round of yearly survey is conducted in the following April/May, roughly 18 months after the start of a given focal cycle and roughly six months after it ended.

The first survey after the assignment of program eligibility falls in the middle of a program cycle during which the families are still receiving assistance, which we refer to as the "during-program" period. Similarly, because both programs provide assistance for 12 consecutive months, the second survey takes place at around six month after the end of the program, which we refer as the "post-program" period. During the data collection, the assistance cycle that generates the discontinuity in eligibility has ended six months prior. Put another way, the allocation mechanism used by aid agencies generates an exogenously determined assignment to assistance receipt for the full population around the eligibility threshold at a given period, and we are able to analyze nationally representative samples of these households in the middle of the one year assistance cycle and six months after the same cycle ends.

Because VASyR rounds are repeated cross-sections, the "follow-up" samples comprise a different set of households than those surveyed in the prior year.[11] However, as the annual assignment mechanism applies to the entire population, one can use random cross-sectional samples that have been subject to the same assignment mechanism for the analysis.

[10] See United Nations High Commissioner for Refugees (2018b,c) for the 2018 report and survey instrument.

[11] A small number of households were repeat-sampled by chance.

Under the assumption that the assignment algorithm randomly allocates cash to households around the eligibility cutoff, the following regression then recovers the reduced-form causal estimates of the during-program effect of cash-based interventions on the focal outcome:

$$y_{i,t} = \alpha + \beta d_{i,t-1} + f(s_{i,t-1}) + \gamma_t + \varepsilon_{i,t} \tag{1}$$

$$\forall s_{i,t-1} \in (c-h, c+h)$$

In Equation 1, $y_{i,t}$ represents the primary and secondary outcomes for household i, observed in year t. We regress $y_{i,t}$ on a binary treatment indicator $d_{i,t-1}$ that equals one if the household was determined to be eligible for cash assistance based on the assessment in period $t-1$, which is the previous calendar year.[12] $s_{i,t-1}$ depicts the continuous running variable, which is the vulnerability score of a household i in period $t-1$. Two continuous local linear functions $f(s_{i,t-1})$ are fit on each side of the eligibility threshold c and the regression sample is restricted to the h score points below and above the threshold, which we determine using the automated optimal bandwidth selection routine developed by Calonico *et al.* (2019). The regression sample includes households from multiple rounds of VASyR, which we account by using survey-year fixed-effects γ_t.[13]

Reduced-form estimates for after-program effects follow a similar approach:

$$y_{i,t} = \alpha + \beta d_{i,t-2} + f(s_{i,t-2}) + \gamma_t + \varepsilon_{i,t} \tag{2}$$

$$\forall s_{i,t-2} \in (c-h, c+h)$$

where the regression discontinuity is based on eligibility determined in period $t-2$, which was 18 months before the households are surveyed and outcomes are collected.

4.2 Fuzzy Regression Discontinuity Design

The reduced form estimates across programs do not take into account non-compliance and are thus not comparable on a "full package receipt" basis. To capture the local average treatment effects (LATE) and standardize the impact estimates across programs and households, we use a fuzzy regression discontinuity design in which the threshold indicator for eligibility is an instrument to predict the amount of cash assistance received by each family, corresponding to the following two-stage-least-squares (2SLS) procedure:

$$y_{i,t} = \alpha + \beta \widehat{aid}_t + f(s_{i,t-j}) + \gamma_{1t} + \varepsilon_{1i,t} \tag{3}$$

[12]The VASyR data collection schedule slightly changes every year, and $t-1$ corresponds to roughly six months after the first assistance received.

[13]For during-program effects, the regression sample specifically includes households who were observed in 2017, 2018, and 2019 and assessed for eligibility in 2016, 2017, and 2018, respectively.

$$j = \{1,2\} \text{ and } \forall s_{i,t-j} \in (c-h, c+h)$$

where $\widehat{aid}_t$ is the predicted assistance that a household receives at period t which we estimate by the following first stage equation:

$$aid_{i,t} = \mu + \lambda d_{t-j} + f(s_{i,t-j}) + \gamma_{2t} + \varepsilon_{2i,t} \tag{4}$$

$$j = \{1,2\} \text{ and } \forall s_{i,t-j} \in (c-h, c+h)$$

where the estimated λ captures the first-stage relationship between eligibility for assistance and the actual assistance received as of t, and β in equation 3 captures the average treatment effect for compliers.

For interpretation, we report the LATE estimates for 175USD per month per family for the multipurpose cash and 27USD per month per person for the food voucher program. These amounts reflect the intended assistance amounts under each program. For sample restriction in 2SLS estimates, we use the MSERD optimal bandwidth h determined in the first stage regression (Calonico *et al.*, 2019). We provide the underlying assumptions for internal validity of our approach and the empirical validity tests in Section 4.5.

RDD estimates have limited external validity away from the identifying threshold in any application. Although we do not address this limitation directly, the local average treatment effects we recover are from a particularly policy-relevant empirical locality in the distribution of households: the margin at which funding increases (or cuts) would expand (or shrink) the programs. This allows us to interpret the findings as the effect of an expansion (or contraction) of these programs.

4.3 Descriptive Statistics

We present descriptive statistics in Table 1 for basic demographic information, data on assistance received, and the primary and secondary outcomes that we use in our analysis. To better reflect the sample local to the discontinuity for which we recover causal estimates, summary statistics are restricted to the sample of households observed six months after beneficiary determination, and were within USD 20 of either program's threshold score.

The sample is relatively young (average age 20.6), with high fertility and dependency rates, has a low average education level, and is balanced by gender. 47 percent of households in the sample received food e-card assistance, and 26 percent received multipurpose cash assistance in addition to food e-card (Table 1, panels A and B).

As noted previously, the cash-based transfers make up a large share of beneficiary families' monthly expenditures and income. For those who received food e-card, the average cumulative amount received as of April of the beneficiary cycle USD 866, or USD 144 per month (32.5 percent of the average monthly total expenditure, and 161 percent of monthly labor income). For those who received multipurpose cash, the average cumulative amount received as of April of the beneficiary cycle is USD 920, or USD 153 per month – 34.6 percent of the average monthly total expenditure, and 172 percent of monthly labor income.[14]

Panel D in Table 1 presents summary statistics of our outcome measures, starting with per capita expenditure (in USD). Total expenditure is calculated by summing the separately asked individual expenditures that the family incurred over the last month, which includes food, rent, energy, transportation, debt payment, household appliances, health and hygiene, education, and a set of other expenditures.

We specify our main outcomes as indices that reflect the mean of unit-standardized sub-components shown in Appendix Table 1. The child hardship index increases with the share of children not going to school, the share of children working, and the share of girls aged 12-17 who are married. The health and healthcare index increases with share of household members who are sick, who required hospitalization, who have a medical condition, or who required primary care. The food coping index increases if the family borrowed food, skipped meals, reduced portions, or searched for a less expensive option than usual, among others. Similarly, the non-food coping index increases with losing or degrading housing, opting for exploitative adult or child work, or reporting of other coping behaviors due to financial distress (see Appendix Table 1 for the full listing of the components of this index).

4.4 Detecting thresholds

The assignment discontinuity arises as a result of program budgets and caseload capacity. For the majority of program-years, the thresholds were rank-based – that is, the assistance was distributed from the most vulnerable to the least vulnerable households based on their relative ranking given by the proxy means test. In some cycles, these budgets were region-specific, and the ranking approach was implemented within each region.[15] In two program-years, the threshold was nationally set at $87 predicted per capita expenditure.

To detect the discontinuity thresholds for program eligibility, we perform an iterative search across potential discontinuity points using the pooled sample of all households for which we have scores and assistance data in any given targeting round.[16] To comport with the assignment mechanism that the implementing agencies used, we undertake the search process separately along the dimensions on which thresholds could differ (by year, program, and where applicable, by region).[17]

[14]Note that the exchange rate was pegged in Lebanon during the study period; the 2020 currency depreciation, inflation, and the rise of black market currency exchange, do not overlap with our study period.

[15]The food e-card program used a national threshold in all targeting rounds, while the multipurpose cash assistance program used a national threshold in 2016, but applied quotas across the four regions of Lebanon in 2017 and 2018.

[16]For each household, we obtained the vulnerability score for all years during which the household was assigned a targeting score. For a family who was surveyed in VASyR 2019 and registered with UNHCR since 2016, for example, we observe the targeting score in 2016, 2017, and 2018.

[17]The search process proceeds as follows: we rank households by their vulnerability score in each targeting cycle and region, conduct an iterative grid search and retain the threshold scores that provide the largest difference in the cumulative amount of assistance between the beneficiaries and non-beneficiaries. Appendix Figure 1 shows the esti-

Table 1: Summary statistics of merged assistance, scores, and outcomes data, 2017-2019 VASyR sample

Statistic	N	Mean	St. Dev.	Min	Max
Panel A: Basics + Demographics					
Survey round	6,767	2,018.47	0.61	2,017	2,019
Predicted expenditure per capita	6,767	81.92	19.99	37.13	133.96
Household size	6,767	5.63	2.15	1	21
Share women	6,767	0.51	0.19	0.00	1.00
Average age	6,767	20.66	9.41	4.67	100.00
Share of dependents	6,767	0.53	0.19	0.00	1.00
Education (yrs) of HoH	6,497	5.51	3.40	0.00	16.00
Avg. education, adults	6,696	5.39	2.78	0.00	16.00
Income (total household)	6,715	89.01	135.56	0.00	733.33
Panel B: Assistance (as of following April, full sample)					
Multipurpose cash (0/1)	6,767	0.26	0.44	0	1
Food e-card (0/1)	6,767	0.47	0.50	0	1
Multipurpose cash (Cumulative USD)	6,767	240.25	423.66	0	1,050
Food e-card (Cumulative USD)	6,767	404.50	494.13	0	3,402
Multipurpose cash (average USD/month)	6,767	40.04	70.61	0	175
Food e-card (average USD/month)	6,767	67.42	82.36	0	567
Panel C: Assistance (as of following April, conditional on receipt)					
Multipurpose cash (Cumulative USD)	1,766	920.58	247.77	175.00	1,050.00
Food e-card (Cumulative USD)	3,158	866.77	349.98	81.00	3,402.00
Multipurpose cash (average USD/month)	1,766	153.43	41.30	29.17	175.00
Food e-card (average USD/month)	3,158	144.46	58.33	13.50	567.00
Panel D: Outcomes					
Total expenditure	6,767	442.01	284.00	0	4,333
H1: Expenditures per capita	6,767	82.80	53.12	0.00	891.67
H2: Child work/education/marriage index	4,994	−0.08	0.89	−0.64	4.76
H3: Healthcare access index	6,767	0.05	1.01	−1.19	4.45
H4a: Food coping index	6,767	0.03	1.00	−1.31	8.68
H4b: Non-food coping index	6,767	0.04	0.99	−1.67	13.46

Notes: Table contains summary statistics for scored households sampled in the 2017-2019 survey rounds. Indices are constructed from the mean of unit-standardized values of index subcomponents.

Appendix Figure 2 shows the average monthly assistance amount by one dollar bins of the distance to the assignment threshold, which was normalized to zero for each program-year. These figures provide clear evidence that (i) the assignment mechanism was discontinuous, as implied by the programmatic description of the beneficiary assignment rule, and (ii) that our methodology accurately recovers the threshold scores used for assignment to beneficiary status.[18]

4.5 Validity Tests

The causal interpretation of the estimates in Equations 1, 2, and 3 relies on the local randomization assumption around the cutoff value of eligibility for cash assistance. In addition, for the estimated local average treatment effects (LATE) in Equation 3 to be valid, the standard instrumental variables assumptions need to hold, among them the most critical is the exclusion restriction. Below, we provide standard empirical tests and additional ones that are specific to our setting to validate the research design. We additionally discuss the first stage and the exclusion restriction.

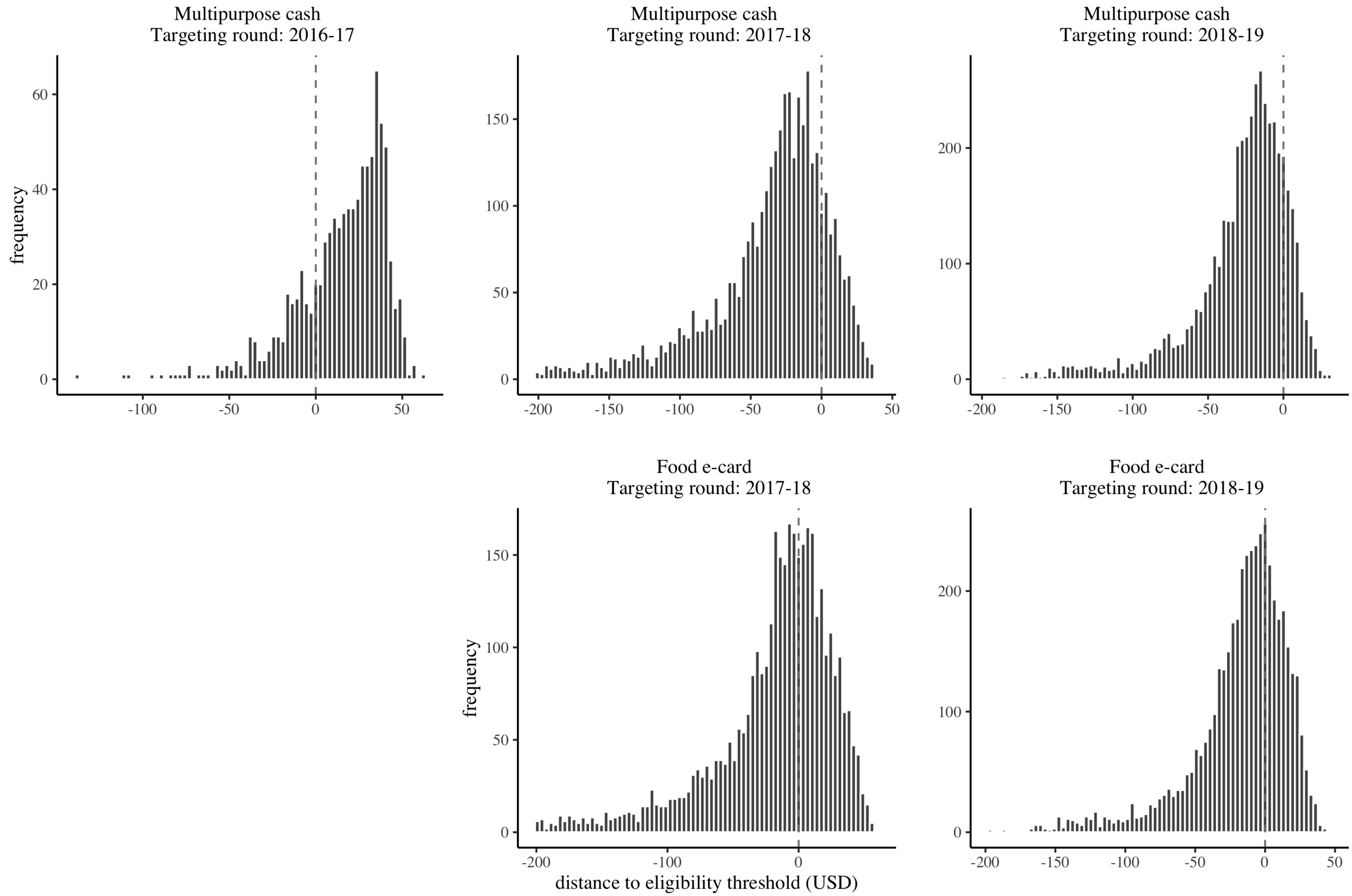
4.5.1 Density in the forcing variable

Using the density of observation frequencies around the threshold, we apply the McCrary (2008) density test to assess whether there is evidence of score manipulation across the assignment threshold. In Figure 3, we show the frequency distribution of observations by targeting scores by dollar bin in each assistance cycle and program. None show a visual sign of density discontinuity around the threshold; we also fail to reject at conventional levels the null hypothesis of no manipulation in the corresponding parametric density tests across all programs and assignment cycles (Appendix Table 3). This result is expected, as the details of the targeting model and the household-level scores are not revealed to refugees or field workers outside a small number of central office staff – making it highly unlikely that manipulation in the forcing variable could occur. Moreover, the targeting model scores are systematically used when making beneficiary assignments for the programs we study with practically no scope for exception. While households can petition for review and redress, intentional features of the targeting program preclude (i) such households knowing either their targeting score/rank or their distance from the threshold/cutoff, and (ii) front-line staff who interact with households from having access to targeting scores, ranks or eligibility thresholds. These features generally preclude opportunistic redress requests based on knowledge of a household's own score or rank, and would also preclude staff's selective encouragement of certain households near the threshold to apply for redress. Furthermore, redress requests are accommodated thorough secondary programs which have different cash transfer amounts and terms of assistance.

mated coefficients by ranking for each of combination of program, year and region (when applicable). We found that the food e-card program in 2016 did not utilize the targeting scores in the same way as other programs and years, and thus did not have a sharp discontinuity in assignment. We discuss this in further detail in Appendix Section A.

[18]We report the dollar value of the implicit threshold for the ranking at which the search coefficient is the global maximum; these values are then reported in Appendix Table 2 by the level of aggregation at which the search was performed. This table also confirms that the search process recovers thresholds that are highly comparable to those set in dollar values explicitly (multipurpose cash in 2016, and food e-card in 2017).

Figure 3: Tests of density around cutoffs



Note: Figure depicts observation density around assignment thresholds (shown by the dashed line) by year and program.

4.5.2 Continuity in prior assistance receipt, first stage and exclusion restriction

To further validate the regression discontinuity design, we use a program feature that is specific to our setting due to the ongoing yearly assistance cycles. While we expect the vulnerability score to determine cash transfers in the next assistance cycle, it should be uncorrelated with assistance receipt within the same cycle. Since we observe the assistance scores of the families for each of the cycles during which they were present in Lebanon, we can directly test this by slightly modifying the specification in Equation 4:

$$aid_{i,t} = \pi + \delta d_t + f(s_{i,t}) + \gamma_{3t} + \varepsilon_{3i,t} \tag{5}$$

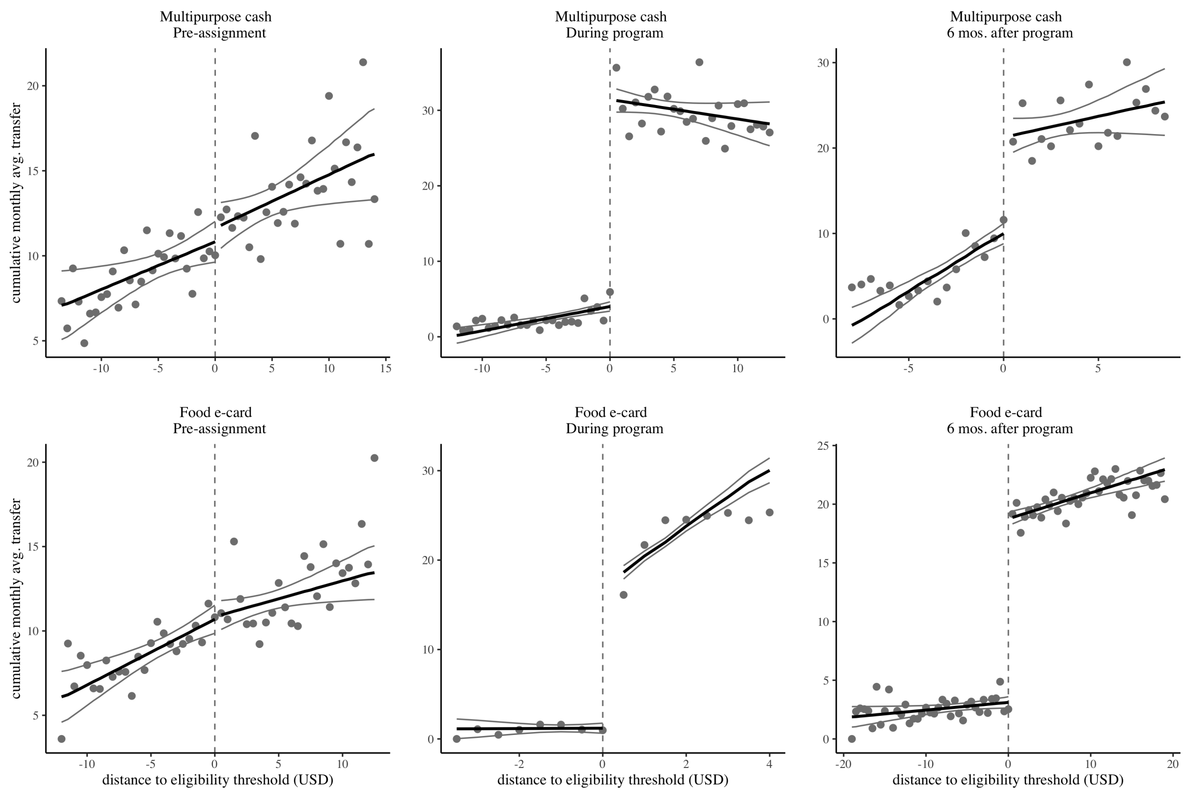
such that if the research design holds, we should estimate a large positive coefficients for λ in Equation 4, indicating a differential cumulative amount of cash assistance between families who are slightly above and slightly below the eligibility threshold over the next two periods for each program while δ in Equation 5 should have no prediction power.

