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The impact of cash transfers on Syrian refugee children in Lebanon

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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.

1. 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 few as 13% of children aged between 3 and 5 were enrolled in early childhood

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education, while enrollment in any educational institution increased to only 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 seasonality of agricultural labor (and the timing of data collection not coinciding with peak agricultural employment) as well as to underreporting of child labor in this context. 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, Bourguignon et al., 2003).

The existing literature on the impact of cash transfers, broadly, on children's well-being is extensive (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. Cahyadi et al. (2020), Ferreira et al. (2009), Galasso (2006), Attanasio et al. (2005), 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 (Cruz et al., 2017; 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). A review of 33 impact evaluations exploring pathways through which social protection (including cash transfers) affect child labor has shown that patterns emerge across different implementation contexts and child labor definitions. Programs that reduce household vulnerability (through for instance cash transfers) reduce child labor in most cases (Dammert et al., 2018).

Although the literature on the impacts of cash transfers on child well-being is vast, the impact in humanitarian contexts has been less explored in general, much less in the context of the Syrian refugee population in Lebanon (Puri et al., 2017; Doocy and Tappis, 2016; Brück et al., 2019). There have been a number of studies on the impact of cash transfers in Lebanon. Lehmann and Masterson (2014) evaluate a winter cash transfer program for Syrian refugees in Lebanon. The study 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, studies such as Lehmann and Masterson (2014) and Battistin (2016) 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 do not provide a thorough examination of children's well-being outcomes, use relatively small sample sizes, and therefore have limited external validity of the scale of the interventions. The broader literature also does not delve into the potentially differential effects of cash transfers over time, especially in terms of children's education, health, and protection outcomes (Chaluda, 2015).

This paper contributes to the existing literature by evaluating the impact 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).

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). 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 use the MMRD approach 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. This enables us to create six sets of pairwise comparisons to identify potentially heterogeneous effects of cash transfers over varying durations of cash receipts. We are able to distinguish between the effects of cash transfers in the short-run, long-run, and when cash receipts are discontinued by comparing each of the treatment statuses to similar households who were never eligible for cash receipts.[1] At the same time, we assess whether the effects of cash transfers change over time by comparing short-run to long-run recipients, and identify the effects of discontinuation of benefits relative to sustained receipts over a duration of approximately 22 months.

Key findings from our analyses reveal that MPC assistance is associated with improved child outcomes at different levels. Compared to children in non-recipient households, MPC assistance reduces the risk of acute illness in children, under 5 years of age, by 10 percentage points in the short-run and by 8 percentage points in the long run compared to discontinued households. Among all treatment groups, the results indicate a decreased need for PHC for young children compared to non-recipient households and increased access to PHC among households who report requiring it for children aged 0 to 5, for long-run recipients by 13.3 per-

[1] We define short-term recipients as those households who have newly received MPC for less than 10 months, long-run 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.

centage points compared to the discontinued group and by 15.8 percentage points compared to non-recipients.

Moreover, these results reveal that MPC leads to improved educational outcomes compared to children in non-recipient households through increased enrollment rates in formal education by 7 percentage points for children in discontinued households and children in long-run recipient households and 8.8 percentage points for children in short-run recipient households. Coupled with significant decreases in enrolment in non-formal education among MPC recipients in the discontinued and long-run groups, results suggest that MPC assistance is associated with a shift from non-formal to formal education. Higher enrolment rates for MPC beneficiaries where also coupled with significant decreases in child labor among all treatment groups by 3.3 percentage points in the discontinued group and 3.7 percentage points in the long-run group.

The results of this paper provide strong implications for educational programming in emergencies as they clearly indicate that the barriers to educational access are not necessarily a result of learning deficiencies but are also economic in nature. On the other hand, results show that while favorable health effects of MPC on pre-primary children tend to diminish in the absence of continued/sustained cash assistance, MPC effects on health, education, child labor, and early marriage tend to persist even after MPC has been discontinued. Hence, policy implications pertaining to the length of the cash cycle proves difficult suggesting the need for further research.

2. Background and conceptual framework

2.1. Syrian refugees in Lebanon and overview of the cash transfer program

In 2017, the World Food Programme (WFP) joined the United Nations Higher Commissioner on Refugees (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 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 results in 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).

Since 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. Balance checks were run on each wave to verify that there were no differences between our treatment groups in access to other cash and non-cash programs (Chaaban et al., 2020).

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. As such, we estimate the net effect of MPC on school enrollment, health status, healthcare access and usage, child labor, and early 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.

2.2. 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). 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.

In theory, increases in income resulting from cash transfers can allow households to cover the direct and indirect costs of healthcare and healthcare access. Direct costs include costs related to healthcare fees and medication such as consultation fees, diagnostic tests, cost-share for hospitalization, etc. while indirect costs include the incurred costs of transportation to healthcare facilities (Gaarder et al., 2010; Lundberg et al., 2010).

[2] The PMT regressions for both cash cycles we look at in this study (2017 and 2018) use a similar list of regressors, although not the same, 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).

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 under 5 years old (Attanasio et al., 2005; Fernald et al., 2009; Gertler, 2004; Rivera et al., 2004; Maluccio and Flores, 2005).

Cash transfers are hypothesized to increase access to education. By increasing income and reducing household liquidity constraints, households are better able to cover direct and indirect costs associated with school enrolment and attendance. Direct costs are the costs directly related to schooling such as school fees, clothing, books, and stationery while indirect costs include the transportation cost to and from school. Cash transfers can help cover part or all of the forgone opportunity costs incurred from children going to school such as forgone earnings (from child labour or children helping in house chores) (Deaton, 1992). Also, to the extent that cash transfers increase the income available to the household, they can affect life-course choices and behaviours of young adults and their parents, especially related to female marriage decisions. For example, the additional cash might reduce female financial dependence on others and reduces the family's need to resort to actions such as marrying off their young girls.

Many studies 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 indicates that cash assistance is associated with a significant reduction in the intensity of child labor (number of working hours) in non-humanitarian settings, with less conclusive evidence on the prevalence of working children in humanitarian settings (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).

3. Data and empirical strategy

3.1. Sample summary

We use data from two waves of household survey data collected in February and March 2019 (wave 1) and in July and August 2019 (wave 2) from repeated cross-sections of Syrian refugee households from three governorates in Lebanon (Mount Lebanon, North Lebanon, and Bekaa). We restrict the analytic sample to individuals who are under the age of 19 years and their corresponding households. This yields a final analytic sample of 6,207 households (2,992 from wave 1 and 3,215 from wave 2) and 24,859 individual observations (11,843 from wave 1 and 13,016 from wave 2).

