The short-lived effects of unconditional cash transfers to refugees

Onur Altındağ and Stephen D. O'Connell

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Abstract

We study two yearlong unconditional cash-based assistance programs for Syrian refugees in Lebanon, and show that they improve consumption, child well-being, food security, and reduce livelihood coping. Despite high transfer values, we find no evidence of lasting effects at six months after either program ends. Households spend transfers on basic needs, and take children out of work and re-enroll them in school. Beneficiaries increase cash savings and their stock of durable goods, but liquidate and spend these assets during or soon after the beneficiary period. The results are similar for longer assistance periods and across transfer modalities.

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

JEL Classification: I38, I32, O12, D74

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Contact Information. Onur Altındağ: Bentley University Department of Economics, IZA Institute of Labor Economics, and Economic Research Forum, oaltindag@bentley.edu. Stephen D. O'Connell: Emory University Department of Economics and IZA Institute of Labor Economics, soconnell@emory.edu.

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1 Introduction

More than 100 countries rely on unconditional cash transfer (UCT) programs to alleviate the self-perpetuating dynamics of poverty. The popularity of UCTs has also changed the structure of social protection programs in humanitarian settings in recent years. As of 2020, a large share of humanitarian assistance is distributed in cash-based programs via United Nations-led response plans, which provide 1.7 billion USD in support globally (United Nations High Commissioner for Refugees, 2021). While the transition from protection to self-reliance approaches aims to recognize the increasingly protracted nature of refugee situations, there is little evidence of whether cash-based humanitarian aid can achieve both immediate relief and longer-term poverty alleviation (Quattrochi *et al.*, 2020; MacPherson and Sterck, 2021). A recent comprehensive review of the UCT literature concludes that whether cash transfers have the potential to alleviate the cycle of poverty depends on core design features, including the size of the transfer, the period over which transfers are sustained, and how funds are used by beneficiaries (Bastagli *et al.*, 2016). In addition to program design, the horizon on which effects persist may also depend on initial capital endowments, market access, and property rights over income, savings, and investments (Blattman *et al.*, 2020; Balboni *et al.*, 2020) – recognizing that when opportunities for the productive use of UCTs are limited by the economic environment, even transfers of large value and long duration might not yield sustained improvements in economic well-being.

Using a pre-registered quasi-experimental design, we estimate the during- and after-program effects of two at-scale cash-based programs that reach more than half of the Syrian refugee population living throughout Lebanon. Our empirical approach uses a threshold-based assignment rule that generates a discontinuity in program eligibility across otherwise-comparable households. We show that a cash-based value voucher for food purchases distributing 1,620 USD to the median-sized household of five over the course of a year increases food expenditure, improves food security, and reduces livelihood coping strategies. A subset of households receive an additional cash transfer of 2,100 USD over the same period, which yields immediate positive effects on expenditure and child well-being, and reduces livelihood coping. 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 within six months after the programs end. Design features do not provide an immediate explanation for the lack of sustained effects. In addition to the transfer values being larger than the vast majority of UCT programs studied in the literature, the modality (cash versus food voucher) or duration (one versus two years) of the transfers does not affect the ability to smooth consumption over a longer horizon. We also show that beneficiaries exhibit forward-looking behavior, with no additional consumption of "temptation goods" (Evans and

Popova, 2017), the removal of children from work and re-enrollment in school, and the building of savings in the form of cash and durable goods. Transfer recipients are more likely to hold savings, but they also report a higher likelihood of spending cash savings to provide basic needs. These results show intent to save, but a lack of capacity to build an asset stock during the transfer period and maintain it afterwards (Karlan *et al.*, 2019).

Our findings add to the growing evidence regarding the effects of cash transfers in humanitarian settings (Hidrobo *et al.*, 2014; Aker, 2017; Sterck and Delius, 2020; MacPherson and Sterck, 2021; Lehmann and Masterson, 2020; Masterson and Lehmann, 2020; Aygün *et al.*, 2021). These results also serve as a point of contrast with the bulk of the cash transfer literature which report positive, although often attenuating, post-program followup effects on consumption and well-being.[1] This paper shows that cash-based humanitarian aid programs effectively provide temporary relief, but are unlikely to achieve longer-term poverty alleviation. We conclude that these challenges to sustained self-reliance are unlikely caused primarily by program design, but are rather a result of the limited capacity of refugees to save and claim the returns of investments.

2 Cash transfers to Syrian refugees in Lebanon

Lebanon hosts approximately 1.5 million Syrian refugees, where the United Nations High Commissioner for Refugees (UNHCR), World Food Programme (WFP), and other national and international NGOs provide cash-based assistance to refugee families in Lebanon. In this paper, we quantify the effect of the multipurpose cash assistance program, which provides a fixed amount of 175 USD per month to eligible families, and the "food e-card", which provides a value voucher for food items of 27 USD per person per month. In the years we study, the cash program annually supported roughly 55,000 families, and the food e-card program assisted over 120,000 families. For targeting and distribution, aid agencies use a regression-based proxy means test which relies on demographic information held by UNHCR and a nationally representative household survey called the Vulnerability Assessment of Syrian Refugees (VASyR). The targeting model generates a continuous measure of predicted expenditure per capita for each household, which is used to rank households in order of priority for assistance programs. The programs we study use the same ranking to assign eligibility based on a standard approach of allocating assistance to the most vulnerable share of the population that the respective program's annual budget can support. This mechanism generates the sharp discontinuities in assignment probability that we use to identify program effects.[2]

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. Importantly, the annual beneficiary assignment and start of the assistance cycle occurs each November, and nationally

[1] Bastagli *et al.* (2016) provides an in-depth review of the UCT literature. For some recent studies, see Aker (2017); Blattman *et al.* (2020); Baird *et al.* (2019); Haushofer and Shapiro (2016, 2018); Haushofer *et al.* (2020); Hidrobo *et al.* (2014); Schwab (2020); Aygün *et al.* (2021); Banerjee *et al.* (2020)

[2] Appendix Figure 1 provides a schematic of a hypothetical program cycle.

representative cross-sectional survey data are collected each year in April.

3 Data

To implement the research design, 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. Next, we describe in further detail the three data sets, which we are able to link using a permanent unique family identifier.

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.[3] 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.[4]

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. In the first step of the analysis, we use a unique identifier to link assistance records to targeting scores in order to detect the implicit assignment threshold for the transfer programs in each year. We detect these thresholds for program eligibility through an iterative search across potential discontinuity points by region, year, and program. The technical details of this process are described in Appendix Section A. 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.

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.[5] The survey is administered over the course of

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

[4] 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.

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

several weeks spanning April and May of each year, is representative by district, and the sample ranges from 4,000 to 5,000 households annually. An advantage of these repeat cross-sectional data from a population subject to a common assignment rule is that attrition concerns typical of sample-based intervention studies are minimized substantially. For our analysis, we obtained four rounds of these data, from 2016 to 2019.

4 Empirical Design

4.1 Sharp Regression Discontinuity Design (RDD)

For identification, we use the quasi-random allocation of cash assistance around the strict thresholds in the targeting score. Households are assigned 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 beneficiaries 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.[6]

The first survey after the assignment of program eligibility falls in the middle of a program cycle, 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. Put another way, the allocation mechanism used by aid agencies generates an exogenously determined assignment to assistance receipt for the 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 assistance cycle and six months after that cycle ends. Because survey rounds are repeated cross-sections, the "follow-up" samples comprise a different set of households than those surveyed in the prior year. 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.

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 an outcome measured 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

[6] See Appendix Figure 2 for a graphic illustration of the program timeline.

eligible for cash assistance based on the assessment in period $t-1$, which is the previous calendar year. $s_{i,t-1}$ 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 by Calonico *et al.* (2019). The regression sample includes households from multiple survey rounds, which we account by using survey-year fixed effects γ_t.[7]

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. 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 that predicts the amount of cash assistance received by each family, corresponding to the following two-stage-least-squares (2SLS) procedure:

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

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

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.

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

For interpretation, we report the LATE estimates for receipt of 175 USD per month per family under the multipurpose cash program, and 27 USD 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 Calonico *et al.* (2019)'s MSERD optimal bandwidth h determined in the first stage regression. We discuss the underlying assumptions for internal validity of our approach and the empirical validity tests in Section 4.4.

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 decreases) would expand (or shrink) the programs. This allows us to interpret the findings as the effects for those households who would be affected by expansion (or contraction) of these programs.

4.3 Outcomes

The transfers make up a large share of beneficiary families' monthly expenditures and income. For those who received the food e-card, the average cumulative amount received as of April of the beneficiary cycle was 866 USD, or 144 USD 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 920 USD, or 153 USD per month – 34.6 percent of the average monthly total expenditure, and 172 percent of monthly labor income.[8]

Our main outcomes are pre-specified indices that take the mean of unit-standardized subcomponents. 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 adverse health 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.[9]

4.4 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 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

[8] We present the descriptive statistics for these variables in Appendix Table 1.

[9] See Appendix Table 2 for the descriptive statistics and the full listing of the components of all the outcome indices.

the research design. We further discuss the first stage and the exclusion restriction.

4.4.1 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. We fail to reject at conventional levels the null hypothesis of no manipulation in the corresponding parametric density tests across all programs and assignment cycles.[10] 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 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.

4.4.2 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:

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

such that if the research design holds, we should estimate a large positive coefficient 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 1 presents the graphical analog to these analyses, in which we plot the amount of program-specific assistance per capita received relative to the assistance eligibility thresholds. Accompanying this, within the same graph, we provide the coefficients from the corresponding regression estimates. The left column in Figure 1 shows that, around the threshold, the targeting score in period t does not predict the amount of assistance received contemporaneously for either

[10] See Appendix Figure 3 for evidence of density continuity; Appendix Table 4 provides formal test statistics.

