Does Conditionality Matter?
An Empirical Analysis of Conditional and Unconditional Cash Transfers in Indonesia

Quentin Liren Jeyaretnam
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Faculty Advisors:
Professor Katharine Sims
Professor Aisha Yusuf

Readers:
Professor Jakina Debnam Guzman
Professor Joshua Hyman

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Abstract

This study evaluates the role of conditionality in cash transfer programs using a natural experiment involving poor households with young children in Indonesia. Between 2007 and 2015, the Indonesian government implemented both a conditional cash transfer (CCT) and a set of two unconditional cash transfers (UCTs) targeting similar groups of poor households with comparable levels of cash payments, where the main difference was in conditioning the CCT on school attendance, health check-up attendance, and receiving immunizations for their children. I exploit the similarity in scope and time of implementation of these programs through a difference-in-difference analysis so as to compare their effects on human capital outcomes of the young children of the programs' beneficiaries and, in turn, estimate the role of conditionality in these programs.

I find that the CCT had a lower impact on these outcomes than did the UCT: small and insignificant effects on years of education and immunizations compared to significant and positive effects of the UCT. Conditionality is associated with increased national exam scores in math and Bahasa Indonesia but did not exhibit a positive externality effect on other education-related outcomes such as attendance of early childhood education programs. Conditionality also appears to be important in alleviating information asymmetries in healthcare-seeking behaviors between households and governments. Where households might view regular doctor visits for their young children as less important than do governments, health-related conditions can communicate to households the need for sustained and consistent health check-ups for their children. I describe potential biases and limitations of my analysis as well as possible policy takeaways for the future implementation of such programs.

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Keywords: Welfare Programs, Conditional Cash Transfers, Unconditional Cash Transfers, Human Capital

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I. Introduction

The past few decades have brought with it significant and innovative developments in the world of welfare payments, most notably the creation of conditional cash transfers (CCTs). As with most welfare programs, CCTs provide poor households with cash payments towards the goal of reducing poverty (Fiszbein et al., 2009). Where CCTs differ from other cash transfer programs, however, is that these payments are conditional on beneficiary households meeting certain stipulations, most often related to human capital investments in the young children of these households – pre and postnatal care, immunizations, growth monitoring, and school attendance, to name a few examples (Fiszbein et al., 2009). Following from the success of Mexico's *Programa Nacional de Educación, Salud y Alimentación* (PROGRESA) in the 1990s, CCTs have spread widely across the developing world (Bastagli et al., 2016), and their effectiveness at improving education and health outcomes for young children is well-documented (Baird et al., 2014).

The counterpart to CCTs, and the traditional model of welfare payments, exists as unconditional cash transfers (UCTs), which similarly target poor households and provide them with cash payments but without the need for beneficiary households to meet any conditions, and these programs have also been shown to be largely successful (Baird et al., 2011). The debate as to which type of cash transfer program, conditional or unconditional, is more desirable is very much active, and the answer remains unclear. Proponents of conditionality argue, for instance, that including conditions allows governments to induce specific desirable behavior changes in households as well as alleviating some of the stigma surrounding welfare payments (de Brauw & Hoddinott, 2011). Opponents of conditionality say, however, that the monitoring and enforcement of conditions is too costly and thus that net welfare is reduced, and that social protection is a human right and thus making their receipt conditional on some action is morally wrong (Freeland, 2007).

Thus, the question of conditionality in cash transfer programs – the role it plays and whether or not it should be included – is very much open, and its answer will be incredibly important for the effective rollout of future programs. Few empirical studies directly comparing CCTs and UCTs exist. Those that do suggest that, in general, CCTs are more effective at improving conditioned outcomes than UCTs (Akresh et al., 2016; Benhassine et al., 2015; de Brauw and Hoddinott, 2011), but with some caveats – Baird et al. (2011), for instance, find that UCTs had a much stronger positive impact on school dropouts, i.e., those who failed to meet the school attendance condition, thus suggesting that the inclusion of conditions might deny benefits to those households that need it the most. Thus, more empirical evidence is needed in order to better understand the role of conditionality.

In this paper, I study two sets of cash transfer programs in Indonesia between 2007 and 2015, one a CCT and the other a set of two UCTs, in order to add to this evidence. I use data from two iterations of the Indonesia Family Life Survey (IFLS), one before and one after the implementation of these programs. The *Program Keluarga Harapan* (PKH) is a CCT first implemented in 2007 and continuing on until at least 2020 that required households to meet requirements related to health (postnatal care and immunizations) and education (primary and secondary school attendance) for their young children, while the *Bantuan Langsung Tunai* (BLT) and the *Bantuan Langsung Sementara Masyarakat* (BLSM), implemented in 2008 and 2013 respectively for less than a year each, are two UCTs, the latter widely considered to be a direct continuation of the former. Both the CCT and the UCT programs targeted poor households, provided similar amounts of cash payments to their beneficiaries, and were implemented nationwide at similar times – in essence, their main difference was in the inclusion or exclusion of conditions. As such, they provide a good setting for a natural experiment in which to assess the role of conditionality in cash transfer programs.

By exploiting variation in time and receipt of the CCT and UCT programs, I am able to employ a difference-in-difference model in order to estimate the relationship between each program and outcomes related to education and health, namely, those outcomes related to the conditions of the PKH, as well as labor, included so as to ascertain some of the potential unintended consequences of including conditions. Within education, I find that the CCT program is associated with, on average, a 0.0625 year decrease in the number of years of education achieved by an individual, although insignificant and so with seemingly no effect, while the UCT program is associated with a 0.178 year increase in the years of education achieved by an individual, significant at the 1% level. Conditionality is also associated with increases in national exam scores in both math and Bahasa Indonesia, the national language of Indonesia, though there was no such positive externality effect on the likelihood of attending early childhood education programs.

Within health, I find that the CCT program is associated with a 0.078 percentage point decrease in the likelihood of a child having received all of their required immunizations, again insignificant and so with seemingly no effect, while the UCT program is associated with a 0.236 percentage point increase in that same likelihood. However, conditionality appears to be important in communicating to households the need for regular health check-ups for their young children, where the CCT is associated with a 0.070 percentage point increase in the likelihood of a child having visited a health clinic in the past month while the UCT is associated with only a 0.014 percentage point increase in that same likelihood.

These results at face value suggest that including conditions actually makes cash transfer programs less effective at improving conditioned outcomes, but that they may exhibit positive externalities on other related outcomes. However, due to limitations of sample size and errors in condition enforcement, these associations may not be causal or could be biased estimates. I also find that, on average, children in households treated by the CCT worked fewer

hours over time while children in households treated by the UCT worked more hours over time, suggesting that conditionality might have disincentivized households from having their children work, though it appears they do not substitute towards investments in education.

Despite the limitations of my analysis, the change in effect from including conditions in these programs still provides some possibly useful insights as to the role of conditionality, which I synthesize into several key takeaways. I conclude that conditions must be carefully constructed and explicit as to what behaviors are being incentivized in order that exactly those behavior changes and no other undesirable and unintended consequences result, and that conditions must be strictly enforced such that their full benefits are obtained in light of their large and documented costs.

The rest of the paper proceeds as follows. I describe the current literature, the setting, and an overview of the programs in Section II. I outline the empirical strategy, data, and summary statistics in Section III and provide the findings of my analysis in Section IV. Section V discusses the limitations, external validity, and policy relevance of my analysis, and concludes.

II. Background

A. Literature Review

CCTs have become an increasingly popular method of poverty alleviation and can now be found implemented throughout the developing world – today, at least one CCT program can be found in over 63 countries (Bastagli et al., 2016), reaching millions of households and families around the world (Robles et al., 2015). Despite their popularity, there is much less evidence on the efficacy and desirability of the actual imposition of conditions, and, in comparing CCTs and UCTs, theoretically, a case can be made for both types of programs.