Figure 4 presents the graphical analog to these analyses, in which we plot the amount of assistance per capita received relative to the assistance eligibility thresholds. Accompanying this, the first column of Table 2 contains the coefficients from the estimation of equation 5 whereas columns 2 and 3 show the regression results from equation 4. The vulnerability score is not correlated with the amount of assistance received within the same assistance cycle for any of the programs around the threshold. This provides a powerful test of balance given that the vulnerability score is solely determined by household observable characteristics at the time of the survey.

Figure 4 also shows that the same vulnerability scores generate a substantial discontinuous jump around the threshold when predicting the future period cash transfers. Table 2 documents these first stage effects that measure the differential cumulative amount of cash in USD received by the eligible households compared to ineligible households who are otherwise similar. For the multipurpose cash program, the difference is around 27 USD per capita per month six months into the program (relative to the non-beneficiary mean of 2 USD), whereas the difference is 11 USD per capita per month six months after the program ends. The gap in assistance amounts narrows due to the fact that beneficiary eligibility has been reassigned when the cycle ends, some of the previous cycle's non-beneficiaries becoming newly eligible for the assistance in the new cycle and vice versa.

For the food e-card program, eligible families are receiving 17 USD per capita per month during the program relative to non-beneficiaries (mean 1 USD), whereas the post-program differences remain at 16 USD. In sum, the first stage estimates indicate that (i) the allocation mechanism works as intended, generating ample random variation around the eligibility cutoff, and (ii) the treatment intensity is substantial, indicating a large cash transfer to the the eligible group relative to the non-beneficiary mean receipt. For example, a family of five individuals that is right above the eligibility threshold for the food e-card program, the differential cumulative amount of assistance for the full

Figure 4: Assistance receipt around threshold



Note: Figure depicts first stage effect of threshold assignment on per capita monthly assistance receipt on optimal bandwidth sample with local linear regression fits and associated 95% confidence intervals. "Pre-assignment" subfigures present a falsification test of whether future assignment (x axis) is related to the amount of assistance received in the prior cycle (y axis).

year assistance cycle amounts to 1020 USD.

In a fuzzy RD design, anticipatory effects could invalidate the assignment scores as a first stage predictor of actual assistance. If households change consumption behavior due to expected future assistance based on their scores, the exclusion restriction assumption would not hold. But because the targeting algorithm changes year to year, and is not shared in detail with field staff nor potentially eligible households during the development of the targeting model, there is no direct way for households to know either their targeting score or whether they are slated to be beneficiaries (or not) in an upcoming cycle. These programmatic features make anticipation of the eligibility determination – at least around the assignment threshold – effectively impossible. Thus the only way that the vulnerability score – unknown to households – has an impact on cash transfers is through assistance eligibility.

4.5.3 Further tests: continuity in pre-assignment outcomes and covariates

We further test for systematic differences in pre-assignment outcome measures across beneficiaries around the eligibility threshold of the upcoming cycle. In the absence of manipulation, the

Table 2: Change in monthly average amount of assistance received across assignment threshold

	Outcome measurement:		
	Pre-assignment	During program	6 mos. after program
	(1)	(2)	(3)
Panel A: Multipurpose cash			
Above threshold	0.80	27.27	11.00
	(0.94)	(0.79)	(1.19)
Benjamini-Hochberg q	0.392	$< 0.001^{***}$	$< 0.001^{***}$
Control Group Mean	8.78	2.21	5.25
Bandwidth	13.64	12.32	8.34
N	2,972	2,520	713
R^2	0.05	0.60	0.44
Panel B: Food e-card			
Above threshold	0.11	16.65	15.69
	(0.63)	(0.54)	(0.38)
Benjamini-Hochberg q	0.862	$< 0.001^{***}$	$< 0.001^{***}$
Control Group Mean	8.64	1.07	2.59
Bandwidth	12.07	3.75	19.05
N	2,907	868	1,756
R^2	0.02	0.85	0.78

Note: This table reports the first stage effect of being above the implicit threshold for assignment to cash transfer receipt on the monthly amount of transfers received. The sample contains all the households within the optimal bandwidth based on the Calonico *et al.* (2019) algorithm. All regressions include survey year fixed effects, a linear term in the poverty score as well as its interaction with the indicator for being above the detected threshold. Estimations are triangular kernel-weighted. The specification, variable definitions, sample, bandwidth selector, and adjustments for multiple hypothesis testing were prespecified for all results contained in this table.
$^{*}q < .1$; $^{**}q < .05$; $^{***}q < .01$

vulnerability scores should be uncorrelated with the outcome measures within the same assistance cycle and any causal impact of the cash transfers should emerge in a future cycle. The empirical specification of the continuity test is similar to equation 1 and as follows:

$$y_{i,t} = \alpha + \beta d_{i,t} + f(s_{i,t}) + \gamma_t + \varepsilon_{i,t} \tag{6}$$

$$\forall s_{i,t} \in (c-h, c+h)$$

where $y_{i,t}$ denotes the outcome measured within the same cycle and prior to the eligibility assignment $d_{i,t}$. We apply these continuity tests on all the primary and secondary outcomes that we assess in our study: contemporaneous cash transfers, expenditure, child hardship index, health access, food coping and non-food coping z-indices.

Appendix Table 4 contains the results of these tests. Only one of ten tests is significant at conventional levels (expenditure at the multipurpose cash program thresholds); its sign is the opposite of one indicating manipulated positive selection. When correcting for multiple hypothesis testing, however, none of the tests of pre-assignment outcomes are statistically significant. We conclude that the running variable can't explain any variation in pre-assignment outcomes.

Similarly, we use the pre-assignment covariates as outcomes in equation 6 to assess the balance of covariates around the threshold for the treatment and the control group. Appendix Table 5 provides the results from these tests on household size, dependency ratio, the household's share of working-age non-disabled men, and the average education level of adults. We find that household size and share of dependents differ around the program threshold at 10% significance level although coefficients go in opposite directions across programs.

To summarize, a large battery of empirical validity tests along with the strong first stage results that indicate the sharp discontinuity around the allocation threshold validate the empirical design. This allows us to interpret any systematic divergence in future outcomes across households around the eligibility threshold as resulting from the two assistance programs studied.

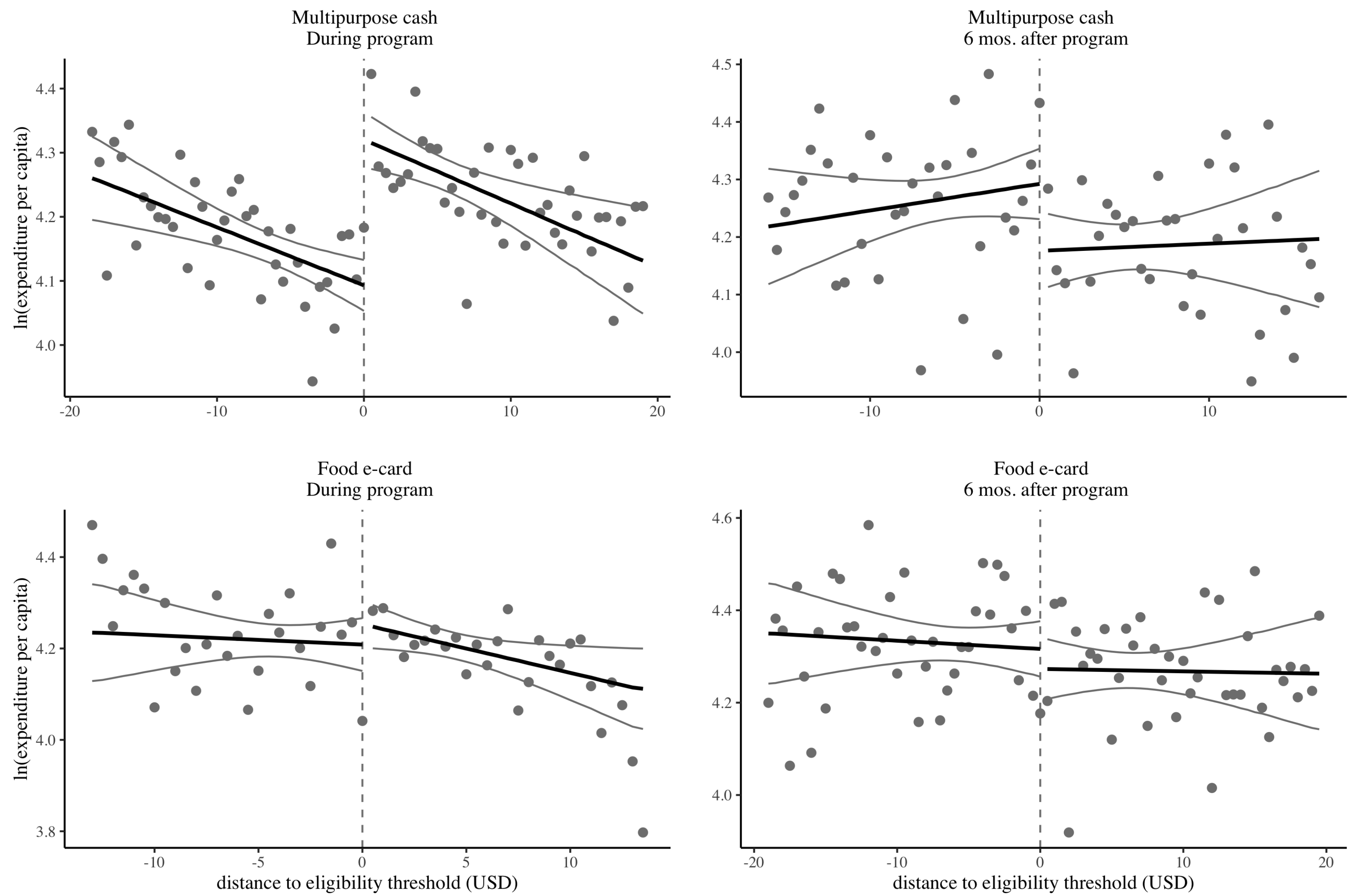
5 Results

We present all results with a graphical representation of the RD analysis, including during- and post-program effects on the focal outcomes for the multipurpose cash and food e-card programs separately. As explained earlier, during-program effects refer to the effects observed six months after the beginning of an annual cycle whereas post-program effects refer to outcomes measured six months after the end of an annual cycle. The corresponding reduced-form point estimates are shown in an accompanying table where we also report the LATE estimates that come from the IV 2SLS specification given in equation 3. Because our main outcomes are indices (excepting log expenditure per capita), we also report the coefficients on the unit-standardized subcomponents of the indices in appendix figures in order to comment on which components drive the results in the overall index. Bandwidths used come from the MSERD selector given by Calonico *et al.* (2019). Conventional standard errors and multiple-hypothesis-adjusted q-values using the Benjamini-Hochberg correction are reported below the coefficient estimates.

Expenditure Figure 5 and Table 3 present the effects of the programs on the natural log of per capita expenditure. The top left panel of Figure 5 shows that households observed across the eligibility threshold for multipurpose cash exhibit a sharp jump in expenditure; an increase of 0.17 log points (18.5 percent), and significant at any conventional level after accounting for multiple hypotheses (Table 3, Panel A, Column 1). The 2SLS estimates indicate a 0.21 log points (23 percent) increase in expenditure among households who received the full intended amount of assistance (Table 3, Panel A, Column 3). The reduced form estimates on post-program effects on expenditure is negative, smaller in magnitude and is not statistically different from zero. The LATE estimates follow the same conclusion. Altogether, the substantial increase in consumption during the program disappears within six months of the end of the assistance cycle. The food e-card program shows positive but more muted impact on expenditure: the increase in overall expenditure is 8 percent, and only significant at the 10 percent level after accounting for multiple hypotheses (Table 3, Panel B, Column 1). The Wald estimates indicate a 10 percent increase in expenditure for full assistance receipt (Table 3, Panel B, Column 3). Note that this program offers a voucher/e-card that can be used to purchase food only. Similar to the cash program, the during-program increases in expenditure do not persist even to six months after the end of the program.

Expenditure sub-components Figure 6 presents 95 percent confidence intervals for reduced-form coefficients on the sub-components of expenditure that include food, rent, energy, health, communications and transport, and spending on new appliances, education, and debt payments. We also add alcohol and tobacco consumption as a separate item to test for increased demand of "temptation" goods (Banerjee and Mullainathan, 2010). The top left panel shows that consumption in essential needs drive the overall increase in expenditure induced by multipurpose cash such as rent, energy, health and hygiene, and debt payment. As intended, the food e-card increases food expenditure markedly with no discernible effect on other types of consumption (Figure 6). Post-program effects generally fail to reject the null for both programs, reflecting the overall null effects on per capita expenditure after the assistance cycle is over. Importantly, we find no evidence of increased consumption on entertainment or tobacco and alcohol for either of the programs at any period that we observe the outcomes.

Figure 5: Effect of assistance on ln(per capita expenditure)



Note: Figure depicts the effect of threshold assignment on per capita monthly expenditure on optimal bandwidth sample with local linear regression fits and associated 95% confidence intervals.

Table 3: Cash transfer effects on per capita expenditure (log)

	Reduced form:		IV LATE:	
	During program	6 mos. after program	During program	6 mos. after program
	(1)	(2)	(3)	(4)
Panel A: Multipurpose cash				
Program effect	0.17	−0.08	0.21	−0.17
	(0.03)	(0.04)	(0.04)	(0.09)
Benjamini-Hochberg q	$< 0.001^{***}$	0.345	$< 0.001^{***}$	0.350
Control Group Mean (levels)	78.34	82.36	77.71	82.52
Bandwidth	16.15	19.68	16.15	19.68
N	3,231	1,710	3,231	1,710
R^2	0.01	0.01	0.01	0.0002
Panel B: Food e-card				
Program effect	0.08	−0.03	0.10	−0.05
	(0.04)	(0.05)	(0.05)	(0.09)
Benjamini-Hochberg q	0.075^{*}	0.719	0.075^{*}	0.717
Control Group Mean (levels)	86.29	88.79	85.56	92.12
Bandwidth	13.46	15.77	13.46	15.77
N	2,958	1,481	2,958	1,481
R^2	0.003	0.002	0.004	0.002

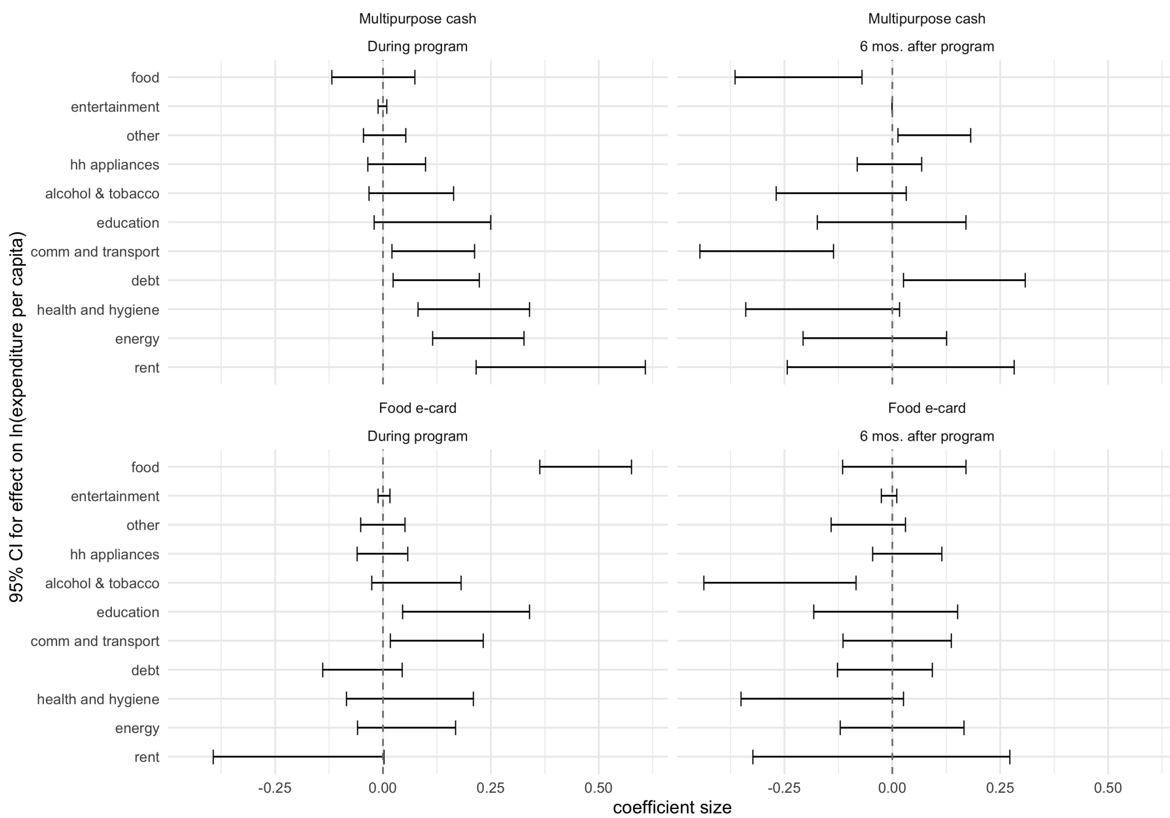
Note: This table reports estimates of the effect of cash transfers on log per capita expenditure. The sample contains all the households within the optimal bandwidth based on the Calonico *et al.* (2019) algorithm. All regressions include survey year fixed effects, a linear term in the poverty score as well as its interaction with the indicator for being above the detected threshold. Estimations are triangular kernel-weighted. The specification, variable definitions, sample, bandwidth selector, and adjustments for multiple hypothesis testing were prespecified for all results contained in this table.
$^{*}q < .1$; $^{**}q < .05$; $^{***}q < .01$

Child welfare Figure 7 and Table 4 present results for the child hardship index, which comprises measures of the presence of the share of children working, the share of children not enrolled in school, and the share of 13 to 17-year-old girls who are married. The multipurpose cash program reduces the child hardship index by 0.23SD on program participants, and we observe no difference between prior recipients versus non-recipients after the program ends. The food e-card program has no impact on the child welfare measure at any point after eligibility assignment.

Child welfare sub-components Appendix Figure 3 shows confidence intervals of program effects among sub-components of the child hardship index, suggesting all of the components (child labor, school disenrollment, and child marriage) contribute to the negative overall effect of cash receipt on the child hardship index.

