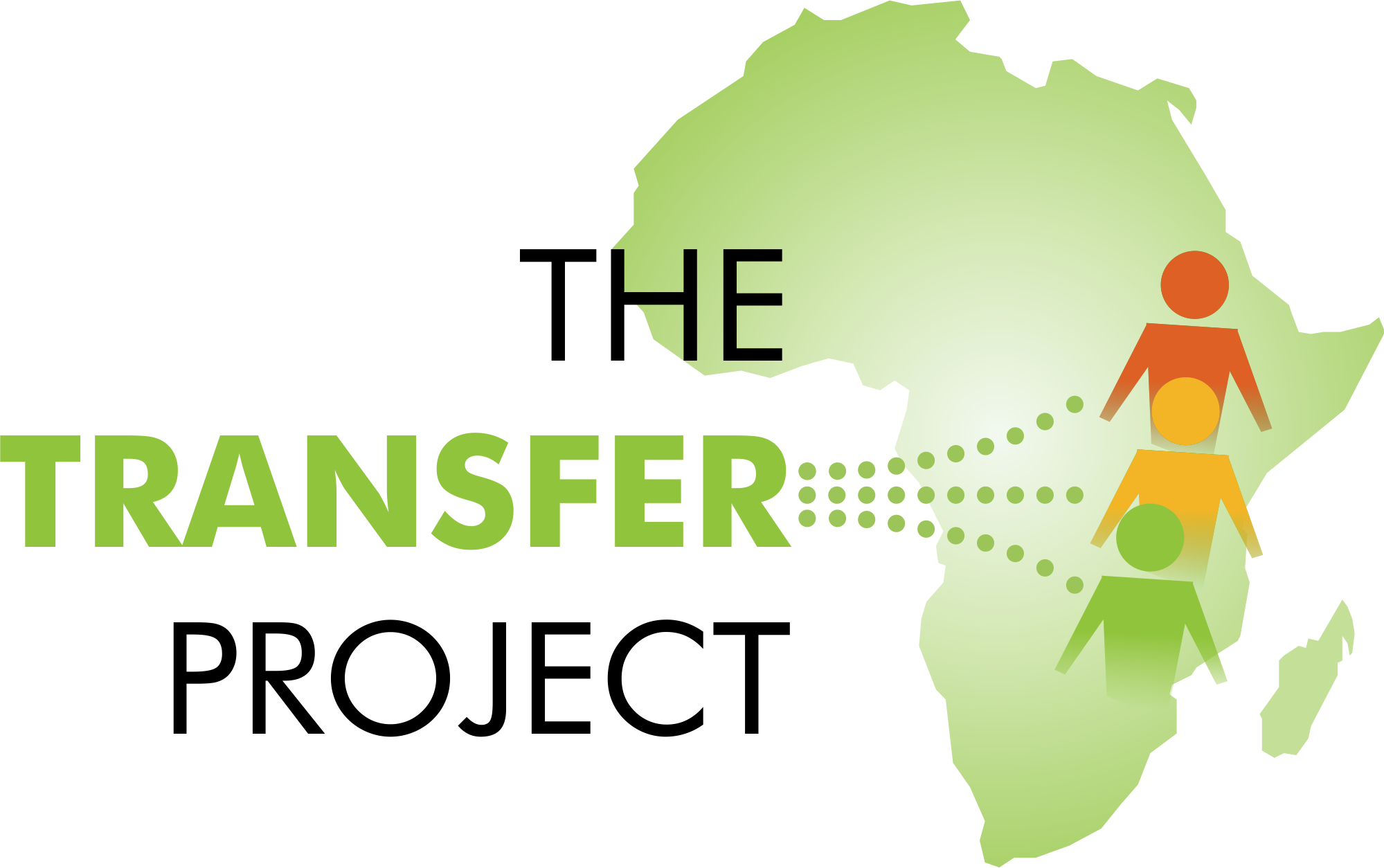
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**Evaluation of No Lost Generation/”Min Ila”, a UNICEF and WFP Cash Transfer Program for Displaced Syrian Children in Lebanon**

Rapid Follow-up Report

| Evaluation of No Lost Generation/”Min Ila”, a UNICEF and WFP Cash Transfer Program for Displaced Syrian Children in Lebanon  Rapid Follow-up Report  May 2017  **Jacobus de Hoop, UNICEF Office of Research - Innocenti**  **Mitchell Morey, American Institutes for Research**  **David Seidenfeld, American Institutes for Research** |
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## Abbreviations and Acronyms

AIR American Institutes for Research

ALP Accelerated Learning Program

DFID Department for International Development

GPS Global Positioning System

HRW Human Rights Watch

IRB Institutional Review Board

IRC International Rescue Committee

MCA Multipurpose Cash Assistance

NGO Nongovernmental Organization

OoR Office of Research – Innocenti

OOSCI Out of School Children

RDD Regression Discontinuity Design

RACE Reaching All Children with Education in Lebanon

SL Statistics Lebanon

UN United Nations

UNHCR United Nations Refugee Agency

UNICEF United Nations Children's Fund

VASyR Vulnerability Assessment of Syrian Refugees in Lebanon

WFP World Food Programme

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Jacobus de Hoop  
Mitchell Morey  
David Seidenfeld

## Executive Summary

This report provides a summary of the preliminary education impacts generated by the No Lost Generation (NLG)/”Min Ila” cash transfer program during the first few months of implementation*.* In 2016, the United Nations Children’s Fund (UNICEF) in partnership with the United Nations World Food Program (WFP) and in coordination with the Ministry of Education and Higher Education (MEHE), started piloting a child-focused cash transfer program designed to lower negative coping strategies harmful to children and remove barriers to primary school aged Syrian children’s school attendance, including financial barriers and reliance on child labor. The pilot also includes a very important household visit and referral component for chidren absent for more than 10 days of school. The evaluation does not focus on this component but it does impact the results of the evaluation. American Institute for Research (AIR) was contracted by UNICEF Lebanon to help UNICEF Office of Research (OoR) design and implement an impact evaluation of the program (with a nonexperimental design), based on baseline data and rapid follow-up data.[[1]](#footnote-2) The purpose of the impact evaluation is to monitor the program’s effects on recipients and provide evidence for decisions regarding the program’s future. The primary goals of this rapid follow-up report are to investigate and discuss the preliminary impacts on education outcomes, specifically enrollment and attendance, after the first few months of program implementation. The endline survey to be carried out later in the year will look at additional indicators of child well-being.

**The Context:** Lebanon has one of the highest per-capita ratios of registered refugees in the world (LCRP 2015-2016).[[2]](#footnote-3) Out of a population of 5.9 million, 1.5 million are displaced Syrians. Most Syrians arrived with limited savings and have struggled to earn steady incomes to meet their families’ basic needs, such as food, healthcare, and shelter. These basic needs tend to require immediate attention, which means that Syrian families often forgo education and its long-term benefits in favor of short-term needs. Consequently, more than 2.6 million children are out of school not only in Lebanon, but also in Syria, Turkey, Jordan, Iraq, and Egypt (UNHCR 2016).[[3]](#footnote-4)

The Reaching All Children with Education (RACE) project in the Lebanese Ministry of Education and Higher Education (MEHE) has partnered with international donors, the United Nations and local nongovernmental organizations (NGOs) to implement education interventions to address this crisis. These efforts include introducing an afternoon shift in public Lebanese primary schools for displaced Syrian and other Non-Lebanese children—the so-called “second shift”. As a result of these efforts, almost 158,000 children were enrolled in formal education for the 2015–16 school year. The 2015 Vulnerability Assessment of Syrian Refugees in Lebanon (known by the acronym VASyR) – a nationally representative assessment carried out by the UNHCR, UNICEF, and WFP – found that Syrian children are more likely to be out of school as their age increases, with a particular increase in dropout rates starting around 10 years of age. The feasibility study for the NLG/Min Ila pilot found that this pattern was due in part to greater household reliance on negative coping strategies such as reducing food in-take and child marriage and especially child labour earnings for older children (aged 10+).

**The Program:** In the 2016–17 school year, UNICEF Lebanon started NLG/”Min Ila” pilot cash programme in partnership with WFP and MEHE. A cash transfer program for displaced Syrian children piloted in the governorates of Mt. Lebanon and Akkar. Syrian children aged five to nine years old who live in the Mt. Lebanon and Akkar governorates and are enrolled in a second shift school receive a basic monthly cash transfer of US$20, roughly equivalent to the average indirect costs to going to school such as transportation, clothes, shoes, and food for school. Syrian children aged 10 to 14 who are enrolled in a second shift school receive a larger monthly education transfer of US$65, to factor in the average indirect costs to going to school and the higher monthly earnings of a working child of this age group.[[4]](#footnote-5) The cash transfer will last for the duration of the school year, and payments are made every month on an ATM card.[[5]](#footnote-6) Although there are no conditions that need to be met in order to receive the money, school attendance is monitored and follow-up household visits are carried out for children who do not attend school regularly. The purpose of these visits are to 1) record reasons for drop-out, and 2) to refer households to existing complementary services, to help children back into school. The cash transfers are, however, stopped for those children and their families that can not be reached by phone or found during households visits during 3 months.

**Evaluation:** The nonexperimental longitudinal study design compares beneficiaries in the pilot governorates of Mt. Lebanon and Akkar with households who would be eligible for the program but who live in the nonprogram governorates of North Lebanon and South Lebanon. The study uses a geographic regression discontinuity design (RDD), where households that are located near the border separating pilot and comparison governorates are compared to each other. The study follows the same households over time, with the baseline data collected from September to October 2016 and rapid follow-up data collected from February to March 2017. The purpose of the study is to measure the immediate effects of the program on children’s school participation. The evaluation includes 1,440 households, with roughly 20 from each of the study’s 74 cadaster clusters, all of which are located by at least one second shift school. UNICEF will conduct more extensive data collection at the beginning of the 2017-2018 school to investigate longer term effects of the program acorss more outcomes. This evaluation was harmonized to a household food security study conducted by WFP, to evaluate the effects of the programme on participating household food consumption over course of the pilot study.

**Impacts on Enrolment:** There is suggestive evidence that the program modestly increased second shift school enrolment. However, school capacity constraints may have dampened this effect because households in the study reported that over half of the second shift schools in the study had reached full capacity during the registration phase, preventing children from enrolling. After the program started, nearly 40 percent of the children in the pilot areas enrolled in a second shift school with another 10 percent expected to enrol but unable due to capacity limits at their nearest second shift school. New second shifts were opened in response to the increased demand in other parts of the pilot governorates; however, this change was not captured by the study because the sample was selected from the areas around existing second shift schools. We therefore currently cannot report with sufficient confidence on the modest remaining increase in school enrolment.

Aggregate MEHE administrative data suggest that formal school enrolment rates of displaced Syrian children increased rapidly across the country from the past (2015-2016) to the current school year. The administrative data show that while 2nd shift enrolment increased 41% nationally, in pilot areas it increased 51%. This study also found that schooling rates increased in both pilot and comparison areas from nearly 60 percent at baseline to nearly 80 percent at follow-up. School enrolment increases were particularly pronounced for children aged 5 to 9, whose self-reported school enrolment increased from slightly over 60 percent to nearly 90 percent.

**Impacts on Attendance:** The program increased school attendance for children who were enrolled in a second shift school by 0.63 days per week on average. This means that children in the pilot attended one month of school days more than children in non-pilot areas. In other terms, children enrolled in a second shift school spent roughly 20 percent more time in school compared to similar children in comparison areas. As a result of the program, children 5 to 9 years old attended second shift school 0.7 more days per week and children 10 to 15 years old attended second shift school 0.5 more days per week compared to similar children in comparison areas. Children receiving the NLG/”Min Ila” cash pilot program benefits attend school on average 4.1 days per week, which means that they are in school over 80 percent of the time possible.

**Reflections on Study and Program Implementation**: This study was designed to capture program effects among children living in the vicinity of an active second shift school. The rationale was that these children could readily enrol in a second shift school in response to the program, allowing the impact evaluation to capture the impact of increasing the demand for education through a cash transfer program. However, over half of all second shift schools in the pilot areas of the study reached full capacity while registering children and had to turn away children who wanted to enroll. While MEHE was prepared to open new second shifts in existing schools to accommodate the increase, as in previous years, in some areas there were no additional school buildings in which to open new second shifts in the vicinity of the beneficaries tracked for this study. This situation may have created a ceiling effect for the study because it is impossible for the program to increase enrolment above the capacity of the second shift schools. In other words, the program was unable to demonstrate its full potential to generate enrolment effects due to the limit on spaces to enrol children in second shift schools. Furthermore, due to the sample being selected from areas with existing schools, enrolment in newly opened second shifts not located near the sampled schools could not be captured.

In this scenario, limited impacts on school enrolment do not necessarily reflect a limited impact on schooling outcomes. On the contrary, assuming that addressing the second shift school capacity constraints would increase school enrolment by an additional 10 percent, the potential for the program to increase both attendance and enrolment is substantive.

UNICEF contracted AIR for the baseline and rapid follow-up study. This report represents the result of that work. UNICEF will continue the evaluation for at least 12 months to measure the impact of the program after a full year of implementation, and to investigate the many domains measured at baseline such as food security, household income, and debt, as well as education and child labor.

## I. Introduction

This report provides the rapid follow-up results of an impact evaluation of a cash transfer program for displaced Syrian primary school aged children living in Lebanon. In 2016, the United Nations Children’s Fund (UNICEF) in partnership with the United Nations World Food Programme (WFP), and in coordination with the Ministry of Education and Higher Education (MEHE), started piloting a child-focused cash transfer program designed to lower negative coping strategies harmful to children and or remove barriers to Syrian children’s school attendance, including financial barriers and reliance on child labor. American Institutes for Research (AIR) was contracted by UNICEF Lebanon to help UNICEF Office of Research (OoR) design and implement an impact evaluation of the program (with a nonexperimental design), using baseline data and rapid follow-up data.[[6]](#footnote-7) The purpose of the impact evaluation is to identify the program’s effects on recipients and provide evidence for decisions regarding the program’s future. The primary purpose of this rapid follow-up report is to present the main education impacts generated by the NLG/”Min Ila” cash transfer program during the first few months of implementation. The endline survey to be carried out at the beginning of the next school year will look at additional indicators of child well-being.

