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Abstract

This paper evaluates the impact of multi-purpose cash assistance on Syrian refugee children living in Lebanon. Using a sharp multidimensional regression discontinuity design, we estimate the program impact of varying cash assistance durations measured over two waves of household survey data collected in 2019. The novel research design enables us to make pairwise comparisons between children from discontinued recipient households (received cash for 12 months then got discontinued in the next cash cycle), short-run cash recipient households (up to 10 months), long-term recipient households (between 16 and 22 months) and non-beneficiary eligible households. Results show that children of any MPC recipient group are transitioning from non-formal to formal schooling while also shifting away from child labor. Cash transfers improve health outcomes for pre-primary and school-aged children and reduce the likelihood of early marriage for girls aged 15-19 years.

Keywords: cash transfers, child well-being, regression discontinuity design, refugees.
JEL Classifications: D60, I15, I25, O12.

Introduction

One in four of the world's children live in a conflict or disaster setting, where meeting children's critical needs is particularly difficult due to increased demand on limited resources, potential displacement and loss of livelihoods (United Nations Children's Fund, 2019). Beyond monetary poverty, children, affected by humanitarian crises suffer from multiple forms of deprivation (malnutrition, stunting, poor health outcomes, less schooling, lower educational attainment, lower future productive capacity and standard of living, lack of protection, increased risk of child labor and child marriage). Notably, children living in conflict affected contexts are two times more likely to be undernourished compared to children living in other low- and middle- income countries and are thus two times more likely to die before the age of 5 years (World Bank, 2011). Uprisings and conflicts in the Middle East and North Africa have prevented around 13 million children from going to school (United Nations Children's Fund, 2015). Poverty and vulnerability faced by children have cumulative and long-term consequences (Barrientos and DeJong, 2006; Yaqub, 2002). Children, in particular, stand at a pivotal node of intervention since investment in their human capital can contribute to breaking the transmission of intergenerational poverty.

Lebanon alone hosts an estimated 1.5 million Syrian refugees over half of whom are children 18 years and younger (Government of Lebanon and United Nations, 2019). The majority of Syrian refugees in Lebanon live in deteriorating socio-economic conditions with limited livelihoods. Based on the 2019 Vulnerability Assessment of Syrian refugees, a yearly multi-purpose household survey targeting Syrian refugees, 73% of registered refugees lived below the poverty line set at USD 114 per person per month, and 55% below the extreme poverty line of USD 87 per person per month (United Nations High Commissioner for Refugees et al., 2019). The share of children below 18 years of age who were reported to have a disability was 3.8%, of which 32% suffered from a speech disability. Almost half children under 2 years of age (48%) were reported to be sick in the 2 weeks preceding the survey, an increase from 34% in 2017. As little as 13% of children aged between 3 and 5 were enrolled in early childhood education while enrollment increased to 69% for children between 6 and 14 years of age, indicating that almost one third were out of school. Two thirds of youth between 15 and 24 years of age were not employed, in education or in training. The percentage of children between the ages of 5 and 17 engaged in child labor was 2.6%, of which more than a quarter (27%) were involved in agriculture. This low rate is most likely due to the underreporting of child labor in this context and the latter peaks during the agriculture season (which did not coincide with the timing of data collection). While boys are more likely to be engaged in child labor (4.4% compared to 0.6%), 27% of girls aged 15 to 19 were married (United Nations High Commissioner for Refugees et al., 2019).

One form of humanitarian intervention that has garnered growing interest in fighting poverty in conflict and crisis affected contexts has been the use of cash transfers, both conditional and unconditional. Proponents of cash assistance argue that it is preferred to in-kind assistance due to its efficiency and the financial autonomy it affords affected populations. The literature also points to cash transfers as a means of easing the financial burden on households and ultimately transferring educational, social, health, and nutrition benefits to children, in effect serving as a form of social safety net (Chaluda, 2015; Mishra and Battistin, 2017).

The existing evidence on the impacts of cash transfers, broadly, on children's well-being is extensive (Bastagli et al., 2016; Kabeer et al., 2012; Fiszbein and Schady, 2009; Carmichael and Rutherford, 2015). There is compelling evidence on the potential impact of cash assistance in addressing multiple forms of deprivation faced by children. In education, the literature points to both conditional and unconditional cash transfers having positive effects on school enrollment especially among high out-of-school populations. Ferreira et al. (2009), Galasso (2006), Attanasio et al. (2005), Chaudhury and Parajuli (2010) Petrosino et al. (2012), Snilstveit et al. (2016), Saavedra and García (2012), and Baird et al. (2013) provide evidence of the efficacy of cash transfers in increasing school enrollment. The literature on child health outcomes is similarly extensive and includes numerous studies demonstrating positive impacts of cash transfers on healthcare access and usage by children and improvements in vaccination coverage (Bastagli et al., 2016; Bassani et al., 2013; Owusu-Addo and Cross, 2014; Ranganathan and Lagarde, 2012; Shei et al., 2014; Perova and Vakis, 2012; Benedetti et al., 2016; Lin and Salehi, 2013; Streuli, 2012). Child protection outcomes, on the other hand, are less explored in this strand of the literature. The evidence for the impacts of cash assistance on social protection outcomes such as child labor, early marriage, and school exit is relatively shallow with mixed/inconclusive findings (Chaluda, 2015; Mishra and Battistin, 2017).

Although there is no shortage of literature on the impacts of cash transfers on child well-being, the impacts in humanitarian contexts are less explored in general, much less in the context of the Syrian refugee population in Lebanon (Puri et al., 2017; Doocy and Tappis, 2016; Bruck et al., 2019). There have been a number of studies on the impact of cash transfers in Lebanon. Lehmann and Masterson (2014) and Battstin (2016) evaluate a winter cash transfer program for Syrian refugees in Lebanon. The studies examined the impact of a cash transfers that is limited in reach as regions with highest number of refugees were not included in the analyses. Further, these studies estimated the impact of cash assistance on a range of household-level outcomes such as consumption, coping strategies, and overall household well-being. School attendance was the only child well-being outcome examined, with mixed findings. As such, these studies suffer from a lack of thorough examination of children's well-being outcomes, relatively small sample sizes, and limited external validity of the scale of the interventions. The broader literature also does not delve into the temporal dynamics of cash transfers especially in terms of children's education, health, and protection outcomes (Chaluda, 2015).

This paper contributes to the existing literature by evaluating the impacts of a large-scale multi-purpose cash (MPC) transfer program on a sample of Syrian refugee children in Lebanon by examining children's health status, access to and usage of healthcare, formal and non-formal school enrollment, child labor, and early marriage. We also exploit changes in the MPC eligibility criteria in Lebanon to identify effects of cash assistance among households with varying durations of exposure. This approach provides valuable insight into the effect of cash assistance over time: do they persist or attenuate following discontinuation? To our knowledge, this contribution is novel in the greater discussion on cash transfer programs and informs the temporal aspects of cash transfers that is crucial in determining the efficacy of such programming (Aizer et al., 2016; Bastagli et al., 2016).

Our empirical strategy goes beyond prior research by employing a multidimensional regression discontinuity which follows from the conditions created by the change in the MPC eligibility criteria. The changes to the formula took place between November 2017 and November 2018 resulting in the creation of four distinct groups of households, or treatment conditions. Households who are newly eligible for MPC, households who were eligible but are no longer (discontinued), and households who maintained their (in)eligibility throughout. In essence, this setting constitutes two treatments and two assignment conditions, which lends itself naturally to a multidimensional regression discontinuity design (MMRD) (Papay et al., 2011; Salti et al., 2020). The MMRD approach, thus, enables us to measure the impacts of varying durations/conditions of cash assistance by comparing beneficiaries to non-beneficiaries as well as beneficiaries from different treatment conditions to each other. We are, thus, able to create six sets of pairwise comparisons identifying the short-term, long-term, and discontinuation of receipts relative to none; short-term relative to sustained (long-term) receipts; and discontinuation of MPC relative to sustained receipts.[1] With this approach, we are able to identify the effects MPC beyond a single cutoff point as in typical RDD applications, but at multiple frontiers of the joint MPC eligibility distribution.

Background and Conceptual Framework

Syrian refugees in Lebanon and overview of the cash transfer program

In 2017, WFP joined UNHCR and other organizations in the delivery of MPC amounting to $175 per household per month to eligible households over a 12-month period. The transfer, which is provided in the form of direct cash given to beneficiaries with no pre- or post-conditions or requirements, aims to alleviate economic hardship and bridge the expenditure gap of households living below the survival minimum expenditure basket set at $87 per person per month. A proxy means testing (PMT) formula is run on a yearly basis to predict household expenditures and

[1] We define short-term recipients as those households who have newly received MPC for less than 10 months, long-term recipients as those who have been receiving MPC across both cycles up to 22 months, and discontinued recipients as those who received MPC for the first 12 months but subsequently became ineligible.

determine their eligibility for the WFP/UNHCR MPC program. The formula uses a set of socio-demographic characteristics from the UNHCR registration database[2] and is re-estimated yearly using newly collected survey data.