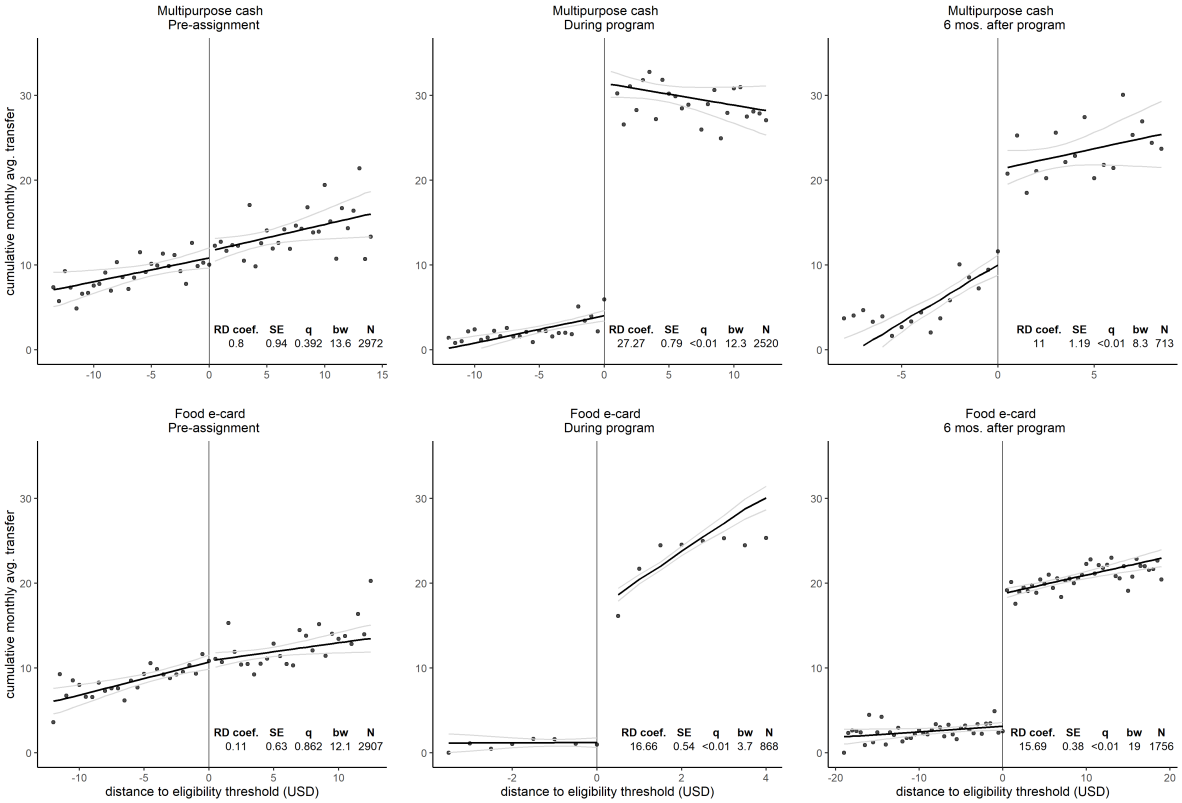
of the programs. The point estimates for both programs are also small and statistically insignificant, which provides a powerful test of balance given that the vulnerability score is solely determined by household observable characteristics at the time of the survey.

The rest of Figure 1 shows that the same vulnerability scores generate a substantial discontinuous jump around the threshold when predicting future cash transfers. The first stage effects measure the differential cumulative amount of transfer received by eligible households compared to otherwise similar ineligible households. 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 exogenous variation around the eligibility cutoff, and (ii) the treatment intensity is substantial, indicating a large discontinuous jump in income for the beneficiary households. For example, for 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 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 proxy means test, 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, itself unknown to households, has an impact on cash transfers is through assistance eligibility.

Figure 1: 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). The specifications, variable definitions, sample, bandwidth selector, and adjustments for multiple hypothesis testing were prespecified for all results contained in this figure.

4.4.3 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 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 is given by:

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

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

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 to all the primary and secondary outcomes that we assess

in our study: contemporaneous cash transfers, expenditure, child hardship index, health access, food coping and livelihood coping z-indices. None of the pre-assignment outcomes are statistically significant after correcting for multiple hypothesis testing.[11] We conclude that the discontinuity does not explain any variation in pre-assignment outcomes. Using the pre-assignment covariates as outcomes in equation 6, we further confirm the balance of covariates around the threshold for the treatment, available in Appendix Table 6. Finally, there is a possibility that assistance receipt in one year could systematically affect the household features used for targeting in a subsequent year. If this were the case, we would expect to see substantial discontinuities in subsequent-year scores around the threshold. We test for this in Appendix Figure 6, and find no evidence that this is the case.

4.4.4 Study limitations and other considerations

Our study has some limitations. First, we cannot empirically estimate local price effects that have been documented in other settings with at-scale cash transfer programs (Filmer *et al.*, 2021). Second, we do not have information on resource sharing among refugees, which might bias our estimates towards zero. Third, we use repeated cross section data and despite being nationally representative, our estimates are based on samples that reflect changes in the population over time. Lastly, estimates for the cash program capture the effects of additional cash on families who already receive food vouchers; we don't estimate cash program effects with non-beneficiary counterfactuals to cash-only recipients. Therefore, the cash program effects we estimate are additive with, and not alternative to, the effects of the food voucher program.

5 Results

We present all results graphically in Figure 2, alongside reduced-form estimates in Table 1 from the estimation of equations 1 and 2. Corresponding 2SLS point estimates are shown in Appendix Table 7. Panel A in Figure 2 and Table 1 present the effects of the programs on the natural log of per capita expenditure. Households eligible for multipurpose cash exhibit a sharp jump in expenditure of 0.17 log points (18.5 percent), significant at any conventional level after accounting for multiple hypotheses (Table 1, Panel A, Column 1). The corresponding 2SLS estimates indicate a 0.21 log points (23 percent) increase in expenditure among households who received the full indented amount of assistance (Appendix Table 7, Panel A, Column 1). The point estimate on post-program effects on expenditure is negative, smaller in magnitude and is not statistically different from zero (Table 1, Panel A, Column 3). The substantial increase in consumption during the program disappears within six months of the end of the assistance cycle. The food value voucher has a positive effect on expenditure, though smaller than that for the cash program: the increase in overall expenditure is 8 percent, and only significant at the 10 percent level after accounting for multiple hypotheses (Table 1, Panel A, Column 2). Similar to the cash program, the during-

[11] Appendix Table 5 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.

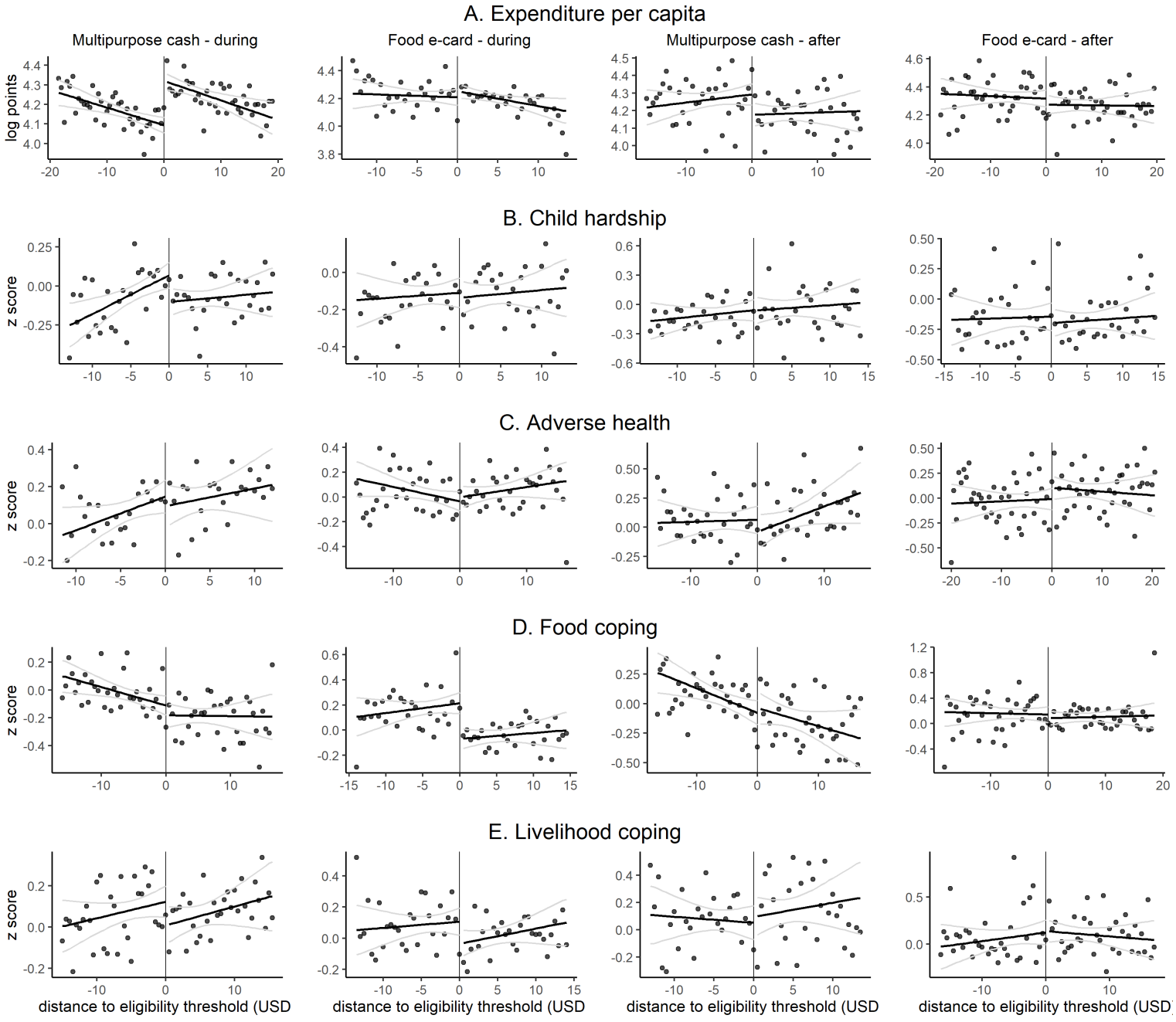
program increases in expenditure do not persist even for six months after the end of the program (Table 1, Panel A, Column 4).

Because our primary outcomes are indices (excepting log expenditure per capita), we also report the coefficients on the unit-standardized sub-components of the indices in Appendix Figures 7-11 to understand the relative contribution of subcomponents to the overall effect. When we investigate the sub-components of increased consumption, we find that essential needs drive the overall increase in expenditure induced by the cash program, which goes to 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.[12] Post-program effects generally fail to reject the null for both programs, reflecting the overall null effects on per capita expenditure shortly after the assistance cycle is over. We find no evidence of increased consumption on "temptation" goods (entertainment or tobacco and alcohol) for either of the programs at any period that we observe the outcomes (Banerjee and Mullainathan, 2010).[13]

[12] See Appendix Figure 7 for the 95 percent confidence intervals of 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.

[13] Ozler *et al.* (2020) show that aid programs in Turkey induce endogenous changes in household composition, which were large enough to reduce estimates of the effects of aid on per capita consumption. We present tests in Appendix Table 8 that show this is unlikely to be the case in Lebanon.

Figure 2: During and after program effects across programs and outcomes



Panel B in Figure 2 and Table 1 show that the cash program reduces child hardship by 0.19SD, with a local average treatment effect on program participants of 0.23SD (Appendix Table 7, Panel B). We observe no difference between prior recipients versus non-recipients after the program ends. The food value voucher has no impact on the child welfare measure at any point after eligibility assignment (Table 1, Panel B). The sub-component analysis suggest that 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 (Appendix Figure 8). Child labor reductions among recipients of the multipurpose cash program are concentrated among boys, as are increases in school enrollment (Appendix Table 9): the cash program appears to induce a shift in the source of household labor supply following the re-enrollment of boys in school, as we also observe increases in men's employment.[14] There is minimal evidence of program effects on any of these outcomes for women or girls.

Panel C in Figure 2 and Table 1 present effects on the adverse 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 person 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 (Figure 2, Panel C and Appendix Figure 9). 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 maintain health.