From both the public and private perspectives, there are strong justifications for attaching conditions to cash transfers. Conditions can help to overcome information

asymmetries between governments and households, where governments might know what actions benefit poor households better than the poor households do themselves. For instance, families may be unaware or unconvinced of the value in educating girls, seeking immunizations, or screening for chronic diseases, whereas governments would recognize the inherent benefits of these actions. Conditions can then induce desirable behavior change amongst poor households faster than other approaches, such as public health or education campaigns. The explicit connection to common indicators of political performance, like school enrollment or health clinic usage, further increases the popularity of conditioning transfers since politicians can point to CCTs as examples of their political successes (de Brauw and Hoddinott, 2011). Furthermore, the actions that households undertake as part of meeting conditions often have positive social externalities that extend beyond the household. For example, actions that improve a country's level of education, healthcare, or environmental protection will naturally improve the quality of life of its residents, whether through better informed and productive citizens, increased life expectancy, or environmental factors such as air quality. As with all positive externalities, they tend to be underprovided by private markets, thus allowing governments to use CCTs to subsidize these actions and help to solve the collective action problems surrounding them.

From the private point of view, the imposition of conditions also has its advantages. Conditions can improve the bargaining power of individuals within a household with regard to resource allocation when that individual's preferences are aligned with the government's, for instance where mothers and fathers might disagree on whether or not to send their daughters to school. Conditions can help to alleviate credit constraints faced by a household in making their investment decisions as well as lessen some of the stigma surrounding welfare payments, since households may feel that they are rightfully earning the cash transfers by meeting the stipulations set forth by the government (de Brauw and Hoddinott, 2011). Similarly, some

conditions also incentivize households to work for their payments, thus helping to solve some of the issues of incentive compatibility where welfare payments might lead to higher levels of unemployment as in classical models of unemployment benefits (Katz and Meyer, 1990). Lastly, Laibson (1997) finds that households with hyperbolic discount functions can act against their own long-term welfare – in such a scenario, conditions act as a constraint to reduce a household's ability to substitute away from future period consumption.

Opponents of conditionality, however, argue that unconditional cash transfers should be preferred. Conditions are costly to administer and monitor and complicate the operations of cash transfer programs – for example, Caldés et al. (2006) estimate that targeting and conditioning make up 60 percent of the total program costs for PROGRESA, 49 percent for the *Programa de Asignación Familiar-Fase II* (PRAF) in Honduras, and 31 percent for the *Red de Protección Social* (RPS) in Nicaragua. Conditions usually also impose direct or opportunity costs on beneficiaries. These costs may reduce their total welfare gains and thus the net benefit of these programs, or conditions may be too difficult to meet, thus potentially excluding households and individuals that might be most in need of cash transfers (Baird et al., 2011). Conditions can also be problematic if they are seen as governments paying households for actions that were formerly freely provided. For example, some argue that the conditions surrounding environmental protections, as included in payments for environmental services (PES) programs, may alter community structure and reduce levels of prosocial behaviors (Ravikumar et al., 2023), though Alix-Garcia et al. (2018) find no such effect when studying Mexico's PES program. The imposition of conditions might also be considered demeaning, implying that poor households do not know what is best for them – Freeland (2007) argues that because social protection is a human right, governments should not make their receipt conditional on any requirement.

As such, the question of whether governments should include conditionality in their cash transfer programs remains open. Empirical evidence is sparse as the number of studies directly comparing CCTs and UCTs remains low, though the literature has been growing in recent years. In one such study of adolescent school-age girls in Malawi, Baird et al. (2011) use a randomized controlled trial with two distinct cash transfer interventions – a UCT arm and a CCT arm conditional on school attendance – to test for the role of conditionality. They find that their CCT arm had a much larger effect on dropout rates, with the UCT arm's impact only 43% as large as the CCT's at the end of the two-year program, thus adding merit to the importance of conditionality in inducing desirable behavior change as discussed above. However, they find that teenage pregnancy and marriage rates were significantly lower amongst the UCT participants, with these effects almost entirely from girls who dropped out of school, suggesting that those most in need of the cash transfer program might be denied benefits due to the imposition of conditions. This unintended consequence of including conditions is notable and casts doubt on the benefits of conditionality, and the lack of other studies leaves this an open question for further research.

In another such study in Burkina Faso, Akresh et al. (2016) randomly assign mothers and fathers to either a CCT or a UCT arm in order to assess the impact of conditionality and differential targeting, with their CCT arm conditioned on school enrollment and attendance for older children and health check-up attendance for younger children. Their results also show that CCTs generally outperform UCTs for these education and health outcomes on which their CCT is conditioned, and they find no significant difference in targeting the programs to mothers versus fathers. However, they do not investigate unconditioned outcomes as in the previous study.

In a study of Labeled Cash Transfers (LCTs) in Morocco, Benhassine et al. (2015) investigate a small cash transfer program targeted to fathers of adolescent, school-aged children

from poor and rural households through a randomized controlled trial. The LCT is distinct from a CCT and similar to a UCT, since it does not include conditions for school attendance but is explicitly named as a program for supporting children's education. Their results show large and significant increases in school attendance rates with almost no difference to these gains when including conditions, and thus they conclude that simply naming a program towards an end, in this case education, is enough to induce desirable behavior change, at least in the context of this study. This finding casts doubt on the necessity of explicit conditions and thus provides an avenue to alleviate some of the administrative costs of condition monitoring. However, a study in Mexico exploiting the incomplete monitoring of the school attendance condition for PROGRESA finds that households that are not monitored are less likely to meet the school attendance condition, thus suggesting the need for further research (de Brauw and Hoddinott, 2011).

We can thus see that some studies indicate that including conditionality does have a much more significant positive impact on conditioned outcomes, but also evidence that UCTs may have advantages in reaching a wider group of households. The problem of denying benefits to those who are unable to meet the conditions remains as a potential concern for CCTs and is critical to the future rollout of cash transfer programs, as this unintended effect may have the consequence of worsening the divide between poor and ultra-poor households. In order to investigate a comparison of CCTs and UCTs and add to our understanding of the role of conditionality in these programs, I use Indonesia as a case for study.

B. Setting: The State of Human Capital in Indonesia

Indonesia, as a rapidly developing economy, provides a unique and interesting setting of study. Many improvements in human capital have been made in the past few decades, but gaps still remain, thus underscoring the importance of understanding the effects of policies that aim to improve human capital in such a setting. I outline several key statistics regarding the

state of education and health in Indonesia so as to provide greater context for the analysis and results in the rest of this paper.

Indonesia holds one of the largest and fastest-growing economies in the world, and thus boasts one of the largest education systems in the world, with over 52 million students and 218,000 schools as of 2017 (Sukmayadi, 2020). Students go through twelve years of basic education from around age 7 to age 18, similar to most public education systems around the world, comprising six years of primary education and six years of secondary education, the latter split into three years each of junior and senior secondary school. Since 1994, education up to the junior secondary level has been compulsory, and in 2013 compulsory education was expanded to include senior secondary school (Sukmayadi, 2020). Students also have the option of undertaking vocational training upon reaching senior secondary school, and an equivalent 12-year Islamic education system also exists. Public school education is free and available to all and is supplemented by private schools in most parts of the country.

Enrollment is generally high due to a number of educational reforms instituted by the government in the late 20th century, though drop-offs in enrollment between school levels can be seen. These reforms include an expansive primary school construction program between 1973 and 1984, at the time the world's most rapid program of its kind ever completed (Duflo, 2001), as well as secondary school reforms such as improved teacher training and an increased supply of textbooks and science equipment (Yeom et al., 2002). The results of Indonesia's efforts are staggering – educational attainment has increased at a rate of 0.26 years of education per chronological year between 2000 and 2015, total student enrollment has increased by 25% between 2000 and 2019, and enrollment of students in senior secondary school has increased from 50% to 71% between 2002 and 2017 (World Bank, 2019).

The state of health in Indonesia, on the other hand, has been improving in the 21st-century but still faces noticeable shortfalls. As of 2008, Indonesia's healthcare spending was

less than 1% of its GDP (Rokx and Elif, 2008), though in the decade since that proportion has increased to about 4% of GDP (ITA, 2019). The proportion of children under five that are underweight has declined significantly over the last few decades from 37.5% in 1989 to 17.9% in 2010, though rates of stunting, thought to be associated with worsened cognitive and economic outcomes later in life (Case and Paxson, 2008), remain high (Cahyadi et al., 2020). Immunization coverage, often correlated with health system performance, is fairly low – as of 2009, only 70% of Indonesians had received the diphtheria, pertussis, and tetanus (DPT) vaccination (Rokx et al., 2009).