All households whose predicted expenditures are below the $87 per person per month line are eligible to receive MPC. However, due to budgetary constraints, not all eligible households are included in the program. UNHCR employs a geographical bottom-up approach for its MPC program by sequentially including households with the lowest predicted expenditures in each region until it reaches the region's allocated proportion. WFP uses the same approach without regional stratification. This creates an arbitrary eligibility cutoff along the PMT score distribution around the last households included and households on either side of this cutoff are arguably similar along observable and unobservable characteristics. This creates a quasi natural experiment where the only systematic difference between households just below and just above the cutoff is access to MPC, so that differences in outcomes can plausibly be attributed to the program (Imbens and Lemieux, 2008).

Given households from our target population are highly vulnerable, they have access to other cash assistance programs, with the largest being the WFP food assistance program that targets all households in our study. Food assistance, which amounts to $27 per person per month over a 12-month period, is provided either through unrestricted cash or through e-vouchers that are redeemable at all WFP-contracted shops across the country. All households with a PMT score below the $87 per person per month poverty line are eligible for this program. UNHCR provided us with a list of the main cash and non-cash programs all households in our sample were receiving. The two other main cash programs that targeted our population of interest are the UNICEF child-focused cash programs and the yearly winter cash assistance program.

The purpose of the cash transfer intervention does not explicitly mention the well-being of children, as in principle, even households with no children can be eligible. Still our paper focuses on two cash cycles that started in November 2017 and 2018 to determine whether the benefits of MPC transfer to children in recipient households (UNHCR, 2020). As such, we estimate the net effect of MPC on school enrollment, health status, healthcare access and usage, child labor, and early

[2] The PMT regressions for both cash cycles we look at in this study (2017 and 2018) use the same regressors extracted from the UNHCR refugee registration database. These include socio-demographic characteristics of the household head, the year of arrival to Lebanon and district of arrival, occupation in Syria, household size, dependency ratio (number of members below 15 or above 64 divided by the number of working-age members), indicator variables for single parent households, the presence of more than 3 dependents in the household, at least 1 dependent with a disability, the share of members with no education, the share of members in each age group, the share of members with a disability, members above 60 with a medical condition (Altindag et al, 2019).

marriage among girls. Further, the re-calibration of the PMT formula between the two cycles led to a sizeable shift in MPC beneficiary households. This shift uniquely positions our paper to investigate the impact of MPC on groups with varying cash assistance durations based on their joint eligibility status for the 2017 and 2018 cash cycles using their 2017 and 2018 PMT scores.

Impact of cash transfers on child well-being

With the recent growth of cash transfer programs in humanitarian contexts, a growing body of literature investigating their overall effectiveness has also emerged. Growing evidence points to cash transfer as an effective intervention to improve child well-being. Cash assistance provides families with supplemental resources enabling them to better meet their children's needs, invest in their capital and break the intergenerational cycle of poverty (Harman, 2018; Bastagli et al., 2016). Understanding how household resources are allocated within the household and how cash interventions affect the most vulnerable in the household is key for designing effective interventions. Children are one of the most vulnerable groups in society since they depend on others and do not command economic resources of their own. Chaluda (2015) notes a consistent positive impact on certain basic child outcomes directly related to cash influx (food consumption, use of health service, school enrolment and attendance) in the cash transfers literature. Outcomes indirectly associated with cash transfers, however, exhibit mixed results. These include child anthropometry, grade attainment and progression, school performance, child labor, and early marriage.

Several evaluations have found a positive impact of cash assistance in increasing children's access to preventive health care (Bastagli et al., 2016; Bassani et al., 2013; Owusu-Addo and Cross, 2014; Ranganathan and Lagarde, 2012; Shei et al., 2014; Perova and Vakis, 2012; Benedetti et al., 2016; Lin and Salehi, 2013; Streuli, 2012) and growth monitoring services, with mixed result on improvement in vaccination coverage. Increased health utilization does not always translate in improved health outcomes among children. Evidence on effects on child morbidity is inconclusive with some findings suggesting a reduction in the incidence of acute malnutrition and illness specifically diarrhea, cold and flu primarily among children younger under 5 years old (Attanasio et al., 2005; Fernald et al., 2009; Gertler, 2004; Rivera et al., 2004; Maluccio and Flores, 2005).

A large number of evaluations have demonstrated that cash transfers increase the demand for education and improve school enrolment and attendance (Petrosino et al., 2012; Snilstveit et al., 2016; Saavedra and García, 2012; Baird et al., 2013) . Stronger evidence on the impact of cash assistance on intermediate education outcomes is noted (such as increasing enrollment, attendance, decreasing drop out) with less conclusive evidence on the impact of cash assistance on second-level outcomes such school performance and cognitive development (Mishra and Battistin, 2017; Chaluda, 2015). In fact, a recent systematic review and a meta-analysis found no significant impact of cash transfers on improving children test scores (Barham et al., 2012).

Few studies evaluate the impact of cash transfers on child protection outcomes (Mishra and Battistin, 2017). While addressing child rights violation demands action in multiple sectors, cash transfers may play a role in reducing child exploitation, by alleviating financial pressures that may force children out of school, into work, and into early marriage. Evidence from non-humanitarian settings indicates that cash assistance is associated with a significant reduction in the intensity of child labor (number of working hours) with less conclusive evidence on the prevalence of working children (Lehmann and Masterson, 2014; Rosas and Sabarwal, 2016; Mishra and Battistin, 2017). Evidence shows that both conditional and unconditional transfers may lead to a significant increase in the age of marriage among girls, and a subsequent delay in the age of first childbirth (Priya et al., 2014; McQueston et al., 2013).

Data and Empirical Strategy

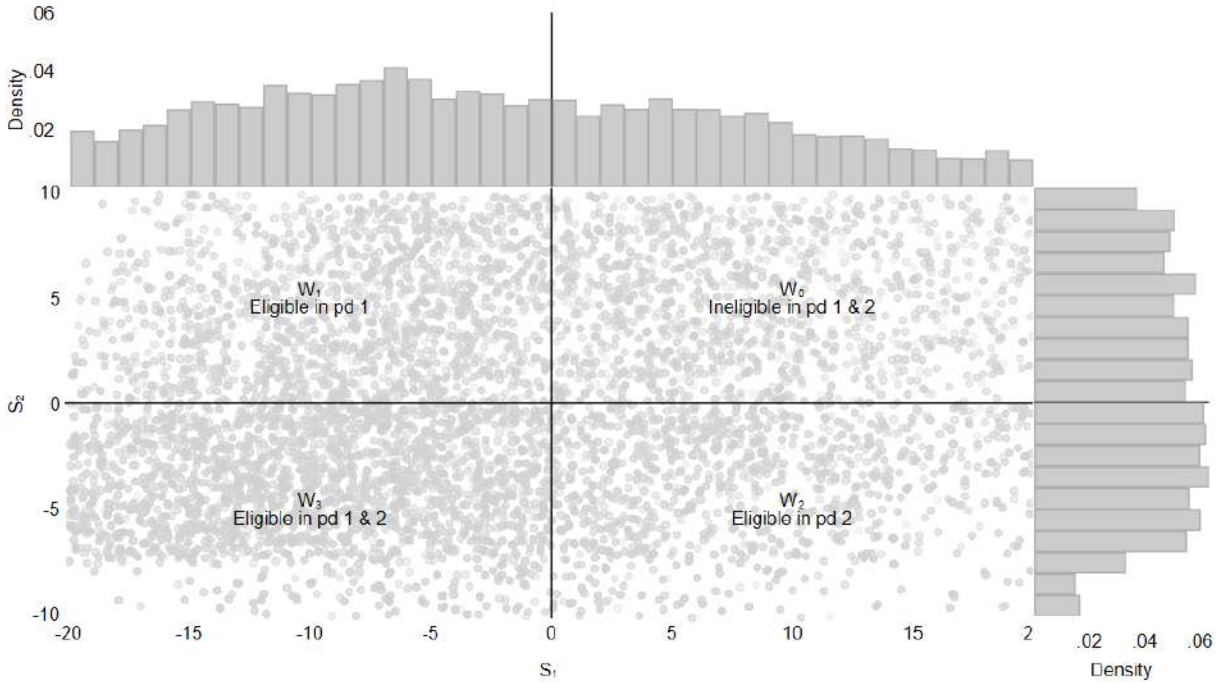
Data and Sample Summary

We draw upon two waves of primary data collected using a multi-purpose household survey in February/March 2019 (wave 1) and again in July/August 2019 (wave 2). The survey design relied on repeated cross-sections of Syrian refugee households from three regions of Lebanon (Mount Lebanon, North Lebanon, and Bekaa) where about 85% of all Syrian refugees in Lebanon resided during the study period, based on the UNHCR registration database. In addition, we restricted the sampling frame to households whose PMT scores were within +/- 10 points of the 2018 MPC eligibility cutoff and +/- 20 points of the 2017 desk score cutoff.[3] We, hereafter, denote the eligibility desk scores as S_1 and S_2 corresponding to the two cycles covered in our data. The sampling followed a standard probability proportional to size approach to maintain representativeness of the population of Syrian refugees in the three regions and within the MPC eligibility cutoff score intervals. We apply sampling weights to all analyses to maintain regional representation, extracted from the UNHCR registry of Syrian refugees in Lebanon at the time of the data collection. We append to the survey data information from the UNHCR refugee registry database, including households' PMT scores in 2017 and 2018 and household access to other assistance programs. The survey data was linked to information from the registration database using a unique scrambled household identifier to form the analytic dataset. The dataset contains demographic information at the household and individual levels including their PMT desk scores and MPC receipt status from 2017 and 2018. Additionally, the data includes information on key health, education, and early marriage outcomes of children under the age of 19 years.