To investigate this further, Appendix Table 13 estimates effects by sex on child labor and school enrollment, as well as adult labor supply. Child labor reductions among recipients of the multipurpose cash program (Panel A) are concentrated among boys, as are increases in school enrollment. These effects are seen alongside increases in men working. The programs thus appear to induce a shift in the source of household labor supply following the re-enrollment of boys in school. We speculate that limited nutrition, typical employment opportunities that require physical

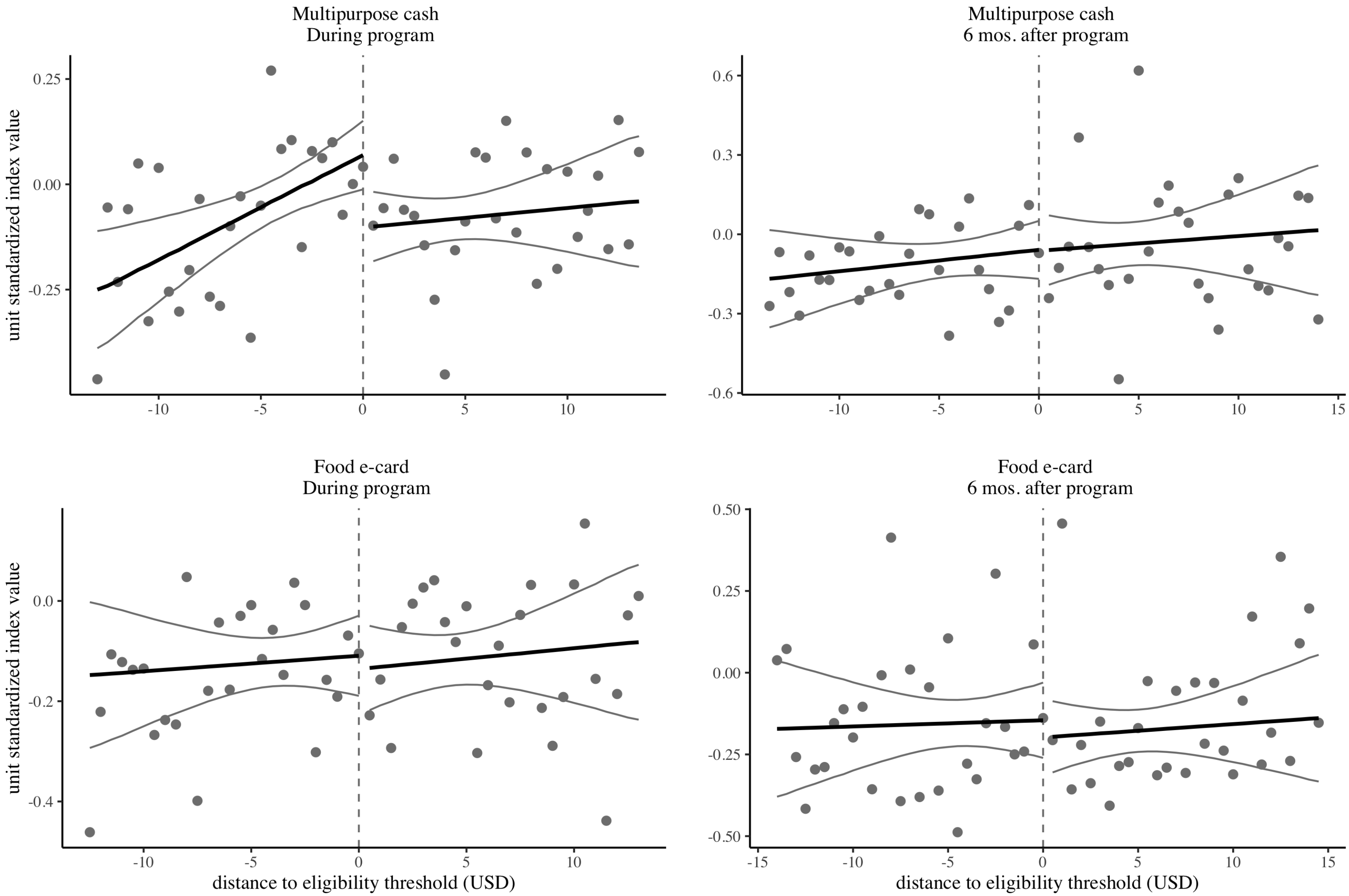
Figure 6: During and after program effects on expenditure subcomponents



Note: Figure depicts the parametric effect of threshold assignment on per capita monthly expenditure and its constituent components. Spans indicate 95% confidence interval.

effort such as construction and farming, and other unobserved changes in labor supply capacity induced by cash transfers might explain the increase in labor supply. The traditional negative relationship between labor supply and income thus may not apply to an extremely poor population (Baird *et al.*, 2018; Banerjee *et al.*, 2020b). There is minimal evidence of program effects on any of these outcomes for women.

Figure 7: Effect of assistance on child hardship index



Note: Figure depicts the effect of threshold assignment on the child hardship index on optimal bandwidth sample with local linear regression fits and associated 95% confidence intervals.

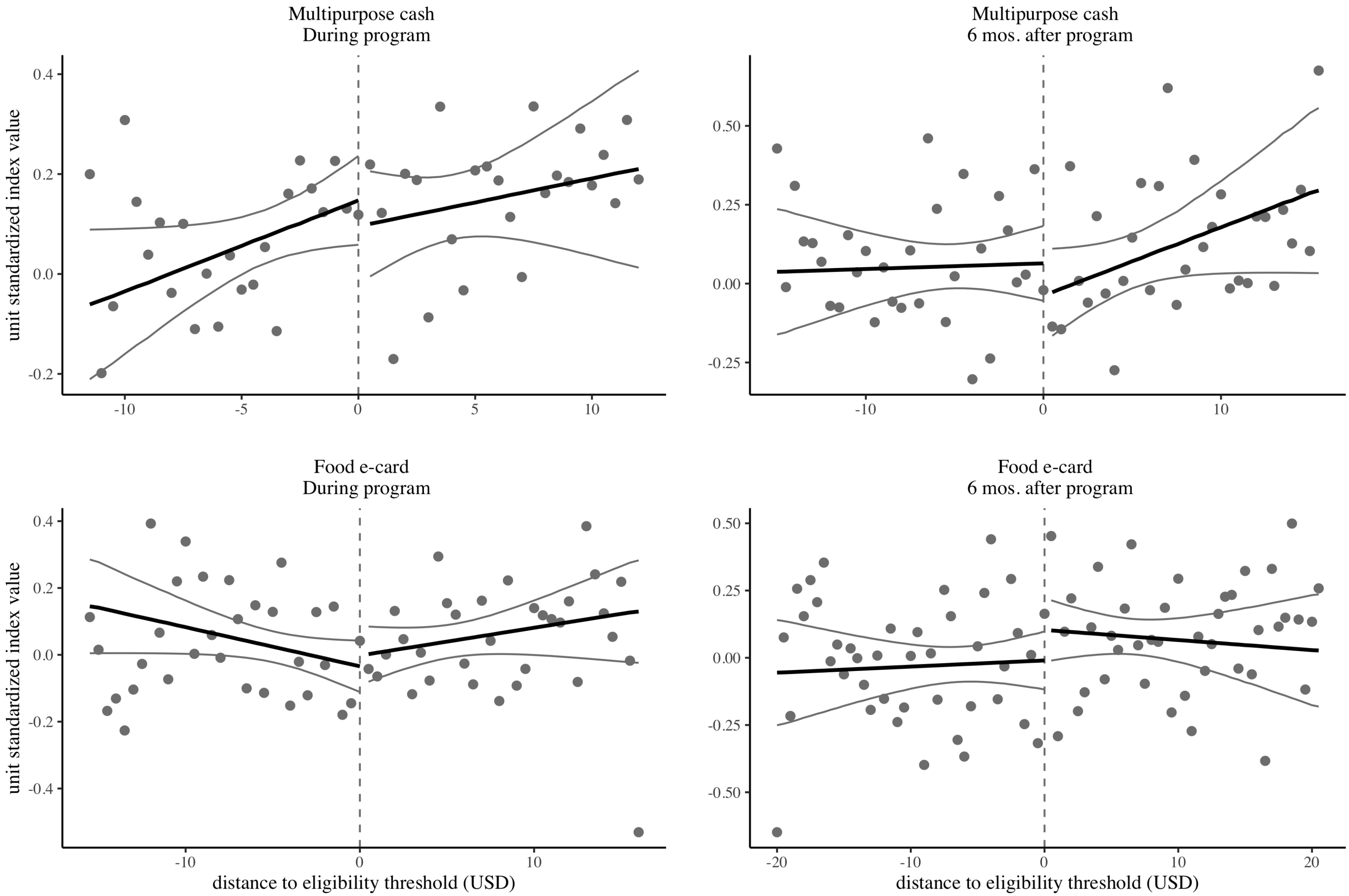
Table 4: Cash transfer effects on child hardship (index)

	Reduced form:		IV LATE:	
	During program	6 mos. after program	During program	6 mos. after program
	(1)	(2)	(3)	(4)
Panel A: Multipurpose cash				
Program effect	−0.19	−0.01	−0.23	−0.01
	(0.06)	(0.09)	(0.08)	(0.21)
Benjamini-Hochberg q	0.007***	0.954	0.007***	0.954
Control Group Mean	-0.08	-0.12	-0.09	-0.14
Bandwidth	13.17	13.85	13.17	13.85
N	2,224	1,050	2,224	1,050
R^2	0.01	0.002	0.01	0.002
Panel B: Food e-card				
Program effect	−0.03	−0.05	−0.03	−0.09
	(0.06)	(0.08)	(0.08)	(0.14)
Benjamini-Hochberg q	0.672	0.719	0.672	0.717
Control Group Mean	-0.14	-0.15	-0.13	-0.16
Bandwidth	12.7	14.56	12.7	14.56
N	2,192	1,166	2,192	1,166
R^2	0.002	0.0004	0.003	−0.001

Note: This table reports estimates of the effect of cash transfers on an index of child hardship. The sample contains all the households within the optimal bandwidth based on the Calonico *et al.* (2019) algorithm. All regressions include survey year fixed effects, a linear term in the poverty score as well as its interaction with the indicator for being above the detected threshold. Estimations are triangular kernel-weighted. The specification, variable definitions, sample, bandwidth selector, and adjustments for multiple hypothesis testing were prespecified for all results contained in this table.

Health and healthcare access Figure 8 and Table 5 present effects on the health index, which is comprised of measures of whether children or adults were sick, whether any member required primary healthcare or hospitalization, or whether any child has a medical condition. Across both programs and measurement periods, there is no distinguishable effect on the index measure of health, nor its sub-components (see Appendix Figure 4). These results are not particularly surprising, as the UNHCR during this period provided highly subsidized healthcare access to all refugees in the country at a network of hospitals and providers covering a wide range of conditions and needs (United Nations High Commissioner for Refugees, 2019). Additional income or food security thus likely only had indirect effects on the ability to access healthcare, such as providing the capacity to obtain transport to a provider.

Figure 8: Effect of assistance on health index



Note: Figure depicts the effect of threshold assignment on health status and access index on optimal bandwidth sample with local linear regression fits and associated 95% confidence intervals.

Table 5: Cash transfer effects on health (index)

	Reduced form:		IV LATE:	
	During program	6 mos. after program	During program	6 mos. after program
	(1)	(2)	(3)	(4)
Panel A: Multipurpose cash				
Program effect	−0.06	−0.11	−0.08	−0.26
	(0.07)	(0.10)	(0.09)	(0.23)
Benjamini-Hochberg q	0.417	0.637	0.417	0.637
Control Group Mean	0.06	0.06	0.05	0.11
Bandwidth	11.72	15.01	11.72	15.01
N	2,408	1,320	2,408	1,320
R^2	0.004	0.01	0.004	0.01
Panel B: Food e-card				
Program effect	0.04	0.12	0.05	0.20
	(0.06)	(0.08)	(0.07)	(0.14)
Benjamini-Hochberg q	0.646	0.719	0.646	0.717
Control Group Mean	0.03	-0.01	0.03	0.01
Bandwidth	15.6	20.08	15.6	20.08
N	3,439	1,820	3,439	1,820
R^2	0.002	0.003	0.002	0.0003

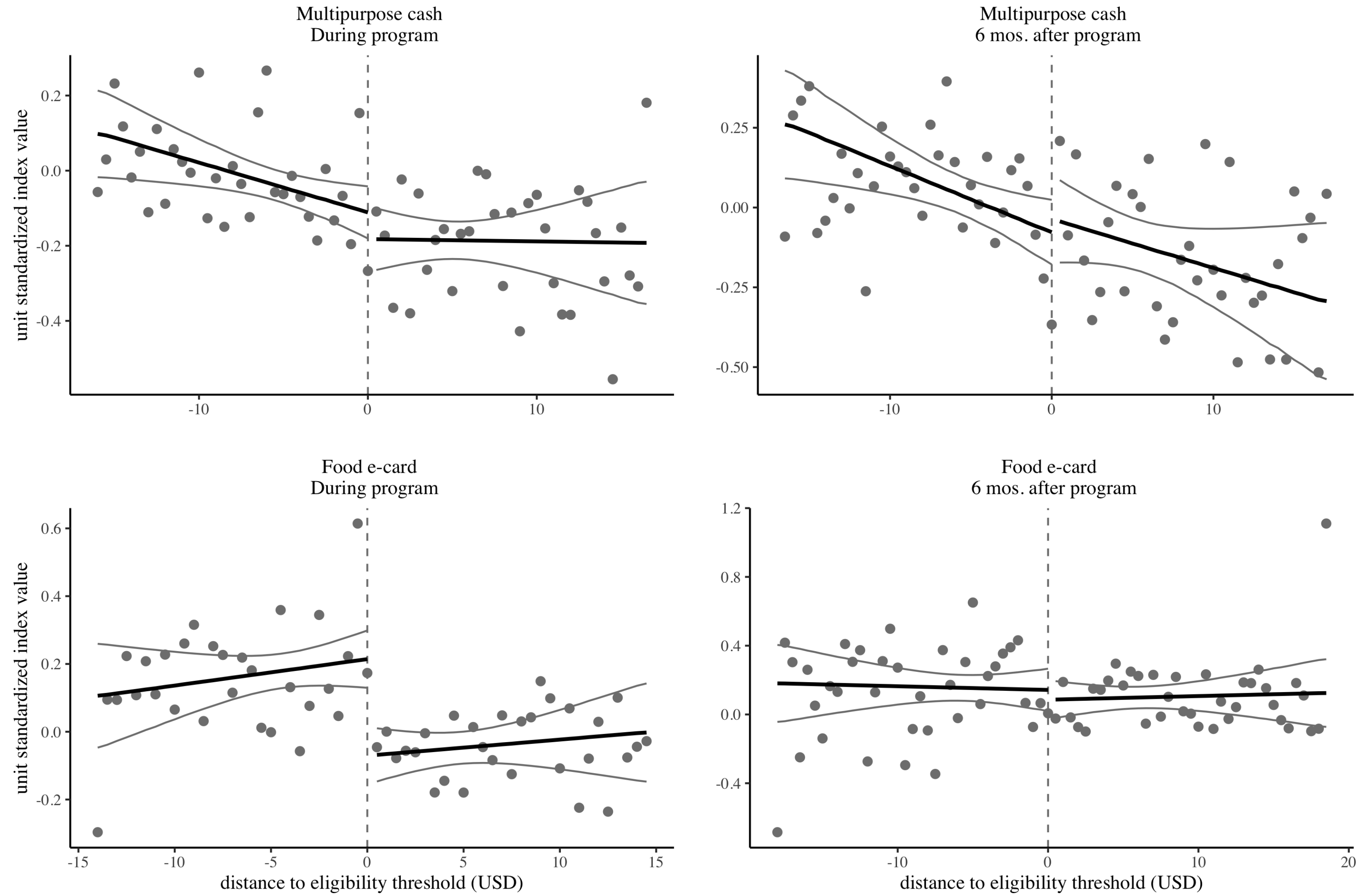
Note: This table reports estimates of the effect of cash transfers on an index of health. The sample contains all the households within the optimal bandwidth based on the Calonico *et al.* (2019) algorithm. All regressions include survey year fixed effects, a linear term in the poverty score as well as its interaction with the indicator for being above the detected threshold. Estimations are triangular kernel-weighted. The specification, variable definitions, sample, bandwidth selector, and adjustments for multiple hypothesis testing were prespecified for all results contained in this table.

Food insecurity Figure 9 and Table 6 contain the effects of the cash and voucher programs on measures of food coping strategies, which comprised eight separately-asked measures of the frequency with which a household engaged in a given food coping strategy in the past week.[19] Striking LATE effects here are found during disbursement of the food e-card program, which reduces the incidence of food coping strategies by .36 SD (Panel B, Column 3 of Table 6). The multipurpose cash program has no contemporaneous effect on food coping, and neither program has effects that last into the post-program period.

Food insecurity sub-components We observe stark improvements in borrowing food, reducing the number of meals, going without food for a day, and reducing portions (bottom left panel of Appendix Figure 5) induced by food e-card program eligibility. Food insecurity, especially insufficient calorie intake, is very common among the refugee population and the effect sizes suggest that the program substantially alleviates food deprivation (Appendix Table 1). The post-program incidence of food coping mechanisms are remarkably similar between households who received one full year of food assistance in the previous cycle to those who were deemed ineligible for the same period (Appendix Figure 5).

[19] The sub-component measures are listed separately in Appendix Table 1.

Figure 9: Effect of assistance on food coping index



Note: Figure depicts the effect of threshold assignment on food coping strategy index on optimal bandwidth sample with local linear regression fits and associated 95% confidence intervals.

Table 6: Cash transfer effects on incidence of food coping strategies (index)

	Reduced form:		IV LATE:	
	During program	6 mos. after program	During program	6 mos. after program
	(1)	(2)	(3)	(4)
Panel A: Multipurpose cash				
Program effect	−0.06	0.06	−0.07	0.13
	(0.06)	(0.09)	(0.07)	(0.20)
Benjamini-Hochberg q	0.381	0.811	0.381	0.811
Control Group Mean	0	0.07	0	0.07
Bandwidth	16.14	16.13	16.14	16.13
N	3,247	1,434	3,247	1,434
R^2	0.01	0.02	0.01	0.02
Panel B: Food e-card				
Program effect	−0.28	−0.05	−0.36	−0.08
	(0.06)	(0.09)	(0.08)	(0.15)
Benjamini-Hochberg q	$< 0.001^{***}$	0.719	$< 0.001^{***}$	0.717
Control Group Mean	0.17	0.16	0.17	0.2
Bandwidth	14.74	18.15	14.74	18.15
N	3,268	1,688	3,268	1,688
R^2	0.01	0.001	0.01	0.002

Note: This table reports estimates of the effect of cash transfers on an index of food coping strategies. The sample contains all the households within the optimal bandwidth based on the Calonico *et al.* (2019) algorithm. All regressions include survey year fixed effects, a linear term in the poverty score as well as its interaction with the indicator for being above the detected threshold. Estimations are triangular kernel-weighted. The specification, variable definitions, sample, bandwidth selector, and adjustments for multiple hypothesis testing were prespecified for all results contained in this table.

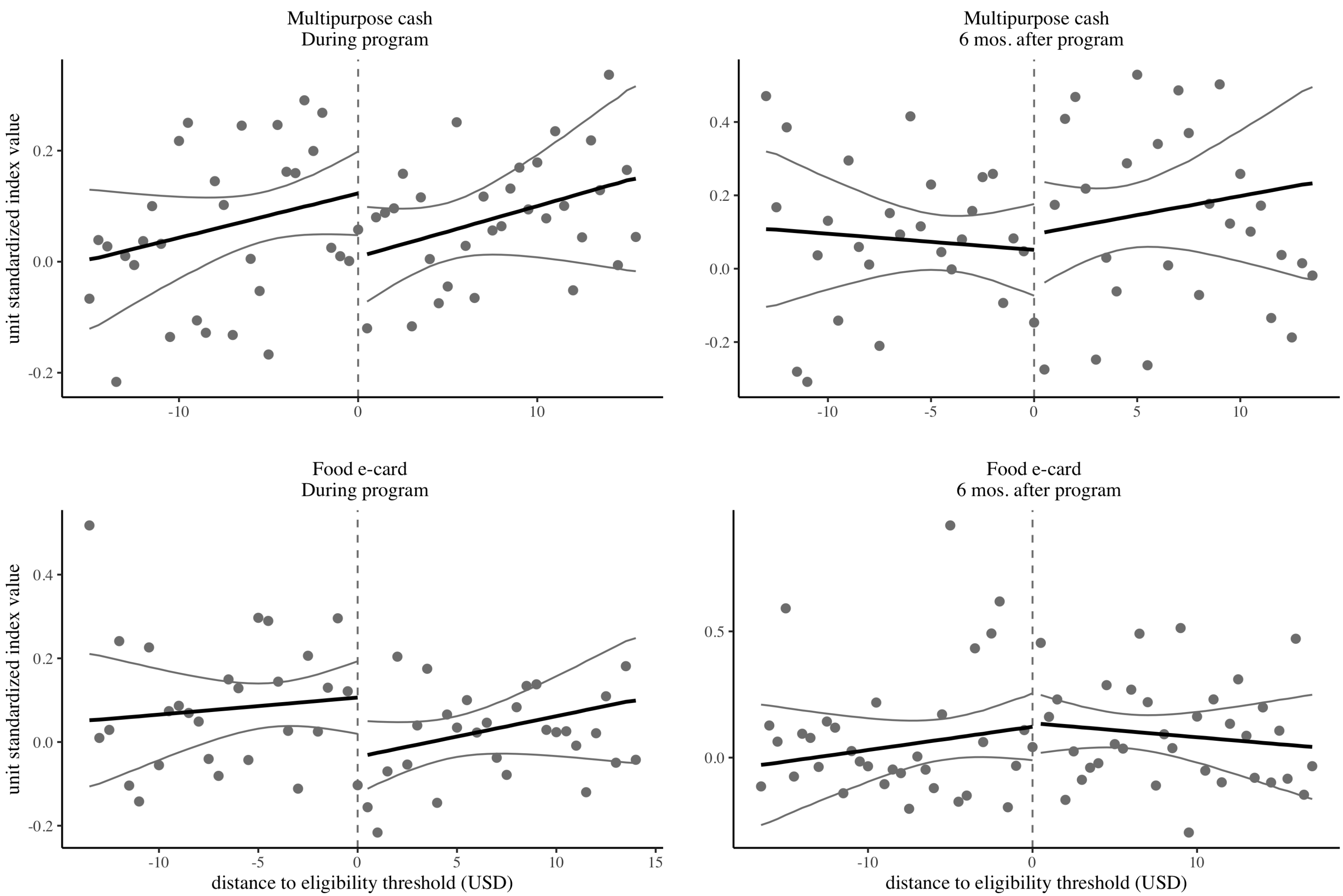
Livelihood coping strategies While beneficiaries are actively receiving assistance, both programs yield significant reductions in livelihood coping strategies (Table 7, column 3 and Figure 10). This is on the order of .15SD for food e-card recipients, and .17SD for multipurpose cash recipients. As with all other outcomes, livelihood coping strategies revert to being indistinguishable between prior recipients versus non-recipients within six months of the end of the assistance cycle.