C. Overview of Programs

On October 1, 2005, in the face of steadily rising global oil prices, the Indonesian government made the decision to cut domestic fuel subsidies in order to ease fluctuations within their budget (Cameron and Shah, 2014). However, this led to sharp price hikes borne mostly by poor households – the price of kerosene, for instance, used for cooking and lighting, rose by 185.7% (Widjaja, 2009). In an attempt to mitigate some of the negative effects of the increase in the price of fuel on poor households, the Indonesian government launched a multi-faceted compensation scheme, the *Program Kompensasi Pengurangan Subsidi Bahan Bakar Minyak* (PKPS-BBM) that year, which included programs for health insurance, the development of rural infrastructure, and school aid (Alatas, 2011). The fourth element of the PKPS-BBM was a temporary UCT, the *Bantuan Langsung Tunai* (BLT), at the time the only nationwide cash transfer program to ever be implemented in Indonesia.

The BLT was targeted at the poorest households in Indonesia, specifically those households with a monthly per capita expenditure below Rp 175,000 (US$11.50), with 18.6 million households (about one-third of all households in Indonesia) receiving the cash transfer of Rp 100,000 (US$7) per month for six months (World Bank, 2006). Due to the lack of a national, unified database of household incomes, the Indonesian government developed and

utilized a proxy-means testing approach in order to identify eligible households. This approach, however, led to fairly severe mistargeting of funds due to the reliance on estimates of household expenditure rather than actual amounts. Cameron and Shah (2014) find that 46.5% of BLT recipients reported having monthly per capita expenditures above the stipulated cutoff and that 48.2% of poor households, i.e., those with monthly per expenditures below the cutoff, did not receive the cash transfer at all. Furthermore, due to the unrestrained nature of the payments, critics of the BLT were concerned that funds would be used for the consumption of non-productive and vice goods, such as cigarettes and alcohol (Alatas, 2011), though Dwiputri (2017) finds no such effect and instead that the BLT contributed negatively to the consumption of cigarettes.

The first round of the BLT ended in December 2006, but it has been revived twice since then. In September 2008, the BLT was reinstated for a seven-month period again in response to rising oil prices, and in June 2013, the Indonesian government introduced for less than a year a new temporary unconditional cash transfer program, the *Bantuan Langsung Sementara Masyarakat* (BLSM), widely considered to be a direct continuation of the BLT and also similar to the BLT in size and scope (World Bank, 2017). As such, my analysis, focusing on the latter two programs, treats them as one set of cumulative temporary UCTs and considers their effects as a whole.

Despite the success of these UCT programs in providing assistance to some poor households, there was still a lot of dissatisfaction with the program and its rollout, leading the Indonesian government to develop new cash transfer and social assistance programs that improved on the design and targeting of the BLT, one such program being the *Program Keluarga Harapan* (PKH) or Family Hope Program. The program was launched in 2007 and was still ongoing as of 2020 (Cahyadi et al., 2020), and, to the best of my knowledge, is still ongoing today, providing quarterly cash payments to poor households with pregnant or

lactating women, children aged 0-15 years, or children aged 16-18 years who had not yet completed 9 years of basic education (Alatas, 2011). Payments were around 15 to 20 percent of annual household income and ranged from Rp 600,000 (US$40) to Rp 2,200,000 (US$145) per household per year (Cahyadi et al., 2020).

Similar to the BLT, household eligibility was determined through a proxy-means testing approach combined with the above-mentioned demographic requirements, and while, to my knowledge, mistargeting has not been brought up as an issue, my data, outlined in Section III.E, suggests otherwise. Unlike the BLT, however, the PKH payments were conditional on households meeting certain requirements depending on the nature of the household. For households with pregnant or lactating women, conditions included attending pre- and post-natal check-ups, consuming iron tablets, and having a trained professional supervise the birth. For households with children aged 0-6 years, conditions included receiving immunizations, taking Vitamin A twice a year, and attending growth-monitoring check-ups. For children aged 7-15 years, conditions included enrolling in school and a minimum attendance rate of 85%. For children aged 16-18 years, the condition was to enroll in school and complete 9 years of basic education (Alatas, 2011). While a system for the enforcement of conditions was in place, stipulating that a first violation would be met with a warning letter, a second with a 10 percent reduction in benefits, and a third with program expulsion, in practice this was not always enforced, with the system beginning in earnest only in 2010 and conditions often not being enforced even beyond that (Cahyadi et al., 2020).

The CCT program was largely considered to be a success in terms of positive increases in health and education outcomes for poor households. In particular, Cahyadi et al. (2020) find that households that received cash transfers from the PKH were 23 percentage points more likely to have childbirth assisted by a trained professional, had school enrollments for the targeted age-group of 7- to 15-year-olds that were 4 percentage points higher than the control

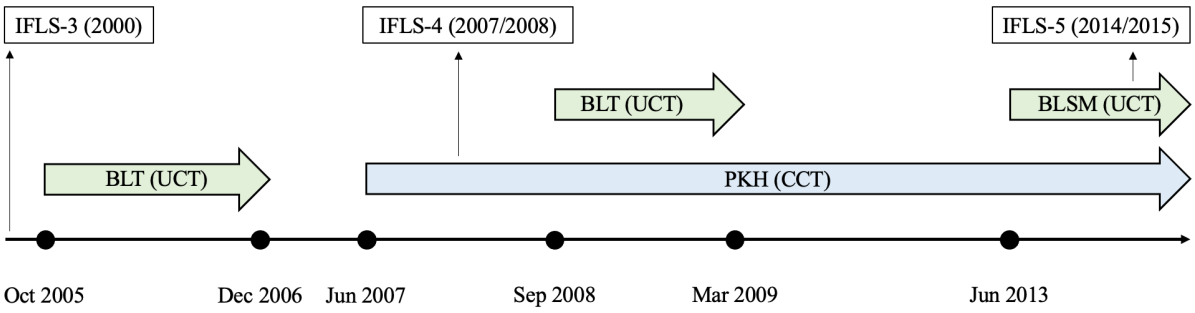

Figure 1: Timeline of Programs and Surveys

group, and, for young children, had a 23 percent reduction in the probability of being stunted. They also document positive cumulative effects of the program on health and education outcomes, supporting the choice of having sustained payments over time rather than temporary assistance, though my results are different and show no such effect.

Figure 1 shows a timeline of the various CCT and UCT programs, as well as the times in which the IFLS survey was implemented, to be elaborated upon in Section III.B.

The similarities of the CCT and UCT programs allow for the role of conditionality in cash transfer programs to be indirectly assessed through a natural experiment. Both programs were large-scale and nationwide, targeted similar groups of households by socioeconomic standards, provided similar levels of monthly payments, and were implemented at similar times. The main difference was in the presence of conditions on the receipt of payments, thus allowing for the effects of both programs on health, education, and labor outcomes to be compared to estimate the impact of conditionality.

III. Data and Empirical Strategy

A. Identification Strategy

I employ a difference-in-difference analysis exploiting variation in time and receipt of the CCT and UCT programs to jointly determine how individuals may be impacted by exposure to each program. The pre-treatment dataset is considered to be the IFLS-4 from 2007/2008, and the post-treatment dataset is considered to be the IFLS-5 from 2014/2015. I define this as such since the PKH began in 2007, the BLT for which there is data was conducted

in 2008, and the BLSM was conducted in 2013, as outlined in Figure 1 in Section II.C. Thus, individuals in the pre-treatment dataset who would be exposed to either treatment would have either just received their first payment (in the case of the CCT) or not yet have received any payment, while individuals in the post-treatment dataset would have been receiving payments for several years. This analysis therefore depends in part on the assumption that any payment received by an individual in the pre-treatment dataset would not have significantly changed any of their outcomes within that first year. This is untestable, but may be reasonable since the outcomes of interest, specifically years of education, faces lags, and thus we would likely not see any effect within the first year of treatment. For outcomes that do not face lags, any significant change in that first year would bias my results downward.