[3] We restrict sample eligibility to +/- 10 points on the 2018 score since the 2018 distribution was the basis of our sampling. While the 2017 score distribution had a larger variance within that range which required a less stringent restriction at +/- 20 points.

For the purposes of this paper, we restrict the analytic sample to individuals who are under the age of 19 years and their corresponding household. This yields a final sample of 6,207 households (2,992 in wave 1 and 3,215 in wave 2) and 24,859 individual observations (11,843 in wave 1 and 13,016 in wave 2). Figure 1 displays the distributions along S_1 and S_2 from the analytic sample that exhibit uniform and smooth distributional properties around the eligibility cutoffs. The figure also displays the division of the sample into 4 mutually exclusive treatment conditions that are denoted as W_0, W_1, W_2, and W_3 corresponding to whether households were recipients of MPC in neither the first nor second period, only the first, only the second, or both, respectively.

Figure 1. S_1 and S_2 desk score distributions



We differentiate the two periods of MPC disbursement because the formula used to determine PMT desk scores changed between the two time periods resulting in changes in the treatment status of refugee households. The change in the formula consequently resulted in groups of households who were eligible for MPC but are no longer (W_1), households who were not eligible for MPC in the first period but are in the second (W_2), and households who maintained eligibility for MPC benefits throughout both time periods (W_3). This distinction allows us to detect potentially heterogeneous effects of receiving cash transfers across these different treatment categories. Moreover, data from the analytic sample show that the change in the desk score calculation resulted in substantial changes in the households receiving MPC between 2017 and 2018. Approximately 42.7% of households observe a change in their treatment status between 2017 and 2018. Specifically, 27.4% of households who received MPC in 2017 were discontinued as a result of the new desk score formula, 15.3% of households who were not receiving any MPC benefits in 2017 are now receiving benefits, and the remaining 57.2% of households maintained receipt (28.8%) or non-receipt (28.5%) of benefits through both periods.[4]

Table 1 shows sample means of observed characteristics of the respondents and their outcomes. We observe a high compliance rate within each treatment condition such that only 4%-4.6% of households who were eligible for MPC in 2017 and 1.4%-2.9% of households who were eligible for MPC in 2018 did not receive their respective MPC benefits. At the same time, we see that approximately 3.1% and 1.6% of ineligible households received MPC benefits in 2017 and 2018, respectively. In terms of the demographic characteristics of the children and adolescents, the overall average age is 8.7 years with means ranging between 8.1 and 9.2 across the treatment conditions. Here, the group W_0 appears to be the youngest, on average, while the group W_3 is the oldest.

At the household level, we use information on the household head to serve as additional covariates that may be correlated with the outcomes and treatment status. We find that the average age, the proportion who are married, and average years of schooling of household heads is somewhat stable across the treatment conditions with average age of about 40 years, approximately 90% are married, and the average years of schooling is 6.14 years. We also see that the proportion of household heads who are female varies between the groups, whereby the group receiving no MPC in either period has the lowest proportion of female household heads at 11%, and the group maintaining MPC status has the highest proportion at 20%. Lastly, the data show some differences in household size across treatment conditions with households in W_0 having 6.3 household members and households in W_3 have 7.1 members, on average. Although we observe some differences in the average characteristics between the four groups, our estimation strategy relies on the average characteristics near the cutoffs to be similar. We will thus test whether these characteristics are significantly different at the relevant points of comparison.

[4] Our sample proportions are in line with UNHCR registry database population proportions, where 24.4% of households in the registry were eligible for MPC in 2017 but no longer in 2018, 15.4% were eligible as of 2018 but not in 2017, and 60.2% of households did not experience a change in their MPC eligibility status.

Table 1. Sample summary statistics, by treatment condition

	W_0	W_1	W_2	W_3	Total
MPC Benefit Status:					
MPC 2017 - Period 1	0.031	0.960	0.032	0.956	0.528
	(0.173)	(0.196)	(0.176)	(0.205)	(0.499)
MPC 2018 - Period 2	0.016	0.019	0.971	0.986	0.462
	(0.126)	(0.137)	(0.168)	(0.118)	(0.499)
Demographics:					
Age	8.14	8.99	8.45	9.23	8.72
	(5.38)	(4.96)	(5.24)	(4.93)	(5.15)
Female	0.49	0.48	0.50	0.50	0.49
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Household Head:					
Age	39.80	39.61	40.54	40.44	40.07
	(9.42)	(7.76)	(8.69)	(8.47)	(8.66)
Female	0.11	0.17	0.12	0.20	0.16
	(0.32)	(0.38)	(0.32)	(0.40)	(0.36)
Married	0.92	0.89	0.91	0.85	0.89
	(0.28)	(0.32)	(0.28)	(0.36)	(0.31)
Years of schooling	6.25	6.57	5.81	5.87	6.14
	(3.17)	(3.29)	(3.11)	(3.42)	(3.28)
Household Size	6.32	6.55	6.91	7.10	6.70
	(2.10)	(1.90)	(2.09)	(1.91)	(2.02)
Child Outcomes (0-5 years):					
Acute illness (last 6 months)	0.48	0.51	0.43	0.44	0.47
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Diahrrea	0.23	0.25	0.22	0.22	0.23
	(0.42)	(0.44)	(0.41)	(0.41)	(0.42)
Respiratory infection	0.11	0.11	0.08	0.10	0.10
	(0.31)	(0.32)	(0.28)	(0.30)	(0.30)
Required PHC	0.53	0.62	0.45	0.55	0.54
	(0.50)	(0.48)	(0.50)	(0.50)	(0.50)
Used PHC	0.50	0.48	0.51	0.50	0.50
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Child Outcomes (6-14 years):					
Enrolled in formal school	0.64	0.61	0.64	0.57	0.61
	(0.48)	(0.49)	(0.48)	(0.49)	(0.49)
Enrolled in non-formal school	0.07	0.14	0.09	0.14	0.11
	(0.25)	(0.35)	(0.29)	(0.35)	(0.32)
Worked (last month)	0.02	0.02	0.02	0.01	0.02
	(0.14)	(0.15)	(0.15)	(0.12)	(0.14)
Required PHC	0.31	0.41	0.25	0.37	0.34
	(0.46)	(0.49)	(0.43)	(0.48)	(0.47)
Used PHC	0.39	0.36	0.40	0.39	0.38
	(0.49)	(0.48)	(0.49)	(0.49)	(0.49)
Early Marriage (15-19 years):					
Married	0.08	0.04	0.08	0.05	0.06
	(0.28)	(0.19)	(0.27)	(0.22)	(0.24)
Observations	**5,654**	**5,360**	**4,804**	**9,056**	**24,874**

Notes: Figures in the table correspond to sample means and standard deviations (in parentheses) using sampling weights.

The lower half of Table 1 displays the means of outcomes of children ages 0-5 years, 6-14 years, and adolescents 15-19 years. We follow this age grouping to conform with compulsory education age requirements imposed in Lebanon, which also serves as the age cutoff for child labor laws in Lebanon (UIS, 2014). As such, we examine different sets of outcomes that are relevant for each age group. For the youngest group, children aged 0-5 years, we collect information on whether the child has contracted any type of acute illness, diarrhea, and respiratory infections in the 6 months prior to the interview. Across the full analytic sample, approximately 47% of all children in this age group were reported to have contracted an acute illness, in general, with almost half reporting having diarrhea. About 10% of all children 0-5 years old were reported to have a respiratory infection in the past 6 months as well. We also asked respondents to report if they have required access to a primary health care (PHC) such as for preventative consultations, acute or chronic illnesses, accident or injury, mental health and diagnostic tests. If they responded as requiring PHC in the past six months we asked the respondents to report whether they accessed any PHC services. Overall, more than half the respondents reported requiring some type of PHC, only half of whom reported actually receiving PHC.

For children who are age-eligible for compulsory schooling in Lebanon, we collect information on their enrollment status in formal and non-formal schooling,[5] whether they engaged in non-home related work in the past month (child labor), whether they required PHC, and if they accessed PHC in the past 6 months. A little over 60% of the sample reported being enrolled in a formal school (public and private schools), the vast majority of which was government formal schools, and 11% enrolled in non-formal schooling (non-formal learning centers such as NGOs and community centers). This also means that almost 30% of school-aged children were out of school entirely. On average, about 2% of all school-aged children reported some type of employment. The proportion of children who work increased with age, up to approximately 8% by age 14 years. A third of school-aged children reported needing PHC in the past 6 months, under 40% of whom actually received PHC.