### Context

Lebanon has one of the highest per-capita ratios of registered refugees in the world (LCRP 2015-2016).[[7]](#footnote-8) Out of a population of 5.9 million, 1.5 million are displaced Syrians. Most Syrians arrived with limited savings and have struggled to earn steady incomes to meet their families’ basic needs, such as food, healthcare, and shelter. These basic needs tend to require immediate attention, which means that Syrian families often must forgo education and its long-term benefits in favor of short-term needs. Consequently, more than 2.6 million children are out of school not only in Lebanon, but also in Syria, Turkey, Jordan, Iraq, and Egypt (UNHCR 2016).[[8]](#footnote-9)

The Reaching All Children with Education (RACE) project in the Lebanese Ministry of Education and Higher Education (MEHE) has partnered with international donors, the United Nations and local nongovernmental organizations (NGOs) to implement education interventions to address this crisis. These efforts include introducing an afternoon shift in public Lebanese primary schools for displaced Syrian children—the so-called “second shift”. As a result of these efforts, almost 158,000 children were enrolled in formal education for the 2015–16 school year. The 2015 Vulnerability Assessment of Syrian Refugees in Lebanon (known by the acronym VASyR) – a nationally representative assessment carried out by the UNHCR, UNICEF, and the World Food Program (WFP) – found that Syrian children are more likely to be out of school as their age increases, with a particular increase in dropout rates starting around 10 years of age.

### Background

In the 2016–17 school year, UNICEF and WFP Lebanon started to roll out a cash-based transfer pilot program for displaced Syrian households with primary school aged children in the governorates of Mt. Lebanon and Akkar. Syrian children aged five to nine who live in the Mt. Lebanon and Akkar governorates and are enrolled in a second shift school receive a basic monthly education transfer of US$20, roughly equivalent to the average indirect costs to go to school (“second shift schools” are public Lebanese primary schools that provide an afternoon shift for displaced Syrian children, known as the “second shift.”) Syrian children aged 10 to 14 who are enrolled in a second shift school receive a larger monthly education transfer of US$65, roughly equivalent to the average monthly indirect school costs as well as a portion of the earnings of a working child.[[9]](#footnote-10) The cash transfer will last for the duration of the school year,and payments are made every month on a bank card. Although there are no conditions that need to be met in order to receive the money, school attendance is monitored and follow-up household visits are carried out for households of children who do not attend school regularly. The objective of the household visits is to record the reason for non-attendance and refer households to a range of existing complementary services that will help them overcome the obstacles to regular school attendance.

### Evaluation

The nonexperimental longitudinal study design compares beneficiaries in the pilot governorates of Mt. Lebanon and Akkar with households that would be eligible for the program but that live in the nonprogram governorates of North Lebanon and South Lebanon and Nabatiyeh. The study uses a geographic regression discontinuity design (RDD), where households located near the border that separates program and nonprogram governorates are compared to each other. The study follows the same households over time, with the baseline data collected in October 2016, and a rapid follow-up data collection in February and March 2017 to measure the immediate effects of the program on children’s school participation. More extensive data collection beyond schooling effects will be carried out at the beginning of the 2017–18 school year.

### Objectives of the Study

The key evaluation topics are as follows:

1. **Immediate schooling effects:** The study aims to provide insight into the direct effects of the program shortly after the start of the 2016–17 school year. In particular, the study will establish the extent to which the program increased school enrollment and the program’s impact on school attendance at the start of the school year.
2. **Schooling effects after the first school year:** The study will also examine wider effects on children’s education outcomes at the beginning of the 2017-18 school year. Topics to be covered include the program’s impacts on satisfactory completion of the school year, (re)enrollment in the next school year, and grade progression; reasons for dropping out during the school year; and other barriers to education.
3. **Broader effects on children’s lives.** The cash transfer pilot program is expected to have cascade effects on other important areas of children’s lives, particularly given that the household visits refer households to a range of services beyond education-focused ones (e.g. protection interventions, health services, etc.). The study will give a comprehensive overview of these effects on negative coping strategies harmful to children, including engagement in (hazardous) child labor, exposure to bullying and violence, risky behavior, aspirations and ambitions for the future, migration, and early child marriage.
4. **Broader effects on children’s households**. The cash provided to households may lead to changes in expenditure patterns, the intra-household allocation of resources, the intra-household division of labor, and food security.
5. **Mediators and heterogeneity.** The impact of the cash transfer program will likely be different for specific subgroups of the Syrian refugee population. The impact evaluation will aim to establish how the effect of the cash transfer program varies across key subgroups—for example, by gender, household size, and the length of time living in Lebanon.

This report focuses on the first objective, immediate schooling effects.

## II. Theory of Change

Policy-relevant research should be built on a theory of change that maps out the causal chain across activities, outputs, outcomes, and impacts, as well as the assumptions that underlie that theory of change.

### Activities

The pilot cash transfer program was specifically designed to address the key demand side barriers to school participation identified previously in nationally representative household surveys:[[10]](#footnote-11) the cost of education and household reliance on children for income generation. It provides income transfers to households for children enrolled in second shift primary schools. Younger children, aged five to nine, receive US$20 each month. Older children, aged 10 to 14, receive a higher amount: US$65 a month. This amount is estimated to offset a portion of the average monthly indirect costs of schooling and earnings of a working child or early marriage, so that the cash transfer program can assist in offseting the opportunity cost of school attendance for older children.

The transfers are unconditional, but labelled. However, UNICEF and WFP encourage school attendance by:

* Raising awareness among stakeholders to contribute to a supportive environment for education.
* Asking households to sign an agreement when they register as pilot beneficiaries. The agreement states that they understand that the cash is intended to facilitate their children’s school attendance, and that they are willing to be visited by pilot actors for referral to complementary services if their children are absent for more than 10 consecutive days.
* During the household visit, recording the reason for non-attendance and referring members to complementary services. The referrals link the cash transfer program to existing complementary services offered by the government, UNICEF and other agencies, which address nonincome-related constraints (such as the need for psychosocial support or difficulty keeping up with the Lebanese curriculum). In this way, the cash transfer program contributes to an integrated package of support.

### Pathways to Impacts

Households with very low levels of consumption spend almost all of their income. We therefore expect that among the beneficiary population, virtually all of the cash transfer will be spent during the initial stages of the program, with spending focused on meeting basic needs such as food, clothing, transport, and shelter. Once immediate basic needs have been met, and possibly after a period of time, the influx of new cash may then trigger further responses within the household economy—for example, the use of services and the ability to free up children to attend school.

Figure 2.1 shows the theory of change developed by UNICEF and RACE for all program activities. This study focuses on the cash transfer part of the program. We highlighted in yellow the pathways of the theory of change relevant for this study in that these are the areas that are measured and examined in the evaluation. We expect the cash transfer to have a direct effect on household consumption and the use of services, and reduce the need to resort to negative coping strategies and specifically reduce child labor and child marriage. The impact of the cash transfer may be smaller or larger depending on local conditions in the household and community. These moderators include household characteristics, access to other services, prices, and shocks. The key point here is that any potential program impact on children must work through the household by influencing spending or time-allocation decisions (including use of services). The link between the household and children can also be moderated by environmental factors, such as distance to schools or health facilities (as indicated in the diagram), and household-level characteristics, such as the mother’s literacy.

Figure 2.1. Theory of Change

Risks:

* Non-Lebanese return/are forced to return to Syria
* No funding available for transfers
* Strategy to expand 2nd shift supply in saturated areas is not successful
* No funding available to expand 2nd shift
* Legal situation of refugees allows engagement in higher value-added types of work

DESIRED SITUATION

Improved well-being of non-Lebanese primary school-aged children due to increased investment in human capital development by households

Improved chances of transition to formal secondary school or TVET (primary completion, formal credentials)

Improved opportunities for non-Lebanese children due to attainment of formal primary education

**Reduced reliance on coping strategies negatively affecting education & nutrition of primary school aged non-Lebanese children**

**Increase in number of non-Lebanese children finishing formal primary education even after dropping out and obtain official primary school credentials**

**Increase in number of non-Lebanese children accessing formal primary education even after dropping out**

Increased uptake of complimentary services & programmes

Social cohesion increases

Assumptions:

* Sufficient capacity of 2nd shift schools to absorb new demand;
* Schools allow children to enrol and write exams even without ID documents;
* Social/complimentary services exist and are functional;
* Common ATM Card System (LOUISE) is established and functional;
* Households will spend cash on nutrition/health as well as indirect costs of education, contributing to overall improved well-being of children;
* 2nd shift schools allow drop-outs to return to classes, as per agreement with RACE PMU.

Spending of benefits in local shops

Importance of education reinforced through messaging

SMS reminders of purpose of cash with notification of payment sent to caregivers

Household visits and referrals to complimentary services provided to households of children who **drop-out** of school

**Households spend more on education and children’s nutrition and health, shoes and appropriate clothing**

**HHs incentivized to send children to formal schools**

**Monthly cash transfers provided to refugee households to send children to formal primary school (2nd shift).**

## III. Study Design

The impact evaluation of Lebanon’s child-focused cash transfer program uses a longitudinal, geographical regression discontinuity design (RDD).[[11]](#footnote-12) The study compares the child-focused cash transfer recipient households from beneficiary governorates (specifically Akkar and Mt. Lebanon where the program is being piloted) with similar households in neighboring governorates (North Lebanon and South Lebanon respectively) that did not begin receiving the transfers during the period of the study.

74 clusters of Cadasters in these governorates were selected for the study: 21 in Akkar, 22 in North Lebanon, 20 in Mt. Lebanon, and 11 in South Lebanon and Nabatiyeh. Cadasters are small administrative geographical units, somewhat comparable in size to census enumeration areas in other countries. These clusters of Cadasters were selected for the study based on their location near the border separating pilot governorates from comparison governorates and for being close to at least one second shift school active during the previous (2015-2016) school year. The impact evaluation tracks the well-being outcomes of children in 1,456 households, roughly 20 in each of the 74 clusters of Cadasters. The selection of the clusters of Cadasters and households within these clusters is described in more detail in a separate baseline report.[[12]](#footnote-13)

A baseline questionnaire was administered in each of the 1,456 households prior to the start of the 2016-2017 school year, i.e. the year in which the pilot cash transfer program started. As part of this baseline data collection, GPS coordinates were collected for each household, enabling us to calculate their distance from the border with its paired governorate. Map 1 shows the geographic distribution of second shift schools and study households. Large circles denote the 2.5 kilometer radius around the second shift schools, small green circles denote each pilot household, and small red triangles denote each comparison household. The majority of households live within a 2.5 kilometer radius from a second shift school and the study design was based on the assumption that children living within this radius would not be out of school due to supply constraints.[[13]](#footnote-14) We will return to this assumption below.

The baseline data show that generally speaking schooling and other welfare outcomes improve as we move to the south of the country. However, outcome variables do not *directly* change as one crosses the Akkar-North and Mt. Lebanon-South borders.[[14]](#footnote-15) This finding lies at the core of the geographical regression discontinuity estimation strategy which we use to examine whether the program resulted in any changes in schooling outcomes. Given that outcome variables did not directly change when crossing the borders separating pilot and comparison areas at baseline and assuming that the pilot program was implemented successfully and reaches only those households living in pilot areas, any direct changes at these borders *at follow-up* can be reasonably attributed to the pilot program itself. Appendix B discusses the technical details of the estimation strategy and the relatively mild underlying assumptions in more detail.

A major reason for choosing a nonexperimental RDD (rather than a randomized, controlled trial) is that a decision was made to roll out the pilot program at scale in the governorates of Akkar and Mt. Lebanon for reasons not related to the impact evaluation. Randomization could not occur either within pilot governorates or at other levels. UNICEF is aware that the current design leaves open the theoretical possibility that observed differences between pilot and comparison households result from an effect other than the cash transfers if circumstances that occur in an early-entry governorate do not occur in delayed-entry governorates (such as differences in local politics). However, there are currently no indications of such structural differences between governorates.

Map 3.1. Treatment and Comparison Areas



### Power to Detect Effects

It is vital to have a sample size that is sufficiently large to detect small but meaningful effects of the cash transfer program. For this reason, we conducted power calculations to determine the minimum detectable effect size for the evaluation. This study relies on an RDD that allows us to detect a program impact on school enrollment of 0.254 standardized mean difference effect size. This means that the study is well powered to detect effects of meaningful size if they exist. Due to the limited number of cadasters in each governorate, this study is unable to estimate impacts at the governorate level with reasonable precision (95 percent confidence), and can only estimate the impacts of the program as a whole.

## IV. Overview of Rapid Follow-up Data Collection

As mentioned above, quantitative baseline data were collected from the sampled households from August to October 2016. UNICEF hired Statistics Lebanon (SL), a research and data collection organization based in Beirut, to oversee and implement quantitative follow-up data collection with the same households via phone calls several months into the program. UNICEF, SL, and AIR facilitated data collection training among SL data collectors during the third week of February 2017. SL began collecting rapid follow-up data in the first week of March in all pilot and comparison governorates.

The rapid follow-up data collection focused on education outcomes for children, specifically enrollment and attendance during the 2016-2017 school year. SL called all households from the baseline study.

### Attrition

Attrition within a sample occurs when households from the baseline sample are missing in the follow-up sample. Mobility, the dissolution of households, and death can cause attrition and make it difficult to locate a household for a second data collection. Attrition causes problems in conducting an evaluation because it not only decreases the sample size (leading to less precise estimates of program impact) but also introduces selection bias to the sample, which will lead to incorrect program impact estimates or change the characteristics of the sample and affect its generalizability.[[15]](#footnote-16) There are two types of attrition: differential and overall. Differential attrition occurs when the pilot and comparison samples differ in the types of individuals who leave the sample. Differential attrition can create biased samples by eliminating the balance between the pilot and comparison groups achieved through the study design at baseline. Overall attrition is the total share of observations missing at follow-up from the original sample. Overall attrition can change the characteristics of the remaining sample and affect the ability of the study’s findings to be generalized to populations outside the study. Ideally, both types should be small.