Panel D in Figure 2 and Table 1 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.[15] Striking improvements in food security are found during disbursement of the food value voucher, which reduces the incidence of food coping strategies by 0.28SD (Table 1, Panel D, Column 2).[16] The multipurpose cash program has no contemporaneous effect on food coping, and neither program has effects that last into the post-program period. We observe stark reductions in the rate of borrowing food, reducing the number of meals, going without food for a day, and reducing portions induced by food e-card program eligibility (Appendix Figure 10). 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. 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.

[14] Due to the survey module used, we are not able to differentiate the type of specific economic activity (whether wage employment, self-employment, or entrepreneurship) that generates these results.

[15] The sub-component measures are listed separately in Appendix Table 2.

[16] Corresponding IV estimates indicate .36 SD improvement among beneficiaries (Appendix Table 7, Panel D, Column 2).

Table 1: Reduced-form RD treatment effects across outcomes

	During program:		After program:	
	Multipurpose cash	Food e-card	Multipurpose cash	Food e-card
	(1)	(2)	(3)	(4)
Panel A: Expenditure per capita				
Program effect	0.17	0.08	−0.08	−0.03
	(0.03)	(0.04)	(0.04)	(0.05)
Benjamini-Hochberg q	$< 0.001^{***}$	0.075^{*}	0.345	0.719
Control Group Mean	78.34	86.29	82.36	88.79
Bandwidth	16.15	13.46	19.68	15.77
N	3,231	2,958	1,710	1,481
R^2	0.01	0.003	0.01	0.002
Panel B: Child hardship				
Program effect	−0.19	−0.03	−0.01	−0.05
	(0.06)	(0.06)	(0.09)	(0.08)
Benjamini-Hochberg q	0.007^{***}	0.672	0.954	0.719
Control Group Mean	-0.08	-0.14	-0.12	-0.15
Bandwidth	13.17	12.7	13.85	14.56
N	2,224	2,192	1,050	1,166
R^2	0.01	0.002	0.002	0.0004
Panel C: Adverse health				
Program effect	−0.06	0.04	−0.11	0.12
	(0.07)	(0.06)	(0.10)	(0.08)
Benjamini-Hochberg q	0.417	0.646	0.637	0.719
Control Group Mean	0.06	0.03	0.06	-0.01
Bandwidth	11.72	15.6	15.01	20.08
N	2,408	3,439	1,320	1,820
R^2	0.004	0.002	0.01	0.003
Panel D: Food coping				
Program effect	−0.06	−0.28	0.06	−0.05
	(0.06)	(0.06)	(0.09)	(0.09)
Benjamini-Hochberg q	0.381	$< 0.001^{***}$	0.811	0.719
Control Group Mean	0	0.17	0.07	0.16
Bandwidth	16.14	14.74	16.13	18.15
N	3,247	3,268	1,434	1,688
R^2	0.01	0.01	0.02	0.001
Panel E: Livelihood coping				
Program effect	−0.12	−0.14	0.04	0.01
	(0.06)	(0.06)	(0.10)	(0.09)
Benjamini-Hochberg q	0.085^{*}	0.068^{*}	0.811	0.872
Control Group Mean	0.06	0.08	0.07	0.07
Bandwidth	15.38	13.53	13.15	16.91
N	3,108	3,013	1,146	1,587
R^2	0.002	0.002	0.002	0.001

Note: This table reports estimates of the effect of cash transfers on outcomes listed in panels. The program effect for expenditure per capita is reported in natural log points; for all index outcomes, the program effect is in units of standard deviations. 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.
$^{*}q < .1$; $^{**}q < .05$; $^{***}q < .01$

While beneficiaries are actively receiving assistance, both programs yield significant reductions in livelihood coping strategies (Panel E of Figure 2 and Table 1). This is on the order of 0.12SD for food value voucher recipients, and 0.14SD for cash recipients (Table 2, Panel 3, Columns 1 and 2). 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. Looking only at magnitudes of these subcomponents, 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, reduce expenditure, downgrade their housing or have children engage in exploitative work (Appendix Figure 11). 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 revert to counterfactual levels and reengage in coping strategies after the cycle ends. All results and conclusions are robust to variations in bandwidth, weighting, and specification choices (Appendix Figures 12-14).

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. In the remainder of the study, we explore potential explanations for this result that can be tested with available data.

6.1 Rent, savings and asset holdings

Next, we investigate whether cash assistance induces savings and asset holdings. Table 2 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, nor in the ability to use savings to cope with liquidity (Panel B). Panels C and D shows neither program enables beneficiaries have any differential savings or ability to use them to smooth consumption after the program ends. In other words, the unconditional cash assistance allows beneficiaries to save temporarily but these savings quickly vanish to buffer consumption.

Durable goods can allow households to engage in productive activity or serve as a non-traditional form of saving (Banerjee *et al.*, 2011). Appendix Figure 15 reports the confidence

Table 2: Cash transfer effects on rent, housing, and savings

	Rent expenditure (USD per capita)	HH paid any rent	HH changed accom. in past 6 mos	HH faced eviction recently	HH has savings	HH spent savings to cope
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Multipurpose cash - during						
Above threshold	3.54***	0.12***	0.03*	−0.002	0.07*	0.09***
	(1.07)	(0.03)	(0.02)	(0.01)	(0.04)	(0.03)
Control Group Mean	12.88	0.42	0.12	0.04	0.31	0.18
Bandwidth	15.15	18.3	16.69	14.1	9.78	7
N	3,058	3,636	3,347	2,868	2,006	1,425
R^2	0.01	0.01	0.002	0.01	0.003	0.01
Panel B: Food e-card - during						
Above threshold	−2.85**	−0.06*	−0.03	0.01	−0.02	−0.02
	(1.30)	(0.03)	(0.02)	(0.01)	(0.03)	(0.02)
Control Group Mean	16.05	0.43	0.14	0.05	0.32	0.21
Bandwidth	13.98	13.55	13.5	13.57	13.73	13.17
N	3,113	3,015	3,006	3,018	3,059	2,950
R^2	0.03	0.02	0.01	0.01	0.01	0.004
Panel C: Multipurpose cash - after						
Above threshold	−0.84	0.02	0.01	−0.01	−0.03	0.002
	(1.51)	(0.04)	(0.03)	(0.02)	(0.04)	(0.04)
Control Group Mean	13.75	0.43	0.13	0.04	0.33	0.19
Bandwidth	20.55	17.38	11.74	12.86	18.4	15.45
N	1,786	1,542	1,022	1,126	1,617	1,367
R^2	0.01	0.0003	0.02	0.02	0.004	0.01
Panel D: Food e-card - after						
Above threshold	−1.03	−0.02	−0.07*	−0.06**	−0.01	−0.04
	(1.84)	(0.04)	(0.04)	(0.02)	(0.04)	(0.04)
Control Group Mean	15.54	0.45	0.14	0.05	0.3	0.21
Bandwidth	15.23	16.13	10.69	10.12	18.1	14.54
N	1,453	1,526	1,030	976	1,682	1,392
R^2	0.001	0.0002	0.005	0.01	0.001	0.003

Note: This table reports estimates of the effect of cash transfers on rent, accommodation, and savings. 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.
*p < .1; **p < .05; ***p < .01

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.

Finally, we also show that the duration over which transfers are sustained, or term structure, is unlikely to be the cause of an inability to see sustained welfare effects. We test this by segmenting the sample into households who were versus weren't eligible for assistance in the prior cycle. The sample of prior eligibles then compares households who received assistance in $t-1$ and t to those who received only in $t-1$, estimating a 2nd-year continuation effect. We then estimate among those we were ineligible in $t-1$, some of whom became eligible in t for a first-year inclusion effect.

For estimation, we use both split samples and a specification in which we use a modified version of Equation 1 that interacts the current and the previous assistance status, as in:

$$y_{i,t} = \alpha + \beta_1 d_{i,t-1} + \beta_2 d_{i,t-2} + \beta_3 (d_{i,t-1} \times d_{i,t-2}) + f(s_{i,t-1}) + \gamma_t + \varepsilon_{i,t} \tag{7}$$

where the interaction effect β_3 permits a test of the null hypothesis of equal ITT effects conditional on prior assistance eligibility.

Table 3 shows the split sample effects as well as the parametric difference given by the interacted specification in Equation 7 for the multipurpose cash and food voucher programs, respectively. We estimate remarkably similar and statistically indistinguishable effects of both programs for all the outcomes 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 transfers. These results suggest that the major determinant of 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 humanitarian aid programs that give poor refugee households in Lebanon sizeable unconditional transfers 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

Table 3: RD effects by previous assistance status

	Outcome:				
	ln(expenditure)	Child hardship	Adverse health	Food coping	Livelihood coping
	(1)	(2)	(3)	(4)	(5)
Panel A: Previous cash beneficiary: No					
Above threshold (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	$< 0.001^{***}$	0.515	0.670	0.080^{*}	0.267
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 cash beneficiary: Yes					
Above threshold (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	$< 0.001^{***}$	$< 0.001^{***}$	0.527	0.506	0.288
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, cash program					
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
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
Panel D: Previous food voucher beneficiary: No					
Above threshold (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	0.032^{**}	0.233	0.123	0.005^{***}	0.225
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 E: Previous food voucher beneficiary: Yes					
Above threshold (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	0.997	0.637	0.312	0.005^{***}	0.150
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 F: Parametric difference, food voucher program					
difference (Panel E - Panel D)	−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

Note: This table reports estimates of the effect of cash transfers on outcomes indicated in the column header. 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. $^{*}q < .1$; $^{**}q < .05$; $^{***}q < .01$

months after the programs end, despite the size of the transfers received.