Finally, we report the percentage of girls aged 15-19 years who are married, including those who report being separated, divorced, or widowed. For the purpose of this paper, we consider the case of a girl who is or has been married by the age of 19 to be an early marriage. Across the full analytic sample, about 13% of girls report being, or having been, married by the age of 19 years. Interestingly, we find the group of households who receive MPC in neither period reporting the highest rate of early marriage at 17%, while the remaining groups report rates between 10% and

[5] Non-formal educational programmes are used to bridge the gap with formal education so that children may eventually integrate into the public school system. Non-formal educational programmes in Lebanon include Community-based Early Childhood Education, Basic Literacy and Numeracy (BLN) and the Accelerated Learning Programme (ALP) that cater for different age groups and education levels. With the strain on the educational system in Lebanon following the crisis, some refugee families choose to register their children in non-formal schools/programmes, especially where public schools are too far to reach or have no space to enroll their children at the appropriate grade levels. Parents also choose non-formal programmes to avoid corporal punishment, difficult curricula, or a lack of attention in public schools. Others choose non-formal education as a first step to ensure children continue learning if they have missed several years of school in order to enrol in public schools later (Human Rights Watch, 2016).

16%. It is important that we acknowledge potential downward biases in the self-reported child labor and early marriage rates as these topics may be considered taboo by the respondents, which could have implications on our subsequent analyses and inferences. Further, the simple between-group mean comparison is observational and not causal, as it depicts average outcomes of children in the various treatment groups, and not in the vicinity of the cutoff points of S_1 and S_2, the proxy measures of vulnerability.

Empirical Strategy

Following Salti et al. (2020) we employ a multidimensional regression discontinuity (MMRD) design with two treatments and two running variables. The two treatments, in this case, refer to the 2017 and 2018 implementations of MPC along with their respective PMT assignment scores. Although the treatments and assignment mechanisms are essentially similar in nature, the recalibration of the PMT scoring formula between the two periods led to the creation of four treatment conditions. This enables us to pool the effects of MPC for those who were initially eligible to receive MPC but were discontinued, those who are newly eligible to receive MPC, and those who maintained eligibility for MPC benefits across both periods. Formally, we denote eligibility for receipt of MPC in 2017 and 2018 with D_1 and D_2, respectively. Eligibility for treatment is assigned using the desk score, S, for each administration of the treatment in time j.

$$D_j = \begin{cases} 1, S_j \leq 0 \\ 0, S_j > 0 \end{cases}; j = 1, 2 \qquad [1]$$

We define the four possible treatment conditions based on eligibility for MPC in 2017 and 2018 as follows:

$$\begin{aligned} W_0 &\equiv (1 - D_1)(1 - D_2) \\ W_1 &\equiv D_1(1 - D_2) \\ W_2 &\equiv D_2(1 - D_1) \\ W_3 &\equiv D_1 D_2 \end{aligned} \qquad [2]$$

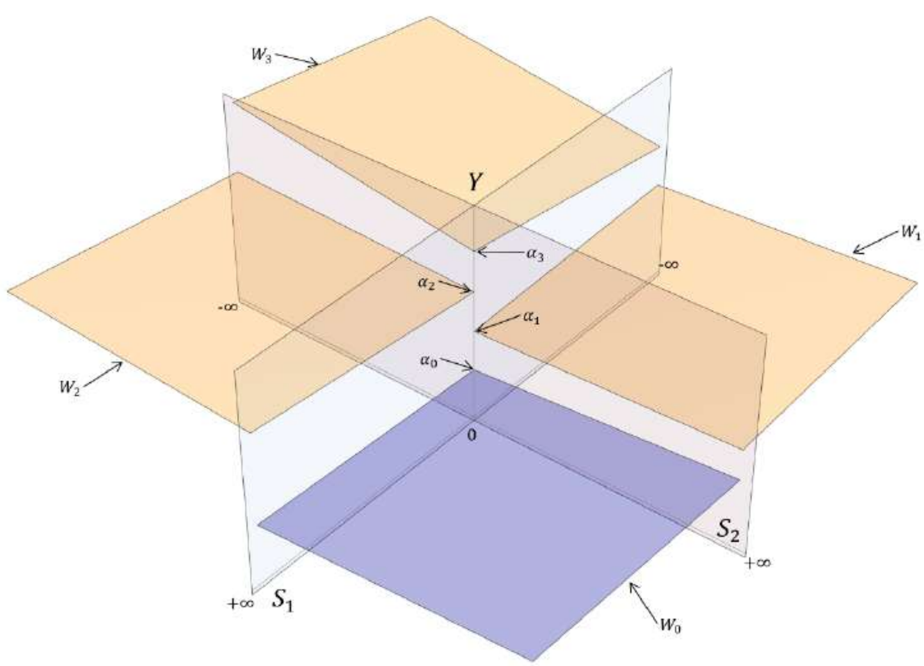
The treatment conditions, W_k, define four separate regions in the (S_1, S_2) space, where W_0 is the group of households who were not eligible for MPC either in 2017 or 2018; W_1 is the group of households who were eligible for MPC in 2017, but were no longer eligible in 2018 as a result of the change in the desk score formula; W_2 is the group of households who were not eligible in 2017, but became eligible in 2018 with the change in the desk score formula; and W_3 is the group of households who were eligible for both rounds of MPC.

Figure 2 illustrates how the MMRD strategy enables us to make six pairwise comparisons of treatment conditions along the cutoff boundaries of S_1 and S_2. This enables us to identify the causal effect of one treatment condition over another by comparing the outcomes of children along these cutoff boundaries (Papay et al., 2011; Reardon and Robinson, 2012; Wong et al., 2013). For instance, the effect of W_1 relative to W_0 is the difference in outcomes between households just left and right of the orthogonal plane $(Y, S_1 = 0, S_2 \geq 0)$ that separates the two treatment areas (see Figure 1). At the same time, this approach identifies a treatment function along each of the four frontiers rather than a single local average treatment effect at one cutoff point as in typical regression discontinuity applications. This enables us to determine the effect of MPC when assignment scores approach each of the cutoffs as well as at points of the distribution away from the cutoff along the desk score axes. As with any RDD, the causal effects identified using this multidimensional approach are local to the frontier cutoffs and, thus, may not hold elsewhere in the joint desk score distribution away from the desk score axes. We represent the sharp MMRD model, assuming perfect compliance with the assignment variables, as:

$$Y_i = \sum_{k=0}^{3} W_{ki}(\alpha_k + \beta_k S_{1i} + \gamma_k S_{2i} + \delta_k S_{1i} S_{2i}) + \varepsilon_i \quad [3]$$

Equation (3) defines four distinct three-dimensional surfaces covering each of the regions of the four treatment conditions defined by W_k. Figure 2 demonstrates how α_k represents the Y-intercept of the surface covering region k at $(S_1, S_2) = (0,0)$. β_k represents the slope of the edge of the same surface when it intersects the orthogonal plane at $S_2 = 0$, and γ_k is the slope of the edge of the surface at $S_1 = 0$. δ_k denotes the gradient of the space inside each region. ε denotes the idiosyncratic error term.

Figure 2. Illustration of equation (3)



The conditional expectation for each region becomes:

$$E[Y_i|W_{ki} = 1] = \alpha_k + \beta_k S_{1i} + \gamma_k S_{2i} + \delta_k S_{1i} S_{2i}; \quad k = 0, 1, 2, 3$$

We can, thus, identify the causal effects from neighboring treatment conditions along the frontiers separating the 4 regions as follows.

Effect of losing MPC eligibility, $W_1(D_1 = 1, D_2 = 0)$, relative to never being eligible, $W_0(D_1 = 0, D_2 = 0)$:

$$\begin{aligned}\tau_{10} &= \lim_{S_1 \to 0^-} E[Y|W_1, S_1, S_2] - \lim_{S_1 \to 0^+} E[Y|W_0, S_1, S_2] \\ &= (\alpha_1 - \alpha_0) + (\gamma_1 - \gamma_0) S_2\end{aligned}$$

Effect of being newly eligible for MPC, $W_2(D_1 = 0, D_2 = 1)$, relative to never being eligible, $W_0(D_1 = 0, D_2 = 0)$:

$$\begin{aligned}\tau_{20} &= \lim_{S_2 \to 0^-} E[Y|W_2, S_1, S_2] - \lim_{S_2 \to 0^+} E[Y|W_0, S_1, S_2] \\ &= (\alpha_2 - \alpha_0) + (\beta_2 - \beta_0) S_1\end{aligned}$$

Effect of maintaining eligibility, $W_3(D_1 = 1, D_2 = 1)$, relative to losing MPC eligibility, $W_1(D_1 = 1, D_2 = 0)$:

$$\begin{aligned}\tau_{31} &= \lim_{S_2 \to 0^-} E[Y|W_3, S_1, S_2] - \lim_{S_2 \to 0^+} E[Y|W_1, S_1, S_2] \\ &= (\alpha_3 - \alpha_1) + (\beta_3 - \beta_1) S_1\end{aligned}$$

Effect of maintaining eligibility, $W_3(D_1 = 1, D_2 = 1)$, relative to being newly eligible for MPC, $W_2(D_1 = 0, D_2 = 1)$:

$$\begin{aligned}\tau_{32} &= \lim_{S_1 \to 0^-} E[Y|W_3, S_1, S_2] - \lim_{S_1 \to 0^+} E[Y|W_2, S_1, S_2] \\ &= (\alpha_3 - \alpha_2) + (\gamma_3 - \gamma_2) S_2\end{aligned}$$

Figure 3 illustrates the treatment function produced by the MMRD estimation for the effects of $W_1|W_0$ and $W_3|W_2$ along S_2 while $S_1 = 0$. We note that the two remaining comparisons between the pair of treatment conditions (W_3, W_0) and (W_1, W_2) evaluated at the limits of S_1 and S_2 would collapse to a single point at $(S_1, S_2) = (0,0)$ as these pairs only meet at the origin.