We investigate attrition at the rapid follow-up by testing for similarities at baseline between (1) pilot and comparison groups for all nonmissing households (differential attrition) and (2) all households at baseline and the remaining households at the rapid follow-up (overall attrition). Fortunately, we do not find any significant differential attrition at the rapid follow-up, meaning that we preserve the benefits of the study design. We also do not find any differences between the study population at baseline and those that remain at the rapid follow-up.

### Overall Attrition

The study lost only four percent of households from the baseline sample with 96 percent responding to the rapid follow-up survey. There were 1,440 households with a child between the age of 5-14 at baseline. Only 54 of these households were not surveyed at the rapid follow up, with 1,387 remaining in the study. We do not find any difference in baseline characteristics between the original sample and the sample that remains. Appendix F shows the test results for all variables at baseline between the remaining sample and the original sample.

### Differential Attrition

We also find no difference in baseline characteristics between the pilot and comparison households that remain in the study at the rapid follow-up, meaning that there is no differential attrition and the benefits of the study design are preserved. Table 4.1 shows the household response rates at the follow-up by pilot status for each governorate.

We tested all the household, young child, and older child outcome measures and comparison variables for statistical differences at baseline between the pilot and comparison groups that remain in the rapid follow-up analysis. None of the indicators is statistically different, demonstrating that on average, people missing from the rapid follow-up sample looked the same at baseline regardless of whether they were from the pilot or comparison group. Appendix F contains the these test results for differential attrition. The similarity of the characteristics of people missing in the follow-up sample between pilot statuses allays the concern that attrition introduced selection bias. Thus, the study maintains strong internal validity created through the geographical RDD, enabling estimated impacts to be attributed to the cash transfer program rather than to differences in the groups resulting from attrition.

**Table 4.1 Attrition by Governorate**

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
|  | **Treatment** | | **Comparison** | |  |
|  | **Akkar** | **Mt. Lebanon** | **North** | **South** | **Total** |
| **Missing** | 8 | 14 | 27 | 5 | 54 |
| **Longitudinal** | 406 | 361 | 389 | 230 | 1,386 |
| **Total** | 414 | 375 | 416 | 235 | 1,440 |
| **Longitudinal (%)** | **98** | **96** | **94** | **98** | **96** |

## V. Rapid Follow-up Education Results

This part of the study investigates the impacts of the NLG/”Min Ila” cash transfer pilot program on education outcomes in second shift schools during the first few months of pilot program implementation. We focus on two primary outcomes: enrollment and attendance. First we investigate program enrolment to understand who ultimately received the program among the treatment and comparison groups.

Pilot Program Enrolment

We investigate pilot program enrolment and find that no one took up the program in comparison areas; thus, the study maintains a good comparison group (i.e. households in the sample living in non-pilot areas did not enrol their children just across the governorate border in pilot areas just in order to benefit from the pilot program). However, only 50% of eligible households in treatment areas enrolled in the pilot program, limiting the extent to which the program reached the targeted eligible beneficiaries. Another 24% of eligible households enrolled in first shift public schools, thus only one-quarter of eligible beneficiaries did not receive the program or attend a public school. The geographical RDD relies on the program enrolment to be strictly enforced at the border so that only eligible households on the treatment side can enrol and not households on the comparison side of the border. The data suggest that this criteria remains upheld because almost no one from the comparison areas enroled in the program. The first figure in Appendix C is a graph that shows no program enrolment in comparison households regardless of their distance to the border. The fiture also shows that only half of the households in pilot areas with children of eligible age enrolled in the program. There are a number of reasons why a household might not have enrolled including that their nearest second shift school was over-enrolled (we have anecdotal evidence that this occurred in a number of schools), they enrolled in a first shift school, they did not know about the program, or they were not interested in the pilot program. The limited enrolment in the program has some affects on the study which we discuss below and in the conclusion section.

**Impacts on School Enrolment**

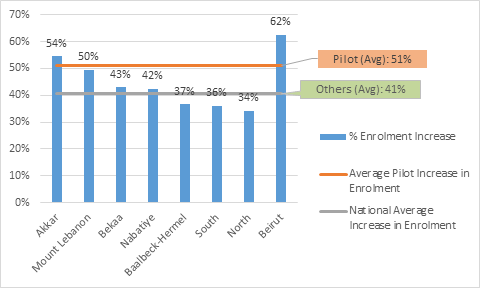
Aggregate MEHE administrative data suggest that formal school enrolment rates of displaced Syrian children increased rapidly across the country from the past (2015-2016) to the current school year. Figure 6.1 shows that average enrolment in second shift schools increased by 51 percent in NLG/Min Ila pilot areas compared to 41 percent in the rest of the country, potentially signalling an impact of the cash transfer program on enrolment outcomes.

The data collected for this study confirm that second shift and overall schooling rates increased substantively in both pilot and comparison areas. Tables 5.1 and 5.2 show enrolment for all children 5 to 14 years old by school type at baseline and follow-up for pilot and comparison governorates. Appendix B contains similar tables for females, males, children 5 to 9 years old, and children 10 to 14 years old. Second shift school enrolment rates increased from about 31 to 51 percent among children in the evaluation sample in treatment areas and from about 41 to 48 percent among children in the comparison areas. Overall school enrolment rates increased from nearly 60 percent at baseline to nearly 80 percent at follow-up in both pilot and comparison areas. School enrolment increases were particularly pronounced for children aged five to nine, whose self-reported school enrolment increased from slightly over 60 percent to nearly 90 percent. We find no differences in program effects between boys and girls.

We currently cannot confirm with certainty that the more pronounced average increase in second shift enrolment in pilot areas (vis-à-vis comparison areas) was driven by the cash transfer program alone. The reason is that, we do not observe a direct change in school enrolment rates when crossing the Akkar-North and Mt. Lebanon South borders at follow-up. School capacity constraints may play an important role. It appears that over half of the second shift schools in the study had reached full capacity during the registration phase, preventing children from enrolling. After the program started, nearly 50 percent of the children in the pilot areas enrolled in a second shift school with another 10 percent expected to enrol but seemingly unable due to capacity limits at their nearest second shift school. New second shifts were opened in response to the increased demand in *other* parts of the pilot governorates; however, this change was not captured by the study because the sample was selected from those households living in the vicinity of pre-existing second shift schools.

These findings are shown more formally using statistical methods in the appendices. Appendix C contains geographic RDD graphs for enrolment and Appendix E contains the regression results.

**Figure 5.1 MEHE Enrolment Increase From 2015 to 2016 School Years**



|  |  |  |  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- |
| **Table 5.1. School Enrolment Treatment governorates** | | | | | | | | |  | |  |
|  |  | Current School Type | | | | | | | |  | |
|  |  | Second shift | First Shift | | Private | Other | Out of school | **Total** | |  | |
| Baseline School Type | Second shift | 22.8% | 5.0% | | 0.9% | 0.0% | 1.8% | 30.6% | | 64.8% | |
| First Shift | 8.4% | 14.0% | | 0.7% | 0.5% | 1.5% | 25.1% | |
| Private | 2.0% | 0.8% | | 2.7% | 0.0% | 0.2% | 5.5% | |
| Other | 1.8% | 0.5% | | 0.6% | 0.1% | 0.6% | 3.6% | |
| Out of school | 15.5% | 3.4% | | 1.1% | 0.1% | 15.1% | 35.2% | |  | |
| **Total** | 50.5% | 23.6% | | 6.0% | 0.8% | 19.2% | 100.0% | |  | |
|  |  | 80.8% | | | | |  |  | |  | |
|  |  |  | |  |  |  |  |  | |  | |

|  |  |  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- |
| **Table 5.2. School Enrolment Comparison governorates** | | | | | | | | |  |  |
|  |  | Current School Type | | | | | |  |
|  |  | Second shift | First Shift | Private | Other | Out of school | **Total** |  |
| Baseline School Type | Second shift | 28.0% | 9.0% | 0.6% | 0.5% | 2.8% | 40.8% | 62.5% |
| First Shift | 1.6% | 7.8% | 0.3% | 0.1% | 0.7% | 10.4% |
| Private | 2.6% | 1.6% | 0.9% | 0.1% | 0.6% | 5.7% |
| Other | 1.5% | 2.4% | 0.5% | 0.2% | 0.9% | 5.5% |
| Out of school | 13.6% | 6.0% | 0.8% | 0.5% | 16.6% | 37.5% |  |
| **Total** | 47.3% | 26.8% | 3.2% | 1.3% | 21.5% | 100.0% |  |
|  |  | 78.5% | | | |  |  |  |

Impacts on Attendance

The program increased school attendance for children who were enrolled in a second shift school by 0.63 days per week on average, which translates into an additional month of attendance for children in the pilot compared to those not in the pilot. In other terms, children enrolled in a second shift school spent roughly 20 percent more time in school compared to similar children in comparison areas. As a result of the program, children 5 to 9 years old attended second shift school 0.7 more days per week and children 10 to 14 years old attended second shift school 0.5 more days per a week compared to similar children in comparison areas. Children receiving the NLG/”Min Ila” piloy program benefits attend school on average 4.1 days per week, which means that they are in school over 80 percent of the time possible. Table 5.3 shows the days attendend in the previous week (out of 5 days maximum) for pilot and comparison children ages 5 to 14 years old. The NLG/Min Ila pilot group consistently demonstrates higher attendance than the comparison children. As we do not have baseline statistics on attendance because we conducted baseline before the school year started we cannot say how these groups changed over time, only how they compare to each other at follow-up.

These findings are again shown more formally using statistical methods in the appendices. Appendix C contains geographic RDD graphs for attendance which shows a clear difference between the pilot and comparison groups at the border and for all distances away from the border. Appendix E contains the regression results.

**Table 5.3. Follow-Up Attendance Results (days in previous week out of 5)**

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| Group | Overall | Girls | Boys | Young (5-9) | Older (10-14) |
| Treatment | 4.33 | 4.3 | 4.3 | 4.3 | 4.4 |
| Comparison | 3.77 | 3.8 | 3.8 | 3.7 | 3.9 |

## VI. Conclusion

This report presents the impacts of the NLG/”Min Ila” pilot cash program on education outcomes (attendance and enrolment) after only a few months of implementation as a rapid follow-up to the baseline data collection. We find a large and significant impact on attendance that occurs for all child subgroups including age and gender. Improving school attendance for children is important because the benefits of schooling acrue with increased time in the classroom. Thus, the more a time a child spends in school the more they are likely to learn and benefit. We also investigated impacts on enrolment. Although we do not find impacts on child enrolment in second shift schools for either age group or gender, we find increased enrolment across the country for these categories, with improvements for both pilot and comparison groups. The limited supply of second shift schools in study areas and recently opened schools in the treatment area that were not located near study populations affect our findings.

### Reflections on Study and Program Implementation

This study was designed to capture program effects among children living in the vicinity of an active second shift school. The rationale was that these children could readily enrol in a second shift school in response to the program, allowing the impact evaluation to capture the impact of increasing the demand for education through a cash transfer program. However, over half of all second shift schools in the pilot areas of the study reached full capacity while registering children and had to turn away children who wanted to enroll. While MEHE was prepared to open new second shifts in existing schools to accommodate the increase, as in previous years, in some areas there were no additional schools in which to open new second shifts. This situation may have created a ceiling effect for the study because it is impossible for the program to increase enrolment above the capacity of the second shift schools. In other words, the program cannot demonstrate its full potential to generate enrolment effects due to the limit on spaces to enrol children in second shift schools. Due to the sample being selected from areas with existing schools, enrolment in newly opened second shifts not located near the sampled schools could not be captured. In this scenario, limited impacts on school enrolment do not necessarily reflect a limited impact on schooling outcomes. On the contrary, assuming that addressing the second shift school capacity constraints would increase school enrolment by an additional 10 percent, the potential for the program to increase both attendance and enrolment is substantive.

UNICEF contracted AIR for the baseline and rapid follow-up study. This report represents the result of that work. UNICEF will continue the evaluation with data collection at the beginning of the 2017-18 school year to measure the impact of the program after a full year of implementation, and to investigate the many domains measured at baseline such as food security, household income, and debt, as well as education and child labor.

## Appendix A. Identification Strategy: Geographical Regression Discontinuity Design

We identify the effect of the NLG/”Min Ila” cash pilot program using an RDD.[[16]](#footnote-17) RDDs can be used to identify program effects when programs are allocated based on an assignment variable. Well known RDD examples include allocation of scholarships and awards based on test scores and allocation of employment and health programs based on the age of the beneficiary. The intuition behind the RDD is that those who are just below the threshold to receive the program (e.g. those whose test score is just too low to get the scholarship or those who are just too young to get the health program) are very similar in all respects to those who are just above the threshold and therefore serve as a valid comparison group. RDDs rely on relatively “mild assumptions” to identify credible program impacts.[[17]](#footnote-18)

In our setting, distance to the pilot governorate border can be interpreted as the assignment variable. Households and children who live just outside the pilot governorate border are likely to be similar to those who live just inside the pilot border and can potentially serve as a credible comparison group.[[18]](#footnote-19)

The geographical RDD[[19]](#footnote-20), [[20]](#footnote-21) is valid when the following assumptions hold:

1. Program allocation is discontinuous at the border.
2. Outcomes, covariates and unobserved characteristics are continuous and similar in value at the border prior to program implementation.
3. Households and individuals do not adjust their location based on the transfer program.
4. The relationship between the assignment variable and the outcome variables is approximated accurately.

Assumption 1 holds as demonstrated by the program implementation graph in Appendix C. Assumption 2 holds as shown in both the baseline report (when discussing balance between T and C) and in this report when discussing how balance is maintained after attrition. Assumption 3 holds because comparison households at baseline did not move across the border at follow up as demonstrated by the attrition analysis. The regression specification is stated below to address Assumption 4.