We differentiate the two MPC disbursement periods because the PMT formula used to determine the desk scores and the associated cutoff value for eligibility changed between the two time periods, resulting in changes in the refugee households' treatment status. The change in formula resulted in four eligibility groups of households: (1) households who are not eligible for MPC in either period (W_0), (2) households who were eligible for MPC in the first period but no longer in the second period (W_1), (3) households who were not eligible for MPC in the first period but are in the second (W_2), and (4) households who maintained eligibility for MPC benefits throughout both time periods (W_3). This distinction allows us to detect potentially heterogeneous effects of cash transfers across these different treatment categories and provide some insights into how the effects of MPC may change over time and how it may affect households who are subject to losing their benefits after a period of being eligible.

Based on data from the analytic sample, the change in PMT formula across the two time periods resulted in substantial changes in the households receiving MPC in 2017 and 2018: 27.4% of households who received MPC in 2017 were discontinued in 2018, 15.3% of households who were not receiving MPC in 2017 started receiving benefits in 2018, and the remaining 57.2% of households maintained the same status of receipt (28.8%) or non-receipt (28.5%) of MPC during both periods.[3]

Table 1 shows sample means of observed characteristics of the respondents and their outcomes. The compliance rate is very high within each eligibility condition with only 4.2% of households who were eligible to receive MPC in 2017 and 1.9% of households who were eligible for MPC in 2018 not receiving their respective MPC benefits (non-compliance by "exclusion"). We also observe that 3.1% and 1.7% of ineligible households received MPC in 2017 and 2018 (non-compliance by "inclusion"), respectively. In terms of the demographic characteristics of the children and adolescents, the average age is 8.7 years with means ranging between 8.1 and 9.2 across the eligibility conditions. Here, the group W_0 (non-eligible) appears to be the youngest, on average, while the group W_3 (eligible in both periods) is the oldest.

At the household level, we use information on the household head 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 an 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 (non-eligible) having 6.3 household members and households in W_3 (eligible in both periods) have 7.1 members, on average.

The lower half of Table 1 separates the means of outcomes of children by age groups: 0–5 years, 6–14 years, and 15–19 years. We follow this age grouping to conform with compulsory

[3] 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 eligibility condition.

	W_0 Non-eligible	W_1 Eligible in period 1 but not in period 2	W_2 Eligible in period 2	W_3 Eligible in both periods	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)
Diarrhea	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 (conditional on requiring it)	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 (conditional on requiring it)	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 : Figuresinthetablecorrespondtosamplemeansandstandarddeviations(inparentheses)usingsamplingweights.

education age requirements imposed in Lebanon, which also serves as the age cutoff for child labor laws in Lebanon (UNESCO Institute for Statistics, 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 include variables measuring 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 had 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 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 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,[4] whether they engaged in work outside the home 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 are government formal

[4] Non-formal educational programs are used to bridge the gap with formal education so that children may eventually integrate into the public school system. Non-formal education programs in Lebanon include Community-based Early Childhood Education (CB-ECE), 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/programs, 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 programs 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 enroll in public schools later (Human Rights Watch, 2016).

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 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 are eligible for MPC in neither period reporting the highest rate of early marriage at 17%, while the remaining groups report rates between 10% and 16%. It is important that we acknowledge potential downward bias in the self-reported child labor and early marriage, as these topics may be considered taboo by the respondents. This could have implications on our subsequent analyses and inferences.

It should be noted that although the table shows differences in the average characteristics across the four groups, this comparison of means is only observational and does not affect the validity of our empirical strategy, since we rely on the similarity of the average characteristics only near the cutoffs for eligibility. To confirm the validity of our approach, we will test whether these characteristics are significantly different at the relevant points of comparison in the vicinity of the cutoff points of $S_2$and S_2, the measures of eligibility for treatment.

3.2. Empirical strategy

We employ a multidimensional regression discontinuity (MMRD) design with two running variables (PMT from 2017 and 2018) and 4 mutually exclusive treatment categories. Technical details of the MMRD approach are provided in the appendix and follow the exposition of the design in Papay et al., (2011), Reardon and Robinson (2012) and Wong et al. (2013). Below, we summarize the intuition behind the approach.

This design preserves much of the spirit of a conventional RDD but involves extensions of the approach on two different levels: i. the treatment and ii. the assignment variables. First, the extension of the RDD framework to four (rather than just two) possible treatment categories expands the set of possible pairwise comparisons across treatment groups from two to six. Thus, the approach provides an estimate of the effect of switching between any two of the four eligibility categories defined above as W_0 (never eligible), W_1 (eligible for MPC 2017 only), W_2 (eligible for MPC 2018 only) and W_3 (always eligible).

We can further distinguish *conceptually* between two "adjacent" categories of treatment, which involve a difference in only one of the MPC receipts (W_0 vs. W_1, W_0 vs. W_2, W_1 vs. W_3, or W_2 vs. W_3), and two non-adjacent categories of treatment, which involve a difference in both MPC receipts (W_0 vs W_3, and W_1 vs W_2). Furthermore, each pairwise comparison of treatment categories also has a programmatic interpretation, based on the time since treatment at which these households are observed:[5]

- $W_1|W_0$ compares discontinued participants to households who were never beneficiaries
- $W_2|W_0$ compares current one-time (and therefore short-run) recipients to households who were never beneficiaries
- $W_3|W_1$ compares current two-time (and therefore long-run) recipients to households who were once beneficiaries but have since been discontinued
- $W_3|W_2$ compares current two-time (and therefore long-run) recipients to current one-time (and therefore short-run) recipients
- $W_3|W_2$ compares current two-time (and therefore long-run) recipients to current one-time (and therefore short-run) recipients
- $W_3|W_0$ compares current two-time (and therefore long-run) recipients to households who were never beneficiaries

Second, the extension of the RDD framework to include two assignment variables means that the assignment variable space is now two dimensional, and two "adjacent" treatment categories are separated not by a scalar cutoff, but by a two-dimensional frontier. With this added dimension, the approach provides an estimate of a local average treatment *function* from moving across "adjacent" treatment categories, rather than the point estimate of a local average treatment effect in a conventional RDD. This local average treatment function is a function of the assignment variable that, in this pairwise comparison of adjacent treatment categories, is *not* crossing its threshold. The effect of crossing the cutoff for one assignment variable can thus vary with the *value* of the other assignment variable. The comparison of non-adjacent treatment categories reduces to a *point* estimate of an average local treatment effect (similar in dimension to the conventional RDD) since such a comparison involves households for whom *both* assignment variables are in vicinity of their cutoffs.