As such we determine the effect of maintaining eligibility,W_3, relative to never being eligible, W_0 as:

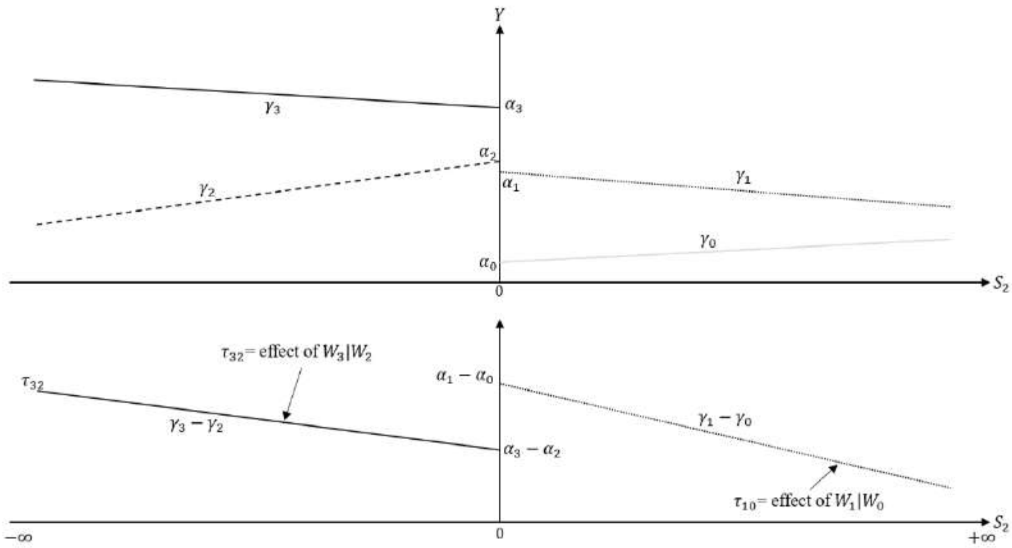
$$\tau_{30} = \lim_{S_1, S_2 \to 0^-} E[Y|W_3, S_1, S_2] - \lim_{S_1 S_2 \to 0^+} E[Y|W_0, S_1, S_2]$$

$$= \alpha_3 - \alpha_0$$

And the effect of losing eligibility, W_1, relative to being newly eligible, W_2, as:

$$\tau_{12} = \lim_{S_1, S_2 \to 0^-} E[Y|W_2, S_1, S_2] - \lim_{S_1 S_2 \to 0^+} E[Y|W_1, S_1, S_2]$$
$$= \alpha_2 - \alpha_1$$

Figure 3. Density test of joint desk score distribution along cutoff boundaries



We estimate the MMRD model parametrically via OLS using observations within an optimal bandwidth following a two-step procedure described in Imbens and Lemieux (2008) using iterative cross-validation to determine the optimal joint bandwidth (h_1, h_2) in the (S_1, S_2) space.[6] We estimate the parameters of the local average treatment function using only observations lying within the optimal bandwidths h_1^* and h_2^* for along the boundary of each of the four aforementioned regions. For instance, we would be estimated the treatment function between W_1 and W_0 as $\hat{\tau}_{10} = E[Y|W_1, -h_1^* \geq S_1 \geq 0, S_2] - E[Y|W_0, 0 \leq S_1 \leq h_1^*, S_2]$.

We can also recover the local average effect on compliers using a fuzzy MMRD where the causal estimand is the sharp MMRD estimator divided by the difference in the conditional probability function of compliance along the relevant treatment boundary frontier (Wong et al., 2013). However, for this paper we chose to estimate the sharp MMRD which is the intent-to-treat (ITT) effect due to the low noncompliance rate as evidenced in Table 1. We acknowledge that the ITT

[6] We follow Papay et al. (2011) to select the bandwidth that minimizes the following cross-validation criterion: $CV_Y(h_1, h_2) = \frac{1}{N}\sum_i \left(Y_i - \hat{Y}_i(S_{1i}, S_{2i}, h_1, h_2)\right)^2$.

estimand will be downward biased. As such, we consider the MMRD estimates presented in this paper to be a conservative lower bound.

MMRD Design Validity

For the MMRD model to yield causal effects, or in our case, functions of MPC on children's health, education, labor, and early marriage outcomes, households must not alter their characteristics in a manner that influences their PMT desk score. Additionally, observed household characteristics must maintain smoothness or continuity along the cutoff boundaries to ensure that the only discontinuities along the cutoff boundaries or frontiers are the households' probability of receiving the treatment(s).

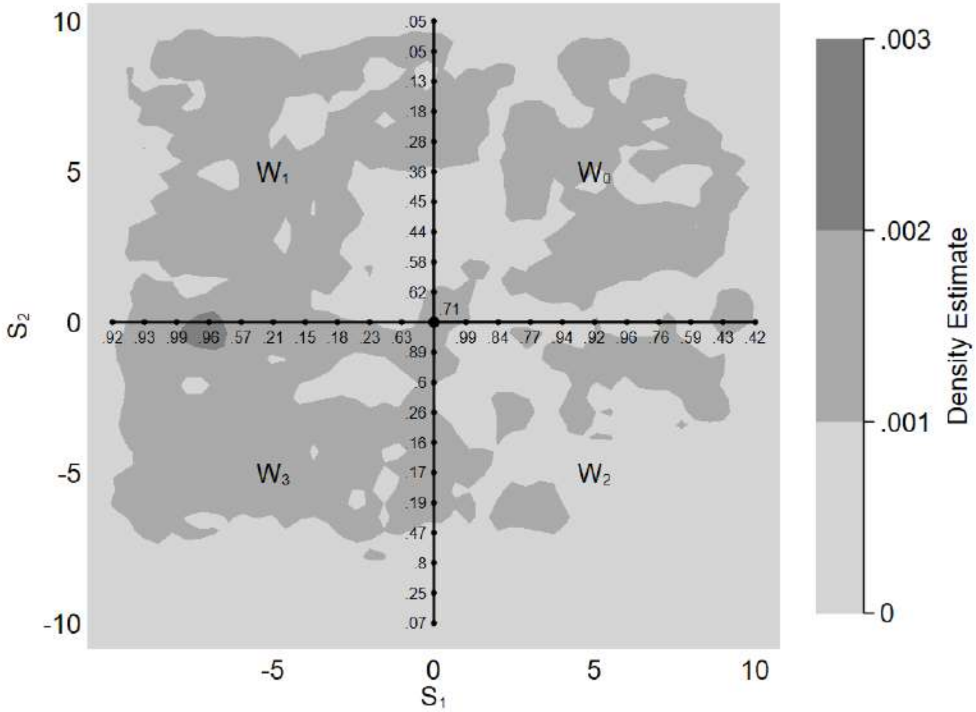
Desk score manipulation

We argue that Syrian refugee households in Lebanon cannot manipulate their position in the desk score distribution, at least not prior to receiving benefits. The eligibility criteria are determined as a predicted score that is a function of household socio-demographic characteristics that are collected as part of household registration into the UNHCR database and are unpublicized. Further, the disbursement of MPC is region specific and as we have discussed earlier creates varying score cutoff points depending on the region. The desk formula is also re-estimated each year, making it more difficult for households to predict or anticipate changes in the eligibility criteria and game the system. It is still possible for households to attempt manipulation of certain characteristics to ensure eligibility for assistance even when eligibility criteria are not explicitly stated.

We conduct a series of tests for score manipulation of desk scores, S_1 and S_2, at the cutoff boundaries using a joint density test that is analogous to the density test developed in McCrary (2008) extended to the two assignment variables case. Cattaneo et al. (2019a) recommend using a local polynomial density estimator along the two assignment variable space to test for manipulation along the boundary cutoffs. This method enables us to test for differences in the density estimates at multiple points along each of the 4 boundary regions. As a result, we are able to provide supporting evidence that households do not manipulate their desk scores overall and at specific points along the 4 boundary regions.

Figure 4 presents a contour plot of the three-dimensional density for S_1 and S_2 where we show the p-values corresponding to the density test statistic at 10 equidistant points along each cutoff boundary. We find that the manipulation test fails to reject the null hypothesis that the densities along each of the 40 points on the boundary frontiers are continuous at the 5% level. However, The test statistic leads us to reject the null hypothesis at three points, $(9,0)$, $(10,0)$, and $(-10,0)$ at the 10% level. We argue that these differences are within the 10% significance threshold with only 3 out of 40 tests being significant.