### Regression Specification

The following regression specification was used to estimate the intent-to-treat effect of the program on the outcome variables of interest:

( 1 )

where is the outcome variable for child in household living in the vicinity of school in governorate at time is an indicator for receiving the transfer, is the assignment variable measuring distance to the border (negative outside the pilot districts and positive inside), is a vector of baseline covariates that include: child age, child gender, parental education, total income per capita. We cluster errors at the ‘cadaster cluster’ level to allow for correlation of the error term within a cluster, meaning for and in the same cadaster cluster.

The term deserves some additional explanation. The RDD in this context relies on households that are located along one of two borders: the Akkar-North border and the Mt. Lebanon-South border. The term represents a fixed effect to account for differences between households located near the two borders (i.e. takes the value 1 for the governorates Akkar and North and 0 otherwise). By including a border fixed effect in all estimating equations, we essentially treat the sample as though all households locate near a single border.

We examined the RDD’s sensitivity to functional form by running several different models. Linear models, higher order polynomial models, and models with interaction terms. Furthermore, the relationship between distance and outcomes may be different on each side of the border. We therefore allowed separate functional forms on each side of the border. Based on visual inspection of standard RDD graphs and the Akaike criterion we currently prefer to rely on a relatively simple linear functional form, the results of which we present below.

We conducted heterogeneity analyses to understand how the transfer affects people in different ways by adding interaction terms to Equation (1). An important dimension of heterogeneity is how the program affect boys versus girls and younger versus older children. However, it is important to be aware that our sample size for heterogeneity analysis is small so it may be impossible to detect the differential impact. The minimum detectable effect size will increase from 0.254 to 0.401.

## Appendix B. Enrollment Statistics by Gender and Age

**Table B.1. Enrolment Rates Treatment Governorates – Girls Only**

|  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
|  |  | Current School Type | | | | | |  |
|  |  | Second shift | First Shift | Private | Other | Out of school | **Total** |  |
| Baseline School Type | Second shift | 23.5% | 5.0% | 1.5% | 0.0% | 1.8% | 31.8% | 65.1% |
| First Shift | 7.7% | 14.2% | 0.5% | 0.5% | 1.7% | 24.6% |
| Private | 2.1% | 0.7% | 2.6% | 0.0% | 0.2% | 5.6% |
| Other | 1.6% | 0.7% | 0.2% | 0.1% | 0.6% | 3.2% |
| Out of school | 18.2% | 3.3% | 0.9% | 0.1% | 12.3% | 34.9% |  |
| **Total** | 53.1% | 23.9% | 5.7% | 0.7% | 16.6% | 100.0% |  |
|  |  | 83.4% | | | |  |  |  |

**Table B.2. Enrolment Rates Comparison Governorates – Girls Only**

|  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
|  |  | Current School Type | | | | | |  |
|  |  | Second shift | First Shift | Private | Other | Out of school | **Total** |  |
| Baseline School Type | Second shift | 29.1% | 8.7% | 0.6% | 0.3% | 2.8% | 41.3% | 62.0% |
| First Shift | 1.7% | 7.7% | 0.1% | 0.1% | 0.7% | 10.3% |
| Private | 2.3% | 1.4% | 1.2% | 0.0% | 0.7% | 5.6% |
| Other | 1.1% | 2.2% | 0.7% | 0.1% | 0.6% | 4.7% |
| Out of school | 13.4% | 6.9% | 1.4% | 0.4% | 16.0% | 38.0% |  |
| **Total** | 47.5% | 26.9% | 4.0% | 1.0% | 20.7% | 100.0% |  |
|  |  | 79.3% | | | |  |  |  |

**Table B.3. Enrolment Rates Treatment Governorates – Boys Only**

|  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
|  |  | Current School Type | | | | | |  |
|  |  | Second shift | First Shift | Private | Other | Out of school | **Total** |  |
| Baseline School Type | Second shift | 22.2% | 5.0% | 0.4% | 0.0% | 1.9% | 29.4% | 64.5% |
| First Shift | 9.0% | 13.7% | 0.8% | 0.6% | 1.3% | 25.5% |
| Private | 1.9% | 0.8% | 2.7% | 0.0% | 0.1% | 5.5% |
| Other | 2.0% | 0.4% | 0.9% | 0.1% | 0.6% | 4.0% |
| Out of school | 13.1% | 3.4% | 1.3% | 0.1% | 17.6% | 35.5% |  |
| **Total** | 48.1% | 23.3% | 6.2% | 0.8% | 21.5% | 100.0% |  |
|  |  | 78.5% | | | |  |  |  |

**Table B.4. Enrolment Rates Comparison Governorates – Boys Only**

|  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
|  |  | Current School Type | | | | | |  |
|  |  | Second shift | First Shift | Private | Other | Out of school | **Total** |  |
| Baseline School Type | Second shift | 27.0% | 9.3% | 0.7% | 0.7% | 2.8% | 40.4% | 63.1% |
| First Shift | 1.6% | 7.8% | 0.4% | 0.0% | 0.7% | 10.5% |
| Private | 2.8% | 1.7% | 0.7% | 0.1% | 0.5% | 5.8% |
| Other | 1.9% | 2.7% | 0.4% | 0.3% | 1.2% | 6.4% |
| Out of school | 13.8% | 5.2% | 0.3% | 0.5% | 17.1% | 36.9% |  |
| **Total** | 47.0% | 26.7% | 2.4% | 1.6% | 22.3% | 100.0% |  |
|  |  | 77.7% | | | |  |  |  |

**Table B.5. Enrolment Rates Treatment Governorates – Young Children (5-9 years)**

|  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
|  |  | Current School Type | | | | | |  |
|  |  | Second shift | First Shift | Private | Other | Out of school | **Total** |  |
| Baseline School Type | Second shift | 24.4% | 5.1% | 1.1% | 0.0% | 1.4% | 32.0% | 64.8% |
| First Shift | 7.1% | 15.0% | 0.7% | 0.5% | 0.5% | 23.7% |
| Private | 2.3% | 0.9% | 2.6% | 0.0% | 0.2% | 5.9% |
| Other | 1.3% | 0.6% | 0.8% | 0.1% | 0.4% | 3.1% |
| Out of school | 19.8% | 4.5% | 1.6% | 0.2% | 9.2% | 35.2% |  |
| **Total** | 54.8% | 26.1% | 6.7% | 0.8% | 11.6% | 100.0% |  |
|  |  | 88.4% | | | |  |  |  |

**Table B.6. Enrolment Rates Comparison Governorates – Young Children (5-9 years)**

|  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
|  |  | Current School Type | | | | | |  |
|  |  | Second shift | First Shift | Private | Other | Out of school | **Total** |  |
| Baseline School Type | Second shift | 29.8% | 9.8% | 0.7% | 0.6% | 1.2% | 42.1% | 65.0% |
| First Shift | 1.9% | 9.6% | 0.5% | 0.1% | 0.1% | 12.1% |
| Private | 2.4% | 1.7% | 1.2% | 0.0% | 0.2% | 5.6% |
| Other | 1.4% | 2.4% | 0.7% | 0.1% | 0.6% | 5.2% |
| Out of school | 17.1% | 7.6% | 1.3% | 0.6% | 8.4% | 35.0% |  |
| **Total** | 52.7% | 31.1% | 4.3% | 1.4% | 10.5% | 100.0% |  |
|  |  | 89.5% | | | |  |  |  |

**Table B.7. Enrolment Rates Treatment Governorates – Older Children (10-14 years)**

|  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
|  |  | Current School Type | | | | | |  |
|  |  | Second shift | First Shift | Private | Other | Out of school | **Total** |  |
| Baseline School Type | Second shift | 20.8% | 4.8% | 0.7% | 0.0% | 2.4% | 28.8% | 64.8% |
| First Shift | 10.0% | 12.8% | 0.6% | 0.6% | 2.8% | 26.7% |
| Private | 1.6% | 0.6% | 2.8% | 0.0% | 0.1% | 5.1% |
| Other | 2.4% | 0.5% | 0.4% | 0.1% | 0.8% | 4.2% |
| Out of school | 10.4% | 1.9% | 0.6% | 0.0% | 22.3% | 35.2% |  |
| **Total** | 45.2% | 20.6% | 5.1% | 0.7% | 28.4% | 100.0% |  |
|  |  | 71.6% | | | |  |  |  |

**Table B.8. Enrolment Rates Comparison Governorates – Older Children (10-14 years)**

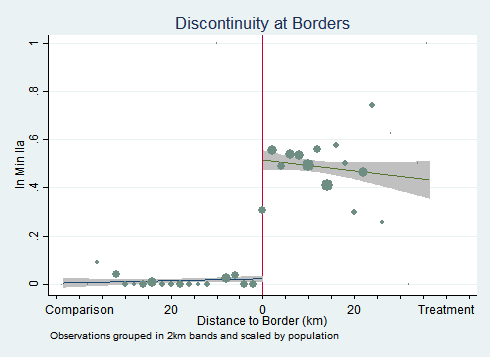
|  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
|  |  | Current School Type | | | | | |  |
|  |  | Second shift | First Shift | Private | Other | Out of school | **Total** |  |
| Baseline School Type | Second shift | 25.4% | 7.9% | 0.5% | 0.3% | 5.0% | 39.1% | 59.1% |
| First Shift | 1.3% | 5.3% | 0.0% | 0.0% | 1.4% | 8.1% |
| Private | 2.7% | 1.3% | 0.6% | 0.2% | 1.1% | 6.0% |
| Other | 1.6% | 2.4% | 0.3% | 0.3% | 1.3% | 6.0% |
| Out of school | 8.7% | 3.9% | 0.2% | 0.3% | 27.9% | 40.9% |  |
| **Total** | 39.8% | 20.8% | 1.6% | 1.1% | 36.7% | 100.0% |  |
|  |  | 63.3% | | | |  |  |  |

## Appendix C. RDD Graphs for Program Implementation, Enrolment, and Attendance

This section contains RDD graphs referenced in the report. RDD graphs are a way to visualize the data given the study design. Recall that the geographic regression discontinuity design of this study compares households on one side of a border to holds of similar distance to the other side of the same border. Only households on one of the sides of the border are eligible to receive the program, thus the treatment and comparison groups are separated by the border. The graphs in this section can be viewed partially as a map. Dots represent clusters of households with bigger dots representing more households. The red line represents the geographic border, so the further a dot is from the red line, the further it is from the border, regardless of the vertical distance between the dot and the X axis (bottom of the graph). The vertical distance between the dot and the bottom of the graph represents the score for that household for the variable presented in the graph, as measured by the Y axis (vertical line).

At baseline, the dots on the left and right side of the border (red line) should be aligned in a mirror image of themselves with the left side looking exactly like the right side, but flipped at the red line. This configuration means that there is no difference between treatment and comparison households. If the program has an impact, then at follow-up we will see a shift between the treatment and comparison groups at the border and the two lines might also have different slopes. The distance between the two lines (the shift) represents the impact of the program.

The first graph in this section Figure C.1 depicts program enrolment. The vertical line represents the proportion of households in a cluster that enrolled in the program. The red line in the middle of the graph represents the border. The dots to the left of the graph represent households in comparison governorates. Notice how all of the dots are at the 0 line for proportion of households that receive the program. This means that households in comparison governorates did not enroll in the program which makes sense because the program was not offered where they live. Now look to the right side of the red line and notice how most of the dots are half way up the chart and sitting around 0.5. This means that roughly half of the households enrolled in the program. All of the households on the right side of the red line had the potential to enroll, but as discussed in the report, there are several reasons why a household who lived in a treatment area might not enroll in the program. The biggest take away from this graph though is that the RDD assumption that there is a discontinuous program implementation at the border holds because only households to the right of the border received the program.

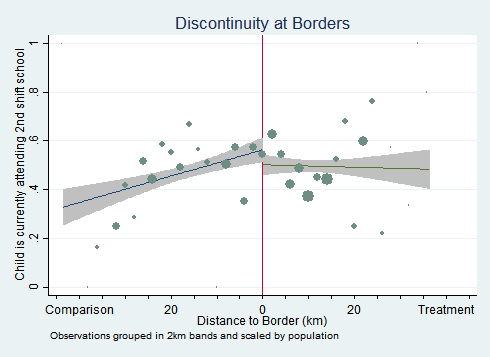
**Figure C.1. Program Implementation**

**Note.** The horizontal axis represents distance (in km) to the border separating the pilot governorates (Akkar and Mt. Lebanon) from the comparison governorates (North Lebanon, South Lebanon, and El Nabatieh). The vertical red line represents the border itself. The vertical axis measures the likelihood that a child is enrolled in school during the rapid follow-up interview. Dots represent local averages for clusters of households. Bigger dots represent more households. Linear OLS regression lines were fitted to the left and the right of the border. Grey areas represent the 95% confidence interval around the regression line.

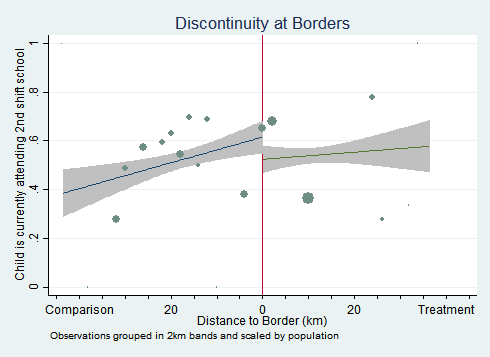
Figure C.2 shows enrolment in second shift schools at follow-up. The grey band around each regression line represents the 95% confidence interval. The grey bands to the left of the center red line overlap with the grey bands to the right of the center red line, thus we cannot claim a statistically significant difference between the the two sides. Figure C.3 and C.4 also show second shift enrolment at follow up for the treatment (right side) and comparison (left side) but disaggregate the data by age for younger and older children. Similar to C.2, the grey shaded areas overlap at the center of the graph meaning that there is not a statistically significant different in enrolment between the treatment and comparison groups. The next section shows these same graphs but includes the baseline measures too.