Just like in the convention RDD, the resulting causal effects identified using an MMRD approach are *local* to the frontier cutoffs and, thus, may not hold everywhere in the joint PMT distribution away from the cutoff axes. The bandwidths determining "proximity" to the cutoff are chosen using an extension of the two-step procedure described in Imbens and Lemieux (2008) developed in Papay et al. (2011). More details on the optimal choice of bandwidths is provided in the appendix.

We use Ordinary Least Squares (OLS) to estimate the MMRD model parametrically for each outcome variable. For simplicity, outcomes of interest are fit as linear functions of the assignment variables. The OLS results from either side of each frontier yield the MMRD estimate of the local average treatment *function* from comparing "adjacent" treatment groups, which will be linear in the assignment variable. To answer the research questions at hand, we report MMRD estimates of the intercept and slope of these local average treatment functions for the following pairwise comparisons: MPC only 2017 vs. none ($W_1|W_0$), MPC only 2018 vs. none ($W_2|W_0$), MPC both years vs only 2017 ($W_3|W_1$), and MPC both years vs only 2018 ($W_3|W_2$). In effect,

$$\tau_{i|j} = \alpha_{i|j} + \beta_{i|j} S_k \quad (1)$$

where $i|j$ is 1|0, 2|0, 3|1 or 3|2 as described above, and k = 1 if $i|j$ is 2|0 or 3|1 and k = 2 if $i|j$ is 1|0 or 3|2.

We also report the point estimate of the local average treatment effect in the comparison of the two "non-adjacent" treatment groups, MPC both years vs. none ($W_3|W_0$), $\tau_{3|0} = \alpha_{3|0}$.[6]

[5] Note that the first 4 of these comparisons are between what we have labeled as "adjacent" treatment groups because the difference between them is in the receipt of only one of the MPC cycles. The last of these comparisons is between two "non-adjacent" treatment groups as it involves comparing two groups who differ on their receipt status of both cycles. Also note that there is one additional possible comparison of non-adjacent treatment groups ($W_2|W_1$) which we do not list here because it does not address any of the research questions we have set out to answer and it does not have meaningful programmatic value.

[6] Because the comparison does not readily inform any programmatic or substantive questions about the effectiveness of the MPC program, we omit from our reported results the point estimate of the local treatment effect from comparing recipients of the other two non-adjacent treatment groups: MPC 2018 only vs. MPC 2017 only ($W_2|W_1$). Findings related to this effect are available from the authors upon request.

We acknowledge that compliance of treatment to eligibility is not perfect in our data and that might warrant estimating a fuzzy MMRD to recover the local average treatment functions for compliers only.[7] However, because of the very low rate of noncompliance (as documented in Table 1), we chose to present the more straightforward results from a sharp MMRD estimation. Our estimates are therefore of the intent-to-treat (ITT) effect. We also acknowledge that the magnitude of the ITT estimand will be downward biased. As such, we consider the MMRD estimates presented in this paper to be a conservative lower bound.

3.3. MMRD design validity

In a similar vein to the traditional RDD framework, the MMRD model yields estimates that can be interpreted as causal effects of MPC on children's health, education, labor, and early marriage outcomes, provided households do not alter their characteristics in a manner that affects the assignment variables, i.e. their PMT scores. Additionally, observed household characteristics must maintain smoothness or continuity along the cutoff boundaries to ensure that any discontinuities along the cutoff boundaries or frontiers are driven only by the discrete jump in households' probability of receiving the treatment(s).

3.3.1. Desk score manipulation

We argue that Syrian refugee households in Lebanon cannot manipulate their position in the PMT score distribution. The score that determines the eligibility for MPC is a function of unpublicized regression coefficients that are re-estimated yearly based on updated survey data and household specific socio-demographic characteristics that are updated regularly as part of household registration exercise into the UNHCR database. Further, the disbursement of MPC is region-specific and creates varying score cutoff points depending on the region. All of these updates and specificities make it very unlikely that households can predict or anticipate changes in the eligibility criteria and manipulate their own scores accordingly.

Still, to ascertain that no manipulation affects the PMT scores we use in our estimations, we conduct a series of tests for manipulation of the PMT scores, S_1 and S_2, at the cutoff boundaries using a joint density test that is a multi-dimensional extension of the density test developed in McCrary (2008). Our tests suggest that at the 5% level, there is no evidence of any discontinuity in the joint density function around the boundaries. Technical details of the tests run are provided in the appendix.

3.3.2. Continuity of confounding factors

The second condition for the validity of a causal reading of the treatment estimates requires that observed characteristics be continuous at the cutoff boundaries. Here, we test whether each of the child's gender and age, the household head's gender, age, marital status, educational attainment, and household size is continuous along the cutoff frontiers. The validity of our approach requires that we find that the treatment functions from estimating equation (1) for these characteristics not have statistically significant intercepts α_{ij} or slopes β_{ij}. Any factors for which we do find a significant change related to treatment would need to be included as control variables in the estimation of equation (1) for outcomes of interest.

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, compared to 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 when we compare long-run to short-run recipients. 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-run 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 recipients.

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 differences in these 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 (1). To further ascertain the validity of our MMRD approach and model specification, we also run the MMRD specification without covariates to compare the results.

3.3.3. Limitations

The external validity of the causal effects estimated in this paper is limited beyond the frontier regions of the (S_1, S_2) space, as with any RDD. 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. The generalizability of the results also do not extend to refugee households without any children 19 years or younger.[8] Another limitation is that due to sample size considerations at the cutoff borders, we pooled the estimation of the treatment functions across regions by normalizing both running variables. In the final regression specification, we control for district fixed effects (at a subregional level), but we miss any possible heterogeneity in treatment effects across regions. From a policy standpoint, our variation in the duration of exposure to MPC benefits is limited to three durations: what we have labeled as short-run exposure (<10 months), what we have labeled as discontinued (12 months and discontinued, observed 2–7 - months since discontinuation), and what we have labeled as long-run exposure (16–22 months). We are therefore constrained in what we can say about duration and persistence by the availability of only these three lengths of exposure in our data. 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. For example, we are unable to link improved access to education with improved learning and eventually improved human capital accumulation.