Unfortunately, while the BLT was first conducted from 2005 to 2006 as described above, I am unable to locate data on that iteration of its implementation. Thus, my analysis is limited to implementations of UCTs after 2007. This then makes the assumption that individuals that received BLT payments prior to 2007 would not have had their outcomes significantly changed by those payments. In actuality, individuals that received the BLT in 2005 would likely have received it in 2008 as well, thus exerting a downward bias on the results for the UCT treatment group in my analysis.

B. Datasets and Variables

The data used in this paper comes from the Indonesia Family Life Survey (IFLS), an ongoing longitudinal survey in Indonesia at the household and individual levels. I use three iterations of the survey – the IFLS-3, fielded in 2000, the IFLS-4, fielded in 2007/2008, and the IFLS-5, fielded in 2014/2015, in order to measure the differences in measures of education, labor, and health as a result of the cash transfer interventions, as well as to run the parallel pre-trends test in Section III.D.

I encode the variables as follows. The treatment variables indicate whether or not a household has ever received the CCT or UCT programs respectively and are derived directly from the survey using indicators for whether or not an individual received the PKH, BLT (2008), and BLSM programs. If a household had ever received the PKH program in 2014/2015, they are considered treated by the CCT, and if a household had ever received the BLT (2008) or BLSM programs in 2014/2015, they are considered treated by the UCT. While receipt of the programs is encoded at the household level, I then attach these household-level responses to each individual child using household IDs included in the survey in order to determine exposure to the treatments at the individual level. I also include in my treatment groups those households from the pre-period (2007/2008) that indicated that they received the PKH, for the CCT treatment, or the BLT (2008), for the UCT treatment. Thus, if a household responded that they received the programs in either the 2007/2008 survey, the 2014/2015 survey, or both, the children in that household are considered treated by the CCT or UCT respectively.

I group the outcome variables into three categories: health, labor, and education. I use these three categories in order to test the impact of the programs on conditioned outcomes, namely education and health variables, as well as to assess any unintended consequences using child labor and other unspecified education outcomes. Within health, the two variables respectively indicate whether or not a child has visited a hospital or *posyandu* (health clinic) in the past month and whether or not an individual has received the full suite of required immunizations as stipulated by UNICEF for which the survey has collected data, namely the Bacillus Calmette-Guérin (BCG), diphtheria, pertussis, and tetanus (DPT), measles, and hepatitis B immunizations (UNICEF, 2022). Within labor, the outcome variable measures the number of hours a child has worked in the previous week, and, due to several exceedingly high outliers, has been winsorized to the bottom 95% of observations.

Within education, the outcome variables measure years of education, whether or not a child has received sufficient education for their age, whether or not a child has ever attended an early childhood education and development (ECED) program, and national exam scores, scored from 0 to 10, in mathematics and Bahasa Indonesia, the national language of Indonesia. The first two variables relate explicitly to the conditions specified in the CCT, while the latter three highlight any additional effects on education outcomes not explicitly mentioned in the conditions. The sufficient education indicator variable is coded to equal 1 if a child has at least as many years of education as their age less six, and 0 otherwise, denoting whether or not a child entered formal education at age 7 and continued attending school every year and is used as a proxy for school attendance. For instance, a child aged 7 would have completed the first year of primary education and so should have one year of education, while a child aged 12 should have completed all of their primary education and so should have six years of education.

C. Estimating Equations

The strategy outlined in Section III.A fits the following regression specification:

(1) $Y_{iht} = c_1 + \beta_1 P_{iht} + \beta_2 C_{iht} + \beta_3 U_{iht} + \gamma_1 (C_{iht} P_{iht}) + \gamma_2 (U_{iht} P_{iht}) + \alpha_{1t} + \boldsymbol{\psi_1 X_{iht}} + \epsilon_{iht}$

where Y_{iht} is the outcome variable for individual *i* in household *h* in year *t*, P_{iht} is a dummy variable for whether or not an individual *i* in household *h* in year *t* is in the post-period (2014/2015 survey), C_{iht} is a dummy variable for whether or not an individual *i* in household *h* in year *t* was exposed to the CCT program, U_{iht} is a dummy variable for whether or not an individual *i* in household *h* in year *t* was exposed to the UCT program, α_{1t} is a year-of-birth fixed effect, and $\boldsymbol{X_{iht}}$ is a vector of controls of household and individual characteristics. The full set of controls are age, sex, household size, whether or not a household owns a television, and the log of household monthly income per capita. The coefficients of interest are γ_1 and γ_2, the difference-in-difference estimators for the CCT and UCT treatments respectively.

By restricting my sample to those individuals that appear in both the pre-treatment and post-treatment datasets, I am also able to create a balanced panel dataset across time which will be used in alternate specifications. While this approach allows for a direct comparison of outcomes by individuals, the tradeoff here is that restricting my sample then limits the number of observations in my analysis. In particular, this restricted analysis fails for some of my outcome variables since there are very few individuals that answered those survey questions in both datasets. As such, I include these results only in addition to my main results using the following specification:

$$(2) \qquad Y_{iht} = c_1 + \alpha_{1t} + \theta_{1h} + \gamma_1(C_{iht}P_{iht}) + \gamma_2(U_{iht}P_{iht}) + \epsilon_{iht}$$

where Y_{iht}, C_{iht}, U_{iht}, P_{iht}, and α_{1t} are as they are described above, and θ_{1h} is a household fixed effect. The coefficients of interest are again γ_1 and γ_2.

D. Identifying Assumption

The identifying assumption is that in the absence of the treatments, the change in outcome variables would not have systematically differed between the treatment and control groups. To examine this, I test for parallel pre-trends between the two groups for a selection of outcome variables using two cohorts of individuals, those from the IFLS-3 (2000) and the IFLS-4 (2007/2008), that would not yet have been exposed to either program according to equation (2). Treatment is determined by having received either the CCT or UCT programs in the IFLS-5 (2014/2015) or IFLS-4 (2007/2008) datasets and Post (P_{iht}) now refers to being in the IFLS-4 (2007/2008) dataset. This test indicates whether those households that would later become treated versus not show differences in trend in the pre-period, i.e., between 2000 and 2007/2008.

Table 1 shows the results of this parallel pre-trends test for the restricted sample, where the coefficients of interest are those of CCT*Post and UCT*Post. I include only years

Table 1: Parallel Pre-trends Test - Restricted Sample

Effect of Programs on Outcomes - Parallel Pre-trends Test - Restricted Sample

VARIABLES	(1) Years of Education	(2) Visited Health Clinic in Past Month
Post	-19.32***	0.240
	[0.617]	[0.336]
CCT*Post	-0.187	0.156
	[0.235]	[0.200]
UCT*Post	-0.293***	0.0543
	[0.096]	[0.042]
Observations	3,886	432
R-squared	0.853	0.101
Number of hhid	1,769	216

Notes: Column (1) specifies a linear regression and column (2) specifies a linear probability model. All specifications include controls for sex, age, year-of-birth fixed effects, and household fixed effects. Sample includes only respondents from the IFLS-3(2000) and IFLS-4 (2007/2008) that appear in both datasets. Stars denote significance at the 1, 5, and 10 percent levels based on unadjusted p-values. Robust standard errors clustered at the household level are shown in parentheses.

of education and whether or not a child visited a health clinic in the past month as my outcome variables due to sparsity of data for the other outcome variables in the IFLS-3 (2000) dataset. While this is not ideal, it is reasonable to assume that these variables are representative of education and health variables respectively in general, and thus we can extrapolate these results to estimate pre-trends for the other education- and health-related outcome variables. However, it is concerning that no outcome variables related to labor are included. Due to limitations of data, I have no way of checking the pre-trends for this category and so will have to assume parallel pre-trends for the sake of my analysis, but this limitation should be noted.

For the CCT treatment group, the coefficients on CCT*Post in both columns (1) and (2) are not significantly different from zero, though their magnitudes are fairly large – column (1) shows that the CCT treatment is associated with a 0.187 year decrease in years of education, while column (2) shows that it is associated with a 15.6 percentage point increase in the likelihood of a child having visited a health clinic in the past month. These coefficients are not negligible, indicating that for this group pre-trends were not perfectly parallel. However, since

they are not significantly different from zero, it is plausible that instead pre-trends were somewhat reasonably consistent with parallel trends prior to the CCT treatment.