Figures C.5-C.9 show attendance in second shift schools for the treatment and comparison groups including disaggregated analysis for age and gender. Notice that the grey shaded areas to the left and the right of the center red line do not overlap with each other. These graphs indicate a statistically significant difference between the treatment and comparison group because the confidence intervals for both groups do not overlap with each other. We do not have attendance data at baseline since those data were collected about the previous school year, so there are not attendance baseline graphs in the next section.

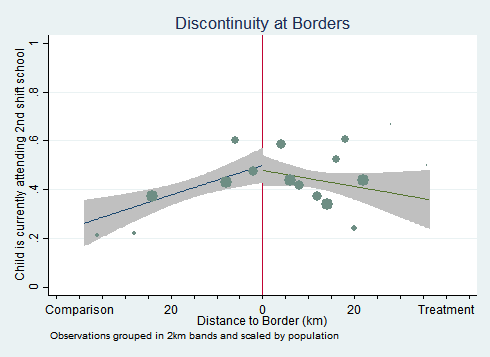
**Figure C.2. Follow-up Enrollment**



**Figure C.3. Children Ages 5-9 Years Old Follow-up Enrollment**

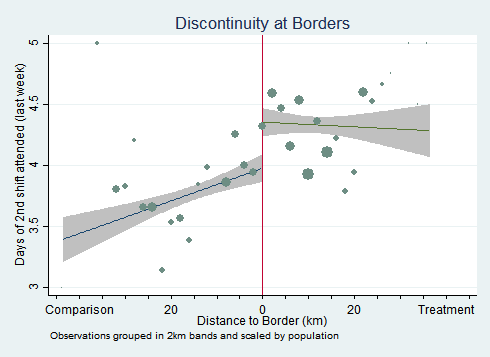


**Figure C.4. Children Ages 10-14 Years Old Follow-up Enrollment**

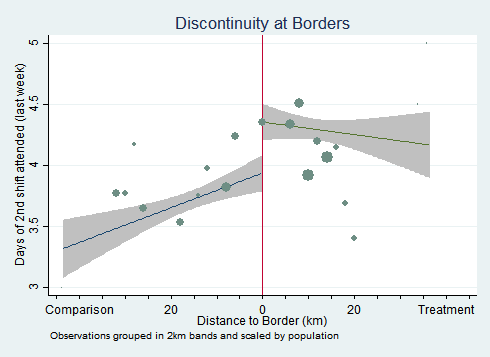


**Note.** The horizontal axis represents distance (in km) to the border separating the pilot governorates (Akkar and Mt. Lebanon) from the comparison governorates (North Lebanon, South Lebanon, and El Nabatieh). The vertical red line represents the border itself. The vertical axis measures the number of days of school attendance in the week prior to the rapid follow-up interview by children attending second shift schools. Dots represent local averages for clusters of households. Bigger dots represent more households. Linear OLS regression lines were fitted to the left and the right of the border. Grey areas represent the 95% confidence interval around the regression lin\e.

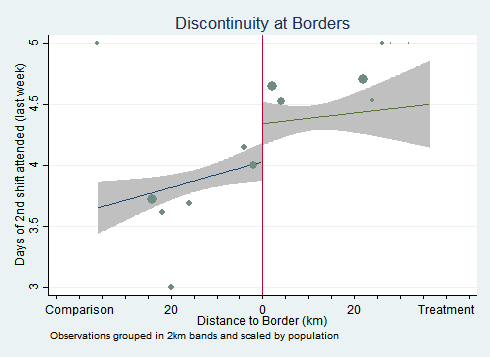
**Figure C.5. Follow-up Attendance for All Children (5-14 Years Old)**



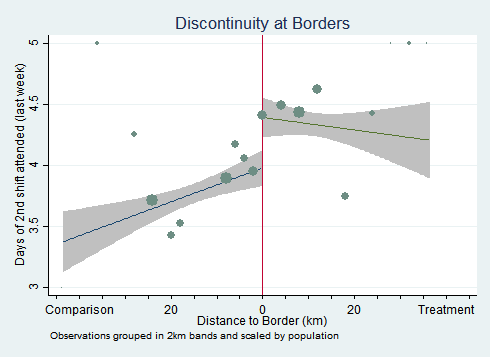
**Figure C.6. Children Ages 5-9 Years Old Follow-up Attendance**



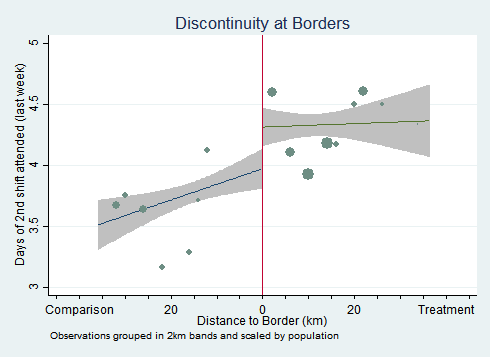
**Figure C.7. Children Ages 10-14 Years Old Follow-up Attendance**



**Figure C.8. Boys Follow-up Attendance**



**Figure C.9. Girls Follow-up Attendance**



## Appendix D. Baseline equivalence graphs

This report’s analysis combines a geographical regression discontinuity design with so-called difference in differences. This estimation procedure entails comparing the Baseline difference between pilot and comparison at the border to the Endline difference in pilot and comparison at the border. We compare the outcomes at Baseline and Endline for several of the important outcomes: enrolment, cost-related dropout, and school expenditures. We do not discuss attendance here because it could not be collected at Baseline, which occurred during the summer holiday. This appendix discusses the Baseline relationship for key outcomes at the governorate border and the Endline relationship at the governorate border.

We find that there is an average increase in the number of children enrolled in 2nd shift schools in pilot governorates, but cannot attribute the change to Min Ila. The reason is that changes in 2nd shift enrollment occur on both sides of the border. Confirming Table 5.1 and 5.2, Figure D.1 shows that average 2nd shift school enrollment increased substantively in pilot governorates from Baseline (flat red line) to Endline (flat blue line). Average 2nd shift school enrollment remained constant in comparison areas. However at Baseline (downward sloping red line) the enrollment was lower near the border and higher further from the border. At Endline (upward sloping blue line), the enrolment is higher nearer to the border and lower further from the border. The key is that the pilot and comparison households at the border are similar at Baseline (red lines) and again similar at Endline (blue lines). Our methodology relies on discontinuous changes at the border so this observation drives our null result.

Figure D.1: Overall Baseline and Endline enrolment



Figures D.2 and D.3 break show that 2nd shift enrollment for girls and for boys both follow the same trend as the overall sample of children. Both boys and girls experienced an unambiguous increased enrolment in pilot areas. Both boys and girls experienced increased enrolment near the border and decreased enrollment further from the border for little change on average.

Figure D.2: Baseline and Endline enrolment for girls

Figure D.3: Baseline and Endline enrolment for boys



There is a similar trend in 2nd shift enrollment for younger and older children. Both groups experience increased enrollment in pilot areas. Both younger and older children nearer to the border in comparison areas saw increased enrolment while those further from the border saw decreased enrolment.

Figure D.4: Baseline and Endline enrolment for younger children (5-9 years)

Figure D.5: Baseline and Endline enrolment for older children (10-14 years)



## Appendix E. Regression Results

The table below provides the results from the regressions that analyze the impact of the program on the two primary outcomes enrolment and attendance. Model (1) investigates the impact of the program on enrolment in a second shift school using a differences in difference analysis in the RDD framework to compare treatment and comparison children while controlling for the distance to the matched pair border (plus age, gender, parents education and household income). The model compares the change between baseline and follow-up between the treatment and comparison groups. The model interacts the differences in differences indicator with distance to the border to account for the RDD framework. Based on this model, there is no impact of the program on enrolment in a second shift school.

Model (2) investigates the impact of the program on days of attendance in a second shift school using a single difference analysis in an RDD framework and controlling for age, gender, parents education and household income. The Treatment variable represents the effect of the program for this model which indicates 0.63 more days of school per week for the treatment group. The three stars mean that it is statistically significant at the p<0.01 value.

**Table E.1. Regression Results**

|  |  |  |
| --- | --- | --- |
| **Dependent Variables** | **(1)** | **(2)** |
| **Child is currently attending second shift school** | **Days of second shift attended (last week)** |
| Treatment | -0.009 | 0.630\*\*\* |
|  | (0.090) | (0.176) |
| Post | 0.231\*\*\* |  |
|  | (0.065) |  |
| Treatment x Post | 0.002 |  |
|  | (0.092) |  |
| Distance to border x Post | 0.009\*\*\* |  |
|  | (0.003) |  |
| Treatment x Post x Distance to border | -0.010\* |  |
|  | (0.006) |  |
| Distance to border x Treatment | 0.001 |  |
|  | (0.005) |  |
| Distance to Border | -0.003 | -0.002 |
|  | (0.004) | (0.005) |
| Border dummy | 0.020 | 0.462\*\*\* |
|  | (0.041) | (0.127) |
| Age | 0.220\*\*\* | 0.081 |
|  | (0.023) | (0.063) |
| Age squared | -0.012\*\*\* | -0.003 |
|  | (0.001) | (0.003) |
| Female | 0.021 | -0.003 |
|  | (0.014) | (0.037) |
| Parent(s) studied beyond primary | 0.014 | 0.109\* |
|  | (0.023) | (0.056) |
| Total income PC USD (past 30 days) | 0.001\*\* | -0.002 |
|  | (0.000) | (0.001) |
| N | 5757 | 2870 |
| R-sqr | 0.076 | 0.189 |
| F | 17.3 | 5.5 |
| \* p<0.10, \*\* p<0.05, \*\*\* p<0.01 |  |  |

## Appendix F. Attrition Tables

**Table F.1. Differential Attrition – Household Demographic Characteristics**

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
|  | Control | | Treatment | | Balance Test | | |
| Variables | Mean | N1 | Mean | N2 | Diff | SE | p-value |
| Household Size | 6.05 | 625 | 6.29 | 774 | 0.07 | 0.21 | 0.73 |
| % Elderly (65+) | 0.01 | 3,777 | 0.01 | 4,862 | 0.00 | 0.00 | 0.51 |
| Children (0-14) per household | 3.36 | 625 | 3.49 | 774 | 0.14 | 0.16 | 0.37 |
| Young children (5-9) per household | 1.44 | 625 | 1.45 | 774 | 0.05 | 0.09 | 0.57 |
| Older children (10-14) per household | 1.04 | 625 | 1.20 | 774 | 0.13 | 0.13 | 0.32 |
| Large household | 0.34 | 625 | 0.41 | 774 | 0.08 | 0.05 | 0.08 |
| Time Displaced (years) | 4.73 | 623 | 4.69 | 773 | 0.43 | 0.55 | 0.44 |
| Distance to border (km) | 19.19 | 625 | 11.27 | 774 | 5.85 | 3.97 | 0.14 |
| Single Parent Household | 0.09 | 597 | 0.07 | 723 | 0.01 | 0.03 | 0.76 |
| Child (0-17) with mother deceased | 0.01 | 2,293 | 0.01 | 2,927 | 0.01 | 0.01 | 0.43 |
| Child (0-17) with father deceased | 0.02 | 2,265 | 0.03 | 2,914 | 0.00 | 0.01 | 0.95 |

Notes: Standard errors are robust to heteroscedasticity and clustered at the 'cadaster cluster' level.

**Table F.2. Differential Attrition – Housing**

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
|  | Control | | Treatment | | Balance Test | | |
| Variables | Mean | N1 | Mean | N2 | Diff | SE | p-value |
| Dwell crowded location | 0.46 | 625 | 0.32 | 774 | -0.07 | 0.09 | 0.39 |
| Dwell poor conditions | 0.11 | 625 | 0.10 | 774 | -0.06 | 0.07 | 0.39 |
| Dwell physical dangers | 0.04 | 625 | 0.03 | 774 | -0.04 | 0.03 | 0.21 |
| Dwell dangerous conditions | 0.03 | 625 | 0.05 | 774 | 0.03 | 0.03 | 0.18 |
| Dwell urgent repairs | 0.06 | 625 | 0.11 | 774 | 0.02 | 0.04 | 0.62 |
| Legal/Electric Energy Source | 0.83 | 623 | 0.92 | 772 | 0.05 | 0.06 | 0.44 |
| Number of rooms | 2.47 | 625 | 2.46 | 774 | -0.19 | 0.19 | 0.32 |
| Rooms (Lg Household) | 2.51 | 542 | 2.48 | 698 | -0.21 | 0.20 | 0.29 |
| Rooms (Sm Household) | 2.45 | 595 | 2.49 | 723 | -0.13 | 0.18 | 0.47 |
| Access to pumped water | 0.40 | 625 | 0.37 | 774 | -0.05 | 0.08 | 0.51 |
| Access to toilet/latrine | 1.00 | 625 | 0.99 | 774 | 0.01 | 0.01 | 0.12 |
| Toilet | 0.57 | 625 | 0.48 | 774 | -0.14 | 0.08 | 0.07 |
| Latrine | 0.42 | 625 | 0.52 | 774 | 0.15 | 0.08 | 0.05 |

Notes: Standard errors are robust to heteroscedasticity and clustered at the 'cadaster cluster' level.