Program design features such as transfer modality, value, or term structure are unlikely to explain these short-lived effects. We additionally demonstrate that beneficiaries are not myopic: they save and invest in durable goods, children are taken out of work and put into school, and the additional income is not spent on temptation goods. Savings and other human and physical capital investments, however, are not sustained after programs end. Hence, the cash-based interventions that we investigate achieve what they are designed to – provide temporary relief to the poor to help cope with day-to-day vulnerability – but program effects do not last. These findings are consistent with an economic environment in which transfers alone might not lead to sustained poverty alleviation (Banerjee *et al.*, 2015; Balboni *et al.*, 2020). Given the economic and legal constraints that refugees face, our results provide insights into a potential lower bound for the horizon on which positive effects of large cash-based interventions are 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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Appendices

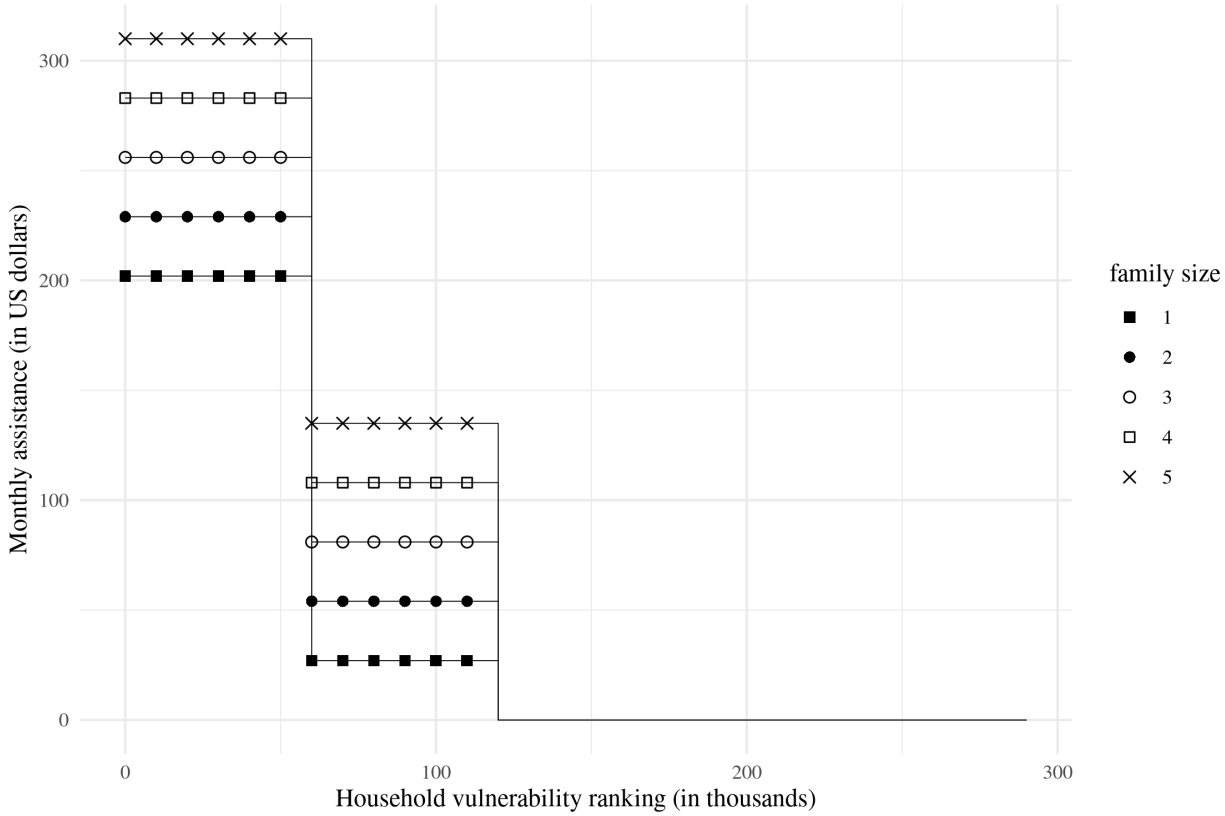
A Threshold detection methodology

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. 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. 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. For each household, we obtained the vulnerability scores 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. 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). 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 4 shows the estimated coefficients by ranking for each of combination of program, year and region (when applicable). 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. Appendix Figure 5 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.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 3 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).

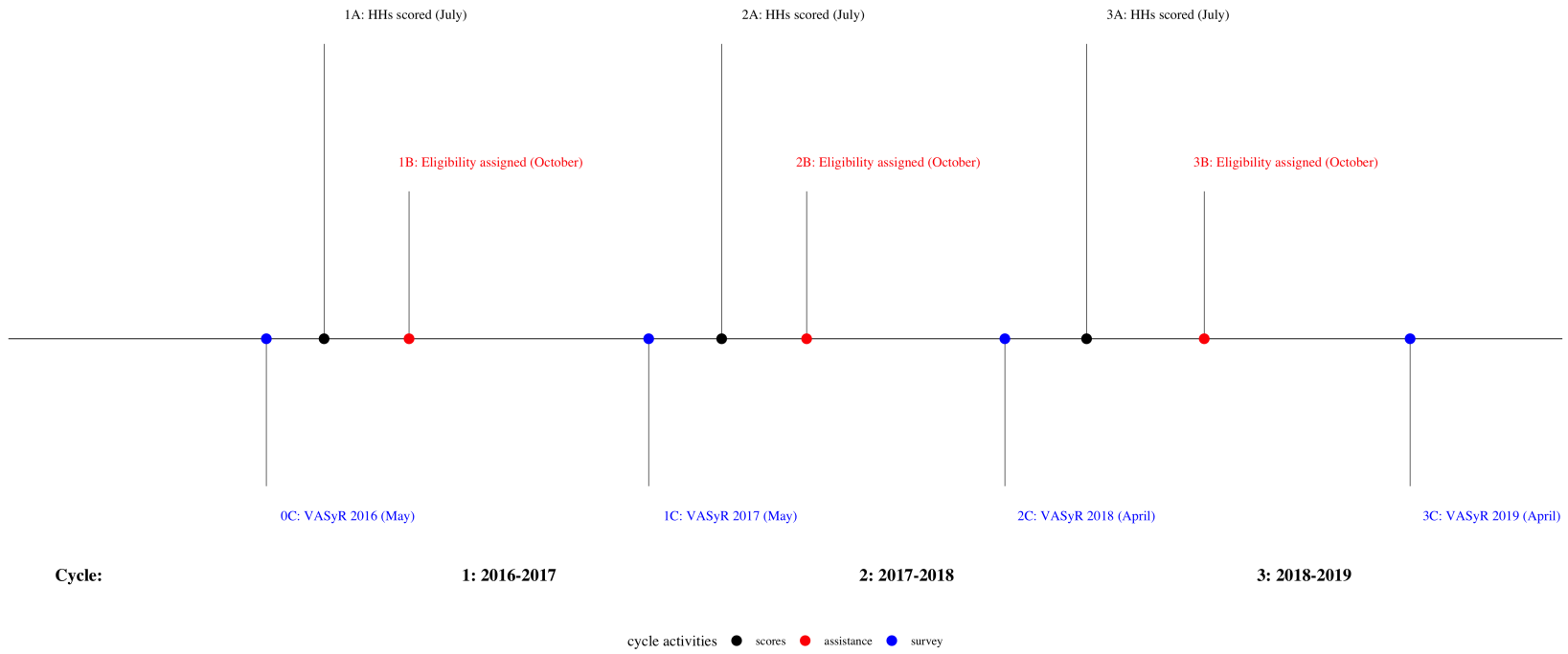
B Appendix Figures and Tables

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



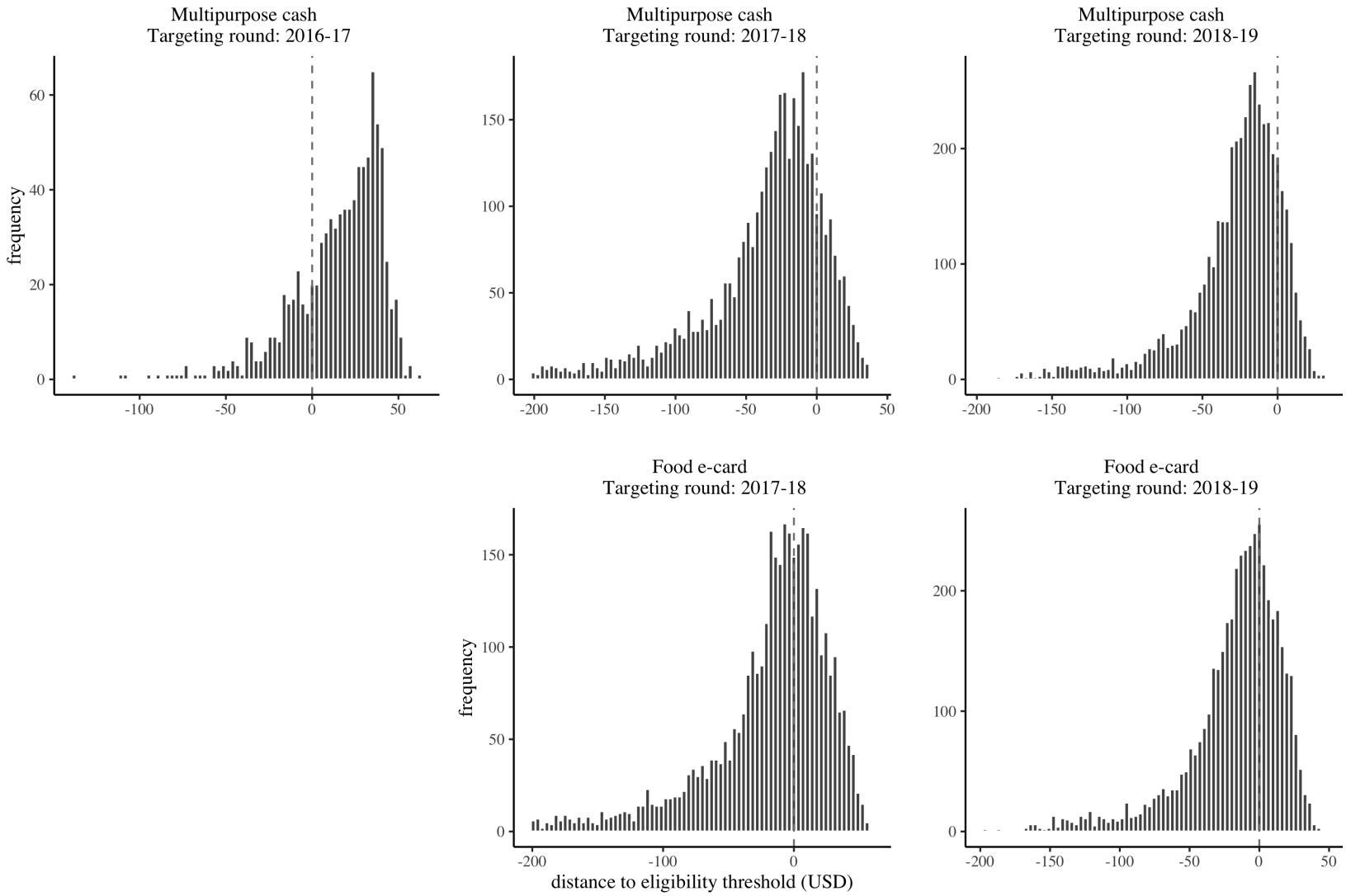
Note: Graph depicts assistance levels by household size across the targeting score rank, under a hypothetical situation in which the multipurpose cash program reaches 60,000 households jointly and the food e-card program reaches 120,000 households. In this scenario, a family of five individuals, for example, would receive 310 USD ($175 + 5 \times 27$) 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 135 USD (5×27), and no assistance if they ranked above 120,000. Note that these figures are for illustrative purposes only, but generally reflect total and relative program sizes in the years studied. 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 Appendix Section A.