For the UCT treatment group, the coefficient on UCT*Post in column (2) is also not significantly different from zero and is rather small, a 5.43 percentage point increase in the outcome likelihood, and so, for the outcome of whether or not a child visited a health clinic in the past month, pre-trends were indeed consistent with parallel trends for this group prior to the UCT treatment. However, the coefficient on UCT*Post for the regression on years of education in column (1) is negative and significant at the 1% level, indicating that, prior to the UCT treatment, the group that would eventually receive the UCT treatment were systematically receiving fewer years of education over time than the untreated group. The sign of this bias is ambiguous – if this trend continued, it would exert a downward bias on the UCT treatment group, but if there were reversion to the mean, for instance where those who dropped out of school reenroll and get back in compliance with compulsory education, then this might exert a positive bias on the UCT treatment group.

Of note is the fact that the coefficient on Post, the topmost variable, in the regression on years of education in column (1) is large, negative, and significant at the 1% level. Without controls, this coefficient should be positive, reflecting the fact that years of education should be increasing with age. Controlling for age and year-of-birth fixed effects should in theory account for this effect, so the fact that this coefficient is now negative suggests some kind of overcorrection. However, I include both age-related controls to be aligned with the rest of the specifications in the main analysis and will be cautious about interpretations in light of these issues.

E. Summary Statistics

Table 2 shows summary statistics from the IFLS-5, the post-treatment dataset, for variables at the individual level, comprising indicators for receipt of the CCT and UCT

Table 2: Summary Statistics for Individual Variables - IFLS-5 (2014/2015)

Variable	Obs	Mean	Std. Dev	Min	Max
Received CCT	9,366	0.0405	0.197	0	1
Received UCT	9,366	0.278	0.448	0	1
Monthly Income per Capita (IDR)	9,366	940,063.90	1,135,697	1,666.67	30,000,000
log(Monthly Income per Capita)	9,366	13.291	1.05	7.419	17.217
Owns a TV	9,366	0.944	0.229	0	1
Household Size	9,366	3.011	0.922	2	8
Age	9,366	6.535	4.296	0	14
Female	9,366	0.486	0.499	0	1
Visited Health Clinic in Past Month	1,218	0.917	0.276	0	1
Received Required Immunizations	1,218	0.735	0.442	0	1
Hours Worked in Past Week	1,689	5.052	5.746	0	21
Years of Education	4,849	4.57	2.473	1	11
Sufficient Education by Age	4,849	0.924	0.265	0	1
Attended ECED Program	4,849	0.768	0.422	0	1
Math Exam Score	1,205	7.521	1.516	1.75	10
Bahasa Indonesia Exam Score	1,205	7.861	1.025	0.8	10

Note: The above variables from the IFLS-5 (2014/2015) are coded at the individual level and cover child respondents aged below 15. Rows 1 and 2 are indicators for whether or not a child received the CCT (PKH) and UCT (BLT/BLSM) treatments respectively. Rows 3 and 4 are continuous variables measuring a household's estimated wealth in Indonesian Rupiah. Owns a TV is an indicator for whether or not a child's household owns a television set. Visited Health Clinic in Past Month is an indicator for whether or not a child went to a hospital or a posyandu (health clinic) for immunizations, health checkups, developmental activities, or other health-related activities in the past month. Received Required Immunizations is an indicator for whether or not a child has received the recommended suite of immunizations (BCG, DPT, measles, and hepatitis B). Hours Worked in Past Week is a continuous variable measuring the number of hours a child worked in the previous week and has been winsorized to the bottom 95%. Sufficient Education by Age is an indicator variable for whether a child has the expected number of years of education for their age. Attended ECED Program is an indicator variable for whether or not a child attended kindergarten, playgroups, or other early childhood education and development services. National exam results are scored out of 10.

programs, measures of wealth and other household and individual characteristics, and outcome variables grouped according to health, labor, and education. It is important to note here the differences in the number of observations for each variable. This is due to a limitation in the survey where not every respondent answered every question, and thus several outcome variables have numerous missing entries. To alleviate this constraint, I limit my analysis to subsets of the full sample that have no missing entries within each category of health, labor, and education. While this allows for a consistent sample size within each category, rigorous comparisons across categories cannot be made because the same individuals do not appear in each subset.

Table 3: Summary Statistics for Individual Variables - IFLS-4 (2007/2008)

Variable	Obs	Mean	Std. Dev	Min	Max
Received CCT	7,316	0.0297	0.169	0	1
Received UCT	7,316	0.308	0.462	0	1
Monthly Income per Capita (IDR)	7,316	473,850.50	597,412.10	2500	10,500,000
log(Monthly Income per Capita)	7,316	12.577	1.038	7.824	16.17
Owns a TV	7,316	0.841	0.366	0	1
Household Size	7,316	3.119	1.063	2	8
Age	7,316	6.342	4.282	0	14
Female	7,316	0.477	0.499	0	1
Visited Health Clinic in Past Month	1,139	0.912	0.283	0	1
Received Required Immunizations	1,139	0.764	0.425	0	1
Hours Worked in Past Week	1,498	5.437	5.032	0	21
Years of Education	3,182	4.284	2.299	1	10
Sufficient Education by Age	3,182	0.831	0.375	0	1
Attended ECED Program	3,182	0.559	0.496	0	1
Math Exam Score	665	7.209	1.059	2.6	9.998
Bahasa Indonesia Exam Score	665	6.677	1.329	2.67	10

Note: The above variables from the IFLS-4 (2007/2008) are coded at the individual level and cover child respondents aged below 15. See Table 2 for a detailed description of each variable.

Table 3 shows summary statistics for the same set of treatment, household and individual characteristics, and outcome variables from the IFLS-4, the pre-treatment dataset, while Table 4 shows those from the IFLS-3, which is used to test the identifying assumption in Section III.D. Table 4 does not include the indicator for whether a child attended any ECED programs since that question was not yet included in this earlier iteration of the survey.

Table 4: Summary Statistics for Individual Variables - IFLS-3 (2000)

Variable	Obs	Mean	Std. Dev	Min	Max
Received CCT	6,142	0.0199	0.139	0	1
Received UCT	6,142	0.225	0.417	0	1
Monthly Income per Capita (IDR)	6,142	189,533.20	204,708	1000	2,750,000
log(Monthly Income per Capita)	6,142	11.664	1.083	6.908	14.827
Owns a TV	6,142	0.678	0.467	0	1
Household Size	6,142	3.391	1.227	2	10
Age	6,142	6.709	4.433	0	14
Female	6,142	0.489	0.499	0	1
Visited Health Clinic in Past Month	697	0.954	0.209	0	1
Received Required Immunizations	697	0.669	0.471	0	1
Hours Worked in Past Week	80	14.275	7.454	0	21
Years of Education	2,910	4.334	2.291	1	12
Sufficient Education by Age	2,910	0.761	0.427	0	1
Math Exam Score	645	6.449	1.535	1.998	9.83
Bahasa Indonesia Exam Score	645	6.904	1.235	2.4	9.8

Note: The above variables from the IFLS-3 (2000) are coded at the individual level and cover child respondents aged below 15. See Table 2 for a detailed description of each variable.