Livelihood coping strategies sub-components There are 26 different components to this index, from which we can glean overall patterns. The effect sizes are large for many of the individual coping strategies, although individual tests do not survive corrections for multiple hypotheses. Looking only at magnitudes of these standardized variables, the overall effects of the multipurpose cash program appear largely attributable to reductions in borrowing money, child labor, buying food on credit, reductions in essential consumption, and the selling of household goods. In the food e-card program, we see reductions in begging, borrowing money, exploitative child labor, and restricting essential consumption. Overall, beneficiary families are less likely to borrow (probably with high interest rates), reduce expenditure, downgrade their housing or have children engage in exploitative work. We then see fall-back effects after the programs end, with an increase in begging and borrowing money, child labor, buying food on credit and selling household goods. As with other outcomes, both programs help reduce the incidence of these coping behaviors, but families converge back to counterfactual levels and reengage in coping strategies after the cycle ends.

5.0.1 Robustness to bandwidth and polynomial

We show in Appendix Figures 7 through 9 that our main results are highly robust across a set of bandwidth and specification choices. Appendix Figure 7 presents our main specifications across a range of bandwidths, Appendix Figure 8 uses a uniform kernel weighting across bandwidths, and 9 estimates the RD specification with a second-order polynomial and triangular kernel weighting. None of these robustness exercises change our findings or conclusions.

Figure 10: Effect of assistance on livelihood coping index



Note: Figure depicts the effect of threshold assignment on the livelihood coping index on optimal bandwidth sample with local linear regression fits and associated 95% confidence intervals.

Table 7: Cash transfer effects on livelihood coping

	Reduced form:		IV LATE:	
	During program	6 mos. after program	During program	6 mos. after program
	(1)	(2)	(3)	(4)
Panel A: Multipurpose cash				
Program effect	−0.12	0.04	−0.15	0.11
	(0.06)	(0.10)	(0.07)	(0.24)
Benjamini-Hochberg q	0.085*	0.811	0.085*	0.811
Control Group Mean	0.06	0.07	0.06	0.1
Bandwidth	15.38	13.15	15.38	13.15
N	3,108	1,146	3,108	1,146
R^2	0.002	0.002	0.003	0.001
Panel B: Food e-card				
Program effect	−0.140	0.015	−0.178	0.026
	(0.063)	(0.091)	(0.080)	(0.159)
Benjamini-Hochberg q	0.068*	0.872	0.068*	0.872
Control Group Mean	0.08	0.07	0.08	0.12
Bandwidth	13.53	16.91	13.53	16.91
N	3,013	1,587	3,013	1,587
R^2	0.002	0.001	0.001	0.001

Note: This table reports estimates of the effect of cash transfers on an index of food coping strategies. The sample contains all the households within the optimal bandwidth based on the Calonico *et al.* (2019) algorithm. All regressions include survey year fixed effects, a linear term in the poverty score as well as its interaction with the indicator for being above the detected threshold. Estimations are triangular kernel-weighted. The specification, variable definitions, sample, bandwidth selector, and adjustments for multiple hypothesis testing were prespecified for all results contained in this table.

5.0.2 Heterogeneity analysis

Heterogeneity analyses along dimensions of household location and demographics can be found in Appendix Figure 10, in which we split samples based on whether the household has a particular characteristic as labeled on the vertical axis. There is minimal evidence of heterogeneity across these measures, with a small number of sub-samples different from their complement.

A few patterns do stand out, however, and are worth noting. Effects on expenditure from the multipurpose cash program are largest among small households, female-headed households, and those with higher education levels. These differences are likely mechanical, as the per-capita transfer is likely larger for these households due to the fact that the multipurpose cash program disburses a set dollar transfer per family regardless of size. We also observe interesting patterns of heterogeneity among effects on the child hardship index, where it appears low-educated households with a higher share of working age men available to support a smaller number of children (via the share of dependents) benefit the most from multipurpose cash in terms of improving children's education and work outcomes. There is some modest heterogeneity in the effects of multipurpose cash on health access and outcomes, in which higher educated families, those with a low share of working age men, and those with a high share of dependents were able to reduce negative health outcomes/access measures. The food e-card program – which scales proportionately with household size – has a larger effect on larger households, as well as those who are less educated, have a higher share of dependents, are male-headed, and/or have a low share of working-age males. There is little evidence of substantive effect heterogeneity along other dimensions.

5.1 Rent, housing, and debt

Table 8 shows that the cash program increases expenditure on rent by nearly 30 percent, 3.50 USD over a control mean of 12.8 USD. Much of this increase comes from households that begin paying any rent (column 2), with minimal evidence of changes in accommodation or eviction (columns 3 and 4). We thus conclude that increase in rent expenditures likely come about as a result of flexible rental arrangements in which refugees pay more when they are liquid.

Cash transfers and vouchers increase households' ability to pay down existing debts and may reduce the need to take on debt. At the same time, a positive income shock will also increase households' creditworthiness. We also estimate the direct effect of programs on holding any debt and the total debt stock held. The final three columns of Table 8 shows that cash program had only a marginally significant four percentage-point reduction in the probability of holding any debt stock – a small effect, given that 91% of counterfactual households hold some debt. We find suggestive effects on total debt stock (column 7): recipients of either program appear to reduce their debt stock by about 10 percent of the counterfactual mean, but these effects are not significant after correcting for multiple hypotheses.

Table 8: Cash transfer effects on rent, housing, and debt

	Outcome:						
	Rent expenditure (USD per capita)	HH paid any rent	HH changed accom. in past 6 mos	HH faced eviction recently	Expenditure on debt (USD per capita)	HH has any debt	ln(outstanding debt)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Multipurpose cash							
Above threshold	3.54	0.12	0.03	−0.002	0.55	−0.04	−0.10
	(1.07)	(0.03)	(0.02)	(0.01)	(0.54)	(0.02)	(0.06)
Benjamini-Hochberg q	0.004***	< 0.001***	0.136	0.895	0.363	0.096*	0.136
Control Group Mean	12.88	0.42	0.12	0.04	1.29	0.91	6.53
Bandwidth	15.15	18.3	16.69	14.1	19.03	14.11	15.62
N	3,058	3,636	3,347	2,868	3,773	2,868	2,855
R^2	0.01	0.01	0.002	0.01	0.003	0.03	0.01
Panel B: Food e-card							
Above threshold	−2.85	−0.06	−0.03	0.01	−0.73	0.01	−0.10
	(1.30)	(0.03)	(0.02)	(0.01)	(0.47)	(0.02)	(0.06)
Benjamini-Hochberg q	0.182	0.182	0.269	0.591	0.210	0.529	0.182
Control Group Mean	16.05	0.43	0.14	0.05	1.35	0.91	6.61
Bandwidth	13.98	13.55	13.5	13.57	14.6	14.57	17.05
N	3,113	3,015	3,006	3,018	3,238	3,233	3,391
R^2	0.03	0.02	0.01	0.01	0.001	0.01	0.01

Note: This table reports estimates of the effect of cash transfers on rent, housing, and debt outcomes. The sample contains all the households within the optimal bandwidth based on the Calonico *et al.* (2019) algorithm. All regressions include survey year fixed effects, a linear term in the poverty score as well as its interaction with the indicator for being above the detected threshold. Estimations are triangular kernel-weighted. The outcomes of the specifications reported in this table were not prespecified.
*q < .1; **q < .05; ***q < .01

6 Why don't effects sustain beyond the assistance cycle?

Despite the large transfer value in both programs and their immediate impact on a battery of well-being measures, families who benefited from a full cycle of assistance soon revert to a similar situation as otherwise comparable non-beneficiary peers. A limited capacity to save, difficulty in coping with large income shocks, debt traps, and the lack of supporting safety nets stand out as potential explanations (Blattman *et al.*, 2020, 2019; Karlan *et al.*, 2019; Banerjee *et al.*, 2011).

6.1 Savings and asset holdings

When households face borrowing constraints and high future discount rates, short-term savings and asset holdings buffer consumption against liquidity shocks (Deaton, 1989). This is especially salient in our setting given that the consumption basket of the extreme poor is inelastic, refugees are typically credit constrained, and their income is volatile.

We first investigate whether cash assistance induces savings and asset holdings. Table 9 provides the reduced-form estimates of saving behavior for beneficiaries of both programs. In Panel A, we see that multipurpose cash eligibility increases the likelihood of having any type of cash savings by an additional 7 percentage points beyond the non-beneficiary rate of 31 percent. Beneficiaries are also 9 percentage points (50 percent of the control mean) more likely to use savings to cope with insufficient liquidity during the same period. In other words, the unconditional cash helps families to save and use savings to adjust consumption when they are not liquid. The food assistance program does not translate to increased savings among recipients, neither ability to use savings to cope with liquidity. Panel B shows that at six months after the program, neither program beneficiaries have any differential savings or ability to use them to smooth consumption. In other words, the unconditional cash assistance allows beneficiaries to save temporarily but these savings quickly vanish to buffer consumption.

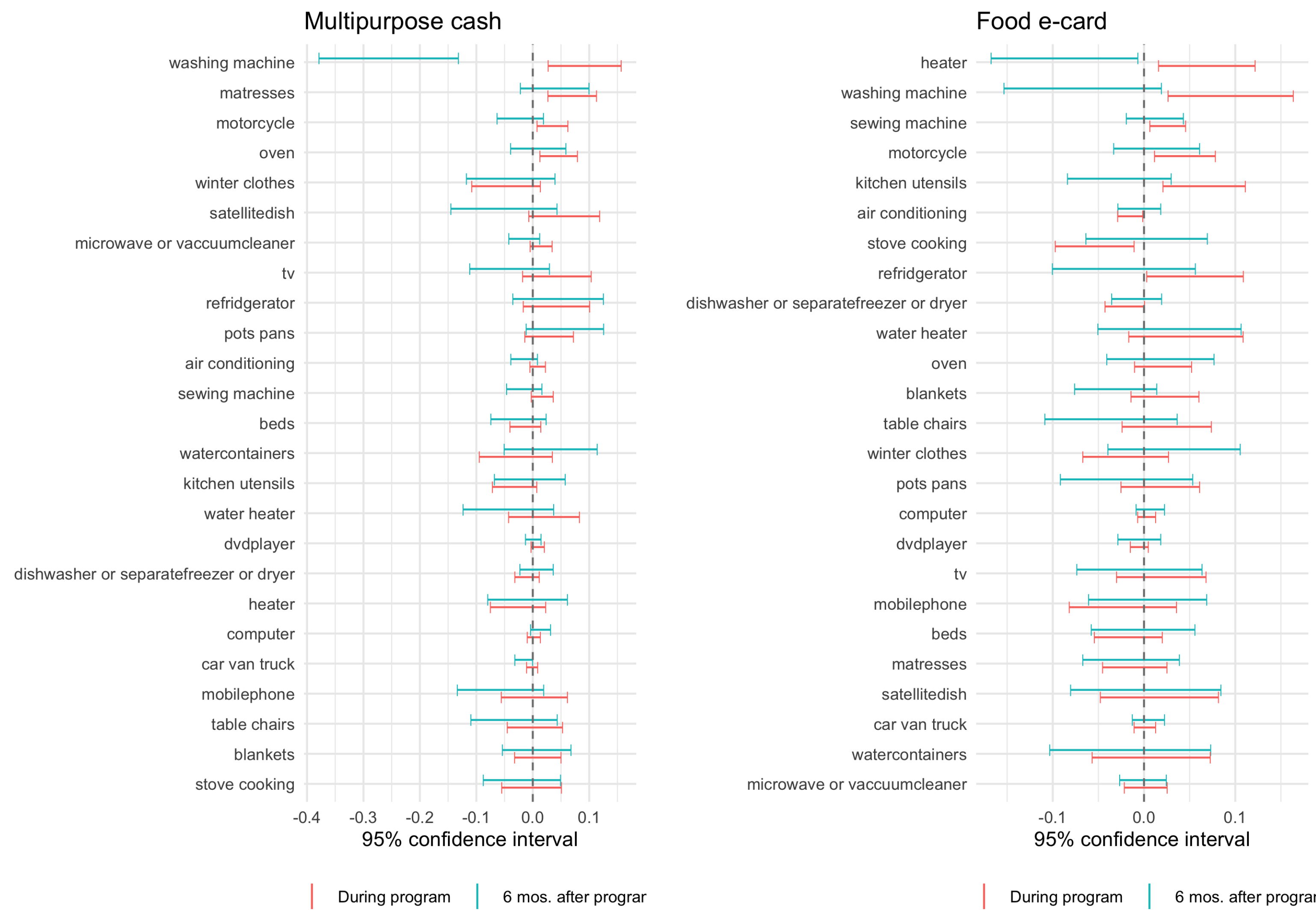
Durable goods allow households to engage in productive activity or serve as a non-traditional form of saving (Banerjee *et al.*, 2011). Figure 11 reports the confidence intervals from reduced-form estimates of program effects on ownership for the set of durable goods and household assets available in the survey data. We find direct effects on owning common basic durable goods from both programs, including ownership of washing machines, mattresses, heaters, ovens, and kitchen utensils. However, these positive effects revert to zero in the post-program period, with previous recipient households no longer more likely to own these items relative to non-recipients. This provides further evidence that households use transfers and voucher assistance to save, but need to liquidate savings within months after the program ends. In other words, durable good savings serve as a tool to adjust consumption to cope with negative income shocks.

Table 9: Cash transfer effects on savings

	Multipurpose cash:		Food e-card	
	HH has savings	HH spent savings to cope	HH has savings	HH spent savings to cope
	(1)	(2)	(3)	(4)
Panel A: During program				
Above threshold	0.07	0.09	−0.02	−0.02
	(0.04)	(0.03)	(0.03)	(0.02)
Benjamini-Hochberg q	0.055*	0.018**	0.587	0.587
Control Group Mean	0.31	0.18	0.32	0.21
Bandwidth	9.78	7	13.73	13.17
N	2,006	1,425	3,059	2,950
R^2	0.003	0.01	0.01	0.004
Panel B: 6 mos. after program				
Above threshold	−0.03	0.002	−0.01	−0.04
	(0.04)	(0.04)	(0.04)	(0.04)
Benjamini-Hochberg q	0.962	0.962	0.720	0.490
Control Group Mean	0.33	0.19	0.3	0.21
Bandwidth	18.4	15.45	18.1	14.54
N	1,617	1,367	1,682	1,392
R^2	0.004	0.01	0.001	0.003

Note: This table reports estimates of the effect of cash transfers on savings and their use. The sample contains all the households within the optimal bandwidth based on the Calonico *et al.* (2019) algorithm. All regressions include survey year fixed effects, a linear term in the poverty score as well as its interaction with the indicator for being above the detected threshold. Estimations are triangular kernel-weighted. The outcomes of the specifications reported in this table were not prespecified.
$^{*}q < .1$; $^{**}q < .05$; $^{***}q < .01$

Figure 11: During-program effects on asset ownership



Note: Figure depicts 95% confidence intervals from the estimation of 1 on indicators of whether a household reports having the indicated asset. Stars indicate significance levels according to multiple hypothesis-adjusted Benjamini-Hochberg q values, where $*q < .1$, $**q < .05$, $***q < .01$.

6.2 Income shocks

Given the evidence against sustained savings, we turn to testing whether fading program effects are a result of income shocks against which households exhaust their savings. Because the assistance cycle reshuffles a subset of households' assistance eligibility across years, the year-over-year transitions into and out of program eligibility generate large income shocks which are independent across years around the program thresholds that form the sample for our empirical design. In a simple counterfactual setting, our during-program intent-to-treat (ITT) effects can be expressed as:

$$\tau = E[y_t|d_{t-1} = 1] - E[y_t|d_{t-1} = 0] \tag{7}$$

where τ reflects the ITT effect conditional on the treatment indicator d_{t-1} determined in period $t-1$. We can then decompose τ into a weighted average of two separate ITT effects, conditional on previous eligibility versus not, captured in d_{t-2}. This decomposition is useful to analyze household behavior following an income shock:

$$\tau_1 = E[y_t|d_{t-1} = 1, d_{t-2} = 0] - E[y_t|d_{t-1} = 0, d_{t-2} = 0] \tag{8}$$

$$\tau_2 = E[y_t|d_{t-1} = 1, d_{t-2} = 1] - E[y_t|d_{t-1} = 0, d_{t-2} = 1] \tag{9}$$

$$\tau = \tau_1 \times P(d_{t-2} = 0) + \tau_2 \times P(d_{t-2} = 1) \tag{10}$$

where Equation 8 captures the impact of a positive income shock for non-beneficiary families who become newly eligible, *i.e.*, $d_{t-2} = 0$ and Equation 9 captures the difference between a continued beneficiary and families receiving a negative income shock via discontinuation, *i.e.*, $d_{t-2} = 1$. A similar τ_1 and τ_2 would provide evidence that program effects are a result of variation in contemporaneous, rather than past, income shocks. In other words, estimating a program effect τ that is independent of a past positive income shock, *i.e.*, d_{t-2}, indicates that previous transfers do not have any longer-term effects beyond the transfer cycle.

For estimation, we use a split-sample graphical analysis as well as a specification in which we use a modified version of Equation 1 that fully interacts the current and the previous assistance status, as in:

$$y_{i,t} = \alpha + \beta_1 d_{i,t-1} + \beta_2 d_{i,t-2} + \beta_3 (d_{i,t-1} \times d_{i,t-2}) + f(s_{i,t-1}) + \gamma_t + \varepsilon_{i,t} \tag{11}$$

where β_1 and $\beta_1 + \beta_3$ recover τ_1 and τ_2, respectively, and the interaction effect β_3 permits a test the null hypothesis of equal ITT effects conditional on prior assistance eligibility ($H_0 : \tau_2 = \tau_1$).