4. Results

4.1. Overview of results

Table 3 shows the main results from running equation (1) 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. All specifications also include wave and district

[7] In a fuzzy MMRD, the causal estimand is the sharp MMRD estimator described above divided by the difference in the conditional probability function of compliance along the relevant treatment boundary frontier (Wong et al., 2013).

[8] The proportion of refugee households without any children under the age of 19 years is 2.2%.

Table 2
Continuity of confounding factors along frontier cutoffs.

		Child		Household Head				
		Female	Age (years)	Female	Age	Married	Years of schooling	Household Size
Discontinued|Non-recipient	intercept	-0.012	0.239	-0.120**	-0.362	0.117**	-0.464	0.02
		(0.038)	(0.526)	(0.054)	(1.270)	(0.047)	(0.507)	(0.312)
	slope	-0.001	-0.103	-0.096*	1.979	0.08	0.029	-0.058
		(0.007)	(0.102)	(0.051)	(1.242)	(0.008)	(0.095)	(0.064)
Short-run|Non-recipient	intercept	-0.024	0.722	-0.123**	0.854	0.099*	0.242	0.083
		(0.038)	(0.538)	(0.059)	(1.225)	(0.054)	(0.528)	(0.325)
	slope	0.005	-0.092	-0.002	1.216	-0.018	0.706	0.064
		(0.004)	(0.064)	(0.005)	(0.946)	(0.015)	(0.550)	(0.033)
Long-run|Discontinued	intercept	-0.021	0.383	0.017	0.016	-0.023**	0.078	0.018
		(0.033)	(0.453)	(0.050)	(1.102)	(0.012)	(0.450)	(0.278)
	slope	-0.004	0.015	0.004	-0.291*	-0.003	-0.014	0.019
		(0.003)	(0.048)	(0.005)	(0.168)	(0.004)	(0.045)	(0.031)
Long-run|Short-run	intercept	-0.009	-0.10	0.003	0.067	-0.006	0.047	-0.002
		(0.033)	(0.473)	(0.045)	(1.062)	(0.039)	(0.481)	(0.292)
	slope	-0.003	0.005	-0.024**	-0.093	0.017*	0.059	0.088
		(0.007)	(0.104)	(0.013)	(0.239)	(0.009)	(0.116)	(0.067)
Long-run|Non-recipient	point estimate	-0.034	0.622	-0.026	−1.124	0.02	0.213	0.141
		(0.035)	(0.499)	(0.052)	(1.156)	(0.045)	(0.499)	(0.324)
	h^*	7.78	4.78	6.80	5.38	6.96	6.39	4.38
	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 using observations $\pm h^*$ from the joint cutoff. h^* denotes the optimal bandwidth as determined by cross-validation. Nh denotes the sample size based on the bandwidth selected. Standard errors clustered at the household level are reported in parentheses. Asterisks denote statistical significance as follows. * p < .10, ** p < .05, and *** p < .01.

Table 3
MMRD estimation of treatment functions at frontier cutoffs.

	Discontinued|Non-recipient		Short-run|Non-recipient		Long-run|Discontinued		Long-run|Short-run		Long-run|Non-recipient		
	intercept	slope	intercept	slope	Intercept	slope	intercept	slope	Point est.	h^*	Nh
Acute Illness (0–5 years):											
Any illness	0.02	-0.012	-0.101*	0.011***	-0.083	-0.002	0.039	0.008	-0.063	6.10	6,280
	(0.063)	(0.012)	(0.057)	(0.004)	(0.064)	(0.006)	(0.059)	(0.013)	(0.061)		
Diahrrea	0.071	-0.013	-0.039	0.002	-0.098*	-0.008*	0.012	0.007	-0.027	8.20	7,179
	(0.052)	(0.010)	(0.044)	(0.003)	(0.051)	(0.004)	(0.044)	(0.009)	(0.047)		
Respiratory infection	-0.094**	0.015*	-0.02	0.001	0.003	0.002	-0.071**	-0.011	-0.092**	1.93	7,468
	(0.039)	(0.008)	(0.035)	(0.002)	(0.037)	(0.003)	(0.033)	(0.007)	(0.037)		
Primary Health Care (0–5 years):											
Required for illness	-0.049	-0.005	-0.084*	0.010***	-0.036	-0.002	-0.001	-0.003	-0.085*	5.67	5,976
	(0.058)	(0.012)	(0.050)	(0.004)	(0.059)	(0.005)	(0.057)	(0.012)	(0.051)		
Used for illness	0.025	0.009	0.084	-0.003	0.133*	0.015**	0.074	-0.001	0.158*	12.19	4,037
	(0.085)	(0.016)	(0.086)	(0.005)	(0.081)	(0.007)	(0.087)	(0.020)	(0.082)		
Primary Health Care (6–14 years):											
Required for illness	-0.231***	0.039***	-0.116**	0.017***	0.107**	0.003	-0.008	-0.009	-0.124***	4.90	9,330
	(0.053)	(0.011)	(0.047)	(0.003)	(0.049)	(0.005)	(0.044)	(0.008)	(0.047)		
Used for illness	-0.001	-0.009	0.053	0	0.149*	0.02***	0.095	-0.003	0.148*	1.94	4,543
	(0.084)	(0.015)	(0.084)	(0.006)	(0.081)	(0.007)	(0.082)	(0.019)	(0.081)		
Education & Labor (6–14 years):											
Formal enrollment	0.076*	-0.003	0.088*	-0.008**	-0.004	-0.001	-0.016	-0.001	0.072	8.34	12,733
	(0.045)	(0.011)	(0.050)	(0.004)	(0.055)	(0.005)	(0.054)	(0.011)	(0.045)		
Non-formal enrollment	-0.069*	0.011	-0.03	0.007***	-0.027	-0.002	-0.066*	-0.006	-0.096***	6.97	11,867
	(0.039)	(0.007)	(0.035)	(0.002)	(0.038)	(0.004)	(0.035)	(0.006)	(0.034)		
Work	-0.033**	0.003	-0.008	0.002*	-0.004	0.001	-0.029**	-0.005*	-0.037***	8.91	12,921
	(0.016)	(0.003)	(0.016)	(0.001)	(0.011)	(0.001)	(0.012)	(0.003)	(0.014)		
Early Marriage (15–19 years):											
Married	-0.066**	0.003	-0.024	0.005***	0.026	0.001	-0.015	-0.002	-0.039	1.67	6,066
	(0.026)	(0.005)	(0.028)	(0.002)	(0.023)	(0.002)	(0.026)	(0.005)	(0.025)		

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. Nh denotes the sample size based on the bandwidth selected. Standard errors clustered at the household level are reported in parentheses. Asterisks denote statistical significance as follows. * p < .10, ** p < .05, and *** p < .01

of residence fixed effects. For each of the first four RDs, the table reports the intercept and slope parameters, α_{ij} (columns (1), (3), (5), (7) and (9)) and β_{ij} (columns (2), (4), (6) and (8)) of the treatment function.