To get a sense of the differences in variables by receipt of the CCT and UCT programs, i.e., treatment status coded as described above, Table 5 shows the means of outcome variables and household and individual characteristics across the two treatments for the IFLS-4 and IFLS-5 datasets. Within the CCT treatment, the data shows that the CCT-treated group was, on average, poorer, as measured by both income per capita and television ownership, from larger households, and older than those untreated by the CCT. Similarly, the UCT-treated group was also, on average, poorer, from larger households, and older than those untreated by the UCT. Within all three datasets, there are many fewer households treated by the CCT as compared to

Table 5: Means of Variables by Treatment Status

	CCT		No CCT		UCT		No UCT		
Variable	**Obs**	**Mean**	**Obs**	**Mean**	**Obs**	**Mean**	**Obs**	**Mean**	**Source**
Years of Education	249	4.723	4,600	4.562	1,597	4.673	3,252	4.519	IFLS-5 (2014/2015)
Sufficient Education by Age	249	0.879	4,600	0.927	1,597	0.884	3,252	0.944	
Attended ECED Program	249	0.659	4,600	0.774	1,597	0.646	3,252	0.828	
Math Exam Score	56	7.378	1,149	7.528	390	7.309	815	7.622	
Bahasa Indonesia Exam Score	56	7.749	1,149	7.867	390	7.733	815	7.922	
Visited Health Clinic in Past Month	43	0.93	1,175	0.917	284	0.898	934	0.923	
Received Required Immunizations	43	0.651	1,175	0.738	284	0.708	934	0.743	
Hours Worked in Past Week	89	7.64	1,600	4.908	597	5.725	1,092	4.683	
Age	379	7.821	8,987	6.481	2,603	7.478	6,673	6.173	
Female	379	0.507	8,987	0.485	2,603	0.483	6,673	0.487	
Household Size	379	3.303	8,987	2.999	2,603	3.189	6,673	2.943	
log(Monthly Income per Capita)	379	12.549	8,987	13.322	2,603	12.854	6,673	13.459	
Owns a TV	379	0.897	8,987	0.946	2,603	0.911	6,673	0.957	
Years of Education	82	3.963	3,100	4.292	1,029	4.033	2,153	4.404	IFLS-4 (2007/2008)
Sufficient Education by Age	82	0.732	3,100	0.834	1,029	0.75	2,153	0.869	
Attended ECED Program	82	0.427	3,100	0.563	1,029	0.426	2,153	0.624	
Math Exam Score	13	5.718	652	6.696	179	6.459	486	6.757	
Bahasa Indonesia Exam Score	13	6.597	652	7.221	179	7.011	486	7.281	
Visited Health Clinic in Past Month	45	0.844	1,094	0.915	352	0.872	787	0.93	
Received Required Immunizations	45	0.733	1,094	0.765	352	0.719	787	0.784	
Hours Worked in Past Week	43	7.721	1,455	5.369	550	5.756	948	5.251	
Age	217	5.899	7,099	6.355	2,252	6.644	5,064	6.207	
Female	217	0.512	7,099	0.476	2,252	0.485	5,064	0.474	
Household Size	217	3.558	7,099	3.106	2,252	3.325	5,064	3.028	
log(Monthly Income per Capita)	217	11.764	7,099	12.602	2,252	12.018	5,064	12.825	
Owns a TV	217	0.604	7,099	0.848	2,252	0.694	5,064	0.906	

Note: Table shows the means of outcome variables and household and individual characteristics by treatment of the CCT and UCT programs respectively. Variables above the line come from the IFLS-5 (2014/2015) and variables below the line come from the IFLS-4 (2007/2008). See Table 2 for a detailed explanation of each variable.

those treated by the UCT – the proportion of CCT-treated households varies between 2% and 4%, while the proportion of UCT-treated households varies between 22% and 30%. This difference can be partially explained by the fact that, as their children grow older, some households in my earlier datasets age out by the later datasets, and thus, since treatment is coded based on indicator variables in the IFLS-5 (2014/2015), some households may not be considered treated despite receiving the CCT later on. Generally, however, it appears that the UCT reached many more households despite similar targeting structures within both programs.

A potential issue that would hinder the validity of my analysis would be if the interaction terms of Post with the CCT and UCT treatments respectively are collinear, as that would make their individual effects impossible to separate from one another. In actuality, they should not be perfectly correlated because of different targeting procedures and eligibility rules for each program. I check the correlations between these interaction terms for both my main analysis and the parallel pre-trends test, and find that the correlations, 0.29 and 0.27 respectively, are reasonably low. Thus, it does not appear that multicollinearity is an issue.

As described in Section II.C, mistargeting of funds was a severe issue for the UCT program, and, while it was not explicitly mentioned as a concern in relation to the CCT, my data suggests similar levels of mistargeting for the CCT program. Figures 2 and 3 show kernel density plots of the log of monthly income per capita by CCT and UCT treatments respectively. If mistargeting was not an issue, we would see less overlap between the two treatment groups since program eligibility was in part determined by a household's income. Instead, we see a significant amount of overlap, suggesting that many households that should have received each program did not, and that many households that should not have received each program did.

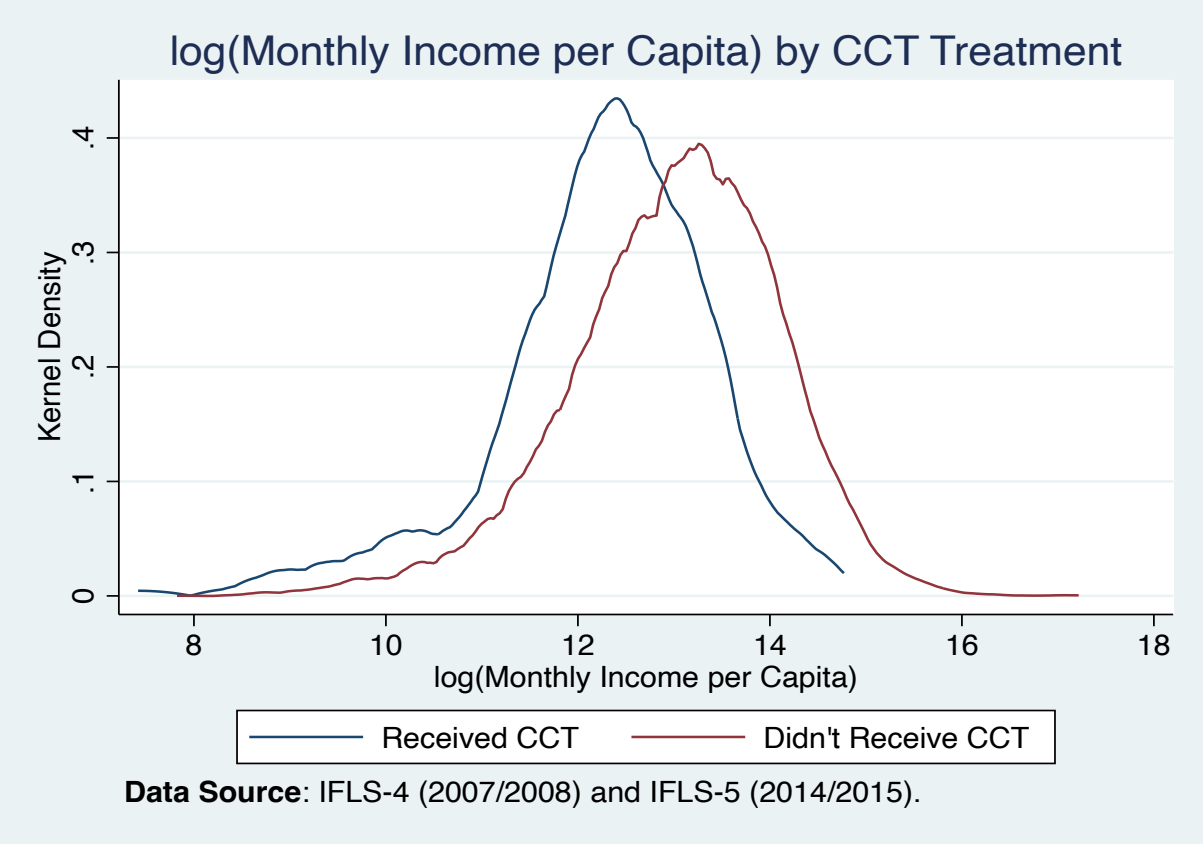

Figure 2: Kernel Density Plot of log of Monthly Income per Capita by CCT Treatment