**Table F.3. Differential Attrition – Economic Wellbeing**

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
|  | Control | | Treatment | | Balance Test | | |
| Variables | Mean | N1 | Mean | N2 | Diff | SE | p-value |
| Household cash earnings PC USD (past 7 days) | 10.05 | 625 | 8.22 | 774 | -4.32 | 1.80 | 0.02 |
| Household in-kind earnings PC USD (past 7 days) | 0.48 | 625 | 1.65 | 774 | 0.67 | 0.46 | 0.15 |
| Total Income USD (past 30 days) | 339.66 | 625 | 307.94 | 774 | -35.8 | 38.62 | 0.36 |
| Income $ (Lg Household) | 448.37 | 214 | 355.15 | 321 | -2.13 | 51.00 | 0.97 |
| Income $ (Sm Household) | 283.07 | 411 | 274.48 | 453 | -72.1 | 34.17 | 0.04 |
| Total Income PC USD (past 30 days) | 56.33 | 625 | 50.77 | 774 | -6.74 | 5.71 | 0.24 |
| Income PC $ (Lg Household) | 57.36 | 214 | 45.10 | 321 | 0.85 | 6.34 | 0.89 |
| Income PC $ (Sm Household) | 55.79 | 411 | 54.78 | 453 | -10.9 | 6.46 | 0.10 |
| Total debt PC USD (past 30 days) | 24.04 | 625 | 26.60 | 774 | 0.06 | 4.77 | 0.99 |
| Debt PC $ (Lg Household) | 15.87 | 214 | 20.10 | 321 | -0.58 | 4.50 | 0.90 |
| Debt PC $ (Sm Household) | 28.30 | 411 | 31.21 | 453 | 1.91 | 5.91 | 0.75 |
| Total Debt PC USD | 32.44 | 625 | 50.02 | 774 | 19.83 | 9.22 | 0.03 |
| Debt PC $ (Lg Household) | 25.99 | 214 | 38.36 | 321 | 9.85 | 10.55 | 0.35 |
| Debt PC $ (Sm Household) | 35.80 | 411 | 58.27 | 453 | 27.93 | 10.29 | 0.01 |
| Not enough food | 0.32 | 624 | 0.52 | 774 | 0.10 | 0.08 | 0.19 |
| Not enough food (Lg Household) | 0.30 | 541 | 0.51 | 698 | 0.11 | 0.08 | 0.19 |
| Not enough food (Sm Household) | 0.32 | 594 | 0.50 | 723 | 0.07 | 0.08 | 0.35 |
| Adult spent day without food | 0.03 | 624 | 0.10 | 774 | 0.01 | 0.03 | 0.85 |
| Adult without food (Lg Household) | 0.03 | 541 | 0.10 | 698 | -0.01 | 0.04 | 0.87 |
| Adult without food (Sm Household) | 0.03 | 594 | 0.10 | 723 | 0.00 | 0.03 | 0.91 |
| Children skipped meal | 0.03 | 624 | 0.13 | 774 | 0.01 | 0.03 | 0.64 |
| Child skipped meal (Lg Household) | 0.03 | 541 | 0.13 | 698 | 0.00 | 0.03 | 0.92 |
| Child skipped meal (Sm Household) | 0.03 | 594 | 0.14 | 723 | 0.01 | 0.03 | 0.66 |
| Any Assistance | 0.98 | 622 | 0.99 | 716 | -0.00 | 0.01 | 0.90 |
| MPCA/LCC Assistance | 0.35 | 622 | 0.39 | 716 | -0.00 | 0.07 | 1.00 |
| WFP Assistance | 0.91 | 622 | 0.91 | 716 | -0.00 | 0.03 | 0.89 |
| UNICEF Winter | 0.11 | 622 | 0.17 | 716 | 0.03 | 0.11 | 0.75 |
| UNHCR Winter ($147) | 0.56 | 622 | 0.61 | 716 | -0.01 | 0.06 | 0.85 |
| UNHCR Winter ($75) | 0.17 | 622 | 0.24 | 716 | 0.10 | 0.05 | 0.07 |

Notes: Standard errors are robust to heteroscedasticity and clustered at the 'cadaster cluster' level.

**Table F.4. Differential Attrition – Adults' Characteristics (Age 17+)**

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
|  | Control | | Treatment | | Balance Test | | |
| Variables | Mean | N1 | Mean | N2 | Diff | SE | p-value |
| Female | 0.51 | 3,442 | 0.51 | 4,402 | 0.01 | 0.02 | 0.79 |
| Age | 32.85 | 3,442 | 33.11 | 4,390 | 0.55 | 0.52 | 0.30 |
| Married | 0.70 | 3,412 | 0.69 | 4,326 | 0.01 | 0.03 | 0.64 |
| Attended middle-school | 0.46 | 2,956 | 0.45 | 3,663 | 0.07 | 0.05 | 0.19 |
| Male grad | 0.47 | 1,508 | 0.44 | 1,918 | 0.15 | 0.05 | 0.01 |
| Female grad | 0.45 | 1,448 | 0.46 | 1,745 | 0.00 | 0.07 | 0.95 |
| Employed (last 7 days) | 0.25 | 3,440 | 0.25 | 3,872 | 0.05 | 0.04 | 0.24 |
| Hours working (last 7 days) | 35.49 | 843 | 28.43 | 955 | 5.75 | 2.95 | 0.06 |
| Employed (in Syria) | 0.34 | 2,855 | 0.36 | 3,664 | 0.04 | 0.04 | 0.33 |
| HH Head: High Skill Job in Syria | 0.10 | 849 | 0.15 | 1,083 | 0.09 | 0.05 | 0.09 |
| Good Health | 0.81 | 3,430 | 0.64 | 4,362 | 0.07 | 0.06 | 0.20 |
| Able to walk | 0.63 | 3,430 | 0.51 | 4,362 | 0.05 | 0.07 | 0.46 |
| Able to sweep | 0.64 | 3,430 | 0.60 | 4,356 | 0.01 | 0.06 | 0.84 |

Notes: Standard errors are robust to heteroscedasticity and clustered at the 'cadaster cluster' level.

**Table F.5. Differential Attrition – Younger Children's Characteristics (Age 5-9)**

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
|  | Control | | Treatment | | Balance Test | | |
| Variables | Mean | N1 | Mean | N2 | Diff | SE | p-value |
| Female | 0.50 | 901 | 0.48 | 1,119 | 0.02 | 0.04 | 0.63 |
| Age | 7.01 | 901 | 7.07 | 1,119 | -0.01 | 0.09 | 0.89 |
| Orphaned | 0.00 | 901 | 0.00 | 1,119 | -0.00 | 0.00 | 0.34 |
| Attended school 2015-16 | 0.66 | 874 | 0.67 | 1,083 | -0.01 | 0.06 | 0.91 |
| Boys In School | 0.68 | 438 | 0.69 | 559 | -0.03 | 0.07 | 0.71 |
| Girls In School | 0.63 | 436 | 0.64 | 524 | 0.02 | 0.07 | 0.80 |
| In School (Lg Household) | 0.59 | 363 | 0.62 | 515 | -0.08 | 0.08 | 0.34 |
| In School (Sm Household) | 0.70 | 511 | 0.72 | 568 | 0.06 | 0.07 | 0.41 |
| Out of school due to cost | 0.52 | 301 | 0.42 | 359 | -0.07 | 0.09 | 0.49 |
| Boys out of school | 0.51 | 140 | 0.41 | 171 | -0.14 | 0.11 | 0.22 |
| Girls out of school | 0.52 | 161 | 0.43 | 188 | -0.01 | 0.11 | 0.95 |
| Out of school (Lg Household) | 0.51 | 148 | 0.42 | 198 | -0.07 | 0.12 | 0.58 |
| Out of school (Sm Household) | 0.52 | 153 | 0.41 | 161 | -0.05 | 0.13 | 0.67 |
| Time to school (min) | 20.46 | 571 | 21.99 | 723 | 2.13 | 1.91 | 0.27 |
| Education Expenses | 95.75 | 600 | 91.45 | 761 | -19.13 | 17.42 | 0.28 |
| Education Expenses (Lg Household) | 104.32 | 225 | 82.43 | 336 | -31.71 | 30.31 | 0.30 |
| Education Expenses (Sm Household) | 90.61 | 375 | 98.59 | 425 | -9.26 | 16.70 | 0.58 |
| Child labor | 0.00 | 877 | 0.00 | 947 | 0.00 | 0.00 | 0.45 |
| Child labor (Lg Household) | 0.00 | 366 | 0.01 | 451 | 0.01 | 0.01 | 0.30 |
| Child labor (Sm Household) | 0.00 | 511 | 0.00 | 496 | -0.00 | 0.00 | 0.34 |
| Working & Out of School | 0.00 | 315 | 0.01 | 311 | 0.02 | 0.03 | 0.36 |
| Working & In School | 0.00 | 583 | 0.01 | 660 | 0.02 | 0.02 | 0.21 |
| Time collecting firewood/water (min) | 35.50 | 877 | 38.30 | 1,054 | 17.21 | 13.07 | 0.19 |
| Firewood (Lg Household) | 14.22 | 366 | 24.99 | 497 | 14.75 | 10.32 | 0.16 |
| Firewood (Sm Household) | 90.61 | 375 | 98.59 | 425 | -9.26 | 16.70 | 0.58 |
| Time doing chores (min) | 134.66 | 877 | 36.94 | 1,055 | -87.84 | 30.67 | 0.01 |
| Job, carries heavy loads | 0.00 | 236 | 0.01 | 257 | 0.01 | 0.02 | 0.35 |
| Job, works with dangerous tools | 0.00 | 235 | 0.01 | 255 | 0.01 | 0.01 | 0.37 |
| Job, exposed to fumes | 0.01 | 236 | 0.00 | 256 | 0.00 | 0.03 | 0.85 |
| Job, exposed to extreme temperature | 0.03 | 233 | 0.01 | 257 | -0.06 | 0.04 | 0.17 |
| Job, exposed to loud noise/vibrations | 0.03 | 236 | 0.00 | 258 | -0.08 | 0.04 | 0.07 |
| Job, exposed to bullying/violence | 0.00 | 236 | 0.01 | 258 | -0.00 | 0.01 | 0.64 |
| Shoes | 0.90 | 873 | 0.92 | 1,090 | 0.10 | 0.05 | 0.04 |
| Winter Clothes | 0.81 | 873 | 0.80 | 1,090 | 0.10 | 0.06 | 0.08 |

Notes: Standard errors are robust to heteroscedasticity and clustered at the 'cadaster cluster' level.

**Table F.6. Differential Attrition – Older Children's Characteristics (Age 10-14)**

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
|  | Control | | Treatment | | Balance Test | | |
| Variables | Mean | N1 | Mean | N2 | Diff | SE | p-value |
| Female | 0.47 | 653 | 0.46 | 926 | -0.03 | 0.04 | 0.49 |
| Age | 11.96 | 653 | 11.85 | 926 | -0.14 | 0.09 | 0.12 |
| Orphaned | 0.01 | 653 | 0.00 | 926 | -0.01 | 0.01 | 0.42 |
| % Child Marriage | 0.00 | 643 | 0.00 | 909 | 0.00 | 0.00 | 0.52 |
| Literate | 0.77 | 651 | 0.77 | 910 | -0.11 | 0.07 | 0.09 |
| Boys Literacy | 0.77 | 345 | 0.75 | 494 | -0.13 | 0.07 | 0.08 |
| Girls Literacy | 0.77 | 306 | 0.80 | 416 | -0.10 | 0.08 | 0.21 |
| Attended school 2015-16 | 0.60 | 634 | 0.66 | 895 | 0.09 | 0.08 | 0.24 |
| Boys In School | 0.59 | 335 | 0.63 | 484 | 0.10 | 0.08 | 0.25 |
| Girls In School | 0.61 | 299 | 0.71 | 411 | 0.09 | 0.08 | 0.30 |
| Out of school due to cost | 0.64 | 256 | 0.62 | 302 | 0.02 | 0.12 | 0.87 |
| Boys out of school | 0.62 | 140 | 0.61 | 179 | -0.01 | 0.14 | 0.94 |
| Girls out of school | 0.67 | 116 | 0.63 | 123 | 0.06 | 0.13 | 0.63 |
| Time to school (min) | 20.61 | 379 | 21.35 | 594 | 1.67 | 2.54 | 0.51 |
| Education Expenses | 85.30 | 398 | 85.88 | 626 | 36.24 | 22.20 | 0.11 |
| Education Expenses (Lg Household) | 100.68 | 201 | 81.97 | 363 | 49.05 | 30.63 | 0.11 |
| Education Expenses (Sm Household) | 69.62 | 197 | 91.28 | 263 | 21.40 | 18.78 | 0.26 |
| Child labor | 0.06 | 649 | 0.05 | 806 | -0.04 | 0.02 | 0.07 |
| Child labor (Lg Household) | 0.06 | 344 | 0.04 | 506 | -0.03 | 0.03 | 0.32 |
| Child labor (Sm Household) | 0.05 | 305 | 0.07 | 300 | -0.04 | 0.03 | 0.11 |
| Working & Out of School | 0.08 | 272 | 0.11 | 285 | -0.03 | 0.04 | 0.40 |
| Working & In School | 0.04 | 394 | 0.03 | 543 | -0.01 | 0.03 | 0.60 |
| Hours working | 33.22 | 32 | 19.79 | 38 | -5.67 | 8.80 | 0.52 |
| Time collecting firewood/water (min) | 38.53 | 649 | 44.16 | 894 | 15.43 | 12.68 | 0.23 |
| Firewood (Lg Household) | 36.86 | 344 | 36.16 | 562 | 6.10 | 16.49 | 0.71 |
| Firewood (Sm Household) | 69.62 | 197 | 91.28 | 263 | 21.40 | 18.78 | 0.26 |
| Time providing care (min) | 125.48 | 649 | 62.67 | 894 | 24.65 | 27.56 | 0.37 |
| Care (Lg Household) | 91.26 | 344 | 50.65 | 562 | 20.29 | 22.89 | 0.38 |
| Care (Sm Household) | 164.08 | 305 | 83.03 | 332 | 15.54 | 43.10 | 0.72 |
| Time doing chores (min) | 133.85 | 649 | 50.28 | 894 | 51.24 | 29.76 | 0.09 |
| Chores (Lg Household) | 106.41 | 344 | 43.93 | 562 | 46.88 | 30.23 | 0.13 |
| Chores (Sm Household) | 164.80 | 305 | 61.02 | 332 | 46.28 | 42.84 | 0.28 |
| Job, carries heavy loads | 0.09 | 215 | 0.08 | 236 | -0.01 | 0.04 | 0.86 |
| Job, works with dangerous tools | 0.09 | 213 | 0.05 | 239 | 0.05 | 0.05 | 0.35 |
| Job, exposed to fumes | 0.08 | 214 | 0.07 | 235 | 0.06 | 0.06 | 0.34 |
| Job, exposed to extreme temperature | 0.12 | 214 | 0.09 | 239 | -0.03 | 0.04 | 0.42 |
| Job, exposed to loud noise/vibrations | 0.05 | 209 | 0.04 | 235 | -0.01 | 0.05 | 0.80 |
| Job, exposed to bullying/violence | 0.02 | 214 | 0.03 | 238 | -0.04 | 0.03 | 0.26 |
| Shoes | 0.93 | 639 | 0.94 | 909 | 0.11 | 0.07 | 0.13 |
| Winter Clothes | 0.82 | 639 | 0.82 | 909 | 0.12 | 0.08 | 0.16 |

Notes: Standard errors are robust to heteroscedasticity and clustered at the 'cadaster cluster' level.