Tables 10 and 11 show the split sample effects as well as the parametric difference given by

Table 10: Multipurpose cash effects by previous assistance status

	Outcome (measured in current cycle):				
	ln(expenditure)	Child hardship	Poor health status and access	Food coping	Livelihood coping
	(1)	(2)	(3)	(4)	(5)
Panel A: Previous beneficiary: No (τ_1)					
above threshold for current cycle	0.17	-0.07	-0.04	-0.15	-0.11
	(0.04)	(0.08)	(0.09)	(0.07)	(0.08)
Benjamini-Hochberg q (interaction)	$< 0.001^{***}$	0.515	0.670	0.080^{*}	0.267
Control Group Mean	81.32	-0.1	0.04	0	0.06
Bandwidth	16.13	13.18	11.84	16.06	15.21
N	2,185	1,400	1,602	2,178	2,059
R^2	0.01	0.01	0.01	0.01	0.001
Panel B: Previous beneficiary: Yes (τ_2)					
above threshold for current cycle	0.18	-0.37	-0.08	0.08	-0.14
	(0.05)	(0.10)	(0.12)	(0.10)	(0.10)
Benjamini-Hochberg q (interaction)	$< 0.001^{***}$	$< 0.001^{***}$	0.527	0.506	0.288
Control Group Mean	73.4	-0.04	0.09	-0.02	0.05
Bandwidth	16.13	13.18	11.84	16.06	15.21
N	1,051	826	832	1,053	1,008
R^2	0.01	0.02	0.01	0.01	0.003
Panel C: Parametric difference ($\tau_2 - \tau_1$)					
difference (Panel B - Panel A)	0.01	-0.30	-0.04	0.23	-0.03
	(0.06)	(0.13)	(0.15)	(0.12)	(0.13)
Benjamini-Hochberg q (interaction)	0.935	0.090^{*}	0.935	0.130	0.935
Control Group Mean	79.22	-0.08	0.06	-0.01	0.05
Bandwidth	16.13	13.18	11.84	16.06	15.21
N	3,236	2,226	2,434	3,231	3,067
R^2	0.02	0.01	0.01	0.01	0.002

Note: This table reports estimates of the effect of cash transfers on outcome indicated in the column header, in which all terms in the main specification are interacted with an indicator for having received assistance in the prior cycle. The sample contains all the households within the optimal bandwidth based on the Calonico *et al.* (2019) algorithm. All regressions include survey year fixed effects, a linear term in the poverty score as well as its interaction with the indicator for being above the detected threshold. Estimations are triangular kernel-weighted. The specification, variable definitions, sample, bandwidth selector, and adjustments for multiple hypothesis testing were prespecified for all results contained in this table.
$^{*}q < .1$; $^{**}q < .05$; $^{***}q < .01$

Table 11: Food e-card effects by previous assistance status

	Outcome (measured in current cycle):				
	ln(expenditure)	Child hardship	Poor health status and access	Food coping	Livelihood coping
	(1)	(2)	(3)	(4)	(5)
Panel A: Previous beneficiary: No (τ_1)					
above threshold for current cycle	0.12	-0.09	0.13	-0.26	-0.10
	(0.05)	(0.08)	(0.07)	(0.08)	(0.08)
Benjamini-Hochberg q (interaction)	0.032^{**}	0.233	0.123	0.005^{***}	0.225
Control Group Mean	88.8	-0.08	0.02	0.14	0.04
Bandwidth	13.23	12.74	15.67	14.24	13.61
N	1,994	1,432	2,352	2,160	2,067
R^2	0.004	0.01	0.002	0.01	0.004
Panel B: Previous beneficiary: Yes (τ_2)					
above threshold for current cycle	0.0003	0.06	-0.13	-0.34	-0.21
	(0.06)	(0.10)	(0.10)	(0.10)	(0.11)
Benjamini-Hochberg q (interaction)	0.997	0.637	0.312	0.005^{***}	0.150
Control Group Mean	83.18	-0.23	0.04	0.21	0.16
Bandwidth	13.23	12.74	15.67	14.24	13.61
N	933	770	1,098	1,005	961
R^2	0.002	0.01	0.004	0.02	0.005
Panel C: Parametric difference ($\tau_2 - \tau_1$)					
difference (Panel B - Panel A)	-0.12	0.16	-0.27	-0.08	-0.11
	(0.08)	(0.13)	(0.12)	(0.13)	(0.13)
Benjamini-Hochberg q (interaction)	0.328	0.352	0.165	0.524	0.495
Control Group Mean	87.03	-0.14	0.03	0.16	0.08
Bandwidth	13.23	12.74	15.67	14.24	13.61
N	2,927	2,202	3,450	3,165	3,028
R^2	0.005	0.01	0.003	0.02	0.01

Note: This table reports estimates of the effect of cash transfers on outcome indicated in the column header, in which all terms in the main specification are interacted with an indicator for having received assistance in the prior cycle. The sample contains all the households within the optimal bandwidth based on the Calonico *et al.* (2019) algorithm. All regressions include survey year fixed effects, a linear term in the poverty score as well as its interaction with the indicator for being above the detected threshold. Estimations are triangular kernel-weighted. The specification, variable definitions, sample, bandwidth selector, and adjustments for multiple hypothesis testing were prespecified for all results contained in this table.
$^{*}q < .1$; $^{**}q < .05$; $^{***}q < .01$

the fully interacted version of Equation 1, given in Equation 11, for the multipurpose cash and food voucher programs, respectively. Appendix Figure 11 contains graphical results. We estimate remarkably similar and statistically indistinguishable effects of both programs for all the outcomes that we measure in our study independent of eligibility in the previous cycle. There is some suggestion that effects on children's education and work (among cash program recipients) improve over time, as well as health outcomes (for food voucher recipients), although these differences do not survive corrections for multiple hypothesis testing. Receiving assistance in a previous period does not appear to have major implications on the impact of current income transfers. These results suggest that the major determinant on expenditure and well-being are from contemporaneous income shocks, and provide further evidence against consumption smoothing, even with longer assistance cycles.

7 Discussion

We study two unconditional cash-based interventions that give poor refugee households in Lebanon a sizeable amount of money over the course of a year. During the program cycle, beneficiaries increase consumption, improve child welfare, increase food security, and reduce livelihood coping strategies. They allocate additional income to essential consumption goods, most notably rent, food, and energy. The documented effects are temporary, however, and do not persist even for six months after the program ends despite the size of the transfers received.

Exploring potential channels, we find evidence contrary to myopic behavior: households do not allocate income to temptation goods, they invest in durable goods, and children are taken out of work and put into school. We then provide evidence that both programs induce saving in cash and nontraditional forms. However, these savings are short-lived and spent to buffer consumption in response to liquidity shocks. We provide direct evidence that the impact of contemporaneous income shocks dwarf past income shocks in their effect on outcomes. Program benefits for continuing recipients are identical to those who are newly included, as effects on expenditure and other outcomes do not compound or accumulate across subsequent assistance cycles.

The volatility of income and expenses faced by poor households make sustained savings improbable to begin with, and costly and short-lived when possible. These findings are consistent with a multifaceted poverty trap in which sizeable cash transfers alone might not lead to sustained poverty alleviation (Banerjee *et al.*, 2015; Balboni *et al.*, 2020). The cash-based interventions that we investigate achieve what they are designed to – provide temporary relief to the extreme poor to help cope with day-to-day vulnerability – but despite the large transfer sizes, program effects do not last.

While refugees face unique challenges in any context, they share many of the same characteristics of the native poor: uncertainty and high volatility in income and expenses, migratory transience, exclusion from formal credit, insurance, savings, and labor markets, and the daily mental and physical strains common among the impoverished. Our results thus provide insight into a potential lower bound of the horizon on which positive effects of large cash-based interventions can be sustained, particularly when targeted to structurally excluded populations who lack access to supporting institutions and safety nets that protect against fall-backs.

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A Accordance with and departures from pre-analysis plan

A.1 Data processing

1. We did not impute missing data or infer variable corrections at the household level from other variables.

2. In the pre-analysis plan, we specified estimating outcomes at intervals of eight, 20, 32, and 44 months after the start of program cycles. This was changed to six and 18 due to a correction in our initial understanding of the program cycles start and end months, and small sample sizes for the 32 (30) and 44 (42) month analyses, mainly arising due to the lack of 2020 VASyR data.

3. We prespecified the potential to need to trim outliers up to the 1% tails, and we preformed this sample trimming due to a small number of inexplicably high values in the measure of expenditure per capita.

4. Also as provided for, we remove households who report zero expenditures in analyses of log expenditure per capita outcomes.

5. We did not perform any other discretionary sample cleaning or restrictions to the data.

A.2 Notes regarding unspecified dimensions of the analysis plan – *force majeure* adaptations

Various aspects of the context and situation that we learned only after working with the data and subsequent communications with partner organizations caused us to depart from certain dimensions of the pre-analysis plan. These were:

1. In 2016, both UNHCR and WFP used a hybrid approach to allocate assistance across households; for the multipurpose cash program, a strict threshold was applied only to those who had previously been receiving multipurpose cash in the prior cycle; therefore, our analysis limits the sample of households scored in 2016 to those that had been receiving in the 2015-16 cycle. WFP also used a hybrid approach in 2016, but this did not use a strict threshold – so analysis of food e-card for households scored in 2016 was dropped.

2. In December 2019, we had expected that the annual survey data (VASyR) would be collected similarly in 2020 and an additional round of data was provided for in the analysis plan. Due to COVID-19 pandemic, this data collection did not happen according to schedule, and was not available for analysis. The entire analysis thus relies on data from 2016 to 2019, rather than 2016 to 2020.

3. The preanalysis plan envisioned a single index for coping mechanisms, which combined food coping strategies and livelihood coping strategies. Given the starkly different nature of

these questions, and recommendations from partner organizations, we split the final primary hypothesis into two hypotheses and outcomes; the additional hypothesis has been factored into our corrections for multiple hypothesis testing throughout.

A.3 Differences from specified dimensions of the analysis plan

We deviated from the analysis plan in a number of small ways due to realizations about the measurement of quality of data fields we had intended to use. These were:

1. The use of "private clinic" in the health index was dropped, as it was not available in all years,
2. We did not use some of the indicators we believed would be indicative of a child working because they were not time-specific in the way that the primary measures of child work were asked,
3. We did not undertake heterogeneity analyses by the presence of children in the household or their demographic structure (it is largely redundant with the split on dependency share) or the presence of protection risks (disability, single woman, medical conditions, etc.) as these variables had low incidence, and
4. Finally, we did not pool the data across the two program cutoffs to estimate a joint effect as specified in the PAP, as such an analysis would generate a result that could be difficult to interpret given the different nature of the two programs (a voucher and unconditional cash).

B Additional Tables and Figures

Appendix Table 1

Statistic	N	Mean	St. Dev.
H2: Share of children not in school	4,911	0.3	0.4
H2: Share of children working	4,911	0.04	0.2
H2: Share 12-17 y.o. girls married	1,845	0.1	0.2
H3: Did not access primary healthcare	6,767	0.6	0.5
H3: Did not access hospital	6,767	0.2	0.4
H3: Presence of medical conditions for adults	6,767	0.1	0.2
H3: Presence of medical conditions for children	6,767	0.1	0.3
H3: Share 0-5 y.o children sick	4,564	0.3	0.4
Coping Food: Relied on less expensive food (# days)	6,767	4.8	2.7
Coping Food: Borrowed food (# days)	6,767	1.2	2.0
Coping Food: Reduced number of meals per day (# days)	6,767	2.9	2.9
Coping Food: Reduced portion size of meal (# days)	6,767	2.8	3.0
Coping Food: Went an entire day without eating (# days)	6,767	0.1	0.5
Coping Food: Restricted consumption of adults (# days)	6,767	2.1	2.9
Coping Food: Sent HH members to eat elsewhere (# days)	6,767	0.1	0.7
Coping Food: Restrict consumption of females (# days)	6,767	0.2	1.0
Coping Non-food: Borrowed money from high interest lender	6,296	0.5	0.5
Coping Non-food: Poor housing quality	6,767	0.6	0.5
Coping Non-food: Faced eviction	6,767	0.05	0.2
Coping Non-food: Sold HH goods	6,767	0.1	0.3
Coping Non-food: Sold assets	6,767	0.03	0.2
Coping Non-food: Reduce health expenditure	6,767	0.5	0.5
Coping Non-food: Reduce education expenditure	6,767	0.3	0.4
Coping Non-food: Spent some or all of HH savings	6,767	0.2	0.4
Coping Non-food: Bought food on credit	6,767	0.8	0.4
Coping Non-food: Sold house	6,767	0.01	0.1
Coping Non-food: Moved to cheaper rent	6,767	0.1	0.3
Coping Non-food: Withdrew children from school	6,767	0.1	0.3
Coping Non-food: Have 6-15 y.o children work	6,767	0.05	0.2
Coping Non-food: Asked for money from strangers	6,767	0.01	0.1
Coping Non-food: Older than 18 y.o accepting exploitative work	6,767	0.02	0.1
Coping Non-food: Under than 18 y.o accepting exploitative work	6,767	0.01	0.1
Coping Non-food: Sent an adult HH member to work elsewhere	6,767	0.02	0.1
Coping Non-food: Sent a child HH member to work elsewhere	6,767	0.01	0.1
Coping Non-food: Marriage of children under 18 y.o	6,767	0.01	0.1
Coping Non-food: Reduce food expenditure	6,767	0.8	0.4

Note: Table contains summary statistics for the sample of scored households sampled in the 2016-2019 survey rounds. Indices are constructed from the mean of unit-standardized values of index subcomponents.

Appendix Table 2: Detected and programmatic assignment thresholds across assistance programs and years

Program	Year	Type of threshold	Region	Detected threshold	Known programmatic threshold
MCAP/MPC	2016	national	all	87.14	87.00
MCAP/MPC	2017	regional	Bekaa	66.80	
MCAP/MPC	2017	regional	BML	72.61	
MCAP/MPC	2017	regional	North	66.22	
MCAP/MPC	2017	regional	South	68.01	
MCAP/MPC	2018	regional	Bekaa	57.11	
MCAP/MPC	2018	regional	BML	66.06	
MCAP/MPC	2018	regional	North	56.94	
MCAP/MPC	2018	regional	South	64.34	
WFP Cash for Food	2016	national	all		
WFP Cash for Food	2017	national	all	87.00	87.00
WFP Cash for Food	2018	national	all	71.67	

Notes: Table contains results of discontinuity grid search process and set thresholds (when applicable).

Appendix Table 3: Density test results across programs and assessment years

Program	Survey year	Bandwidth (L)	Bandwidth (R)	p-value	t-stat	N
Multipurpose Cash	2017	13.22	12.54	0.24	1.17	881
Multipurpose Cash	2018	19.87	18.66	0.73	0.35	3806
Multipurpose Cash	2019	13.25	9.49	0.75	-0.32	4577
Food e-card	2018	28.71	32.14	0.58	0.56	3806
Food e-card	2019	21	22.79	0.83	-0.21	4577

Notes: Table contains results of density test of manipulation in the forcing variable from McCrary (2008).

Appendix Table 4: Effect of assistance on primary outcomes measured pre-assignment

	Outcome measurement:				
	ln(EPC)	Child work/education/marriage (z)	Health access (z)	Food coping (z)	Non-food coping (z)
	(1)	(2)	(3)	(4)	(5)
Panel A: Multipurpose cash					
Above threshold	−0.08	0.09	−0.10	−0.03	0.10
	(0.04)	(0.06)	(0.08)	(0.06)	(0.06)
Benjamini-Hochberg q	0.270	0.399	0.399	0.858	0.382
Control Group Mean	76.58	-0.13	0.07	-0.01	0.06
Bandwidth	12.05	11.84	9.08	12.41	17.79
N	2,592	2,140	1,999	2,709	3,775
R^2	0.02	0.002	0.003	0.003	0.01
Panel B: Food e-card					
Above threshold	−0.02	0.07	0.002	−0.02	−0.08
	(0.04)	(0.06)	(0.06)	(0.06)	(0.06)
Benjamini-Hochberg q	0.858	0.399	0.979	0.892	0.399
Control Group Mean	86.11	-0.09	0.03	0.06	0.07
Bandwidth	12.69	16.06	13.37	15.61	12.84
N	2,981	2,822	3,198	3,649	3,079
R^2	0.01	0.002	0.001	0.001	0.003

Note: This table reports the results of a falsification test of assignment to assistance receipt on prior assistance receipt, The sample contains all the households within the optimal bandwidth based on the Calonico *et al.* (2019) algorithm. All regressions include survey year fixed effects, a linear term in the poverty score as well as its interaction with the indicator for being above the detected threshold. Estimations are triangular kernel-weighted. The specification, variable definitions, sample, bandwidth selector, and adjustments for multiple hypothesis testing were prespecified for all results contained in this table.

$^{*}q < .1$; $^{**}q < .05$; $^{***}q < .01$

Appendix Table 5: Effect of assistance on household characteristics measured pre-assignment

	Outcome measurement:			
	Avg. education yrs., adults	Household size	Share of dependents	Share working-age men
	(1)	(2)	(3)	(4)
Panel A: Multipurpose cash				
Above threshold	0.08	0.19	0.10	0.02
	(0.07)	(0.13)	(0.04)	(0.08)
Benjamini-Hochberg q	0.399	0.399	0.117	0.892
Control Group Mean	-0.22	5.9	0.4	-0.07
Bandwidth	10.44	13.74	16.94	11.39
N	2,058	2,997	3,605	2,181
R^2	0.01	0.02	0.01	0.01
Panel B: Food e-card				
Above threshold	−0.02	−0.31	−0.06	−0.01
	(0.06)	(0.12)	(0.05)	(0.06)
Benjamini-Hochberg q	0.892	0.117	0.399	0.892
Control Group Mean	-0.09	5.49	0.29	0.05
Bandwidth	11.72	13.61	12.62	13.23
N	2,788	3,268	3,024	2,827
R^2	0.005	0.01	0.01	0.003

Note: This table reports the results of a falsification test of assignment to assistance receipt on pre-assignment covariates, The sample contains all the households within the optimal bandwidth based on the Calonico *et al.* (2019) algorithm. All regressions include survey year fixed effects, a linear term in the poverty score as well as its interaction with the indicator for being above the detected threshold. Estimations are triangular kernel-weighted. The specification, variable definitions, sample, bandwidth selector, and adjustments for multiple hypothesis testing were prespecified for all results contained in this table.

$^{*}q < .1$; $^{**}q < .05$; $^{***}q < .01$

Appendix Table 6: Effect of Assistance on Subcomponents of Expenditure: During program

	Index subcomponent:										
	food	rent	health and hygiene	energy	hh appliances	education	comm and transport	debt	entertainment	alcohol & tobacco	other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Multipurpose cash											
Above threshold	−0.02	0.41***	0.21***	0.22***	0.03	0.11*	0.12**	0.12**	−0.001	0.07	0.004
	(0.05)	(0.10)	(0.07)	(0.05)	(0.03)	(0.07)	(0.05)	(0.05)	(0.005)	(0.05)	(0.02)
Benjamini-Hochberg q	0.805	< 0.001	0.005	< 0.001	0.461	0.179	0.036	0.036	0.839	0.292	0.875
Control Group Mean	33.71	13.92	14.64	9.39	1.53	3.14	5	2.39	1.01	1.81	1.81
Bandwidth	10.15	15.71	14.57	13.64	15.52	10.37	13.07	15.51	11.09	15.49	20.56
N	2,083	3,168	2,937	2,775	3,133	2,127	2,665	3,129	2,276	3,129	4,030
R^2	0.002	0.02	0.02	0.01	0.01	0.02	0.01	0.02	0.001	0.01	0.001
Panel B: Food e-card											
Above threshold	0.47***	−0.19*	0.06	0.06	−0.001	0.19***	0.13**	−0.05	0.003	0.08	−0.0001
	(0.05)	(0.10)	(0.07)	(0.06)	(0.03)	(0.07)	(0.06)	(0.05)	(0.01)	(0.05)	(0.03)
Benjamini-Hochberg q	< 0.001	0.146	0.544	0.542	0.998	0.053	0.088	0.542	0.744	0.311	0.998
Control Group Mean	34.59	17.35	16.77	9.93	1.34	3.91	5.47	2.51	1.02	3.06	2.08
Bandwidth	15.9	15.4	12.27	12.66	12.37	9.64	10.36	11	14.99	13.14	17.06
N	3,486	3,395	2,767	2,849	2,789	2,206	2,348	2,497	3,304	2,948	3,721
R^2	0.03	0.03	0.01	0.001	0.001	0.004	0.004	0.001	0.002	0.004	0.0003

*p < .1; **p < .05; ***p < .01

This table reports estimates of the effect of predicted expenditure per capita on the independent variables that compose the child education/work/marriage index. The dependent variables are the share of children working, the share of children not in school, and the share of females under 17 that are married. The sample contains all the households within the optimal bandwidth based on the Calonico *et al.* (2019) algorithm. All regressions include region and survey year fixed effects, a linear term in the poverty score as well as its interaction with the indicator for being above the detected threshold. Estimations are triangular kernel-weighted.