For most of our estimated treatment lines in the first 8 columns of the table, the difference in intercepts α_{ij} is far larger than the difference in the slopes β_{ij}, 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 β_{ij}. 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 10%), and long-run recipients are 8 percentage points less likely to report acute illness in children than discontinued households although the estimate is not statistically significant. The picture becomes more granular when we look at different types of illness separately: the risk of diarrhea in young children is 9.8 percentage points lower in long-run recipient households than discontinued households (significant at 10%), 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-run recipients when compared to short-run recipients (by 7.1 percentage points) and non-recipients (by 9.2 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 (8.4 percentage points) and long-run recipients (8.5 percentage points) to non-recipients.

Among households who report requiring PHC for children aged 0 to 5 years, MPC improves the household's access to PHC. This improvement is highly significant among long-run 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-run recipients (12.4 percentage points) when compared to non-recipients. However, we find a significant increase in the need for PHC when we compare long-run recipients to the discontinued group. For those households who do report requiring PHC for their children aged 6 to 14, long-run 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 7.6 percentage points and is significant at the 10% level for children in discontinued households and children in long-run recipient households (compared to non-recipients). The effect is slightly larger (8.8 percentage points) and significant (10%) 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-run 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 for children in households with long-run MPC compared to short-run and non-recipients respectively. 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, ranging between 2.4 and 6.6 percentage points, and significant at 5% for girls in discontinued households compared to non-recipients.

4.2. Robustness checks

Results based on MMRD indicate that cash transfers to Syrian refugee households have significant effects on children's health, access to education, child labor, and the risk of early marriage. Table A1 in the appendix shows that the MMRD coefficient estimates without covariates are qualitatively 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 discontinuities in observed characteristics along the cutoff points do not confound the estimation of the causal treatment functions.[9] We also disaggregate 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 follow the method suggested by Imbens and Lemieux (2008) and Cattaneo et al. (2019b) of using a placebo cutoff to test for spurious results, or discontinuities at non-boundary points. We generate random placebo cutoffs along the two desk scores in the W_0 region. Using the control subsample, we run an MMRD estimation similar to the one described above but using these random artificially generated cutoffs. The null hypothesis that there are no significant treatment effects around these artificial frontiers would suggest that the results from the MMRD with the actual cutoffs reported in Table 3 are not spurious.

Table 4 shows 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, only three 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.

5. 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

[9] We test the sensitivity of the MMRD model specification by employing a quadratic functional form for the assignment scores and, again, find no substantive differences in the regression results.

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

	Discontinued|Non-recipient		Short-run|Non-recipient		Long-run|Discontinued		Long-run|Short-run		Long-run|Non-recipient		
	intercept	slope	intercept	slope	Intercept	slope	intercept	slope	Point est.	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
	(0.108)	(0.026)	(0.108)	(0.018)	(0.112)	(0.019)	(0.111)	(0.030)	(0.106)		
Diahrrea	0.044	-0.019	0.081	-0.005	0.045	-0.002	0.007	-0.006	0.089	11.61	2075
	(0.094)	(0.024)	(0.105)	(0.017)	(0.092)	(0.016)	(0.102)	(0.029)	(0.101)		
Respiratory infection	-0.146**	0.020*	-0.062	0.009	0.037	0.008	-0.047	-0.022	-0.109*	12.93	2075
	(0.073)	(0.012)	(0.082)	(0.012)	(0.066)	(0.011)	(0.072)	(0.019)	(0.066)		
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
	(0.099)	(0.024)	(0.097)	(0.014)	(0.107)	(0.017)	(0.103)	(0.029)	(0.100)		
Used for illness	-0.103	0.023	-0.113	-0.001	-0.048	0.000	-0.038	0.004	-0.151	12.31	1091
	(0.144)	(0.038)	(0.151)	(0.025)	(0.160)	(0.027)	(0.162)	(0.049)	(0.152)		
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
	(0.095)	(0.024)	(0.096)	(0.018)	(0.104)	(0.018)	(0.100)	(0.026)	(0.105)		
Used for illness	-0.009	-0.014	-0.075	-0.001	0.057	0.005	0.124	0.008	0.048	14.21	763
	(0.180)	(0.048)	(0.181)	(0.028)	(0.180)	(0.028)	(0.175)	(0.048)	(0.193)		
Education & Labor (6–14 years):											
Formal enrollment	-0.015	0.001	0.006	0.000	-0.002	0.000	-0.023	-0.001	-0.017	5.71	2305
	(0.113)	(0.036)	(0.114)	(0.022)	(0.126)	(0.023)	(0.125)	(0.038)	(0.125)		
Non-formal enrollment	0.161**	-0.044**	0.028	-0.008	-0.081	-0.005	0.051	0.012	0.08	8.9	2549
	(0.069)	(0.019)	(0.058)	(0.010)	(0.062)	(0.011)	(0.062)	(0.014)	(0.065)		
Work	-0.058*	0.004	-0.029	0.003	0.004	0.005	-0.026	0.003	-0.055*	11.26	2559
	(0.034)	(0.007)	(0.037)	(0.005)	(0.039)	(0.006)	(0.041)	(0.012)	(0.032)		
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
	(0.043)	(0.011)	(0.047)	(0.005)	(0.054)	(0.008)	(0.056)	(0.015)	(0.045)		

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. Nh denotes the sample size based on the bandwidth selected. Standard errors clustered at the household level are reported in parentheses. Asterisks denote statistical significance as follows. * p < .10, ** p < .05, and *** p < .01.