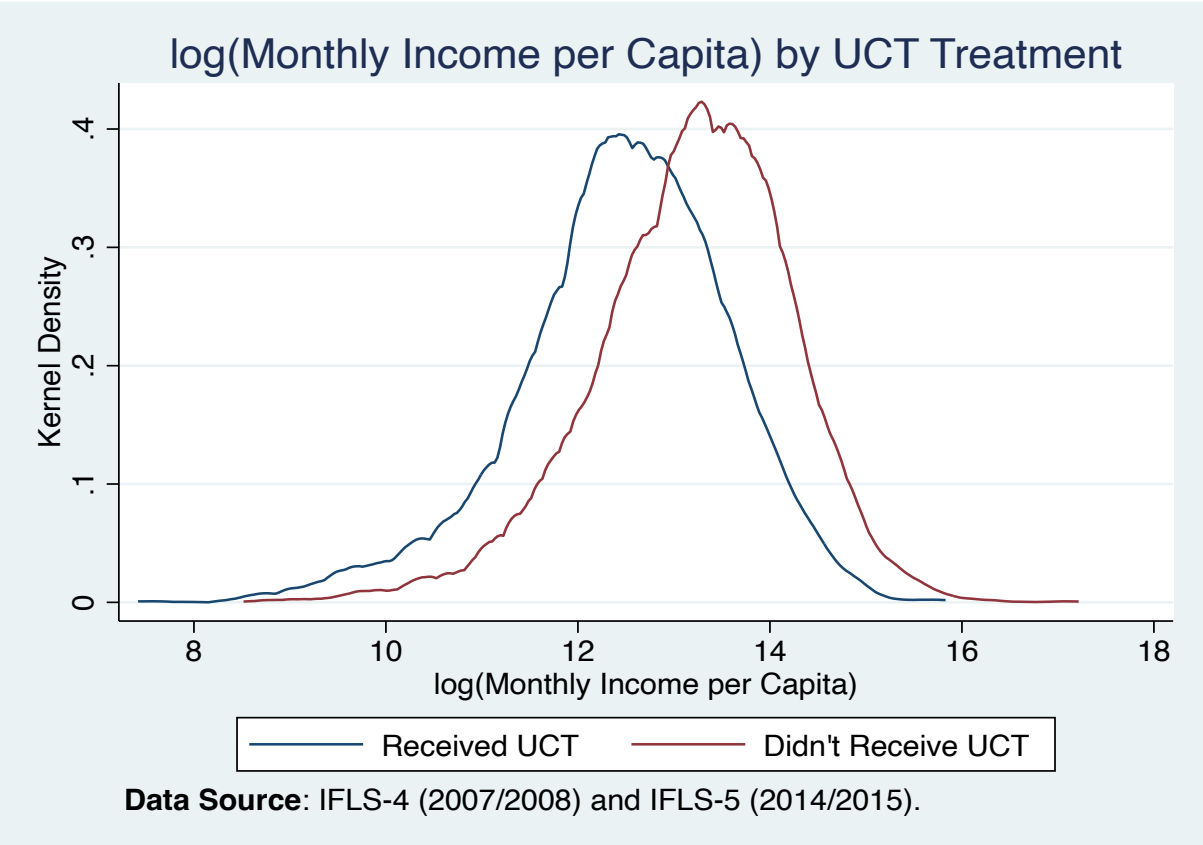

Figure 3: Kernel Density Plot of log of Monthly Income per Capita by UCT Treatment

Tables 6 and 7 show summary statistics for the restricted sample of panel data across the IFLS-4 (2007/2008) / IFLS-5 (2014/2015) and the IFLS-3 (2000) / IFLS-4 (2007/2008) datasets. Since this is panel data on the same set of children between time periods, later samples are older, have more years of education, and work more. Since it is unlikely that no child drops out of school or repeats a grade, the decrease in the sufficient education by age variable is similarly unsurprising.

Table 6: Summary Statistics for Panel Data - IFLS-4 / IFLS-5

	IFLS-4 (2007/2008)		IFLS-5 (2014/2015)	
Variable	**Obs**	**Mean**	**Obs**	**Mean**
Received CCT	3,272	0.0416	3,272	0.0431
Received UCT	3,272	0.334	3,272	0.323
Age	3,272	3.465	3,272	10.322
Female	3,272	0.472	3,272	0.472
Years of Education	428	1.584	428	8.488
Sufficient Education by Age	428	0.977	428	0.949
Attended ECED Program	3,167	0.262	3,167	0.787
Visited Health Clinic in Past Month	176	0.926	176	1
Hours Worked in Past Week	81	3.148	81	7.321

Note: Includes only respondents from the IFLS-4 (2007/2008) and the IFLS-5 (2014/2015) that appear in both datasets. See Table 2 for a detailed description of each variable.

Table 7: Summary Statistics for Panel Data - IFLS-3 / IFLS-4

	IFLS-3 (2000)		IFLS-4 (2007/2008)	
Variable	**Obs**	**Mean**	**Obs**	**Mean**
Received CCT	2,167	0.0235	2,167	0.025
Received UCT	2,167	0.287	2,167	0.304
Age	2,167	3.091	2,167	10.461
Female	2,167	0.483	2,167	0.482
Years of Education	198	1.424	198	8.162
Sufficient Education by Age	198	0.979	198	0.874
Visited Health Clinic in Past Month	216	0.939	216	1

Note: Includes only respondents from the IFLS-3 (2000) and the IFLS-4 (2007/2008) that appear in both datasets. See Table 2 for a detailed description of each variable.

IV. Results

A. Education

The raw comparison of averages with no controls for years of education is shown in Table 8. I find that the difference-in-difference estimators for the CCT treatment and UCT treatment are increases of 0.38 years and 0.48 years respectively, indicating that both programs were associated with positive changes in years of education.

Table 9 shows the results from the linear and probit regression estimations for the variables years of education, sufficient education by age, ECED program attendance, and national exam scores in Bahasa Indonesia and mathematics, and these include the full set of controls from equation (1) in Section III.C. Considering the effects of the CCT program first, I find that the difference-in-difference estimate of the CCT is a 0.0625 year decrease in the years of schooling achieved by an individual, as specified in column 1, although this result is not significantly different from zero. The probit regressions for sufficient education by age and ECED program attendance – columns (2) and (3) – tell a similar story, with coefficients on the CCT interaction term of -0.0215 and -0.015 respectively. Translating these into marginal effects on outcome likelihood, I find that an individual in the post-treatment sample that received the CCT is 0.00375 percentage points less likely to have a sufficient number of years of education for their age, and 0.00458 percentage points less likely to have attended ECED programs as a young child – as with column (1), neither estimate is significantly different from zero. The effect on national exam scores, while not significant, is positive, with a treated

Table 8: Means of Years of Education by Cohort and Treatment Status, Main Experiment

	Treatment Status					
Cohort	**CCT Treated**	**CCT Untreated**	**Difference**	**UCT Treated**	**UCT Untreated**	**Difference**
IFLS-4 (2007/2008)	3.96	4.33	0.37	4.05	4.45	0.4
IFLS-5 (2014/2015)	4.64	4.63	-0.01	4.68	4.6	-0.08
Difference	0.68	0.3	0.38	0.63	0.15	0.48

Note: Sample includes only respondents from the IFLS-4 (2007/2008) and IFLS-5 (2014/2015), and estimation does not include any controls.

Table 9: Regression Results - Education

Effect of Programs on Education Outcomes

VARIABLES	(1) Years of Education	(2) Sufficient Education by Age	(3) Attended ECED Program	(4) Bahasa Exam Score	(5) Math Exam Score
Post	5.860***	0.375***	0.843	-0.0269	-0.00860
	[0.235]	[0.054]	[2.387]	[0.220]	[0.442]
Received CCT	0.0474	-0.0175	0.0766	-0.423	-0.711*
	[0.108]	[0.154]	[0.172]	[0.293]	[0.402]
Received UCT	-0.309***	-0.273***	-0.261***	-0.170*	-0.108
	[0.044]	[0.064]	[0.061]	[0.098]	[0.126]
CCT*Post	-0.0625	-0.0215	-0.0150	0.445	0.734
	[0.121]	[0.196]	[0.184]	[0.309]	[0.452]
UCT*Post	0.178***	-0.0210	-0.168**	0.0337	-0.0932
	[0.050]	[0.083]	[0.071]	[0.113]	[0.157]
Observations	8,039	8,039	8,039	1,687	1,687
R-squared	0.876			0.120	0.108

Note: All specifications include controls for sex, household size, television ownership, and the log of monthly income per capita, and columns (1) and (3) to (5) also include controls for age and year-of-birth fixed effects. Columns (1), (4), and (5) estimate linear regressions, while columns (2) and (3) estimate probit regressions. Sample includes only respondents from the IFLS-4 (2007/2008) and IFLS-5 (2014/2015). Stars denote significance at the 1, 5, and 10 percent levels based on unadjusted p-values. Robust standard errors clustered at the household level are shown in parentheses.

individual in the post-treatment sample achieving, on average, increases of 0.445 points (p-value: 0.150) and 0.734 points (p-value: 0.105) for scores in Bahasa Indonesia and mathematics respectively as compared to an untreated individual in the pre-treatment sample.