Figure 4. Density test of joint desk score distribution along cutoff boundaries



Notes: figures along the S_1 and S_2 axes denote p-values of the manipulation test statistics at the corresponding (S_1, S_2) boundary coordinate.

Continuity of confounding factors

The second condition requires that observed characteristics are not discontinuous at the cutoff boundaries. Here, we estimate equation (3) where we use the covariates as dependent variables to test for discontinuities at the cutoff frontiers. Here, we test whether the child's gender and age, the household head's gender, age, marital status, educational attainment, and household size are continuous along the cutoff frontiers. This means that the desirable outcome of this test is such that the treatment function along the covariates are not statistically significant to ensure that the estimated effects of MPC can be construed as causal.

Table 2 presents the results of the tests for covariate continuity. We find that children included in the analytic sample do not differ significantly along any of the cutoff boundaries in terms of gender, age, or household size. We also find that the characteristics of their household heads are largely similar in terms of age and educational attainment. We find statistically significant differences in the proportion of household heads who are female between households who are discontinued (W_1) and short-run (W_2) recipients, and non-recipient households (W_0) at the 5% and 10% levels respectively. We also find that the proportion of households whose head is female decreases with S_2. Household heads of discontinued and short-run recipients are also more likely to be married relative to their non-recipient counterparts, while the household heads of long-term recipients (W_3)

are less likely to be married than the discontinued group and slightly more likely to be married relative to the short-run group.

Based on these results, we cannot conclude that the covariates female household head and married household head satisfy the smoothness condition for identification of the causal effects of MPC. However, to ensure that these potential differences in the observed factors do not confound the identification of the ITT effect functions along the cutoff boundaries, we include all covariates in the main regression specification described in equation (3). In addition to the covariate adjustment, we run the MMRD specification both with and without covariates to assess whether any differences in the covariates pose a credible threat to the validity of the MMRD estimates.

Limitations

The causal effects we estimate in this paper may have limited external validity beyond the frontier regions of the (S_1, S_2) space. Households and children who are far enough from these borders may not react similarly to a change in their MPC status as those households near the borders. Another drawback of the analysis presented in this paper is that we normalized both running variables and pooled the estimation of the treatment functions across regions due to sample size considerations at the cutoff borders. In the final regression specification, we include district fixed effects that are nested within regions to account for geographic differences in outcomes, but again we are not able to test for possible heterogeneous treatment effects across regions. From a policy standpoint, our variation in the duration of exposure to MPC benefits is limited to three durations (< 10 months, 12 months and discontinued, and 16-22 months) we are unable to compute an exact optimal duration of benefits for sustained impacts on child well-being. We also do not collect information on children's home learning environment, parental engagement, or their time use before and after receipt of MPC benefits. Lastly, although we discuss the theoretical underpinnings of the relationship between cash transfers and child well-being, we are not able to empirically confirm these links.

Results

Overview of results

Table 3 shows the main results from running equation (3) for each of the combinations of treatments while controlling for child's age and gender, and at the household level, household size, and the gender, age, marital status, and educational attainment of the household head. In all specifications, we include wave and district of residence fixed effects. The first 4 RDs yield estimates for two parameters from equation 3: the difference in intercepts *a* (columns (1), (3), (5) and (7)) and the difference in slopes *b* (columns (2), (4), (6) and (8)) between the treatment line and the comparison line around the threshold for the running variable in question. The fifth RD yields an estimate of a single parameter: the difference in the outcome between long-term recipients and the control group around the threshold for both running variables (column (9)).

For most of our estimated treatment lines in the first 8 columns of the table, the difference in intercepts is far larger than the difference in the slopes, and the resulting estimated effect is largely driven by the difference in intercepts and persists in the relevant range of comparison even when it has the opposite sign of the difference in slope. In cases where the slope is not statistically significant, but the intercept is, the treatment effect is constant along the frontier. In our presentation of the results and the discussion of their magnitudes, we therefore focus on the estimated difference in the intercepts around the boundary.

We find some evidence that multi-purpose cash reduces the risk of acute illness in young children. Short-run recipient households are 10 percentage points less likely to report that their children aged 5 or less suffered an acute illness compared to non-recipient households (significant at 5%), and long-term recipients are 8 percentage points less likely to report acute illness in children than discontinued households (significant at 10%). The picture becomes more granular when we look at different types of illness separately: the risk of diarrhea in young children is 8.9 percentage points lower in long-term recipient households than discontinued households (significant at 5%), whereas the risk of respiratory infection in children 5 or less is significantly lower among discontinued households than in non-recipients (9.4 percentage points lower, significant at 5%), and significantly lower (at 5%) in long-term recipients when compared to short-run recipients (by 7 percentage points) and non-recipients (by 9 percentage points).

All treatment groups report lower need for PHC for their young children, and more so for longer term recipients, a finding consistent with the lower incidence of acute illness. The reduction in need is significant at 10% and similar in magnitude when comparing short-run recipients (by 8.4 percentage points) and long-term recipients (by 8.5 percentage points) to non-recipients.

Among households who report requiring PHC for children aged 0 to 5, MPC improves the household's access to PHC. This improvement is highly significant among long-term recipients, with a probability of access higher by 13.3 percentage points compared to the discontinued group and by 15.8 percentage points compared to non-recipients.

The results are qualitatively similar for children aged 6 to 14. For this age group too, MPC seems to reduce the need for PHC, and significantly so (at 5%) for the discontinued (23 percentage points), short-run (11.6 percentage points) and long-term recipients (12.6 percentage points) when compared to non-recipients. However, we find a significant increase in the need for PHC when we compare long-term recipients to the discontinued group. For those households who do report requiring PHC for their children aged 6 to 14, long-term MPC significantly improves their chance of accessing the needed care when compared to discontinued households and non-recipients.

MPC improves enrolment in formal schooling. This improvement is of around 7 percentage points and is significant at the 10% level for children in discontinued households and children in long-term recipient households (compared to non-recipients). The effect is stronger (8.8 percentage points) and more significant (5%) for short-run recipients compared to non-recipients. Much of this improvement seems to be coming from children transitioning out of non-formal schooling as we find significantly lower enrolment in non-formal schools among MPC recipients. Enrolment in non-formal education is 6.9 percentage points lower among children in discontinued households compared to non-recipients. Long-term receipt of MPC also significantly reduces enrolment in non-formal schooling by 6.6 percentage points when compared to short-run receipt and by 9.6 percentage points when compared to non-receipt.

The receipt of cash lowers the risk of child labor for all treatment groups. This added protection is significant at 5% and lowers the risk by 3.3 percentage points for children in discontinued households compared to non-recipients, and by 2.9 and 3.7 percentage points respectively for children in households with long-term MPC compared to short-run and non-recipients. So the effect seems to persist even after discontinuation, and seems to grow with longer exposure to MPC. Our regressions for early marriage include only data on girls 15 to 19 years old. The estimated effect of MPC is negative for all treatment groups and significant at 5% for girls in discontinued households compared to non-recipients.

Robustness checks

Our MMRD results suggest that multi-purpose cash transfers to Syrian refugee households have significant effects on children's health, access to education, withdrawal from the labor market, and reduction in early marriages. Table A1 in the appendix shows that the coefficient estimates from the MMRD without covariates are virtually unchanged from the estimation results presented in Table 3 that includes observable characteristics as covariates, with only minor changes in the significance level of 4 coefficients while the magnitude and signs are all unchanged and still statistically significant. This result provides (1) evidence of the soundness of the identification strategy and (2) evidence that any discontinuities in observed characteristics along the cutoff points do not confound the estimation of the causal treatment functions. We also disaggregated the results by gender to test for possible heterogeneity in the treatment functions for girls relative to boys. We find no substantive differences in the effects of MPC by gender, however, by pooling the subgroups we are able to improve the precision of our estimates.

To further confirm the validity of our findings, we employ a placebo cutoff to test for spurious regression results, or discontinuities at non-boundary points as suggested by Imbens and Lemieux (2008) and Cattaneo et al. (2019b). We randomly generated false cutoffs along S_1 and S_2 in the control, or W_0, region away from the actual cutoff boundaries with the other treatment conditions. Using only observations from the control region, we replicate the MMRD estimation procedure described in equation (3) using these placebo cutoffs. The null hypothesis is that there are no treatment effects at non-discontinuity frontiers. Failing to reject the null hypothesis would suggest that discontinuity estimates at the actual cutoff boundaries reported in Table 3 are not spurious.

Table 4 presents the results of the placebo cutoff test for the full sample replicating the results of Table 3 but with false cutoffs in the non-recipient region. Of the 99 parameter estimates we only find 7 estimates that are statistically significant at the 10% level, four of which are significant at the 5% level. This result shows that the outcome functions are, for the most part, smooth along S_1 and S_2 in areas away from the cutoff boundaries. We have little reason to believe that the estimated effects shown in Table 3 are spurious.