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

	Discontinued|Non-recipient		Short-run|Non-recipient		Long-run|Discontinued		Long-run|Short-run		Long-run|Non-recipient		
	intercept	slope	intercept	slope	Intercept	slope	intercept	slope	Point est.	h*	Nh
Acute Illness (0–5 years):											
Any illness	0.011	−0.011	-0.106**	0.011*	-0.082*	−0.001	0.034	0.006	−0.071	6.10	6,280
	(0.078)	(0.015)	(0.045)	(0.006)	(0.049)	(0.007)	(0.068)	(0.014)	(0.075)		
Diahrrea	0.062	−0.011	−0.044	0.003	-0.098*	-0.008*	0.008	0.007	−0.036	8.2	7,187
	(0.066)	(0.012)	(0.060)	(0.006)	(0.058)	(0.005)	(0.051)	(0.010)	(0.060)		
Respiratory infection	-0.093*	0.015**	−0.021	0.001	0.002	0.002	-0.070**	-0.011*	-0.090*	10.93	7,476
	(0.051)	(0.007)	(0.048)	(0.004)	(0.043)	(0.004)	(0.035)	(0.006)	(0.049)		
Primary Health Care (0–5 years):											
Required for illness	−0.054	−0.004	-0.085*	0.009*	−0.034	−0.002	−0.004	−0.004	-0.088*	5.67	5,976
	(0.072)	(0.014)	(0.049)	(0.005)	(0.066)	(0.007)	(0.067)	(0.014)	(0.049)		
Used for illness	0.001	0.012	0.083	−0.004	0.154**	0.016*	0.072	0	0.155*	12.19	4,037
	(0.106)	(0.019)	(0.115)	(0.012)	(0.066)	(0.009)	(0.106)	(0.023)	(0.094)		
Primary Health Care (6–14 years):											
Required for illness	-0.232***	0.038***	-0.115**	0.017***	0.107*	0.003	−0.01	−0.009	−0.124**	4.90	9,330
	(0.062)	(0.012)	(0.058)	(0.007)	(0.055)	(0.006)	(0.051)	(0.009)	(0.055)		
Used for illness	0.003	−0.01	0.051	0	0.154*	0.020**	0.106	−0.002	0.157*	10.94	4,543
	(0.101)	(0.018)	(0.105)	(0.012)	(0.090)	(0.008)	(0.094)	(0.022)	(0.080)		
Education & Labor (6–14 years):											
Formal enrollment	0.084**	−0.004	0.096**	-0.008*	0	−0.001	−0.012	0.002	0.084*	8.34	12,733
	(0.040)	(0.014)	(0.048)	(0.005)	(0.065)	(0.006)	(0.067)	(0.014)	(0.048)		
Non-formal enrollment	-0.075**	0.012**	−0.027	0.006*	−0.025	−0.001	-0.073**	−0.007	-0.100***	6.97	11,867
	(0.030)	(0.006)	(0.048)	(0.003)	(0.044)	(0.005)	(0.033)	(0.007)	(0.038)		
Work	-0.035*	0.003	−0.008	0.002	−0.002	0.001	-0.029**	-0.006**	-0.037***	8.91	12,921
	(0.020)	(0.003)	(0.021)	(0.002)	(0.014)	(0.001)	(0.015)	(0.003)	(0.013)		
Early Marriage (15–19 years):											
Married	-0.054*	0.001	−0.018	0.004	0.026	0.001	−0.009	−0.001	−0.027	10.67	6,066
	(0.030)	(0.006)	(0.039)	(0.004)	(0.024)	(0.002)	(0.033)	(0.006)	(0.035)		

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. Standard errors clustered at the household level are reported in parentheses. Asterisks denote statistical significance as follows. * p < .10, ** p < .05, and *** p < .01.

argue that these results, at least, hold true in conflict and crisis affected contexts and over the duration of MPC benefits studied in this paper.

MPC lowers the likelihood that children aged 0–5 years report contracting any acute illnesses among recipient households in the short-run and long-run 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 needed than non-recipients. These results are in line with findings from Samuels et al. (2020) who show that access to MPC helps Syrian refugees cover the costs of prescription medicine and improves their access to healthcare. Discontinued and non-recipient households are more likely to stop accessing needed medication and treatment. Findings from Lyles et al. (2021) suggest that MPC may improve access to healthcare and medication for chronic diseases and reduce hospitalization among children. However, MPC maybe insufficient on its own to address health utilization and expenditures and would need to be implemented in tandem with other health interventions such as subsidized care costs and health education. Andrade et al. (2012) and Attanasio et al. (2005) show that cash transfers in Colombia and Brazil led to improved health outcomes, especially for children younger than 6 years. Moreover, the qualitative literature of Syrian refugees in Jordan have shown supportive evidence as well in terms of increased access to healthcare; beneficiary households from the UNICEF cash programme, in response to the question 'what has the cash grant helped covered?', respondents referred mainly to their ability to cover 'children medical needs' such as being able to take them to get treated (Overseas Development Institute, 2017).

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 illness, 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-run recipients of MPC. But similar to the findings for 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-run 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 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 the probability of transition to formal education from non-formal settings depends on 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. Samuels et al. (2020) show that Syrian refugee recipients of MPC in Lebanon use their transfers to cover education expenses such as school fees, transportation costs, books, stationery and uniforms. Non-recipients mentioned that access to MPC would allow them to enrol their children in school.

Our findings on the effects of MPC on educational access are in line with findings from the broader literature (Ahmed et al., 2007; Galasso, 2006; Schady and Araujo, 2006; Schultz, 2004). Qualitative evidence from KIIs with caretakers of Syrian refugee children in the Bekaa region in Lebanon, found similar evidence confirming that households receiving MPC are more likely to enroll their children in school with their children attending school more consistently (Foster, 2015). Moreover, in Jordan, while quantitative evidence found that the UNHCR cash transfer program did not have any impact on educational outcomes of Syrian children of beneficiary households, evidence from qualitative analysis found that cash assistance enabled their sons to remain in school instead of needing to work (Overseas Development Institute, 2017). On the other hand, Breisinger et al. (2018) found no statistically significant impact of the Takaful program 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 enrolment 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-run 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-run 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. This is supported by some evidence in the qualitative literature (focus group discussions and in-depth phone interviews) from MPC beneficiary households, discontinued households and households who have never received MPC Although considered as a sensitive topic with a strong reluctance of respondents to discuss, results revealed that improvements in the financial circumstances of a family, including through MPC, reduces the need to resort to early marriage (Overseas Development Institute, 2020). We show that MPC has some protective effects in the short- and long-run 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).

While the main purpose of cash transfers is to alleviate poverty and credit constraints for poor households, cash is hypothesized to have smaller tangible impacts namely on women's empowerment and household decision making processes. The theoretical literature assumes that economic empowerment- in terms of giving women more control over resources or more access to productive assets- is linked to both increased decision-making power for women and improved outcomes for children (Behrman, Mitchell, Soo, & Bravo, 2010). Hence, giving money to women being a feature of many conditional cash transfer programmes around the world. Giving money to women is thought to increase their bargaining power at home and give them control over household resources (Holmes & Jones, 2010). In humanitarian contexts, there is a gap in the evidence base around intra-household bargaining especially between men and women. Although we are unable to address this gap in this paper, this topic warrants future research to focus on evaluating the impacts of cash transfers on the dynamics of intra-household decision making and whether these programs can shift the balance of power within the household.