Appendix Figure 2: Timeline of cycle activities and data observation



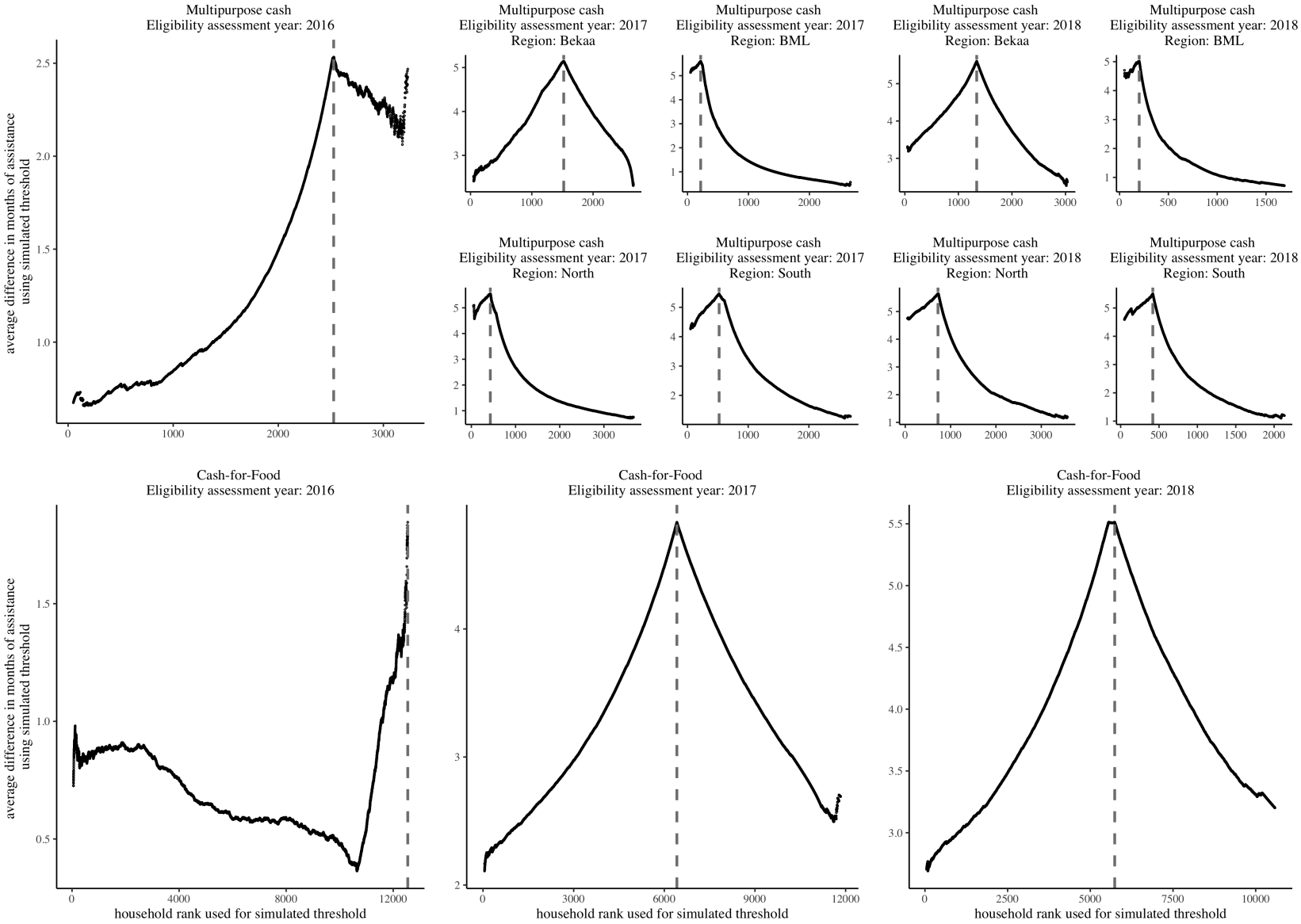
Note: Graph depicts timing of assistance cycle activities and data collection. From June to August, the targeting model is developed, targeting scores are assigned to "active" households, and beneficiary status for the upcoming cycle is determined. In November, the new assistance cycle begins. Households 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.

Appendix Figure 3: Tests of density around cutoffs



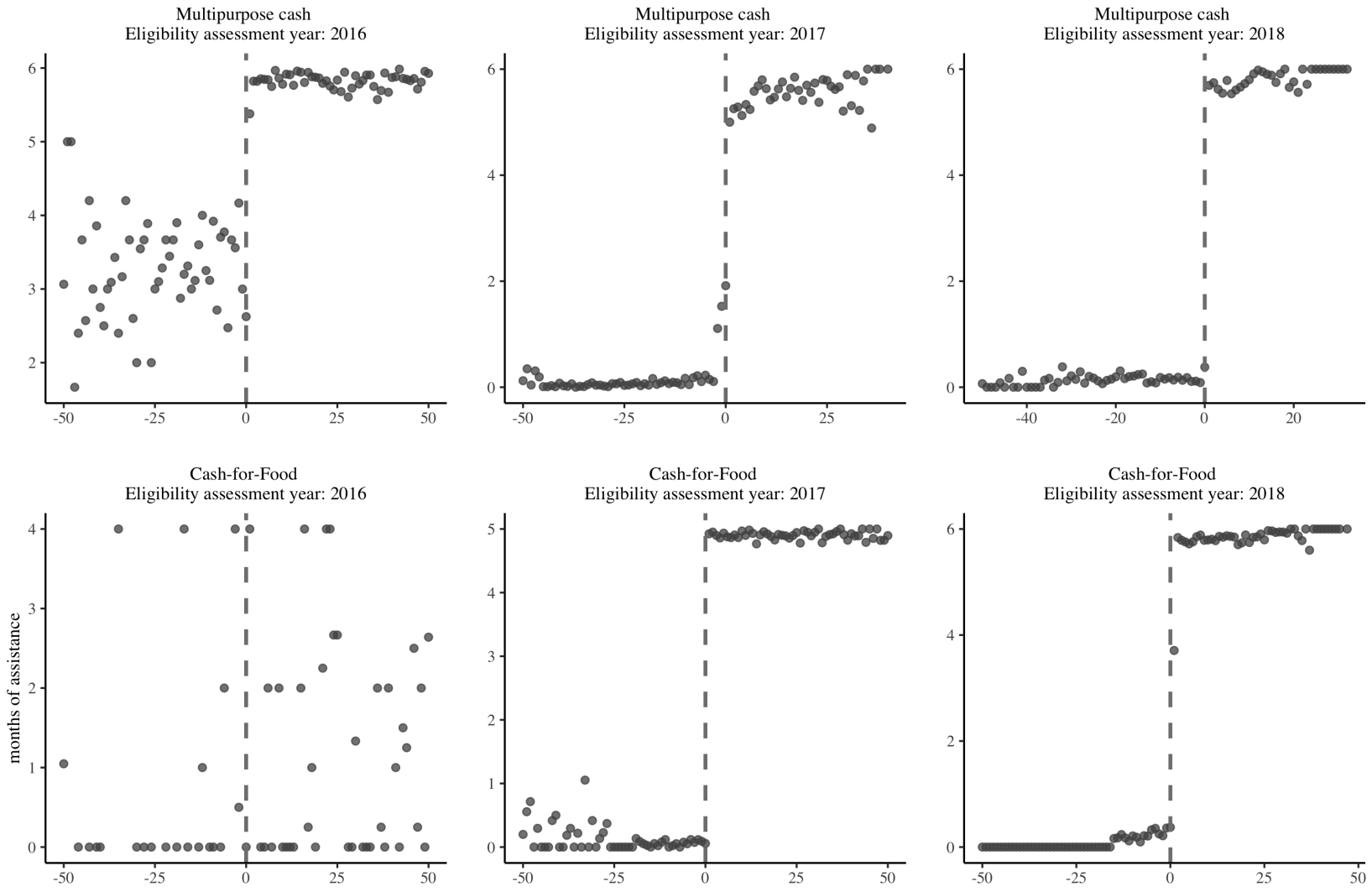
Note: Figure depicts observation density around assignment thresholds (shown by the dashed line) by year and program.

Appendix Figure 4: Depiction of threshold detection procedure



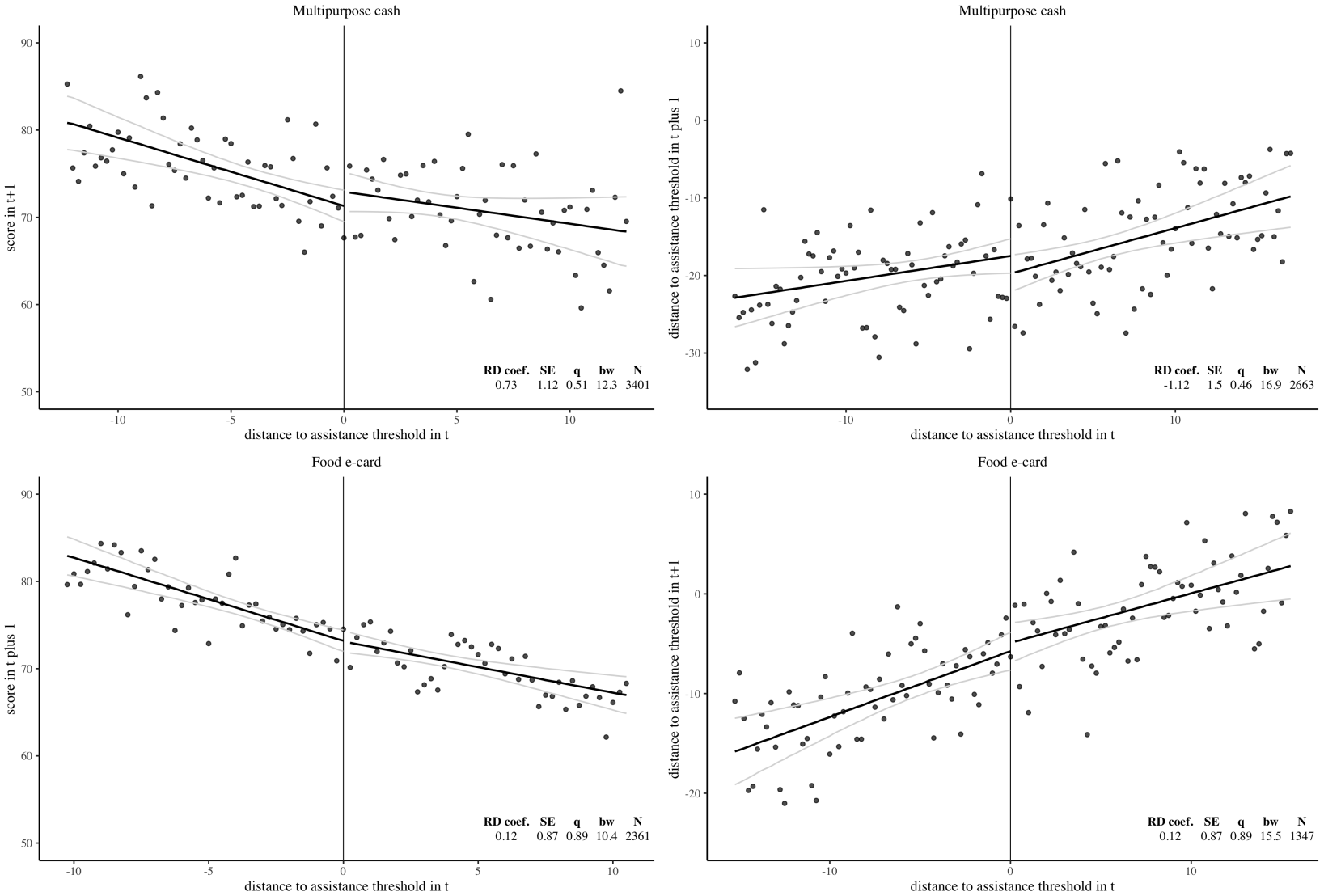
Note: Figure contains graphical depiction of the grid search process used to detect the discontinuity in assignment probability across programs and years.