**Table F.7. Attrition – Household Demographic Characteristics**

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
|  | Full Sample | | Remaining | | Balance Test | | |
| Variables | Mean | N1 | Mean | N2 | Diff | SE | p-value |
| Household Size | 6.17 | 1,456 | 6.18 | 1,399 | 0.02 | 0.01 | 0.04 |
| % Elderly (65+) | 0.01 | 8,967 | 0.01 | 8,639 | 0.00 | 0.00 | 0.99 |
| Children (0-14) per household | 3.42 | 1,456 | 3.43 | 1,399 | 0.01 | 0.01 | 0.07 |
| Young children (5-9) per household | 1.44 | 1,456 | 1.44 | 1,399 | 0.01 | 0.01 | 0.22 |
| Older children (10-14) per household | 1.13 | 1,456 | 1.13 | 1,399 | 0.00 | 0.00 | 0.37 |
| Large household | 0.38 | 1,456 | 0.38 | 1,399 | 0.00 | 0.00 | 0.08 |
| Time Displaced (years) | 4.70 | 1,453 | 4.71 | 1,396 | 0.01 | 0.01 | 0.07 |
| Distance to border (km) | 14.74 | 1,456 | 14.81 | 1,399 | 0.06 | 0.05 | 0.29 |
| Single Parent Household | 0.08 | 1,371 | 0.08 | 1,320 | 0.00 | 0.00 | 0.75 |

Notes: Standard errors are robust to heteroscedasticity and clustered at the 'cadaster cluster' level.

**Table F.8. Attrition – Housing**

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
| Variables | Full Sample | | Remaining | | Balance Test | | |
| Mean | N1 | Mean | N2 | Diff | SE | p-value |
| Dwell crowded location | 0.39 | 1,456 | 0.38 | 1,399 | -0.00 | 0.00 | 0.80 |
| Dwell poor conditions | 0.11 | 1,456 | 0.11 | 1,399 | -0.00 | 0.00 | 0.12 |
| Dwell physical dangers | 0.04 | 1,456 | 0.04 | 1,399 | -0.00 | 0.00 | 0.49 |
| Dwell dangerous conditions | 0.04 | 1,456 | 0.04 | 1,399 | -0.00 | 0.00 | 0.68 |
| Dwell urgent repairs | 0.09 | 1,456 | 0.09 | 1,399 | -0.00 | 0.00 | 0.54 |
| Legal/Electric Energy Source | 0.88 | 1,452 | 0.88 | 1,395 | 0.00 | 0.00 | 0.55 |
| Number of rooms | 2.46 | 1,456 | 2.47 | 1,399 | -0.00 | 0.01 | 0.89 |
| Rooms (Lg Household) | 2.49 | 1,291 | 2.49 | 1,240 | -0.00 | 0.01 | 0.65 |
| Rooms (Sm Household) | 2.47 | 1,374 | 2.47 | 1,318 | -0.00 | 0.01 | 1.00 |
| Access to pumped water | 0.38 | 1,456 | 0.38 | 1,399 | -0.00 | 0.00 | 0.33 |
| Access to toilet/latrine | 1.00 | 1,456 | 0.99 | 1,399 | -0.00 | 0.00 | 0.03 |
| Toilet | 0.52 | 1,456 | 0.52 | 1,399 | -0.00 | 0.00 | 0.71 |
| Latrine | 0.48 | 1,456 | 0.48 | 1,399 | 0.00 | 0.00 | 0.77 |
| Rent (USD monthly) | 173.43 | 1,415 | 173.13 | 1,359 | -0.49 | 0.44 | 0.27 |
| Rent $ (Lg Household) | 174.06 | 1,254 | 173.64 | 1,204 | -0.75 | 0.42 | 0.08 |
| Rent $ (Sm Household) | 174.86 | 1,334 | 174.64 | 1,279 | -0.43 | 0.46 | 0.34 |

Notes: Standard errors are robust to heteroscedasticity and clustered at the 'cadaster cluster' level.

**Table F.9. Attrition – Economic Wellbeing**

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
| Variables | Full Sample | | Remaining | | Balance Test | | |
| Mean | N1 | Mean | N2 | Diff | SE | p-value |
| Household cash earnings PC USD (past 7 days) | 8.96 | 1,456 | 9.04 | 1,399 | 0.04 | 0.05 | 0.38 |
| Household in-kind earnings PC USD (past 7 days) | 1.17 | 1,456 | 1.13 | 1,399 | -0.05 | 0.05 | 0.29 |
| Total Income USD (past 30 days) | 321.79 | 1,456 | 322.11 | 1,399 | -0.42 | 1.01 | 0.68 |
| Income $ (Lg Household) | 391.11 | 551 | 392.44 | 535 | 0.52 | 1.74 | 0.76 |
| Income $ (Sm Household) | 279.59 | 905 | 278.57 | 864 | -1.44 | 1.30 | 0.27 |
| Total Income PC USD (past 30 days) | 53.34 | 1,456 | 53.25 | 1,399 | -0.21 | 0.18 | 0.26 |
| Income PC $ (Lg Household) | 49.79 | 551 | 50.00 | 535 | 0.12 | 0.21 | 0.58 |
| Income PC $ (Sm Household) | 55.50 | 905 | 55.26 | 864 | -0.32 | 0.25 | 0.21 |
| Total debt PC USD (past 30 days) | 25.42 | 1,456 | 25.46 | 1,399 | -0.03 | 0.24 | 0.89 |
| Debt PC $ (Lg Household) | 18.11 | 551 | 18.41 | 535 | 0.18 | 0.12 | 0.13 |
| Debt PC $ (Sm Household) | 29.87 | 905 | 29.83 | 864 | -0.10 | 0.35 | 0.78 |
| Total Debt PC USD | 41.52 | 1,456 | 42.16 | 1,399 | 0.60 | 0.30 | 0.05 |
| Debt PC $ (Lg Household) | 32.89 | 551 | 33.41 | 535 | 0.38 | 0.23 | 0.10 |
| Debt PC $ (Sm Household) | 46.78 | 905 | 47.58 | 864 | 0.84 | 0.45 | 0.06 |
| Not enough food | 0.43 | 1,455 | 0.43 | 1,398 | -0.00 | 0.00 | 0.52 |
| Not enough food (Lg Household) | 0.42 | 1,290 | 0.42 | 1,239 | -0.00 | 0.00 | 0.80 |
| Not enough food (Sm Household) | 0.42 | 1,373 | 0.42 | 1,317 | -0.00 | 0.00 | 0.54 |
| Skipped meal | 0.27 | 1,455 | 0.27 | 1,398 | 0.00 | 0.00 | 0.69 |
| Skipped meal (Lg Household) | 0.26 | 1,290 | 0.27 | 1,239 | 0.00 | 0.00 | 0.37 |
| Skipped meal (Sm Household) | 0.26 | 1,373 | 0.26 | 1,317 | 0.00 | 0.00 | 0.73 |
| Adult spent day without food | 0.07 | 1,455 | 0.07 | 1,398 | 0.00 | 0.00 | 0.22 |
| Adult without food (Lg Household) | 0.07 | 1,290 | 0.07 | 1,239 | 0.00 | 0.00 | 0.39 |
| Adult without food (Sm Household) | 0.07 | 1,373 | 0.07 | 1,317 | 0.00 | 0.00 | 0.21 |
| Children skipped meal | 0.08 | 1,455 | 0.08 | 1,398 | -0.00 | 0.00 | 0.81 |
| Child skipped meal (Lg Household) | 0.08 | 1,290 | 0.08 | 1,239 | 0.00 | 0.00 | 0.72 |
| Child skipped meal (Sm Household) | 0.09 | 1,373 | 0.09 | 1,317 | -0.00 | 0.00 | 0.89 |
| Any Assistance | 0.99 | 1,393 | 0.99 | 1,338 | 0.00 | 0.00 | 0.77 |
| MPCA/LCC Assistance | 0.37 | 1,393 | 0.37 | 1,338 | -0.00 | 0.00 | 0.81 |
| WFP Assistance | 0.91 | 1,393 | 0.91 | 1,338 | -0.00 | 0.00 | 0.09 |
| UNICEF Winter | 0.14 | 1,393 | 0.14 | 1,338 | -0.00 | 0.00 | 0.86 |
| UNHCR Winter ($147) | 0.59 | 1,393 | 0.59 | 1,338 | -0.00 | 0.00 | 0.68 |
| UNHCR Winter ($75) | 0.21 | 1,393 | 0.21 | 1,338 | 0.00 | 0.00 | 0.15 |

Notes: Standard errors are robust to heteroscedasticity and clustered at the 'cadaster cluster' level.

**Table F.10. Attrition – Adults' Characteristics (Age 17+)**

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
| Variables | Full Sample | | Remaining | | Balance Test | | |
| Mean | N1 | Mean | N2 | Diff | SE | p-value |
| Female | 0.51 | 3,997 | 0.51 | 3,847 | 0.00 | 0.00 | 0.91 |
| Age | 32.99 | 3,991 | 33.00 | 3,841 | 0.02 | 0.02 | 0.49 |
| Married | 0.69 | 3,943 | 0.70 | 3,795 | 0.00 | 0.00 | 0.93 |
| Literate | 0.84 | 3,978 | 0.84 | 3,828 | -0.00 | 0.00 | 0.87 |
| Male Literacy | 0.87 | 1,958 | 0.87 | 1,884 | 0.00 | 0.00 | 0.36 |
| Female Literacy | 0.81 | 2,017 | 0.80 | 1,941 | -0.00 | 0.00 | 0.17 |
| Attended middle-school | 0.46 | 3,373 | 0.45 | 3,246 | -0.00 | 0.00 | 0.09 |
| Male grad | 0.46 | 1,745 | 0.45 | 1,681 | -0.00 | 0.00 | 0.35 |
| Female grad | 0.46 | 1,628 | 0.45 | 1,565 | -0.01 | 0.00 | 0.04 |
| Employed (last 7 days) | 0.25 | 3,730 | 0.25 | 3,582 | 0.00 | 0.00 | 0.54 |
| Employed (in Syria) | 0.35 | 3,323 | 0.35 | 3,196 | -0.00 | 0.00 | 0.63 |
| HH Head: High Skill Job in Syria | 0.12 | 984 | 0.12 | 948 | 0.00 | 0.00 | 0.74 |
| Good Health | 0.71 | 3,971 | 0.71 | 3,821 | -0.00 | 0.00 | 0.98 |
| Able to walk | 0.56 | 3,971 | 0.57 | 3,821 | 0.00 | 0.00 | 0.54 |
| Able to sweep | 0.62 | 3,968 | 0.62 | 3,818 | 0.00 | 0.00 | 0.46 |

Notes: Standard errors are robust to heteroscedasticity and clustered at the 'cadaster cluster' level.