6. 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 effects of cash transfers over time and varying receipt conditions, 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. 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.

CRediT authorship contribution statement

Wael Moussa: Conceptualization, Methodology, Formal analysis, Writing – original draft, Writing – review & editing. **Alexandra Irani:** Conceptualization, Project administration, Data curation, Writing – original draft, Writing – review & editing. **Nisreen Salti:** Conceptualization, Methodology, Validation, Writing – original draft, Writing – review & editing. **Rima Al Mokdad:** Data curation, Writing – original draft, Writing – review & editing. **Zeina Jamaluddine:** Conceptualization, Resources, Data curation, Writing – original draft, Writing – review & editing. **Jad Chaaban:** Conceptualization, Funding acquisition, Supervision, Writing – review & editing. **Hala Ghattas:** Conceptualization, Writing – review & editing.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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Technical appendix

Detailed data description

The survey design relied on repeated cross-sections of Syrian refugee households from the three governorates of Lebanon where close to 85% of all Syrian refugees in Lebanon resided during the study period, based on the UNHCR registration database (Mount Lebanon, North Lebanon, and Bekaa).

The sampling frame is further restricted to only households whose PMT scores were within a 10-point range of the 2018

MPC eligibility cutoff and a 20-point range of the 2017 eligibility cutoff.[10] The full range of PMT scores in 2018 is 24.6 to 424.5, whereby the sampling frame consists of 31.6% of the total refugee population and 57.3% of all MPC eligible households. The sample was selected following a standard probability proportional to size. We apply sampling weights extracted from the UNHCR registry of Syrian refugees in Lebanon to all analyses to maintain regional representation. We append to the survey data information from the UNHCR refugee registry database, including households' PMT scores and household access to other assistance programs. Information from the registration database is appended to survey data using a unique scrambled household identifier.

Fig. A1 displays the distribution along S_1 and S_2 from the analytic sample and shows that the distribution is uniform and smooth around the eligibility cutoffs. The figure also shows as 4 quadrants the 4 mutually exclusive treatment conditions, denoted as W_0, W_1, W_2, and W_3 corresponding to different permutations of MPC receipt status for the two periods (W_0 = never received, W_1=received only in the first period, W_2= received only in the second period, andW_3 = received in both periods).

Details of the empirical strategy

We denote eligibility for treatment (receipt of MPC) by D_1 and D_2, for 2017 and 2018 respectively. Eligibility for treatment is determined by the desk score, S, for each administration of the treatment in time j.

$$D_j = \left.\begin{matrix} 0, & S_j > 0 \\ 1, & S_j \leq 0 \end{matrix}\right\}; \quad j = 1,2 \qquad [1]$$

The four possible eligibility conditions defined above can be expressed as:

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

Fig. 2 shows 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 allows 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). To illustrate, the effect of W_2 relative to W_0 is the difference in outcomes between households just left and right of the orthogonal plane $(Y, S_1 \geq 0, S_2 = 0)$ that separates the two treatment areas (see Fig. A2). The multiple dimensions allow us to identify a treatment "function" along each of the four frontiers rather than a single local average treatment effect at one cutoff point as we would in a standard regression discontinuity application. This enables us to determine the effect of MPC when assignment scores approach the cutoff for eligibility for one treatment along the desk score axis for the other treatment. The resulting causal effects identified using an MMRD approach are local to the frontier cutoffs and, thus, may not hold everywhere 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:

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

Equation (3) defines four surfaces covering each of the regions of the partition defined by the four treatment conditions $W_k, fork = 0,..,3$. As illustrated in Fig. 2, α_k represents the Y-intercept of the plane covering region k at $(S_1, S_2) = (0,0)$. β_k represents the slope of the intersection of the same plane with the orthogonal plane at $S_2 = 0$, and γ_k is the slope of the intersection of the plane with the orthogonal plane at $S_1 = 0$. δ_k denotes the gradient of the plane inside each region. ε is an idiosyncratic error term.

The conditional expectation for each region becomes:

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

We can identify the causal effects from neighboring treatment conditions along the frontiers separating the four regions as follows:

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

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

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

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

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

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

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

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

To illustrate these effects graphically, Fig. A3 shows the treatment function produced by the MMRD estimation on the orthogonal plane along S_2 and Y, while $S_1 = 0$ for the comparisons of discontinuation to ineligibility ($W_1|W_0$, top panel) and long-run treatment to recent eligibility ($W_3|W_2$, bottom panel).

The remaining comparisons between the treatment conditions (W_3, W_0) and (W_1, W_2) happen at the limits of S_1 and S_2, so they are reduced to a single point at $(S_1, S_2) = (0,0)$ as these pairs only

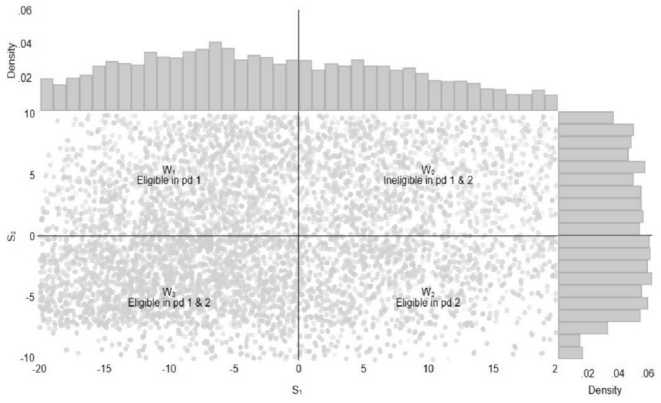

Fig. A1. S_1 and S_2 and desk score distributions.

[10] 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.