Appendix Figure 5: Depiction of first stage, by year



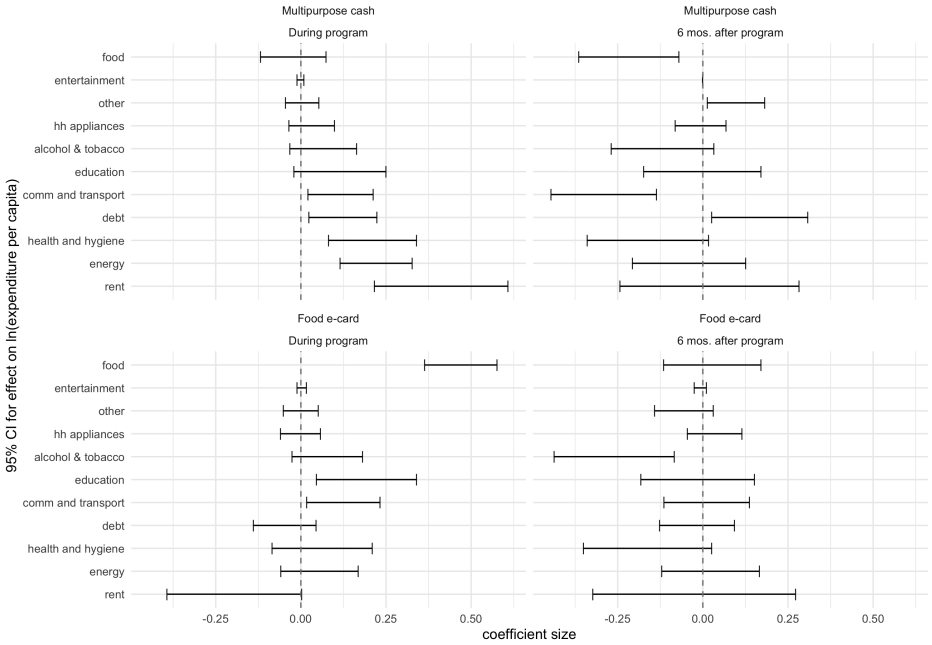
Note: Figure depicts first-stage of assignment discontinuity as measured in monthly dollars of assistance per capita.

Appendix Figure 6: Test of score continuity at thresholds



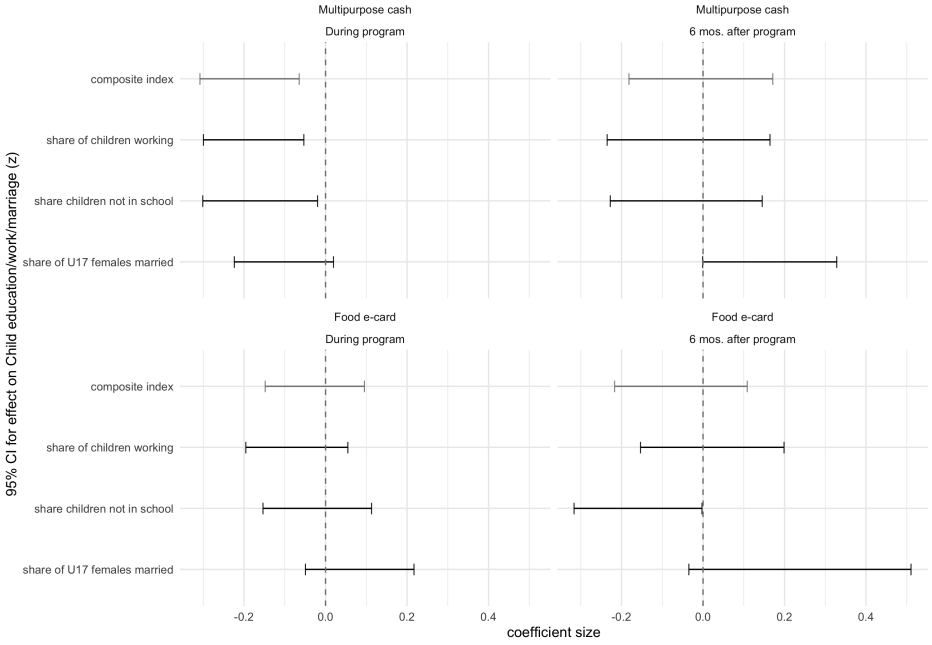
Note: Figure depicts test of subsequent year targeting score continuity across the identifying threshold.

Appendix Figure 7: During and after program effects on expenditure subcomponents



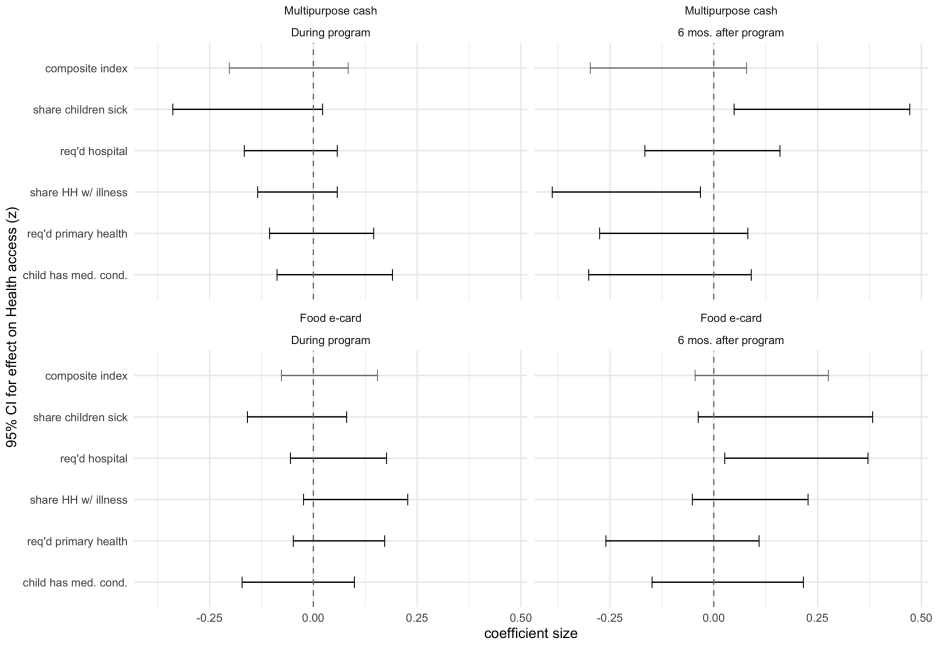
Note: Figure depicts the parametric effect of threshold assignment on per capita monthly expenditure and its constituent components. Spans indicate 95% confidence interval.

Appendix Figure 8: During and after program effects on child hardship index subcomponents



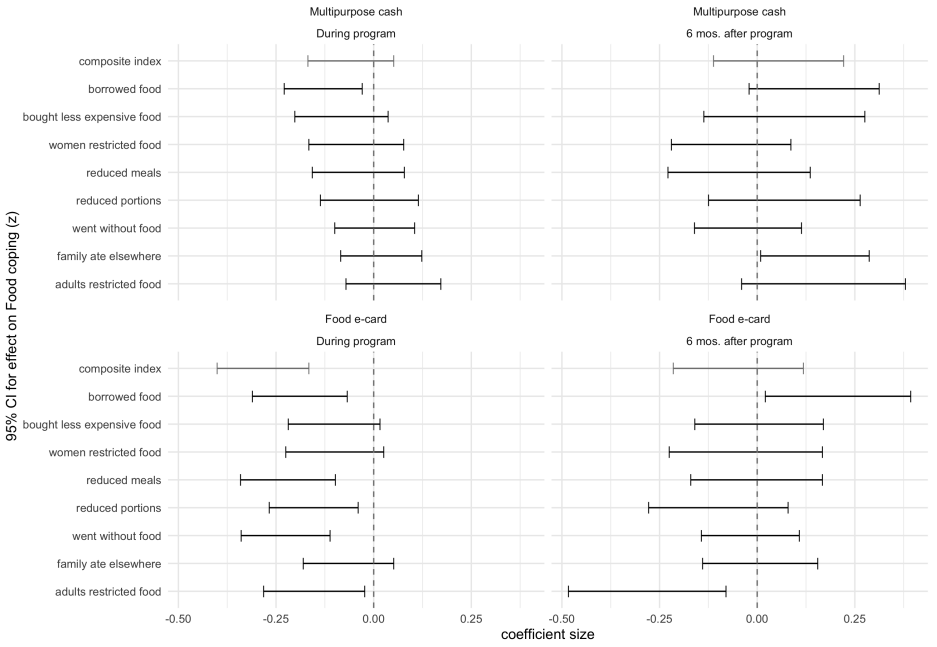
Note: Figure depicts the parametric effect of threshold assignment on the child hardship index and its constituent components. Spans indicate 95% confidence interval.

Appendix Figure 9: During and after program effects on adverse health index subcomponents



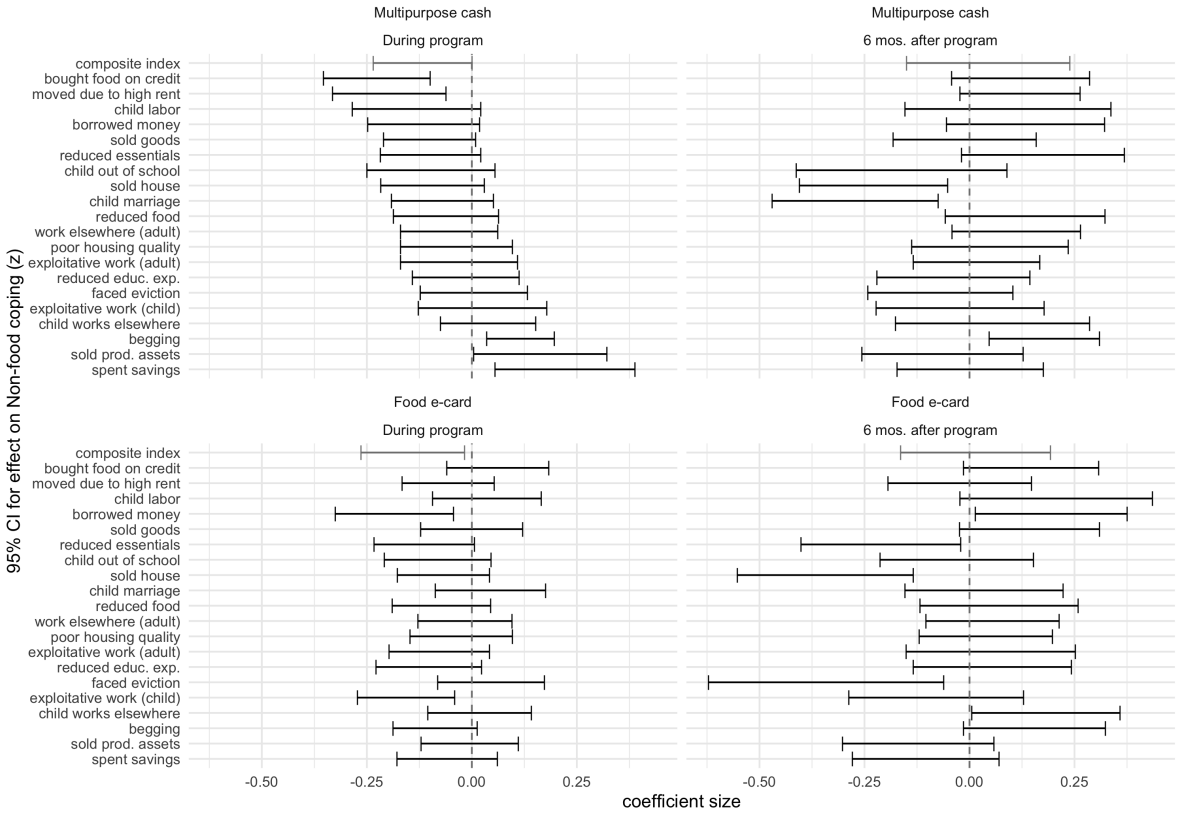
Note: Figure depicts the parametric effect of threshold assignment on the health status and access index and its constituent components. Spans indicate 95% confidence interval.