Appendix Table 7: Effect of Assistance on Subcomponents of Expenditure: 6 mos. after program

	Index subcomponent:										
	food	rent	health and hygiene	energy	hh appliances	education	comm and transport	debt	entertainment	alcohol & tobacco	other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Multipurpose cash											
Above threshold	-0.22^{***}	0.02	-0.16^{*}	−0.04	−0.01	−0.001	-0.29^{***}	0.17^{**}	0.00	−0.12	0.10^{**}
	(0.07)	(0.13)	(0.09)	(0.09)	(0.04)	(0.09)	(0.08)	(0.07)	(0.00)	(0.08)	(0.04)
Benjamini-Hochberg q	0.019	0.982	0.152	0.909	0.982	0.995	0.002	0.06	NaN	0.213	0.06
Control Group Mean	34.76	14.78	15.61	9.33	1.59	4.07	5.17	2.35	1	1.53	1.53
Bandwidth	14.43	20.95	15.84	11.99	20.81	17.87	10.91	17.28	9.55	14.64	15.08
N	1,263	1,814	1,406	1,045	1,806	1,580	958	1,536	817	1,281	1,325
R^2	0.01	0.002	0.01	0.004	0.003	0.01	0.01	0.02		0.02	0.005
Panel B: Food e-card											
Above threshold	0.03	−0.02	-0.16^{*}	0.02	0.03	−0.01	0.01	−0.02	−0.01	-0.26^{***}	−0.06
	(0.07)	(0.15)	(0.10)	(0.07)	(0.04)	(0.09)	(0.06)	(0.06)	(0.01)	(0.09)	(0.04)
Benjamini-Hochberg q	0.87	0.87	0.512	0.87	0.87	0.87	0.87	0.87	0.87	0.044	0.756
Control Group Mean	35.91	16.67	17.11	10.42	1.64	4.18	5.82	2.12	1.04	3.14	1.83
Bandwidth	18.06	15.71	14.34	15.7	18.47	20.1	15.86	18.28	29.17	10.87	17.24
N	1,679	1,490	1,382	1,487	1,712	1,822	1,499	1,695	2,355	1,054	1,613
R^2	0.002	0.0001	0.002	0.001	0.002	0.0003	0.003	0.002	0.002	0.01	0.002

$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$
This table reports estimates of the effect of predicted expenditure per capita on the independent variables that compose the child education/work/marriage index. The dependent variables are the share of children working, the share of children not in school, and the share of females under 17 that are married. The sample contains all the households within the optimal bandwidth based on the Calonico *et al.* (2019) algorithm. All regressions include region and survey year fixed effects, a linear term in the poverty score as well as its interaction with the indicator for being above the detected threshold. Estimations are triangular kernel-weighted.

Appendix Table 8: Effect of Assistance on Individual Variables for Child Education/Work/Marriage: During program

	Index subcomponent:		
	share of children working	share children not in school	share of U17 females married
	(1)	(2)	(3)
Panel A: Multipurpose cash, during program			
Above threshold	−0.18	−0.16	−0.10
	(0.06)	(0.07)	(0.06)
Benjamini-Hochberg q	0.015**	0.039**	0.098*
Control Group Mean	-0.01	-0.05	-0.2
Bandwidth	17.98	13.7	17.8
N	2,911	2,279	1,184
R^2	0.004	0.01	0.005
Panel B: Food e-card, during program			
Above threshold	−0.07	−0.02	0.08
	(0.06)	(0.07)	(0.07)
Benjamini-Hochberg q	0.408	0.773	0.408
Control Group Mean	-0.05	-0.11	-0.13
Bandwidth	13.23	13.58	17.23
N	2,243	2,285	1,035
R^2	0.001	0.004	0.01
Panel C: Multipurpose cash, 6 mos. after program			
Above threshold	−0.04	−0.04	0.16
	(0.10)	(0.09)	(0.08)
Benjamini-Hochberg q	0.729	0.729	0.153
Control Group Mean	0	-0.11	-0.21
Bandwidth	13.08	16.04	21.05
N	975	1,210	686
R^2	0.001	0.01	0.01
Panel D: Food e-card, 6 mos. after program			
Above threshold	0.02	−0.16	0.24
	(0.09)	(0.08)	(0.14)
Benjamini-Hochberg q	0.799	0.134	0.134
Control Group Mean	-0.05	-0.14	-0.16
Bandwidth	14.63	19.81	9.85
N	1,159	1,477	304
R^2	0.0004	0.003	0.01

This table reports estimates of the effect of predicted expenditure per capita on the independent variables that compose the child education/work/marriage index. The dependent variables are the share of children working, the share of children not in school, and the share of females under 17 that are married. The sample contains all the households within the optimal bandwidth based on the Calonico *et al.* (2019) algorithm. All regressions include region and survey year fixed effects, a linear term in the poverty score as well as its interaction with the indicator for being above the detected threshold. Estimations are triangular kernel-weighted.

Appendix Table 9: Effect of Assistance on Individual Variables for Health Access: During program

	Index subcomponent:				
	Child has med. cond.	Req'd hospital	Req'd primary health	Share children sick	Share HH w/ illness
	(1)	(2)	(3)	(4)	(5)
Panel A: Multipurpose cash, during program					
Above threshold	0.05	−0.05	0.02	−0.16	−0.04
	(0.07)	(0.06)	(0.06)	(0.09)	(0.05)
Benjamini-Hochberg q	0.575	0.575	0.748	0.430	0.575
Control Group Mean	0.12	0	0.08	-0.08	0.01
Bandwidth	17.61	19.94	13.79	9.51	19.65
N	3,529	3,919	2,811	1,350	3,874
R^2	0.004	0.002	0.005	0.01	0.003
Panel B: Food e-card, during program					
Above threshold	−0.04	0.06	0.06	−0.04	0.10
	(0.07)	(0.06)	(0.06)	(0.06)	(0.06)
Benjamini-Hochberg q	0.602	0.512	0.512	0.602	0.512
Control Group Mean	0.1	-0.02	0.01	-0.05	0.02
Bandwidth	13.66	14.83	17.07	20.32	10.59
N	3,041	3,274	3,721	2,923	2,406
R^2	0.002	0.001	0.002	0.0004	0.01
Panel C: Multipurpose cash, 6 mos. after program					
Above threshold	−0.10	−0.003	−0.10	0.26	−0.21
	(0.10)	(0.08)	(0.09)	(0.11)	(0.09)
Benjamini-Hochberg q	0.371	0.969	0.371	0.055*	0.055*
Control Group Mean	0.12	0.04	0.08	-0.12	-0.02
Bandwidth	18.15	20.9	15.72	16.21	11.35
N	1,604	1,809	1,392	943	990
R^2	0.004	0.001	0.01	0.01	0.01
Panel D: Food e-card, 6 mos. after program					
Above threshold	0.03	0.20	−0.07	0.17	0.09
	(0.09)	(0.09)	(0.09)	(0.11)	(0.07)
Benjamini-Hochberg q	0.717	0.125	0.534	0.265	0.358
Control Group Mean	0.08	-0.07	-0.02	-0.1	-0.04
Bandwidth	18.08	16.48	13.89	15.66	20.33
N	1,681	1,558	1,330	962	1,838
R^2	0.001	0.01	0.002	0.004	0.001

This table reports estimates of the effect of predicted expenditure per capita on the independent variables that compose the child education/work/marriage index. The dependent variables are the share of children working, the share of children not in school, and the share of females under 17 that are married. The sample contains all the households within the optimal bandwidth based on the Calonico *et al.* (2019) algorithm. All regressions include region and survey year fixed effects, a linear term in the poverty score as well as its interaction with the indicator for being above the detected threshold. Estimations are triangular kernel-weighted.

Appendix Table 10: Effect of Assistance on Individual Variables for Food Coping: During program

	Number of days in week that household:							
	borrowed food	without food	eating elsewhere	less expensive food	reduced meals	reduced portions	women restricted cons.	adults restricted cons.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Multipurpose cash, during program								
Above threshold	−0.13	0.003	0.02	−0.08	−0.04	−0.01	−0.04	0.05
	(0.05)	(0.05)	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Benjamini-Hochberg q	0.088^{*}	0.948	0.948	0.696	0.824	0.948	0.824	0.824
Control Group Mean	-0.08	-0.04	-0.01	0.04	-0.02	-0.06	0.05	0.06
Bandwidth	14.97	10.31	14.77	15.56	15.79	13.15	17.74	15.76
N	3,028	2,117	2,979	3,144	3,186	2,681	3,544	3,181
R^2	0.02	0.01	0.01	0.01	0.01	0.01	0.001	0.003
Panel B: Food e-card, during program								
Above threshold	−0.19	−0.22	−0.06	−0.10	−0.22	−0.15	−0.10	−0.15
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)
Benjamini-Hochberg q	0.005^{***}	$< 0.001^{***}$	0.277	0.120	$< 0.001^{***}$	0.016^{**}	0.137	0.035^{**}
Control Group Mean	0.06	0.03	0.01	0.11	0.13	0.16	0.02	0.18
Bandwidth	13.1	13.07	15.41	13.98	13.96	16.24	16.21	13.52
N	2,941	2,936	3,396	3,113	3,111	3,549	3,542	3,013
R^2	0.01	0.01	0.001	0.003	0.01	0.01	0.002	0.005
Panel C: Multipurpose cash, 6 mos. after program								
Above threshold	0.15	−0.02	0.15	0.07	−0.05	0.07	−0.07	0.17
	(0.08)	(0.07)	(0.07)	(0.11)	(0.09)	(0.10)	(0.08)	(0.11)
Benjamini-Hochberg q	0.299	0.739	0.288	0.671	0.711	0.671	0.671	0.299
Control Group Mean	-0.06	-0.08	-0.05	0.11	0.1	0.06	0.01	0.17
Bandwidth	15.48	15.18	22.78	11.46	15.59	13.4	16.08	12.25
N	1,370	1,334	1,951	997	1,376	1,169	1,431	1,068
R^2	0.003	0.01	0.004	0.02	0.02	0.03	0.002	0.02
Panel D: Food e-card, 6 mos. after program								
Above threshold	0.21	−0.02	0.01	0.01	−0.001	−0.10	−0.03	−0.28
	(0.10)	(0.06)	(0.08)	(0.08)	(0.09)	(0.09)	(0.10)	(0.10)
Benjamini-Hochberg q	0.120	0.988	0.988	0.988	0.988	0.744	0.988	0.048^{**}
Control Group Mean	-0.03	0.01	0	0.15	0.12	0.14	0.06	0.24
Bandwidth	13.72	27.69	12.18	16.05	18.11	15.05	16	13.39
N	1,312	2,281	1,170	1,515	1,684	1,440	1,510	1,282
R^2	0.004	0.002	0.01	0.0001	0.0000	0.001	0.001	0.01

This table reports estimates of the effect of predicted expenditure per capita on the individual food coping variables. The dependent variables are reported in the number of days each mechanism occured during a week. The sample contains all the households within the optimal bandwidth based on the Calonico *et al.* (2019) algorithm. All regressions include district and survey year fixed effects, a linear term in the poverty score as well as its interaction with the indicator for being above the detected threshold. Estimations are triangular kernel-weighted.

Appendix Table 11: Effect of Assistance on Individual Variables for Non-Food Coping: During program

Coping subcomponent:

Panel A: Multipurpose cash, during program

	begging	borrowed money	exploitative work (child)	child labor	child marriage	child works elsewhere	exploitative work (adult)	faced eviction	bought food on credit	moved due to high rent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Above threshold	0.12	−0.11	0.03	−0.13	−0.07	0.04	−0.03	0.005	−0.23	−0.20
	(0.04)	(0.07)	(0.08)	(0.08)	(0.06)	(0.06)	(0.07)	(0.06)	(0.07)	(0.07)
Benjamini-Hochberg q	0.033**	0.231	0.823	0.231	0.430	0.663	0.792	0.944	0.020**	0.033**
Control Group Mean	-0.05	0.11	-0.05	0.08	0.03	0	0.01	-0.02	0.1	-0.03
Bandwidth	14.22	13.06	8.73	13.97	14.46	15.39	13.58	13.73	12.07	9.79
N	2,880	2,497	1,781	2,848	2,924	3,110	2,765	2,791	2,482	2,009
R^2	0.005	0.02	0.005	0.001	0.003	0.001	0.0004	0.0002	0.01	0.01

Panel B: Multipurpose cash, during program

	child out of school	poor housing quality	reduced educ. exp.	reduced essentials	reduced food	sold prod. assets	sold goods	sold house	spent savings	work elsewhere (adult)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Above threshold	−0.10	−0.04	−0.01	−0.10	−0.06	0.16	−0.10	−0.09	0.22	−0.05
	(0.08)	(0.07)	(0.06)	(0.06)	(0.06)	(0.08)	(0.06)	(0.06)	(0.09)	(0.06)
Benjamini-Hochberg q	0.396	0.746	0.868	0.231	0.523	0.176	0.231	0.282	0.045**	0.523
Control Group Mean	0.1	0.03	0.03	0.05	0.03	0.03	-0.03	0.02	-0.08	-0.01
Bandwidth	13.75	12.12	14.69	16.82	14.01	11.44	16.44	15.13	6.98	15.98
N	2,800	2,489	2,959	3,370	2,856	2,355	3,306	3,054	1,421	3,216
R^2	0.002	0.004	0.002	0.001	0.001	0.003	0.003	0.002	0.01	0.004

Panel C: Multipurpose cash, 6 mos. after program

	begging	borrowed money	exploitative work (child)	child labor	child marriage	child works elsewhere	exploitative work (adult)	faced eviction	bought food on credit	moved due to high rent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Above threshold	0.18	0.13	−0.02	0.09	−0.27	0.06	0.02	−0.07	0.12	0.12
	(0.07)	(0.10)	(0.10)	(0.13)	(0.10)	(0.12)	(0.08)	(0.09)	(0.08)	(0.07)
Benjamini-Hochberg q	0.070*	0.376	0.920	0.767	0.070*	0.852	0.920	0.767	0.376	0.376
Control Group Mean	-0.06	0.1	0.03	0.08	0.02	0.02	-0.05	0.04	0.04	-0.07
Bandwidth	20.45	14.58	16.13	13.98	14.07	12.73	13.21	13.51	16.69	17.56
N	1,780	1,241	1,434	1,224	1,234	1,112	1,148	1,181	1,487	1,554
R^2	0.005	0.02	0.002	0.002	0.01	0.003	0.001	0.01	0.01	0.004

Panel D: Multipurpose cash, 6 mos. after program

	child out of school	poor housing quality	reduced educ. exp.	reduced essentials	reduced food	sold prod. assets	sold goods	sold house	spent savings	work elsewhere (adult)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Above threshold	−0.16	0.05	−0.04	0.18	0.13	−0.06	−0.01	−0.23	0.002	0.11
	(0.13)	(0.09)	(0.09)	(0.10)	(0.10)	(0.10)	(0.09)	(0.09)	(0.09)	(0.08)
Benjamini-Hochberg q	0.420	0.852	0.856	0.376	0.376	0.792	0.947	0.080*	0.984	0.376
Control Group Mean	0.13	0.08	0.08	0.02	0.01	0.06	0.01	0.03	-0.05	-0.05
Bandwidth	12.49	13.73	17.05	13.66	14.72	18.83	14.91	15.95	15.36	17.14
N	1,095	1,202	1,513	1,196	1,286	1,644	1,308	1,416	1,355	1,521
R^2	0.01	0.02	0.01	0.004	0.004	0.002	0.005	0.01	0.01	0.004

This table reports estimates of the effect of predicted expenditure per capita on months of assistance received. The dependent variable is the number of months a household has received assistance from the program. The sample contains all the households within the optimal bandwidth based on the Calonico *et al.* (2019) algorithm. All regressions include district and survey year fixed effects, a linear term in the poverty score as well as its interaction with the indicator for being above the detected threshold. Estimations are triangular kernel-weighted.

Appendix Table 12: Effect of Assistance on Individual Variables for Non-Food Coping: During program

Coping subcomponent:

Panel A: Food e-card, during program

	begging	borrowed money	exploitative work (child)	child labor	child marriage	child works elsewhere	exploitative work (adult)	faced eviction	bought food on credit	moved due to high rent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Above threshold	−0.09	−0.18	−0.16	0.04	0.05	0.02	−0.08	0.05	0.06	−0.06
	(0.05)	(0.07)	(0.06)	(0.07)	(0.07)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Benjamini-Hochberg q	0.444	0.110	0.110	0.783	0.711	0.869	0.522	0.711	0.577	0.577
Control Group Mean	0.01	0.07	0.03	0.04	0.01	-0.01	-0.01	0.01	0.07	0.01
Bandwidth	18.03	10.67	13.06	15.29	13.03	15.81	15.35	12.86	12.48	15.97
N	3,895	2,278	2,932	3,374	2,925	3,467	3,386	2,896	2,811	3,502
R^2	0.003	0.01	0.005	0.002	0.001	0.0005	0.001	0.0004	0.001	0.001

Panel B: Food e-card, during program

	child out of school	poor housing quality	reduced educ. exp.	reduced essentials	reduced food	sold prod. assets	sold goods	sold house	spent savings	work elsewhere (adult)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Above threshold	−0.08	−0.02	−0.10	−0.11	−0.07	−0.005	0.0001	−0.07	−0.06	−0.02
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Benjamini-Hochberg q	0.522	0.865	0.444	0.433	0.522	0.981	0.999	0.522	0.577	0.869
Control Group Mean	0.05	0	0.12	0.05	0.01	-0.02	0.06	0	0	0.01
Bandwidth	14.99	13.35	14.01	14.33	14.5	15.99	13.95	16.33	13.08	17.52
N	3,304	2,985	3,117	3,184	3,221	3,507	3,110	3,562	2,937	3,799
R^2	0.001	0.001	0.003	0.002	0.002	0.0005	0.003	0.0005	0.01	0.001

Panel C: Food e-card, 6 mos. after program

	begging	borrowed money	exploitative work (child)	child labor	child marriage	child works elsewhere	exploitative work (adult)	faced eviction	bought food on credit	moved due to high rent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Above threshold	0.16	0.20	−0.08	0.21	0.03	0.18	0.05	−0.34	0.15	−0.02
	(0.09)	(0.09)	(0.11)	(0.12)	(0.10)	(0.09)	(0.10)	(0.14)	(0.08)	(0.09)
Benjamini-Hochberg q	0.192	0.170	0.704	0.192	0.784	0.176	0.738	0.170	0.192	0.792
Control Group Mean	-0.01	-0.02	0.02	0.03	0.04	-0.04	0.04	0.11	-0.01	0.01
Bandwidth	10.79	14.55	16.43	10.76	17.57	14.75	14.58	10.12	17.92	17.08
N	1,045	1,393	1,555	1,039	1,640	1,412	1,396	976	1,665	1,600
R^2	0.01	0.01	0.001	0.004	0.0005	0.004	0.002	0.01	0.004	0.001

Panel D: Food e-card, 6 mos. after program

	child out of school	poor housing quality	reduced educ. exp.	reduced essentials	reduced food	sold prod. assets	sold goods	sold house	spent savings	work elsewhere (adult)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Above threshold	−0.03	0.04	0.06	−0.21	0.07	−0.12	0.14	−0.34	−0.10	0.06
	(0.09)	(0.08)	(0.10)	(0.10)	(0.10)	(0.09)	(0.09)	(0.11)	(0.09)	(0.08)
Benjamini-Hochberg q	0.784	0.738	0.738	0.170	0.704	0.368	0.207	0.020^{**}	0.445	0.704
Control Group Mean	0.05	0.02	0.08	0.01	-0.02	0.03	0.02	0.06	0	-0.04
Bandwidth	16.65	18.86	14.09	13.18	12.48	15.74	19.01	15.61	14.54	13.53
N	1,568	1,740	1,360	1,263	1,192	1,491	1,753	1,476	1,392	1,294
R^2	0.0001	0.0002	0.001	0.005	0.004	0.001	0.003	0.01	0.003	0.001