Discussion

In this paper, we contribute to the literature on the effectiveness of cash transfers in emergency/ humanitarian contexts and, more broadly, to issues of conditionality and temporality that are central to the cash transfer debate. First, our results provide some evidence that the benefits of cash transfers for displaced populations carry over to the children of the receiving households in terms of improved health, education, protection from child labor and early marriage outcomes. Second, this finding supports the hypothesis that household decision makers do not always require external incentives, i.e. conditions on receipt of cash transfers, to optimize household welfare including that of their children. Lastly, our results shed some light on the question "do households require a sustained inflow of cash, or is a one-time investment enough to avoid mean reversion?" We show that sustained improvements in children's well-being depend on the outcome of interest to policy makers and that there is no 'one size fits all' policy solution. We argue that these results, at least, hold true in conflict and crisis affected contexts.

MPC lowers the likelihood that children aged 0-5 years report contracting any acute illnesses among recipient households in the short-run and long-term relative to non-recipients, potentially signaling that children of these households are exhibiting greater improvements in their overall health status than their counterparts (Pellerano et al., 2014; Clare O'Brien et al., 2013; Owusu-Addo and Cross, 2014). We confirm these improvements with, generally, lower incidences of specific acute illnesses such as diarrhea and respiratory infections. Health outcomes of children 5 years and younger exhibit a lower likelihood of reporting needing PHC due to illnesses in recipient households relative to non-recipients, which corroborates our findings for acute illnesses. Another encouraging result is that recipient households, especially in the long run, are more likely to use

PHC services when required than non-recipients. These results are in line with findings from (Andrade et al., 2012; Attanasio et al., 2005) who show that cash transfers in Colombia and Brazil led to improved health outcomes, especially for children younger than 6 years.

The short-run improvements in health outcomes of pre-primary aged children are not sustainable in the long run when MPC benefits are discontinued. Conversely, when households are provided with a second cycle of cash transfers, the initial improvements in the incidence of acute illnesses, needing PHC, and using PHC are maintained in the long run. For one type of acute illness, however, we find that one cycle of cash transfers lowers the incidence of respiratory infections in early childhood and that these effects do not attenuate with the loss of MPC benefits. Altogether, these findings contribute to filling a gap in the literature and suggest that, at least for most acute illnesses, refugee families may rely on MPC support to increase and maintain investments in health as well as in living conditions conducive to better health in early childhood.

We also find negative effects of cash transfers on the need for PHC because of illness among school-aged children. Unlike the findings for children 5 years of age or younger, MPC lowers the probability of school-aged children needing PHC in all three of our different exposure categories to MPC: households whose MPC was discontinued, short- and long-term recipients of MPC. But similar to the findings from the youngest age group, PHC utilization does depend on continued MPC as children of households with discontinued benefits are just as likely to use PHC when ill as non-recipients, whereas only long-term recipients use PHC at a significantly higher rate when required. This suggests that while an initial cash investment may mitigate certain risk factors for illness, actually receiving adequate health services is still subject to financial constraints.

This paper contributes to the evidence base by providing some insight into the household decision-making process involving the choices faced by households to enroll their child in formal education, non-formal education, or participate in income generating activities. Among school-aged children, our results show that MPC significantly shifts children away from non-formal schooling and child labor participation into formal education for both boys and girls. This is an important finding in the context of education in emergencies, where the main vehicle for intervention is non-formal education (NFE). NFE interventions are typically created as a strategy to facilitate the transition of children who have lost access to schooling back into the formal education system via accelerated learning curricula (Chopra and Adelman, 2017). The finding that children are more likely to transition to formal education from non-formal settings depends on their receipt of cash transfers potentially means that attendance in NFE programs alone might not be enough for children to successfully get back 'on track' with their formal schooling. MPC leads to lower child labor at the same time, which may also signal, at least partially, that MPC may compensate the household for the foregone opportunity of income that could be generated from child labor.

Our findings on the effects of MPC on educational access are in line with findings from the literature (Bastagli et al., 2016; Ahmed et al., 2007; Galasso, 2006; Schady and Araujo, 2006; Schultz, 2004). Breisinger et al. (2018) found no statistically significant impact of the Takaful programme in Egypt on school enrolment. The study explains that enrolment rates are already high and the conditionality of school attendance was not yet applied at the time of the study.

Our findings are relatively novel in the humanitarian context as well as in the context of unconditional cash transfers where a dearth of evidence exists (Baird et al., 2013; Mishra and Battistin, 2017). Previous evidence from Lebanon includes a study by De Hoop et al. (2018) that finds no impact on enrolment. The study mentions that the lack of detected impact could be due to the fact that schools were already running at full capacity in the study pilot areas. An earlier study by Lehmann and Masterson (2014) found that children from households receiving cash assistance were 6 percentage points more likely to be enrolled in school.

Additionally, we find that the favorable effects on enrollment and child labor materialize in the short-run and persist over time, even when MPC benefits are discontinued. MPC, thus, offers a protective mechanism for children that increases enrollment and lowers the risk of child labor. This is an especially salient finding when households tend to rely on child labor as a means of consumption smoothing in the face of economic shocks (De Hoop and Rosati, 2014). In the context of the Syrian refugee population in Lebanon, this protective effect of cash is resilient to the discontinuation of MPC, and is cumulative, as long-term receipt lowers the likelihood of child labor significantly more than in the short-run. From a policy and cost effectiveness perspective, this shows that lasting effects on formal school enrollment and child labor may be achieved without continuous MPC inflow.

We find that MPC has a negative effect on the likelihood of early marriage among girls aged 15-19 years. Our estimates show that these effects are significant and largest among girls from households whose MPC benefits were discontinued. Although the effects for the short- and long-term recipient groups are still negative relative to the non-recipients, they are slightly smaller in magnitude and are not statistically significant. Nevertheless, this finding is important in the context of providing refugee families some form of protection against forced or unwanted marriages for girls. We show that MPC has some protective effects in the short- and long-term for girls as Syrian refugee families in Jordan and Lebanon are increasingly resorting to early marriages as a means to mitigate household poverty (Watkins and Zyck, 2014; Thompson, 2012).

Conclusion

The results of this paper provide some supporting evidence that MPC provided to Syrian refugee households in Lebanon improves health outcomes for pre-primary children as well as health and educational outcomes for school-aged children. This paper also shows that MPC can have protective effects for school-aged children by reducing their risk of child labor and reducing the likelihood of early marriage for girls aged 15-19 years. Specifically, our findings show that children of MPC recipients are transitioning from non-formal to formal schooling while also shifting away from child labor. This is an important result for educational programming in emergencies as it shows that the barriers to educational access are not necessarily a result of learning deficiencies but are also economic in nature.

Although our findings are not original in the broader literature on cash transfers, this paper adds to the literature by providing some insight into the temporal dimension of cash transfer effects, especially in protracted displacement settings. We find that the favorable health effects of MPC on pre-primary children tend to diminish in the absence of continued/sustained cash assistance, while MPC effects on health, education, child labor, and early marriage tend to persist even after MPC has been discontinued. These two findings show that the policy solution to the duration of cash transfers is not a simple one. The answer is different for different outcomes, and as such, policy makers may have to face the difficult task of deciding which child outcomes to prioritize.

This paper is not without its limitations. As with all inferences made with RDD, the external validity of our findings is limited to households near the eligibility cutoffs. Although we employ an MMRD design which allows us to extend the limits of the study's external validity, the findings are still limited to households neighboring the eligibility frontiers. It is important to note that while the changes in eligibility criteria for cash benefits resulted in original research, we show that these changes are accompanied with consequences for households who were ultimately cut off. One other important limitation of this paper is that we are unable to link improved access to education with improved learning and eventually improved human capital accumulation. We believe that future research investigating whether the beneficial effects of MPC during childhood and adolescence translate to substantial gains, economic or otherwise, over the life-cycle would be a considerable contribution to the emerging literature in conflict and emergency contexts.

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Table 2. Continuity of confounding factors along frontier cutoffs

		Child		Household Head				
		Female	Age (years)	Female	Age	Married	Years of schooling	Household Size
$W_1\|W_0$	$\hat{\alpha}_1 - \hat{\alpha}_0$	-0.02	0.26	-.120**	-0.362	.117**	-0.464	0.02
	$\hat{\gamma}_1 - \hat{\gamma}_0$	-0.054	0.48	-.096*	1.979	0.08	0.029	-0.058
$W_2\|W_0$	$\hat{\alpha}_2 - \hat{\alpha}_0$	-0.046	0.347	-.123**	0.854	.099*	0.242	0.083
	$\hat{\beta}_2 - \hat{\beta}_0$	-0.026	0.087	-0.002	1.216	-0.018	0.706	0.064
$W_3\|W_1$	$\hat{\alpha}_3 - \hat{\alpha}_1$	0.002	-0.089	0.017	0.016	-.023**	0.078	0.018
	$\hat{\beta}_3 - \hat{\beta}_1$	0.007	-0.035	0.004	-.291*	-0.003	-0.014	0.019
$W_3\|W_2$	$\hat{\alpha}_3 - \hat{\alpha}_2$	-0.005	-0.004	0.003	0.067	-0.006	0.047	-0.002
	$\hat{\gamma}_3 - \hat{\gamma}_2$	-0.001	-0.023	-.024**	-0.093	.017*	0.059	0.088
$W_3\|W_0$	$\hat{\alpha}_3 - \hat{\alpha}_0$	0.007	-0.133	-0.026	-1.124	0.02	0.213	0.141
	h*	7.775	4.776	6.793	5.383	6.959	6.394	4.378
	Nh	19,640	14,501	5,398	4,640	5,439	5,216	4,007