Appendix Figure 10: Effects of transfers on subcomponents of the food coping strategy index



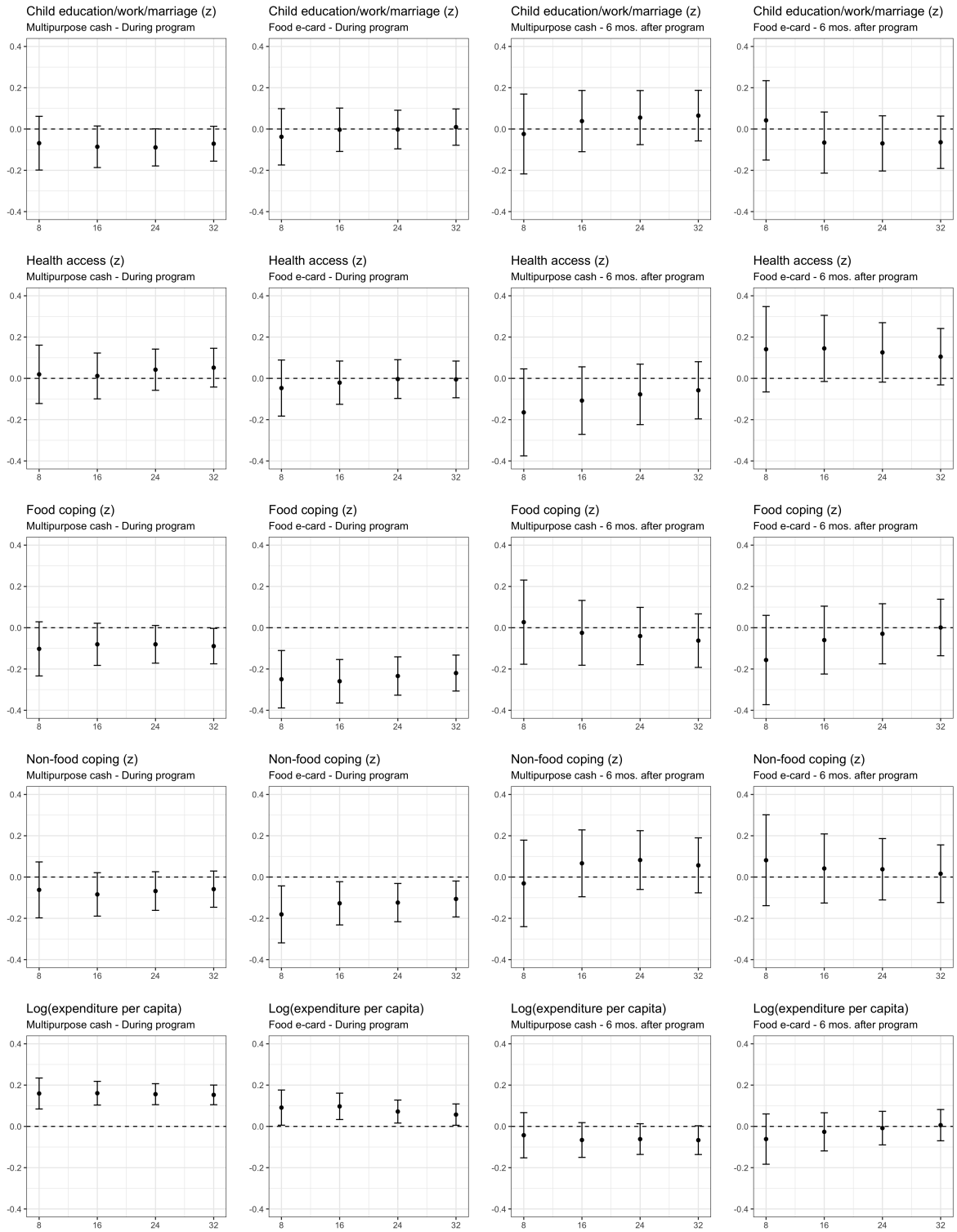
Note: Figure depicts the parametric effect of threshold assignment on the food coping index and its constituent components. Spans indicate 95% confidence interval.

Appendix Figure 11: Effects of transfers on subcomponents of the livelihood coping index



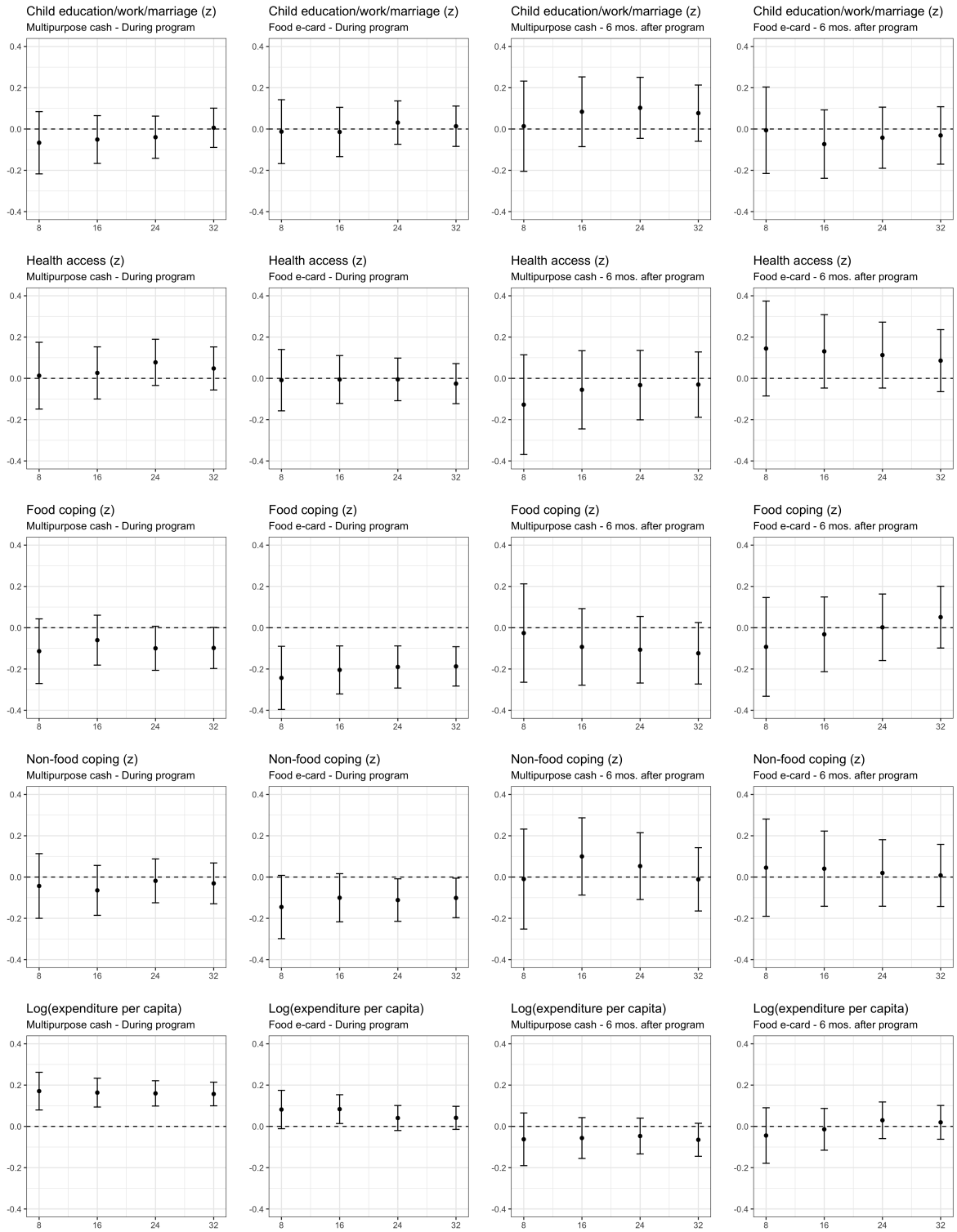
Note: Figure depicts the parametric effect of threshold assignment on the livelihood coping index and its constituent components. Spans indicate 95% confidence interval.

Appendix Figure 12: Robustness: local linear specification with triangular kernel weights, varying bandwidths