**Table F.11. Attrition – Younger Children's Characteristics (Age 5-9)**

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
| Variables | Full Sample | | Remaining | | Balance Test | | |
| Mean | N1 | Mean | N2 | Diff | SE | p-value |
| Female | 0.49 | 2,093 | 0.49 | 2,020 | -0.00 | 0.00 | 0.50 |
| Age | 7.04 | 2,093 | 7.05 | 2,020 | 0.00 | 0.00 | 0.63 |
| Orphaned | 0.00 | 2,093 | 0.00 | 2,020 | 0.00 | 0.00 | 0.12 |
| Literate | 0.45 | 2,053 | 0.45 | 1,982 | -0.00 | 0.00 | 0.21 |
| Boys Literacy | 0.45 | 1,038 | 0.44 | 1,006 | -0.00 | 0.00 | 0.33 |
| Girls Literacy | 0.45 | 1,015 | 0.45 | 976 | -0.00 | 0.00 | 0.33 |
| Attended school 2015-16 | 0.66 | 2,029 | 0.66 | 1,957 | -0.00 | 0.00 | 0.85 |
| Boys In School | 0.69 | 1,031 | 0.69 | 997 | 0.00 | 0.00 | 0.66 |
| Girls In School | 0.64 | 998 | 0.64 | 960 | -0.00 | 0.00 | 0.44 |
| In School (Lg Household) | 0.61 | 905 | 0.61 | 878 | -0.00 | 0.00 | 0.08 |
| In School (Sm Household) | 0.71 | 1,124 | 0.71 | 1,079 | 0.00 | 0.00 | 0.32 |
| Out of school due to cost | 0.46 | 684 | 0.46 | 660 | 0.00 | 0.00 | 0.43 |
| Boys out of school | 0.45 | 324 | 0.46 | 311 | 0.00 | 0.01 | 0.36 |
| Girls out of school | 0.47 | 360 | 0.47 | 349 | 0.00 | 0.00 | 0.76 |
| Out of school (Lg Household) | 0.46 | 353 | 0.46 | 346 | 0.00 | 0.00 | 0.94 |
| Out of school (Sm Household) | 0.46 | 331 | 0.46 | 314 | 0.01 | 0.01 | 0.33 |
| Time to school (min) | 21.33 | 1,342 | 21.31 | 1,294 | -0.01 | 0.12 | 0.94 |
| Education Expenses | 93.26 | 1,410 | 93.35 | 1,361 | -0.12 | 0.79 | 0.88 |
| Education Expenses (Lg Household) | 91.40 | 581 | 91.21 | 561 | -0.59 | 1.08 | 0.59 |
| Education Expenses (Sm Household) | 94.56 | 829 | 94.85 | 800 | 0.28 | 1.03 | 0.79 |
| Child labor | 0.00 | 1,893 | 0.00 | 1,824 | 0.00 | 0.00 | 0.07 |
| Child labor (Lg Household) | 0.00 | 841 | 0.00 | 817 | 0.00 | 0.00 | 0.23 |
| Child labor (Sm Household) | 0.00 | 1,052 | 0.00 | 1,007 | 0.00 | 0.00 | 0.37 |
| Working & Out of School | 0.01 | 649 | 0.01 | 626 | 0.00 | 0.00 | 0.26 |
| Working & In School | 0.01 | 1,290 | 0.01 | 1,243 | 0.00 | 0.00 | 0.13 |
| Time collecting firewood/water (min) | 37.25 | 2,001 | 37.03 | 1,931 | -0.21 | 0.45 | 0.64 |
| Firewood (Lg Household) | 20.87 | 888 | 20.42 | 863 | -0.44 | 0.71 | 0.54 |
| Firewood (Sm Household) | 94.56 | 829 | 94.85 | 800 | 0.28 | 1.03 | 0.79 |
| Time providing care (min) | 77.66 | 2,001 | 77.13 | 1,931 | -0.39 | 1.10 | 0.72 |
| Care (Lg Household) | 28.76 | 888 | 29.22 | 863 | 0.60 | 0.33 | 0.07 |
| Care (Sm Household) | 116.68 | 1,113 | 115.85 | 1,068 | -0.89 | 1.82 | 0.62 |
| Time doing chores (min) | 82.26 | 2,002 | 81.30 | 1,932 | -0.82 | 1.04 | 0.43 |
| Chores (Lg Household) | 49.00 | 888 | 48.46 | 863 | -0.28 | 1.03 | 0.79 |
| Chores (Sm Household) | 108.77 | 1,114 | 107.81 | 1,069 | -1.05 | 1.63 | 0.52 |
| Job, carries heavy loads | 0.01 | 508 | 0.01 | 493 | 0.00 | 0.00 | 0.41 |
| Job, works with dangerous tools | 0.01 | 505 | 0.01 | 490 | 0.00 | 0.00 | 0.37 |
| Job, exposed to fumes | 0.01 | 507 | 0.01 | 492 | 0.00 | 0.00 | 0.17 |
| Job, exposed to extreme temperature | 0.02 | 505 | 0.02 | 490 | 0.00 | 0.00 | 0.14 |
| Job, exposed to loud noise/vibrations | 0.02 | 509 | 0.02 | 494 | 0.00 | 0.00 | 0.19 |
| Job, exposed to bullying/violence | 0.00 | 509 | 0.00 | 494 | -0.00 | 0.00 | 0.54 |
| Shoes | 0.91 | 2,035 | 0.91 | 1,963 | -0.00 | 0.00 | 0.03 |
| Winter Clothes | 0.80 | 2,035 | 0.80 | 1,963 | 0.00 | 0.00 | 0.43 |

Notes: Standard errors are robust to heteroscedasticity and clustered at the 'cadaster cluster' level.

**Table F.12. Attrition – Older Children's Characteristics (Age 10-14)**

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
| Variables | Full Sample | | Remaining | | Balance Test | | |
| Mean | N1 | Mean | N2 | Diff | SE | p-value |
| Female | 0.46 | 1,638 | 0.46 | 1,579 | 0.00 | 0.00 | 0.93 |
| Age | 11.89 | 1,638 | 11.90 | 1,579 | 0.00 | 0.01 | 0.58 |
| Orphaned | 0.01 | 1,638 | 0.01 | 1,579 | 0.00 | 0.00 | 0.05 |
| % Child Marriage | 0.00 | 1,610 | 0.00 | 1,552 | 0.00 | 0.00 | 0.16 |
| Literate | 0.78 | 1,619 | 0.77 | 1,561 | -0.00 | 0.00 | 0.21 |
| Boys Literacy | 0.76 | 870 | 0.76 | 839 | -0.00 | 0.00 | 0.28 |
| Girls Literacy | 0.79 | 749 | 0.79 | 722 | -0.00 | 0.00 | 0.33 |
| Attended school 2015-16 | 0.64 | 1,584 | 0.64 | 1,529 | -0.00 | 0.00 | 0.95 |
| Boys In School | 0.61 | 850 | 0.61 | 819 | -0.00 | 0.00 | 0.94 |
| Girls In School | 0.67 | 734 | 0.67 | 710 | 0.00 | 0.00 | 0.98 |
| In School (Lg Household) | 0.60 | 916 | 0.60 | 890 | -0.00 | 0.00 | 0.75 |
| In School (Sm Household) | 0.69 | 668 | 0.69 | 639 | 0.00 | 0.00 | 0.61 |
| Out of school due to cost | 0.64 | 578 | 0.63 | 558 | -0.00 | 0.00 | 0.28 |
| Boys out of school | 0.63 | 331 | 0.62 | 319 | -0.00 | 0.00 | 0.25 |
| Girls out of school | 0.66 | 247 | 0.65 | 239 | -0.00 | 0.01 | 0.54 |
| Out of school (Lg Household) | 0.63 | 367 | 0.62 | 357 | -0.01 | 0.00 | 0.18 |
| Out of school (Sm Household) | 0.65 | 211 | 0.65 | 201 | -0.00 | 0.01 | 0.94 |
| Time to school (min) | 21.00 | 1,008 | 21.06 | 973 | 0.06 | 0.11 | 0.58 |
| Education Expenses | 85.64 | 1,063 | 85.66 | 1,024 | -0.11 | 0.94 | 0.91 |
| Education Expenses (Lg Household) | 88.25 | 582 | 88.63 | 564 | -0.44 | 0.87 | 0.61 |
| Education Expenses (Sm Household) | 82.49 | 481 | 82.01 | 460 | 0.39 | 1.70 | 0.82 |
| Child labor | 0.05 | 1,511 | 0.05 | 1,455 | -0.00 | 0.00 | 0.34 |
| Child labor (Lg Household) | 0.05 | 875 | 0.05 | 850 | -0.00 | 0.00 | 0.72 |
| Child labor (Sm Household) | 0.06 | 636 | 0.06 | 605 | -0.00 | 0.00 | 0.56 |
| Working & Out of School | 0.09 | 579 | 0.10 | 557 | 0.00 | 0.00 | 0.05 |
| Working & In School | 0.04 | 975 | 0.04 | 937 | -0.00 | 0.00 | 0.13 |
| Hours working | 25.69 | 73 | 25.93 | 70 | 0.02 | 0.47 | 0.97 |
| Time collecting firewood/water (min) | 42.03 | 1,601 | 41.80 | 1,543 | -0.19 | 0.44 | 0.66 |
| Firewood (Lg Household) | 37.41 | 933 | 36.42 | 906 | -0.88 | 0.73 | 0.23 |
| Firewood (Sm Household) | 82.49 | 481 | 82.01 | 460 | 0.39 | 1.70 | 0.82 |
| Time providing care (min) | 89.46 | 1,601 | 89.09 | 1,543 | -0.04 | 1.18 | 0.97 |
| Care (Lg Household) | 67.57 | 933 | 66.07 | 906 | -0.91 | 1.36 | 0.50 |
| Care (Sm Household) | 120.04 | 668 | 121.84 | 637 | 1.20 | 2.30 | 0.60 |
| Time doing chores (min) | 86.93 | 1,601 | 85.43 | 1,543 | -1.13 | 1.28 | 0.38 |
| Chores (Lg Household) | 70.23 | 933 | 67.65 | 906 | -1.84 | 1.55 | 0.24 |
| Chores (Sm Household) | 110.26 | 668 | 110.71 | 637 | -0.23 | 2.45 | 0.93 |
| Job, carries heavy loads | 0.09 | 465 | 0.09 | 451 | 0.00 | 0.00 | 0.78 |
| Job, works with dangerous tools | 0.07 | 466 | 0.07 | 452 | 0.00 | 0.00 | 0.89 |
| Job, exposed to fumes | 0.08 | 463 | 0.08 | 449 | -0.00 | 0.00 | 0.43 |
| Job, exposed to extreme temperature | 0.10 | 467 | 0.10 | 453 | -0.00 | 0.00 | 0.88 |
| Job, exposed to loud noise/vibrations | 0.05 | 458 | 0.05 | 444 | 0.00 | 0.00 | 0.86 |
| Job, exposed to bullying/violence | 0.02 | 466 | 0.02 | 452 | 0.00 | 0.00 | 0.14 |
| Shoes | 0.94 | 1,607 | 0.93 | 1,548 | -0.00 | 0.00 | 0.84 |
| Boys own shoes | 0.93 | 862 | 0.93 | 830 | -0.00 | 0.00 | 0.30 |
| Girls own shoes | 0.94 | 745 | 0.94 | 718 | 0.00 | 0.00 | 0.71 |
| Shoes (Lg Household) | 0.93 | 925 | 0.93 | 897 | -0.00 | 0.00 | 0.24 |
| Shoes (Sm Household) | 0.94 | 682 | 0.94 | 651 | 0.00 | 0.00 | 0.52 |
| Winter Clothes | 0.82 | 1,607 | 0.82 | 1,548 | 0.00 | 0.00 | 0.26 |
| Boys winter clothes | 0.83 | 862 | 0.83 | 830 | 0.00 | 0.00 | 0.59 |
| Girls winter clothes | 0.81 | 745 | 0.81 | 718 | 0.01 | 0.00 | 0.17 |
| Winter clothes (Lg Household) | 0.83 | 925 | 0.83 | 897 | 0.01 | 0.00 | 0.13 |
| Winter clothes (Sm Household) | 0.81 | 682 | 0.81 | 651 | -0.00 | 0.00 | 0.59 |

Notes: Standard errors are robust to heteroscedasticity and clustered at the 'cadaster cluster' level.

1. Statistics Lebanon was contracted by UNICEF to conduct the baseline data collection. [↑](#footnote-ref-2)
2. Government of Lebanon and the United Nations (2014). Lebanon Crisis Response Plan 2015-2016. [↑](#footnote-ref-3)
3. Missing Out: Refugee Education in Crisis. (2016). UNHCR. Available:   
   <http://uis.unesco.org/sites/default/files/documents/missing-out-refugee-education-in-crisis_unhcr_2016-en.pdf>. [↑](#footnote-ref-4)
4. UNICEF and the WFP provided the program to children 15 and 16 years old if their closest school would enroll them, though schools would not guarantee they would accept them. The study will track children in this age range to determine whether they received the transfer, and then analyze this age group separately in the follow-up round if there is a sufficient number of beneficiaries. [↑](#footnote-ref-5)
5. WFP, UNHCR, UNICEF and the Lebanon Cash Consortium (LCC) provide humanitarian assistance to vulnerable populations in Lebanon through cash transfers. They deliver such assistance in Lebanon through creditable and re-chargeable prepaid cards called the “Lebanon One Unified Inter-Organizational System for E-cards” (LOUISE). The goal of this system is to foster cost-efficiency, harmonization and simplification at the beneficiary level.  The agencies worked together to develop, organize, and implement the system. [↑](#footnote-ref-6)
6. Statistics Lebanon was contracted by UNICEF to conduct the baseline data collection. [↑](#footnote-ref-7)
7. Government of Lebanon and the United Nations (2014). Lebanon Crisis Response Plan 2015-2016. [↑](#footnote-ref-8)
8. Missing Out: Refugee Education in Crisis. (2016). UNHCR. Available:   
   <http://uis.unesco.org/sites/default/files/documents/missing-out-refugee-education-in-crisis_unhcr_2016-en.pdf>. [↑](#footnote-ref-9)
9. UNICEF provided the program to children 15 and 16 years old if their closest school would enroll them, though schools would not guarantee they would accept them. The study will track children in this age range to determine whether they received the transfer, and then analyze this age group separately in the follow-up round if there is a sufficient number of beneficiaries. [↑](#footnote-ref-10)
10. VASyR 2015 [↑](#footnote-ref-11)
11. Nonexperimental designs do not manipulate the selection process to determine who receives the program, while randomized, controlled trials use a lottery process to select who will receive the program and who will not. [↑](#footnote-ref-12)
12. De Hoop, Morey, and Seidenfeld, “Evaluation of NLG/Min Ila, a UNICEFand WFP Cash Transfer Program for Displaced Syrian Children in Lebanon Baseline Report.” February 2017. UNICEF Lebanon [↑](#footnote-ref-13)
13. During the 2015-2016 school year, UNICEF and Caritas ran a school bus program, providing transport to school for children living far from a second shift school. This program was stopped in the governorates of Akkar and Mt. Lebanon during the 2016-2017 school year, but continued in the rest of the country. However, as children living within the 2.5 km radius from a second shift school do not benefit from the bus services, this program is unlikely to confound the results presented in this report. [↑](#footnote-ref-14)
14. This finding is in line with the fact that more generally geographic, economic, and political situations do not change when crossing these same borders. Education policies, moreover, are determined centrally and do not differ across governorates. [↑](#footnote-ref-15)
15. What Works Clearinghouse (http://ies.ed.gov/ncee/wwc/documentsum.aspx?sid=19) [↑](#footnote-ref-16)
16. Gertler, P. J.; Martinez, S., Premand, P., Rawlings, L. B. and Christel M. J. Vermeersch, 2010, Impact Evaluation in Practice: Ancillary Material, The World Bank, Washington DC (www.worldbank.org/ieinpractice). [↑](#footnote-ref-17)
17. Lee, D. and T. Lemieux. (2010). “Regression Discontinuity Designs in Economics”, *Journal of Economic Literature*, 48(2), 281-355. [↑](#footnote-ref-18)
18. Importantly, this “geographical RDD” identifies the effect of the program on those households and children living close to the border (the so-called “local treatment effect”), which may or may not be identical to the effect of the program on the full sample in the pilot governorate. Because within governorate differences in the characteristics of Syrian refugees tend to be limited, this issue appears to be of secondary concern. [↑](#footnote-ref-19)
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