Notes: Figures in the table represent parameter estimates corresponding to the treatment functions following OLS estimation of equation [3] using observations $\pm h^*$ from the joint cutoff. h^* denotes the optimal bandwidth as determined by cross-validation. Asterisks denote statistical significance as follows. * p<.10, ** p<.05, and *** p<.01

Table 3. MMRD estimation of treatment functions at frontier cutoffs

	$W_1 \mid W_0$		$W_2 \mid W_0$		$W_3 \mid W_1$		$W_3 \mid W_2$		$W_3 \mid W_0$		
	$\hat{\alpha}_1 - \hat{\alpha}_0$	$\hat{\gamma}_1 - \hat{\gamma}_0$	$\hat{\alpha}_2 - \hat{\alpha}_0$	$\hat{\beta}_2 - \hat{\beta}_0$	$\hat{\alpha}_3 - \hat{\alpha}_1$	$\hat{\beta}_3 - \hat{\beta}_1$	$\hat{\alpha}_3 - \hat{\alpha}_2$	$\hat{\gamma}_3 - \hat{\gamma}_2$	$\hat{\alpha}_3 - \hat{\alpha}_0$	h*	Nh
Acute Illness (0-5 years):											
Any illness	.020	-.012	-.101**	.011**	-.083*	-.002	.039	.008	-.063	6.10	6,280
Diahrrea	.071	-.013	-.039	.002	-.098**	-.008*	.012	.007	-.027	8.20	7,179
Respiratory infection	-.094**	.015**	-.020	.001	.003	.002	-.071**	-.011*	-.092**	10.93	7,468
Primary Health Care (0-5 years):											
Required for illness	-.049	-.005	-.084*	.010*	-.036	-.002	-.001	-.003	-.085*	5.67	5,976
Used for illness	.025	.009	.084	-.003	.133**	.015**	.074	-.001	.158**	12.19	4,037
Primary Health Care (6-14 years):											
Required for illness	-.231***	.039***	-.116***	.017***	.107***	.003	-.008	-.009	-.124***	4.90	9,330
Used for illness	-.001	-.009	.053	.000	.149**	.020***	.095	-.003	.148**	10.94	4,543
Education & Labor (6-14 years):											
Formal enrollment	.076*	-.003	.088**	-.008*	-.004	-.001	-.016	-.001	.072*	8.34	12,733
Non-formal enrollment	-.069**	.011**	-.030	.007**	-.027	-.002	-.066***	-.006	-.096***	6.97	11,867
Work	-.033**	.003	-.008	.002*	-.004	.001	-.029***	-.005**	-.037***	8.91	12,921
Early Marriage (15-19 years):											
Married	-.066**	.003	-.024	.005*	.026	.001	-.015	-.002	-.039	10.67	6,066

Notes: Figures in the table represent parameter estimates corresponding to the treatment functions following OLS estimation of equation [3] using observations $\pm h^*$ from the joint cutoff. h^* denotes the optimal bandwidth as determined by cross-validation. Asterisks denote statistical significance as follows. * p<.10, ** p<.05, and *** p<.01

Standard errors are not reported for brevity. The full table of coefficients with reported standard errors can be made available upon request.

Table 4. MMRD estimation of placebo treatment functions using false cutoffs

	$W_1\|W_0$		$W_2\|W_0$		$W_3\|W_1$		$W_3\|W_2$		$W_3\|W_0$		
	$\hat{\alpha}_1-\hat{\alpha}_0$	$\hat{\gamma}_1-\hat{\gamma}_0$	$\hat{\alpha}_2-\hat{\alpha}_0$	$\hat{\beta}_2-\hat{\beta}_0$	$\hat{\alpha}_3-\hat{\alpha}_1$	$\hat{\beta}_3-\hat{\beta}_1$	$\hat{\alpha}_3-\hat{\alpha}_2$	$\hat{\gamma}_3-\hat{\gamma}_2$	$\hat{\alpha}_3-\hat{\alpha}_0$	h*	Nh
Acute Illness (0-5 years):											
Any illness	0.084	-0.024	0.075	-0.001	-0.096	-0.008	-0.087	-0.015	-0.012	12.12	2075
Diahrrea	0.044	-0.019	0.081	-0.005	0.045	-0.002	0.007	-0.006	0.089	11.61	2075
Respiratory infection	-.146***	.020*	-0.062	.009*	0.037	0.008	-0.047	-0.022	-.109**	12.93	2075
Primary Health Care (0-5 years):											
Required for illness	-0.069	0.019	0.003	0.003	0.004	0.007	-0.068	-0.03	-0.065	13.93	2073
Used for illness	-0.103	0.023	-0.113	-0.001	-0.048	0	-0.038	0.004	-0.151	12.31	1091
Primary Health Care (6-14 years):											
Required for illness	0.071	-0.013	0.061	-0.004	-0.038	-0.006	-0.029	-0.008	0.033	9.06	2548
Used for illness	-0.009	-0.014	-0.075	-0.001	0.057	0.005	0.124	0.008	0.048	14.21	763
Education & Labor (6-14 years):											
Formal enrollment	-0.015	0.001	0.006	0	-0.002	0	-0.023	-0.001	-0.017	5.71	2305
Non-formal enrollment	.161**	-.044**	0.028	-0.008	-0.081	-0.005	0.051	0.012	0.08	8.9	2549
Work	-.058*	0.004	-0.029	0.003	0.004	0.005	-0.026	0.003	-.055*	11.26	2559
Early Marriage (15-19 years):											
Married	0.033	0.002	-0.027	0.006	-0.049	-0.003	0.011	0.003	-0.016	15.47	1253

Notes: Figures in the table represent parameter estimates corresponding to the treatment functions following OLS estimation of equation [3] using observations $\pm h^*$ from the joint cutoff. h^* denotes the optimal bandwidth as determined by cross-validation. Asterisks denote statistical significance as follows. * p<.10, ** p<.05, and *** p<.01. Standard errors are not reported for brevity. The full table of coefficients with reported standard errors can be made available upon request.

Table A1. MMRD estimation of treatment functions at frontier cutoffs – without covariates

	$W_1 \mid W_0$		$W_2 \mid W_0$		$W_3 \mid W_1$		$W_3 \mid W_2$		$W_3 \mid W_0$		
	$\hat{\alpha}_1 - \hat{\alpha}_0$	$\hat{\gamma}_1 - \hat{\gamma}_0$	$\hat{\alpha}_2 - \hat{\alpha}_0$	$\hat{\beta}_2 - \hat{\beta}_0$	$\hat{\alpha}_3 - \hat{\alpha}_1$	$\hat{\beta}_3 - \hat{\beta}_1$	$\hat{\alpha}_3 - \hat{\alpha}_2$	$\hat{\gamma}_3 - \hat{\gamma}_2$	$\hat{\alpha}_3 - \hat{\alpha}_0$	h*	Nh
Acute Illness (0-5 years):											
Any illness	.011	-.011	-.106**	.011**	-.082*	-.001	.034	.006	-.071	6.10	6,286
Diahrrea	.062	-.011	-.044	.003	-.098**	-.008*	.008	.007	-.036	8.20	7,187
Respiratory infection	-.093**	.015**	-.021	.001	.002	.002	-.070**	-.011*	-.090**	10.93	7,476
Primary Health Care (0-5 years):											
Required for illness	-.054	-.004	-.085*	.009*	-.034	-.002	-.004	-.004	-.088*	5.67	5,982
Used for illness	.001	.012	.083	-.004	.154**	.016**	.072	.000	.155**	12.19	4,040
Primary Health Care (6-14 years):											
Required for illness	-.232***	.038***	-.115***	.017***	.107***	.003	-.010	-.009	-.124***	4.90	9,356
Used for illness	.003	-.010	.051	.000	.154**	.020***	.106*	-.002	.157**	10.94	4,552
Education & Labor (6-14 years):											
Formal enrollment	.084**	-.004	.096**	-.008*	.000	-.001	-.012	.002	.084**	8.34	12,781
Non-formal enrollment	-.075**	.012**	-.027	.006**	-.025	-.001	-.073***	-.007*	-.100***	6.97	11,913
Work	-.035**	.003	-.008	.002	-.002	.001	-.029***	-.006***	-.037***	8.91	12,970
Early Marriage (15-19 years):											
Married	-.054**	.001	-.018	.004	.026	.001	-.009	-.001	-.027	10.67	6,091

Notes: Figures in the table represent parameter estimates corresponding to the treatment functions following OLS estimation of equation [3] using observations $\pm h^*$ from the joint cutoff. h^* denotes the optimal bandwidth as determined by cross-validation. Asterisks denote statistical significance as follows. * p<.10, ** p<.05, and *** p<.01. Standard errors are not reported for brevity. The full table of coefficients with reported standard errors can be made available upon request.