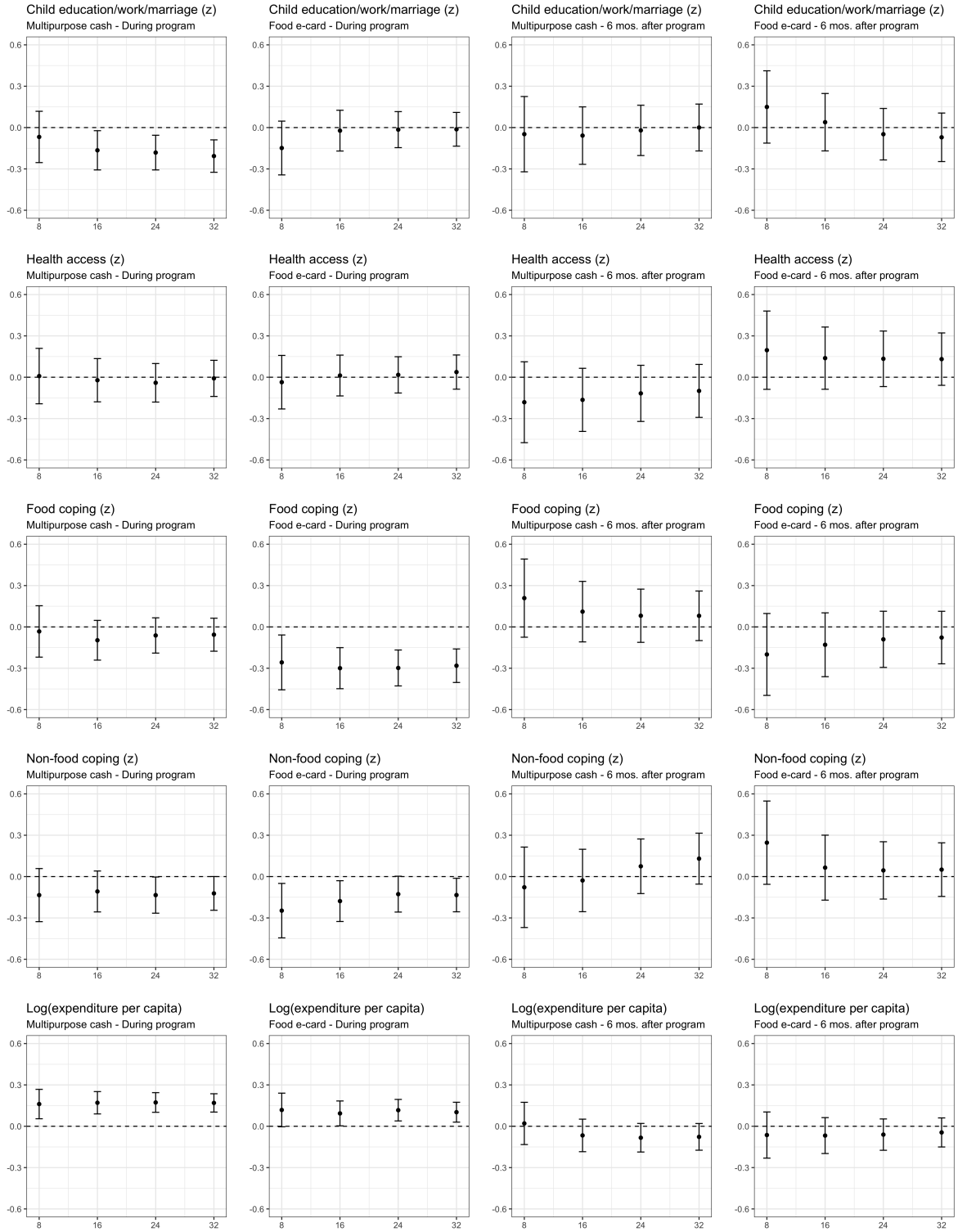
Note: Figure contains graphical depiction of coefficents from robustness specifications that vary the bandwidth used to determine the sample. These specifications were not prespecified.

Appendix Figure 13: Robustness: local linear specification with uniform kernel weights, varying bandwidths



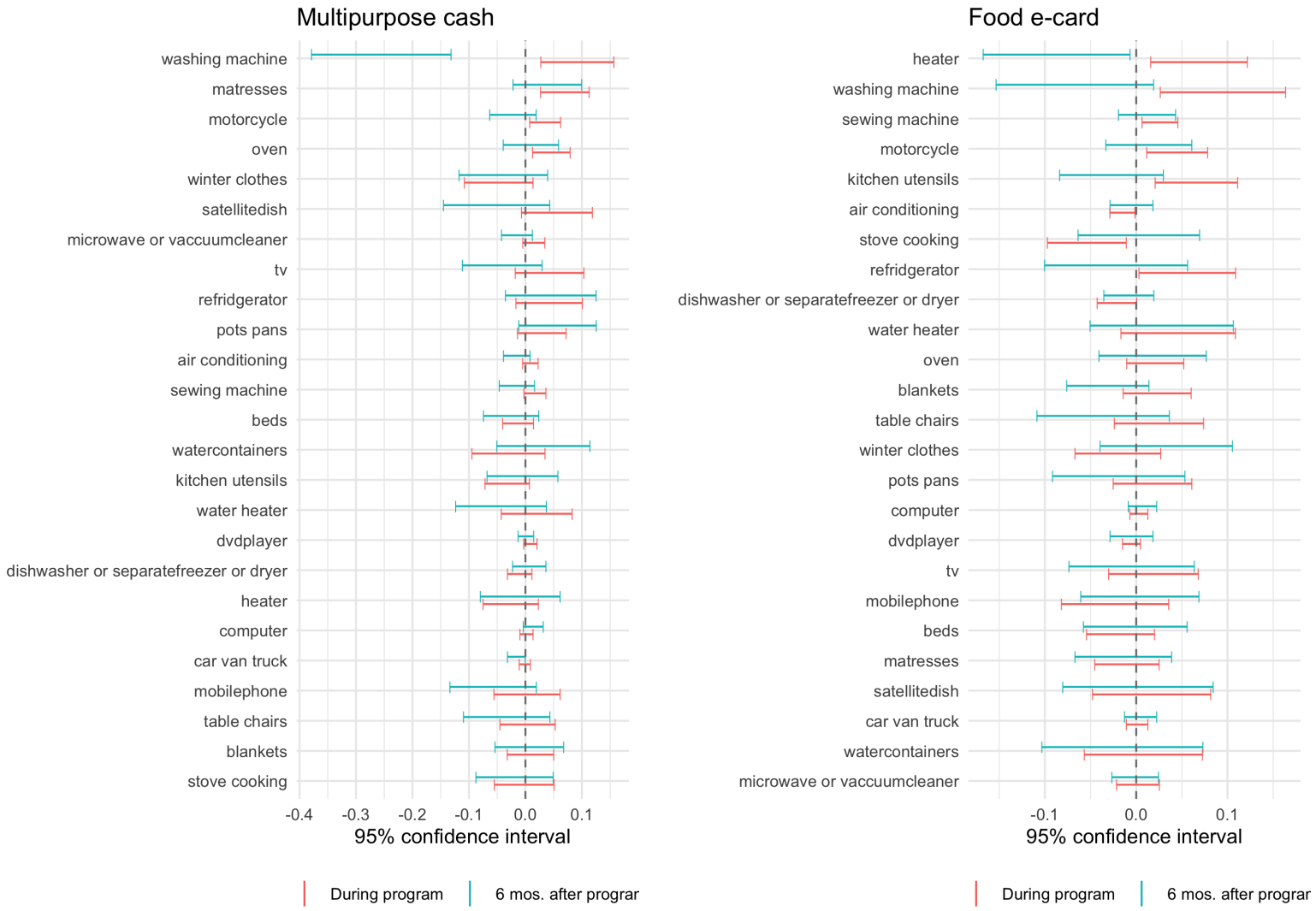
Note: Figure contains graphical depiction of coefficents from robustness specifications using a uniform kernel and varying bandwidths to determine the sample. These specifications were not prespecified.

Appendix Figure 14: 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 15: 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 $*q < .1$, $**q < .05$, $***q < .01$.

Appendix 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. 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. Note that the exchange rate was pegged in Lebanon during the study period.

Appendix Table 2

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 3: Detected and programmatic assignment thresholds across assistance programs and years

Program	Year	Type of threshold	Region	Detected threshold	Known 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 4: 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 5: Effect of assistance on primary outcomes measured pre-assignment

	Outcome measurement:				
	ln(EPC)	Child hardship (z)	Adverse health (z)	Food coping (z)	Livelihood 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 6: 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 7: 2SLS RD treatment effects across outcomes

	During program:		After program:	
	Multipurpose cash	Food e-card	Multipurpose cash	Food e-card
	(1)	(2)	(3)	(4)
Panel A: Expenditure per capita				
Program effect	0.21	0.10	−0.17	−0.05
	(0.04)	(0.05)	(0.09)	(0.09)
Benjamini-Hochberg q	$< 0.001^{***}$	0.075^{*}	0.350	0.717
Control Group Mean	77.71	85.56	82.52	92.12
Bandwidth	16.15	13.46	19.68	15.77
N	3,231	2,958	1,710	1,481
R^2	0.01	0.004	0.0002	0.002
Panel B: Child hardship				
Program effect	−0.23	−0.03	−0.01	−0.09
	(0.08)	(0.08)	(0.21)	(0.14)
Benjamini-Hochberg q	0.007^{***}	0.672	0.954	0.717
Control Group Mean	-0.09	-0.13	-0.14	-0.16
Bandwidth	13.17	12.7	13.85	14.56
N	2,224	2,192	1,050	1,166
R^2	0.01	0.003	0.002	−0.001
Panel C: Adverse health				
Program effect	−0.08	0.05	−0.26	0.20
	(0.09)	(0.07)	(0.23)	(0.14)
Benjamini-Hochberg q	0.417	0.646	0.637	0.717
Control Group Mean	0.05	0.03	0.11	0.01
Bandwidth	11.72	15.6	15.01	20.08
N	2,408	3,439	1,320	1,820
R^2	0.004	0.002	0.01	0.0003
Panel D: Food coping				
Program effect	−0.07	−0.36	0.13	−0.08
	(0.07)	(0.08)	(0.20)	(0.15)
Benjamini-Hochberg q	0.381	$< 0.001^{***}$	0.811	0.717
Control Group Mean	0	0.17	0.07	0.2
Bandwidth	16.14	14.74	16.13	18.15
N	3,247	3,268	1,434	1,688
R^2	0.01	0.01	0.02	0.002
Panel E: Livelihood coping				
Program effect	−0.15	−0.18	0.11	0.03
	(0.07)	(0.08)	(0.24)	(0.16)
Benjamini-Hochberg q	0.085^{*}	0.068^{*}	0.811	0.872
Control Group Mean	0.06	0.08	0.1	0.12
Bandwidth	15.38	13.53	13.15	16.91
N	3,108	3,013	1,146	1,587
R^2	0.003	0.001	0.001	0.001

Note: This table reports estimates of the effect of cash transfers on outcomes listed in panels. The program effect for expenditure per capita is reported in natural log points; for all index outcomes, the program effect is in units of standard deviations. 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 8: Cash transfer effects on expenditure, household/case size, and expenditure per capita

	ln(exp per HH member) (HH size from survey)	ln(exp per case member) (Case size from admin. data)	ln(total expenditure)	HH size (from survey)	Case size (from admin.)
	(1)	(2)	(3)	(4)	(5)
Panel A: Multipurpose cash - during					
Above threshold	0.14	0.10	0.11	−0.25	−0.07
	(0.04)	(0.04)	(0.04)	(0.20)	(0.18)
Benjamini-Hochberg q	0.005***	0.052*	0.027**	0.278	0.697
Control Group Mean	73.59	4.24	5.94	6.23	5.79
Bandwidth	8.01	8.07	11.42	7.1	7.46
N	1,501	1,508	2,156	1,339	1,402
R^2	0.02	0.01	0.01	0.003	0.01
Panel B: Food e-card - during					
Above threshold	0.08	0.08	0.11	0.29	0.25
	(0.04)	(0.04)	(0.05)	(0.13)	(0.13)
Benjamini-Hochberg q	0.051*	0.051*	0.051*	0.051*	0.051*
Control Group Mean	86.29	4.33	5.91	5.52	5.23
Bandwidth	13.46	13.76	12.44	12.99	11.68
N	2,958	3,023	2,773	2,916	2,636
R^2	0.003	0.002	0.01	0.01	0.02

Note: This table reports estimates of the effect of cash transfers on alternative measures of per capita expenditure and household or case size. 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 Table 9: 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.
$^{*}q < .1$; $^{**}q < .05$; $^{***}q < .01$