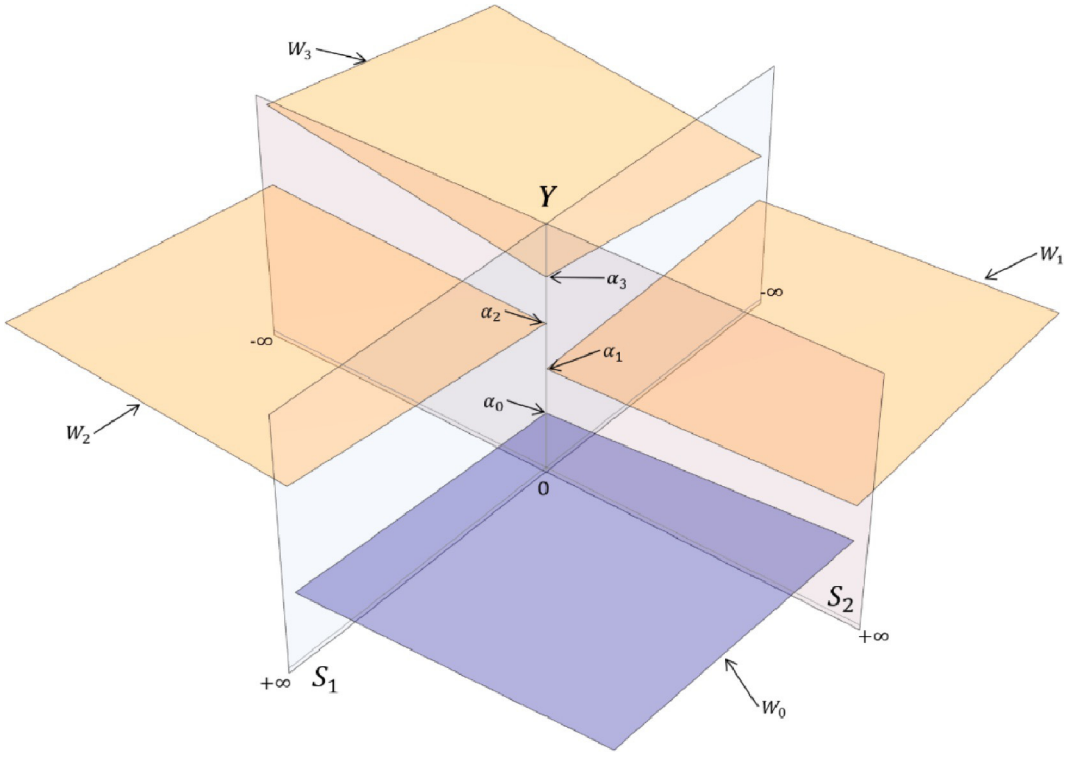

Fig. A2. Illustration of equation (3).

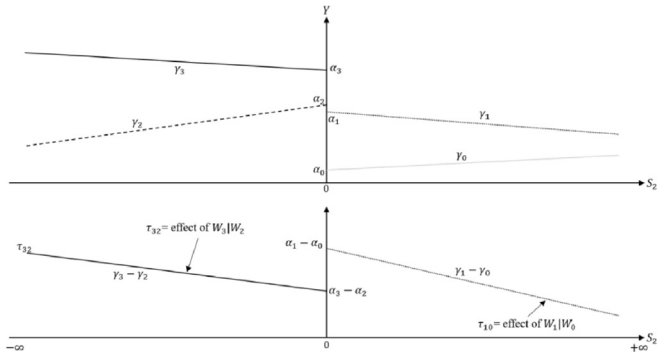

Fig. A3. Cross-sectional illustration of Fig. A2 at the S_1 cutoff.

meet at the origin. To complete our notation, the effect of maintaining eligibility,W_3, relative to never being eligible, W_0 can be written as:

$$\tau_{30} = E[Y|W_3, S_1, S_2] - \lim_{S_1 S_2 \to 0^+} E[Y|W_0, S_1, S_2] = \alpha_3 - \alpha_0$$

And the effect of becoming eligible losing eligibility W_2 compared to losing eligibility W_1 is denoted as:

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

Choice of optimal bandwidth

Observations used in the estimation are those lying within an optimal bandwidth of each boundary. Bandwidths are chosen following the two-step procedure described in Imbens and Lemieux (2008). We use iterative cross-validation to determine the optimal joint bandwidth (h_1, h_2) in the (S_1, S_2) space.[11] 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 estimate the treatment function between W_1 and W_0 as $\hat{\tau}_{10} = E[Y|W_1, -h_1^* \geq S_1 \geq 0, S_2] - E[Y|W_0, 0 \leq S_1 \leq h_1^*, S_2]$.

Tests for the manipulation of PMT scores

Cattaneo et al. (2019a) recommend using a local polynomial density estimator along the two assignment variables space to test for manipulation along the boundary cutoffs. This method enables

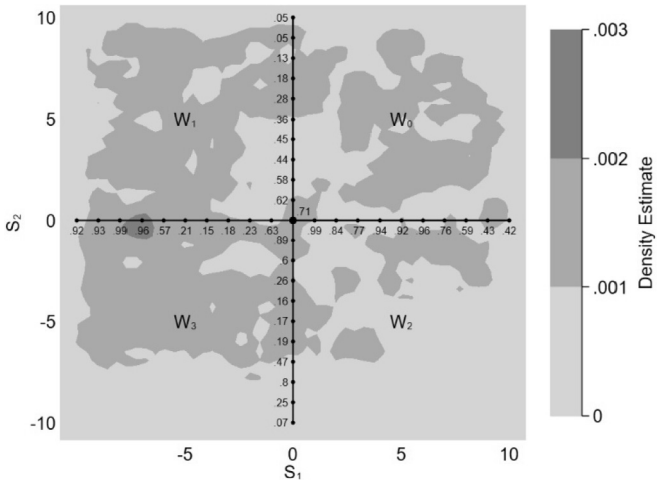

Fig. A4. Density test of joint desk score distribution along cutoff boundaries. Notes: figures along the and axes denote p-values of the manipulation test statistics at the corresponding boundary coordinate.

[11] We follow Papay et al. (2011) to select the bandwidth that minimizes the following cross-validation criterion:$CV_Y(h_1, h_2) = \frac{1}{N}\Sigma_i^N (Y_i - \hat{\mu}(X_{1i}, X_{2i}, h_1 h_2))^2$

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.

Fig. A4 is a contour plot of the joint density for S_1 and S_2, where we show the p-values corresponding to the density test statistic at 10 equidistant points within 10 units of each cutoff boundary. We find that the manipulation test fails to reject the null hypothesis at the 5% level that the density is continuous along the boundaries at each of the 40 points. However, the test statistic does leads us to reject the null hypothesis at the 10% level at three points, $(9, 0)$, $(10, 0)$, and $(-10, 0)$. We argue that these differences are within the 10% significance threshold with only 3 out of 40 tests being significant.

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