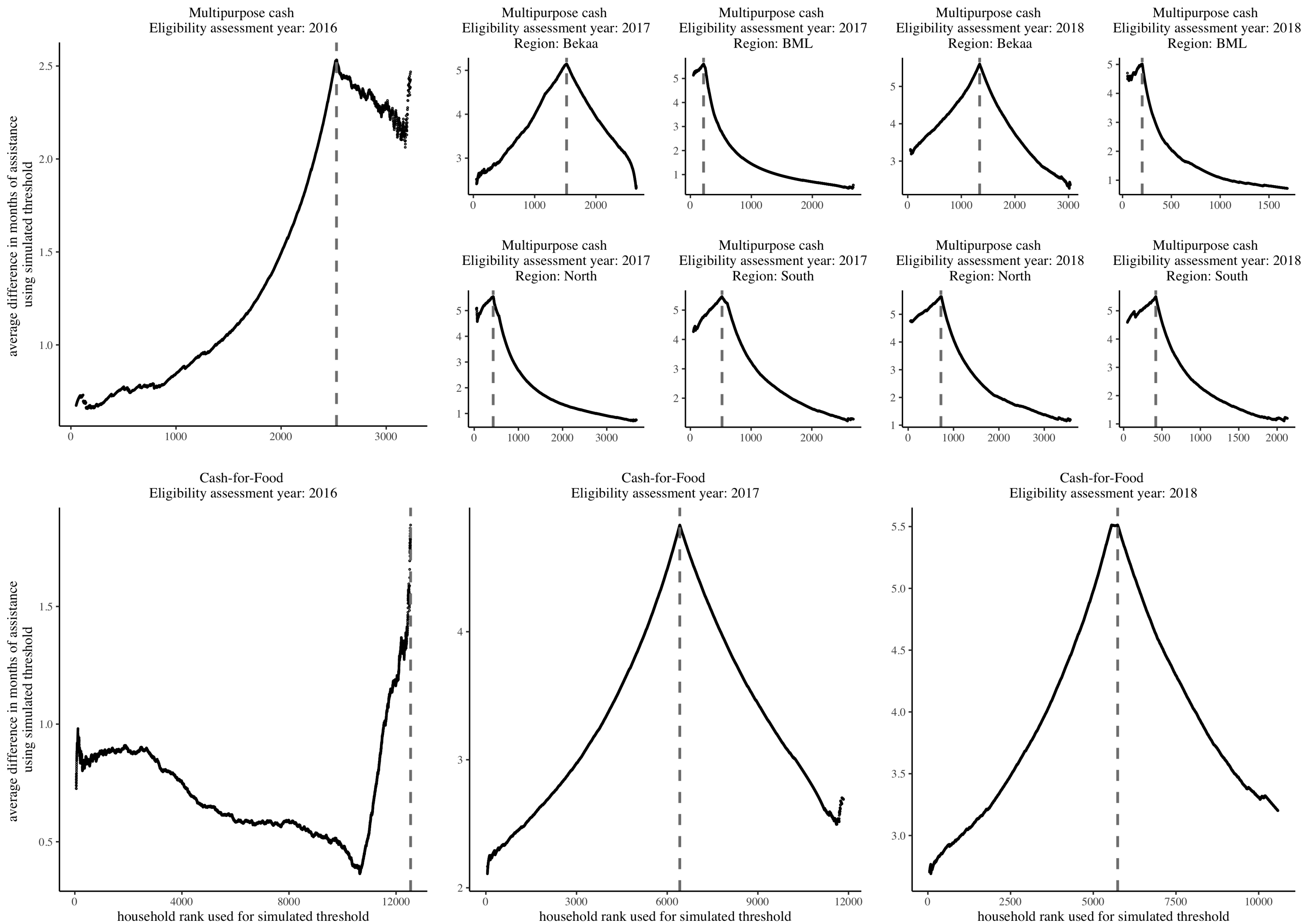
This table reports estimates of the effect of predicted expenditure per capita on months of assistance received. The dependent variable is the number of months a household has received assistance from the program. The sample contains all the households within the optimal bandwidth based on the Calonico *et al.* (2019) algorithm. All regressions include district and survey year fixed effects, a linear term in the poverty score as well as its interaction with the indicator for being above the detected threshold. Estimations are triangular kernel-weighted.

Appendix Table 13: Cash transfer effects on adult and child labor supply

	Outcome:					
	Share adult men working	Share adult women working	Share boys working	Share boys out of school	Share girls working	Share girls out of school
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Multipurpose cash						
Above threshold	0.09	0.02	−0.05	−0.13	−0.01	−0.02
	(0.04)	(0.02)	(0.02)	(0.04)	(0.01)	(0.04)
Benjamini-Hochberg q	0.044**	0.555	0.006***	0.006***	0.656	0.669
Control Group Mean	0.51	0.07	0.06	0.3	0.02	0.26
Bandwidth	9.41	13.69	12.46	12.28	16.96	11.8
N	1,657	2,733	1,661	1,643	2,150	1,553
R^2	0.05	0.005	0.01	0.01	0.002	0.01
Panel B: Food e-card						
Above threshold	0.05	−0.03	−0.02	0.003	−0.01	0.001
	(0.03)	(0.01)	(0.02)	(0.03)	(0.01)	(0.03)
Benjamini-Hochberg q	0.240	0.240	0.466	0.979	0.790	0.979
Control Group Mean	0.59	0.07	0.06	0.28	0.01	0.24
Bandwidth	15.81	14.3	16.47	14.75	14.93	15.44
N	3,095	3,098	2,112	1,924	1,890	1,946
R^2	0.07	0.002	0.002	0.005	0.003	0.003

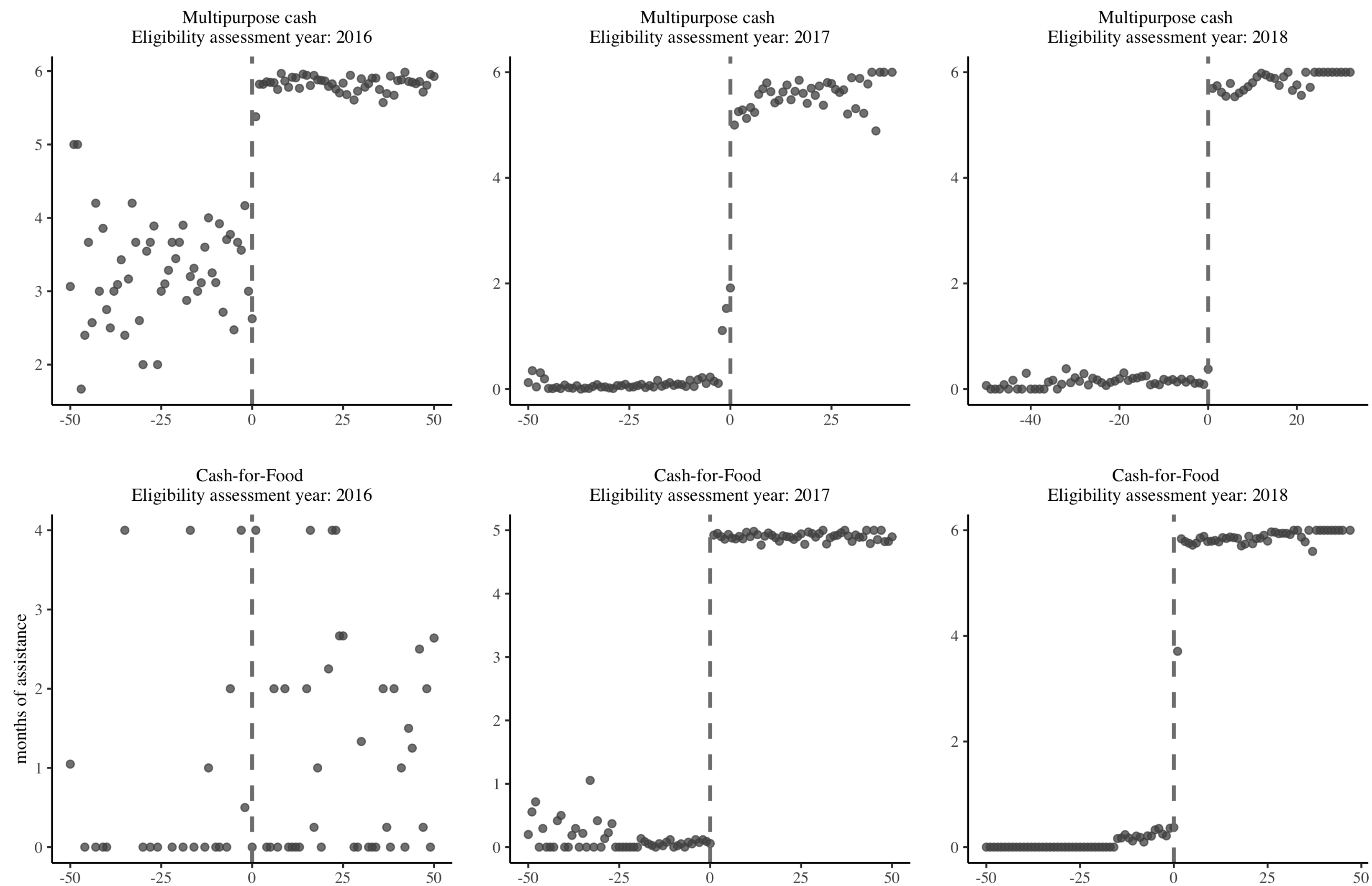
Note: This table reports estimates of the effect of cash transfers on labor supply and education outcomes. The sample contains all the households within the optimal bandwidth based on the Calonico *et al.* (2019) algorithm. All regressions include survey year fixed effects, a linear term in the poverty score as well as its interaction with the indicator for being above the detected threshold. Estimations are triangular kernel-weighted. The outcomes of the specifications reported in this table were not prespecified.
$^{*}q < .1$; $^{**}q < .05$; $^{***}q < .01$

Appendix Figure 1: Depiction of threshold detection procedure



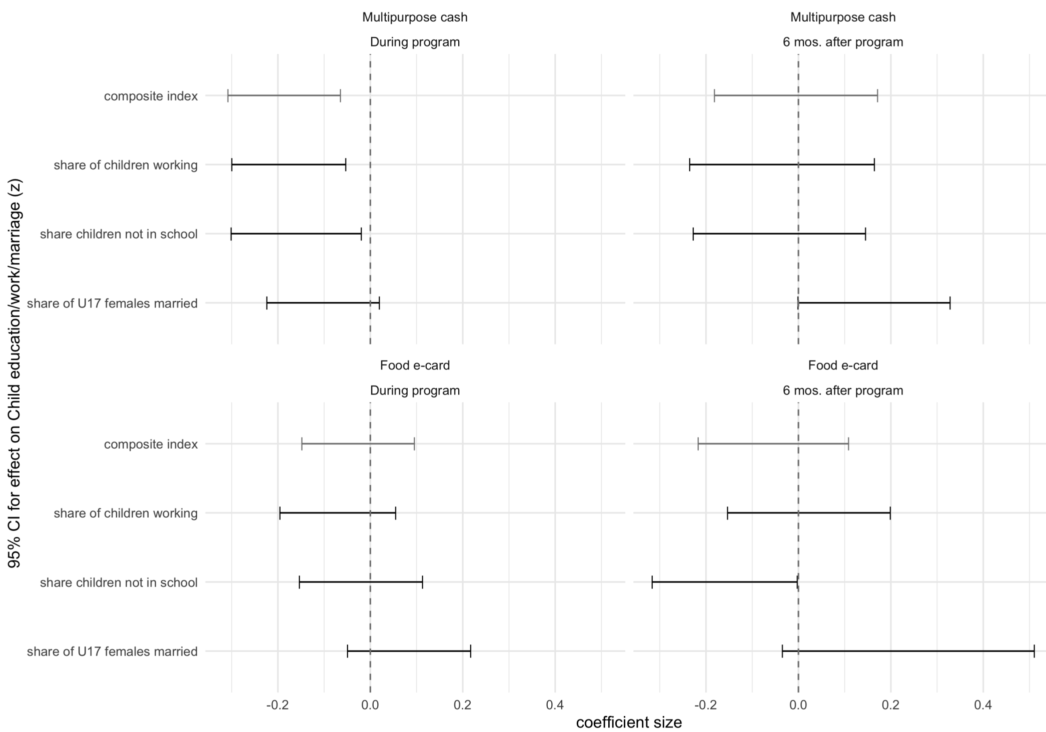
Note: Figure contains graphical depiction of the grid search process used to detect the discontinuity in assignment probability across programs and years.

Appendix Figure 2: Depiction of first stage, by year



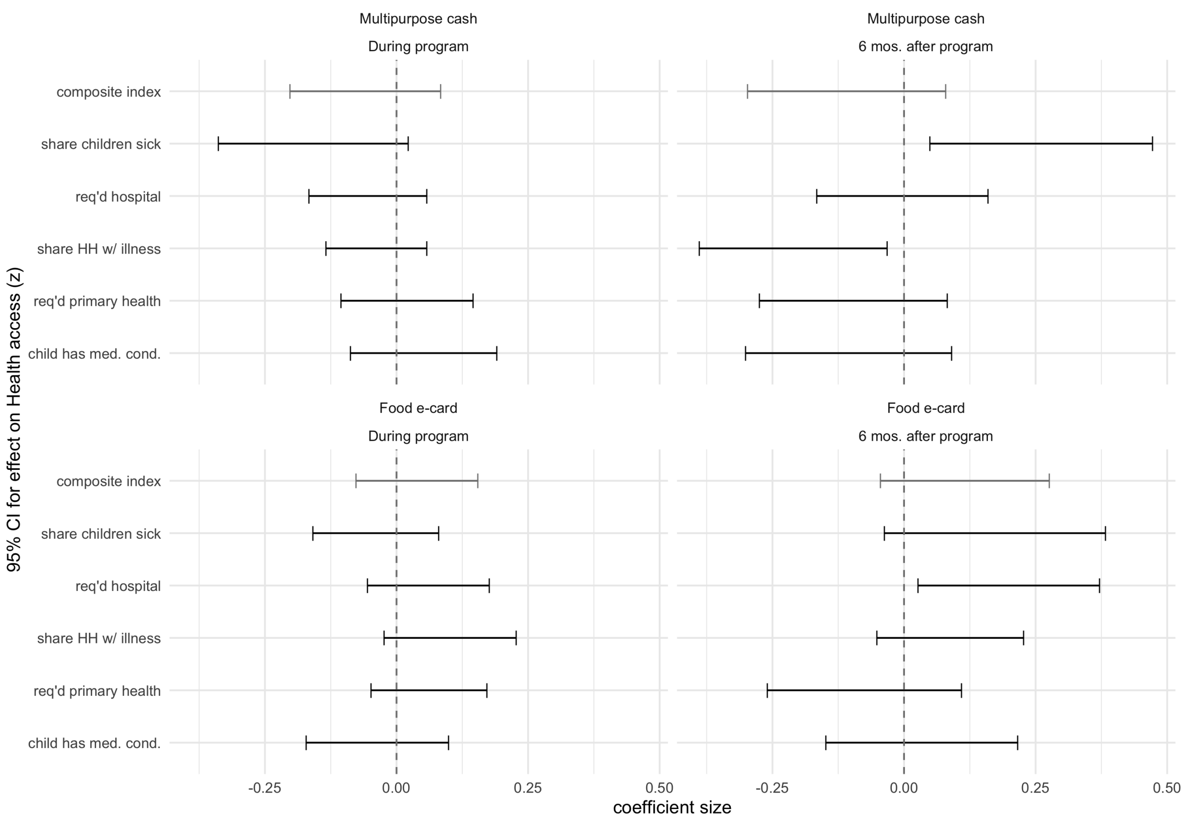
Note: Figure depicts first-stage of assignment discontinuity as measured in monthly dollars of assistance per capita.

Appendix Figure 3: During and after program effects on child hardship index subcomponents



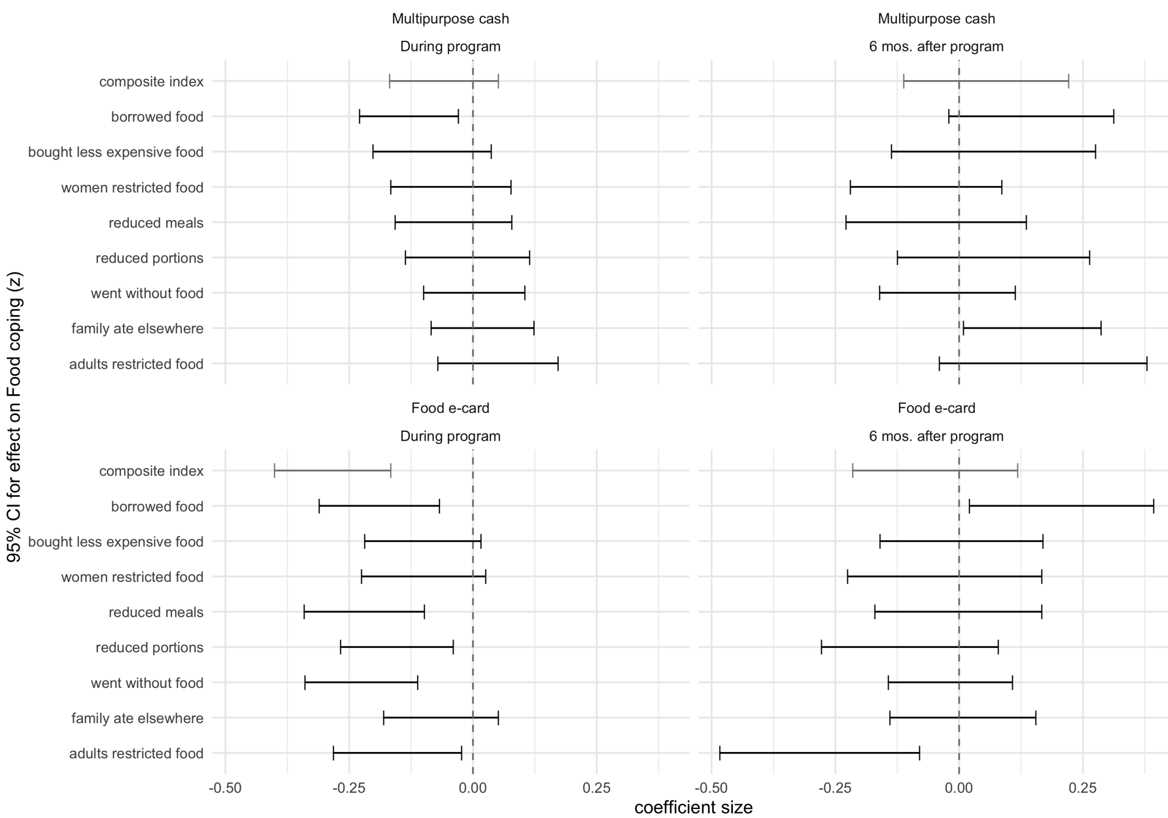
Note: Figure depicts the parametric effect of threshold assignment on the child hardship index and its constituent components. Spans indicate 95% confidence interval.

Appendix Figure 4: During and after program effects on health index subcomponents



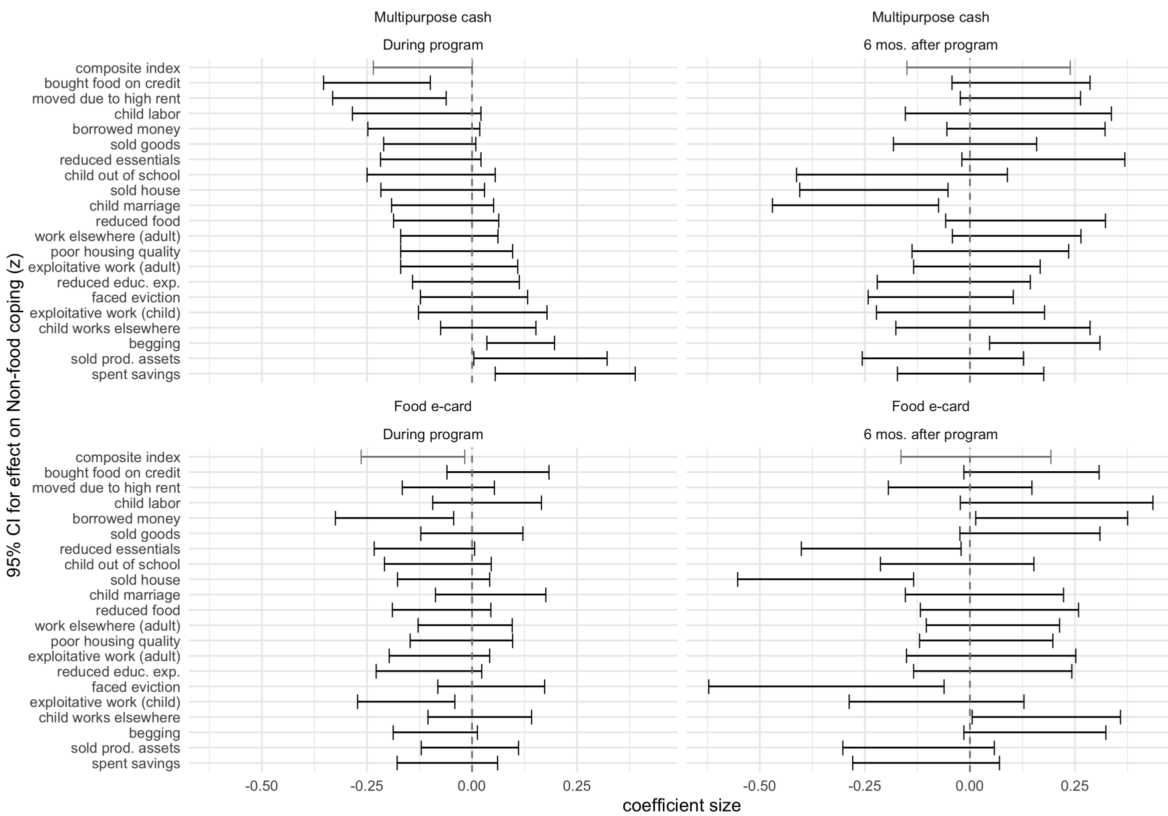
Note: Figure depicts the parametric effect of threshold assignment on the health status and access index and its constituent components. Spans indicate 95% confidence interval.

Appendix Figure 5: Effects of transfers on subcomponents of the food coping strategy index



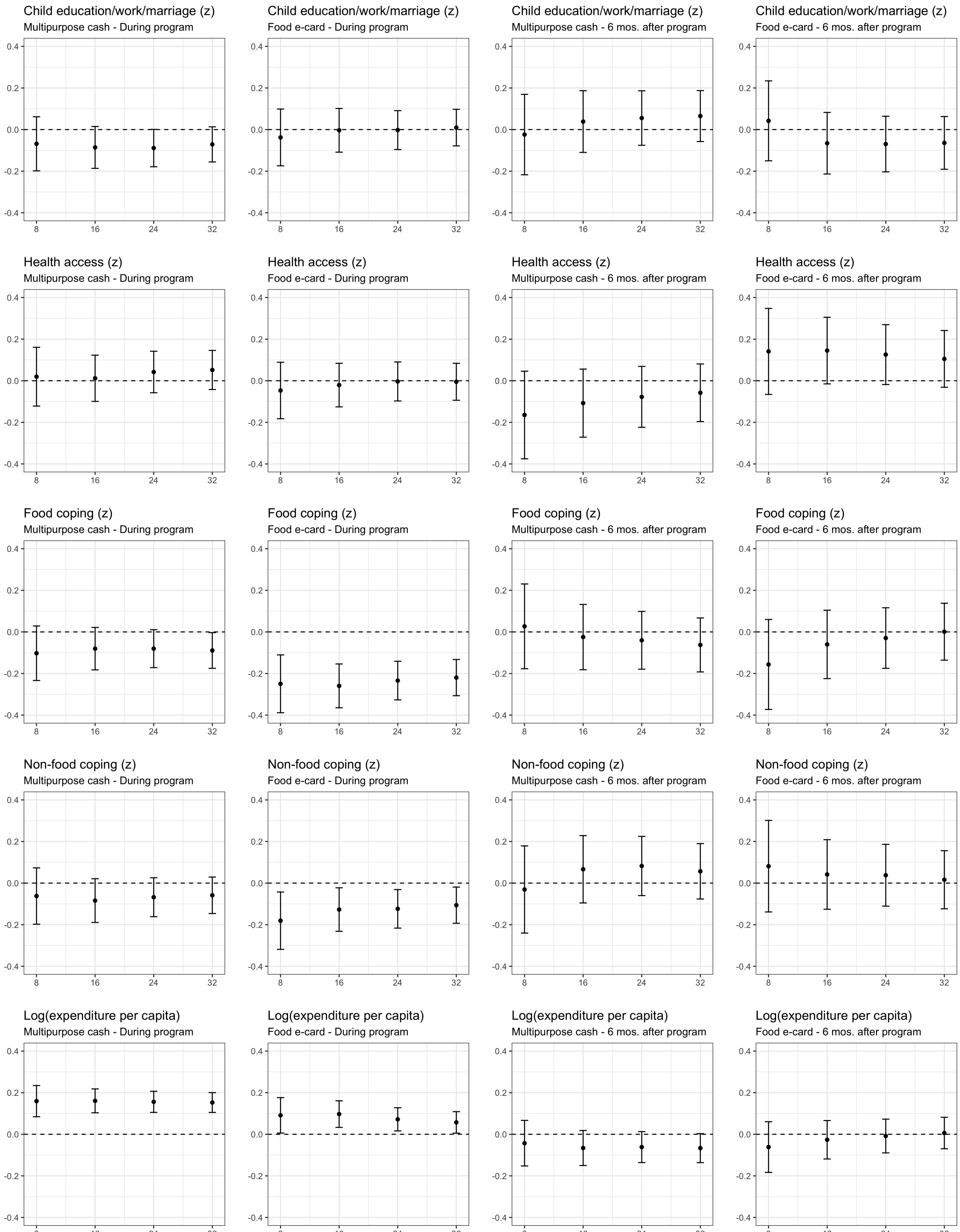
Note: Figure depicts the parametric effect of threshold assignment on the food coping index and its constituent components. Spans indicate 95% confidence interval.

Appendix Figure 6: Effects of transfers on subcomponents of the livelihood coping index



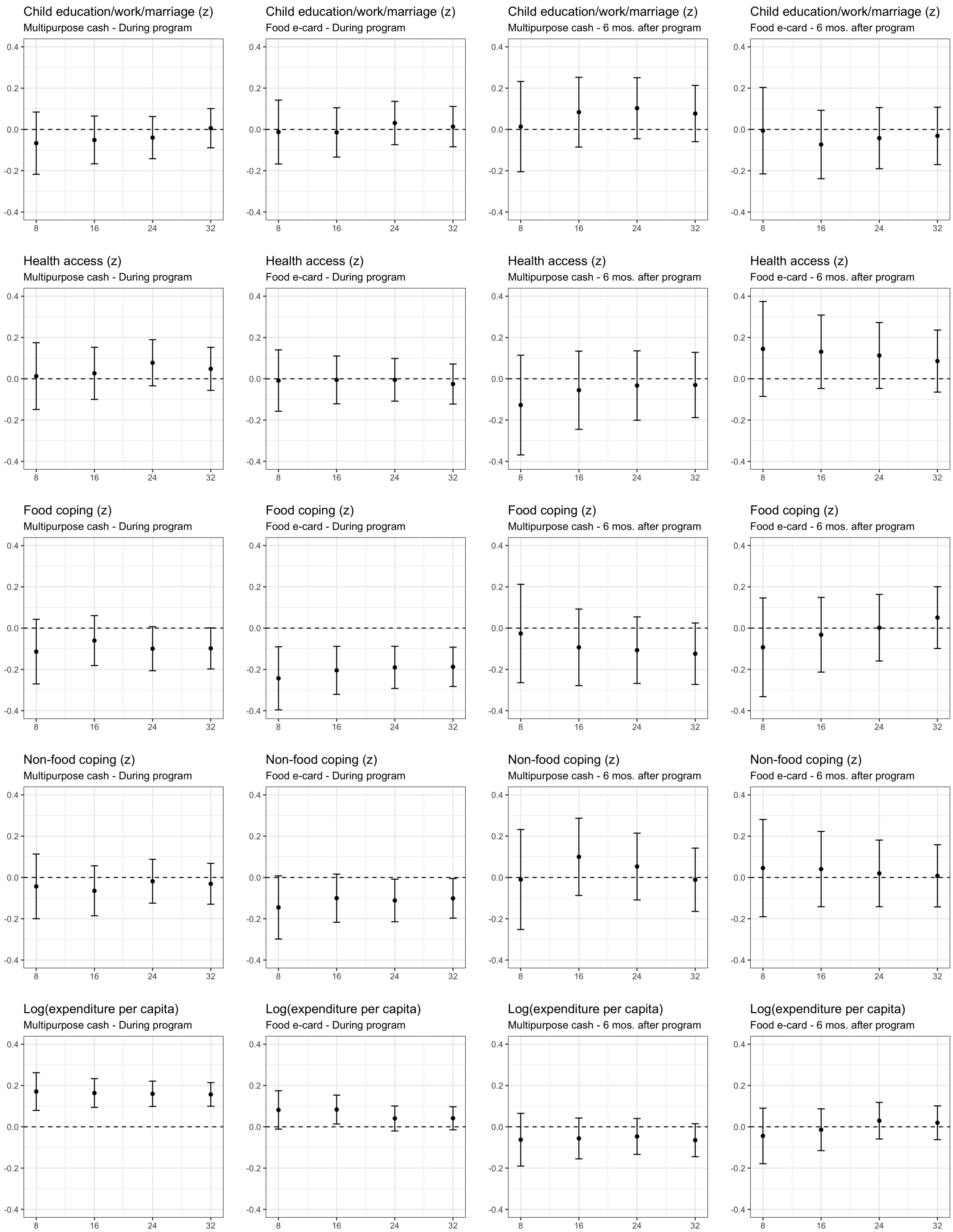
Note: Figure depicts the parametric effect of threshold assignment on the livelihood coping index and its constituent components. Spans indicate 95% confidence interval.

Appendix Figure 7: Robustness: local linear specification with triangular kernel weights, varying bandwidths



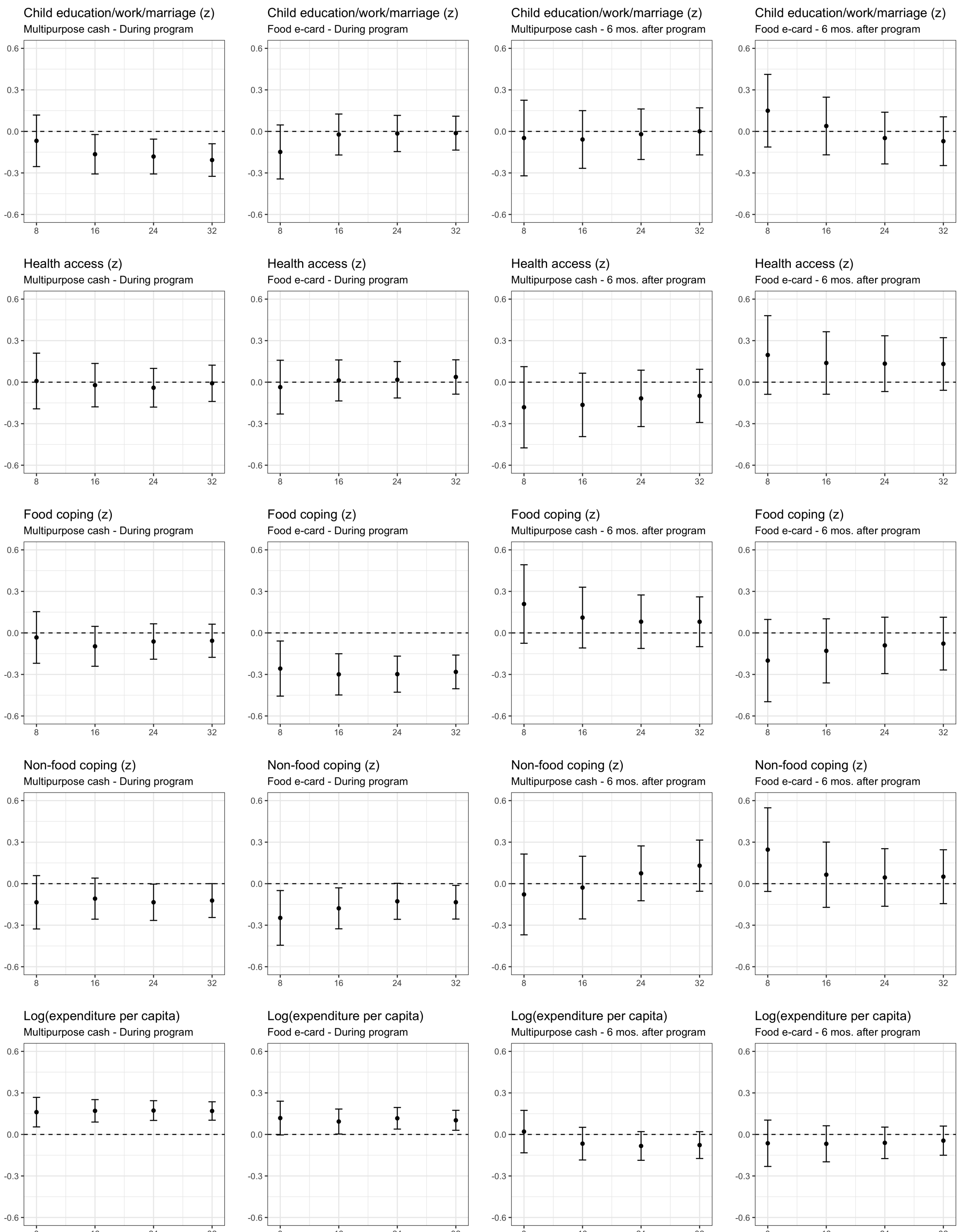
Note: Figure contains graphical depiction of coeffecents from robustness specifications that vary the bandwidth used to determine the sample. These specifications were not prespecified.

Appendix Figure 8: Robustness: local linear specification with uniform kernel weights, varying bandwidths



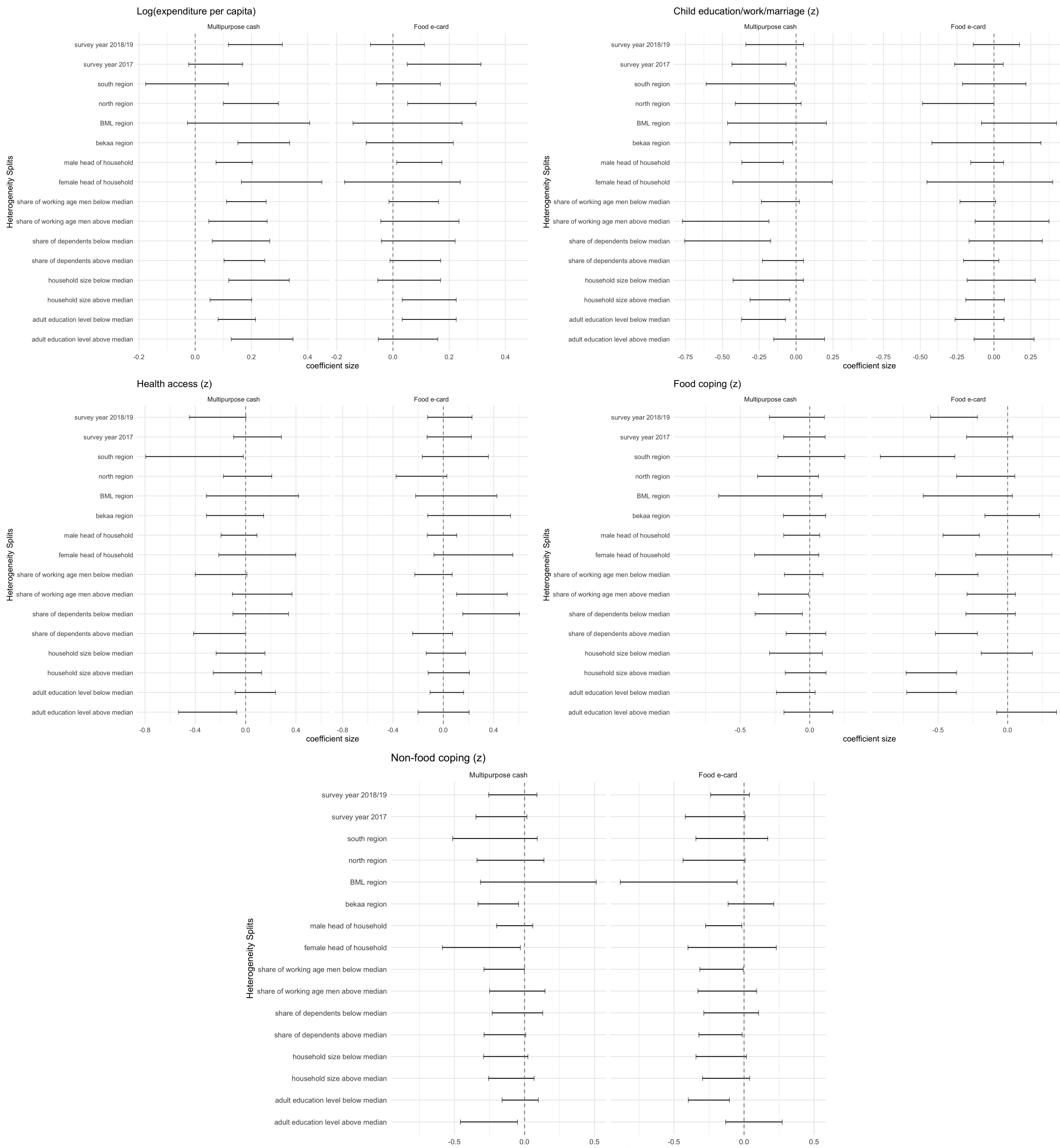
Note: Figure contains graphical depiction of coefficents from robustness specifications using a uniform kernel and varying bandwidths to determine the sample. These specifications were not prespecified.

Appendix Figure 9: Robustness: local linear specification with triangular kernel weights and local second-order polynomial specification, varying bandwidths



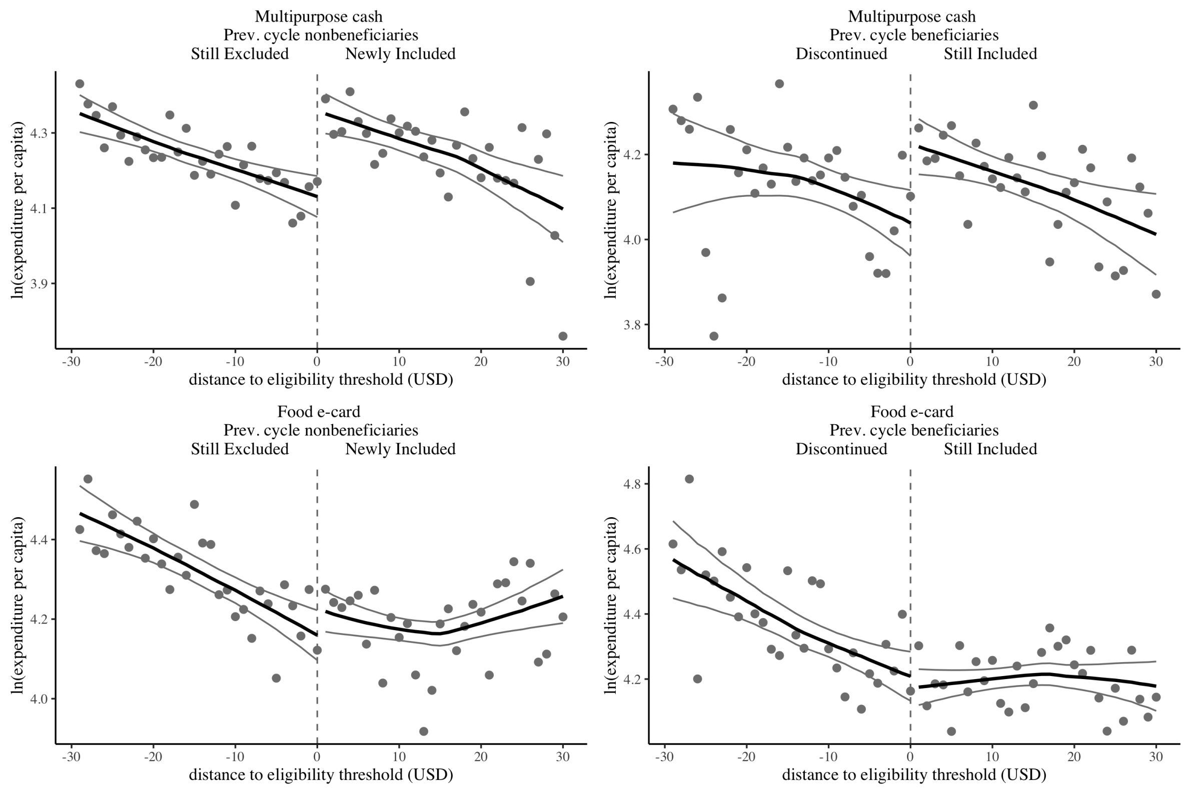
Note: Figure contains graphical depiction of coefficents from robustness specifications that include second-order polynomials in the running variable and varying bandwidths to determine the sample. These specifications were not prespecified.

Appendix Figure 10: Heterogeneity sample splits, during-program effects across outcomes



Note: Figure contains confidence intervals for reduced-form program effects by the sample indicated.

Appendix Figure 11: During-program effects on expenditure of transfers by previous recipient status



Note: Figure depicts the effect of threshold assignment on the the natural log of expenditure with LOESS regression fits and associated 95% confidence intervals.