The results in Table 9 suggest that the CCT program had little to no effect on a child's education-related outcomes and may have even had a negative impact on the years of education achieved by a child. This is surprising given the program's explicit inclusion of school attendance within its conditions, which would suggest a strongly positive impact of the program. This result might be explained by the lack of enforcement of the conditions until at least 2010, which would lead to households being considered treated without their children actually attending school and thus a negative result as seen here. While I lack the data to investigate the actual enforcement of conditions, this result would be in line with the conclusions drawn by de Brauw and Hoddinott (2011).

The probit estimate in column (2) of Table 9 shows a negative and almost zero impact of the CCT program on having achieved a sufficient number of years of education for a child's age. This is reasonable due to the fact that my sample only includes children aged below 15, the age of completing junior secondary school, and while there are drop-offs in school attendance at transition points between primary, junior secondary, and senior secondary school, school attendance rates are generally much higher at lower levels of education. This suggests that all children, no matter their treatment status, were attending school and keeping up with their education.

The results in columns (3) to (5) of Table 9 give a sense of some of the unintended consequences of conditionality, specifically those unintended consequences related to education outcomes. The probit estimate in column (3) shows a negative and near-zero impact of the CCT program on whether or not a child attended ECED programs while in their early childhood. My hypothesis here was that, akin to the Labeled Cash Transfers as investigated by Benhassine et al. (2015), the inclusion of primary and above education within the CCT's conditions would have a positive externality effect on other related education outcomes. Thus, by receiving the CCT and seeing the explicit focus on primary and above education, heads of households might then alter other education investment decisions for their children and opt to send them for ECED programs, for instance. The near-zero result shown above suggests that this is not the case and is consistent with the possibility that instead explicit conditions are necessary in order to induce desirable behavior change in households.

The effects on exam scores are positive and significant at the 15% level, and this suggests that being treated by the CCT may potentially have resulted in improved performance at school above any improvements from simply having more years of education or attending more school, since columns (1) and (2) of Table 9 have negative or near-zero results. The exact mechanism through which this increased performance is achieved is unclear, but some

potential reasons might be that children in households that receive the CCT spend less time working and thus have more time to focus on schoolwork, or that the labeling of the program towards education then leads individuals to spend more time on it.

Looking now at the effects of the UCT program, I find that the effect of being in the post-treatment sample and receiving the UCT is a 0.178 year increase in the years of schooling achieved by an individual as compared to one that is untreated in the pre-treatment sample, and that this effect is significant at the 1% level. The probit models in columns (2) and (3) of Table 9 yield negative coefficients of -0.0210 and -0.168 respectively, with the former not significantly different from zero and the latter significant at the 5% level. The marginal effects on the likelihood of each outcome variable show that a treated individual in the post-treatment sample is 0.0037 percentage points less likely to have a sufficient number of years of education for their age and 0.0512 percentage points less likely to have attended ECED programs in their early childhood as compared to an untreated individual in the pre-treatment sample. Neither of the effects on national exam scores is significantly different from zero, with an individual that received the UCT in the post-treatment sample achieving, on average, 0.0337 more points on the Bahasa Indonesia exam and 0.0932 fewer points on the mathematics exam as compared to an untreated individual in the pre-treatment sample.

The significantly positive impact of the UCT program on the years of education achieved by a child suggests that, despite the lack of explicit conditions for receiving payments from the UCT program, households increased their education investments as a result of the temporary cash assistance. That we see such a positive effect indicates that the cumulative impact of temporary transfers, as with the BLT (2008) and then BLSM (2013) programs, can be strongly positive when it comes to effects on education. What is surprising, however, is that treated households were less likely to send their children to ECED programs. This might be due to the fact that the benefits of ECED programs are not as widely known as compared to

the benefits of basic primary education, for instance, and so households might only choose to alter their investment decisions towards primary and above education.

The role of conditionality here is difficult to parse out due to the presence of bias and the lack of condition enforcement in the CCT program, particularly for the years of education outcome. However, it does appear that conditionality is important even only to the end of labeling cash transfer programs towards some goal, as evidenced by the increases in exam scores amongst the CCT group. Conversely, the results also suggest that labeling alone may be insufficient, and that clear and specific conditions are necessary to induce behavior change as seen by the lack of an effect on ECED investment decisions.

The dramatic reduction in the coefficients of CCT*Post and UCT*Post in column (1) of Table 9 as compared to the raw data in Table 8 is of note, especially since the effect of the

Table 10: Regression Results - Years of Education - Stepwise Addition of Controls

Stepwise Addition of Controls - Effect of Programs on Years of Education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Years of Education	Years of Education	Years of Education	Years of Education	Years of Education	Years of Education	Years of Education
Post	0.117*	0.107*	0.106	0.0751	0.0734	0.169***	6.446***
	[0.065]	[0.065]	[0.065]	[0.067]	[0.067]	[0.025]	[0.043]
Received CCT	-0.0809	-0.0687	-0.0666	-0.0567	-0.0586	0.0437	0.0474
	[0.202]	[0.205]	[0.205]	[0.204]	[0.205]	[0.109]	[0.108]
Received UCT	-0.367***	-0.324***	-0.323***	-0.291***	-0.290***	-0.313***	-0.309***
	[0.082]	[0.084]	[0.084]	[0.085]	[0.085]	[0.045]	[0.044]
CCT*Post	0.163	0.156	0.155	0.163	0.155	-0.0468	-0.0625
	[0.250]	[0.252]	[0.252]	[0.252]	[0.252]	[0.121]	[0.121]
UCT*Post	0.508***	0.477***	0.477***	0.471***	0.471***	0.184***	0.178***
	[0.114]	[0.115]	[0.115]	[0.115]	[0.114]	[0.050]	[0.050]
Owns a TV		0.206**	0.205**	0.179**	0.177**	0.284***	0.279***
		[0.082]	[0.082]	[0.084]	[0.083]	[0.049]	[0.048]
Household Size			-0.00814	0.00297	0.00409	-0.0129	-0.0102
			[0.024]	[0.024]	[0.024]	[0.013]	[0.013]
log(Monthly Income per Capita)				0.0502*	0.0521**	0.0568***	0.0550***
				[0.026]	[0.026]	[0.011]	[0.011]
Female					0.225***	0.208***	0.205***
					[0.050]	[0.020]	[0.020]
Age						0.918***	
						[0.004]	
Observations	8,039	8,039	8,039	8,039	8,039	8,039	8,039
R-squared	0.006	0.007	0.007	0.007	0.009	0.873	0.876

Note: Column (7) includes year-of-birth fixed effects. Sample includes only respondents from the IFLS-4 (2007/2008) and IFLS-5 (2014/2015). Stars denote significance at the 1, 5, and 10 percent levels based on unadjusted p-values. Robust standard errors clustered at the household level are shown in parentheses.

CCT program is now near-zero and negative. I perform a stepwise addition of controls to this linear regression to test which controls are driving this change, the results of which are shown in Table 10.

The largest changes in the coefficients for the interaction terms are seen in column (6), which controls for age, and column (7), which includes year-of-birth fixed effects. These columns also show a very large increase in their R-squared values. Thus, it appears that age-related controls are the cause of the dramatic reduction in the coefficients of interest. Since years of education will naturally increase as a child gets older, assuming that child continues to go to school, the raw data faces an upward bias from the omission of these controls, and thus their inclusion is necessary and the downward change they exert makes sense.

B. Health

The raw comparison of averages with no controls for the immunization requirement indicator variable is shown in Table 11. I find that the difference-in-difference estimator for the CCT treatment is a 5 percentage point decrease in the average probability of a child having received all of their required immunizations, indicating that the CCT program was associated with a negative change in the likelihood of a child receiving all of their immunizations, and that the estimator for the UCT treatment is a 7 percentage point increase in that same probability, indicating a positive change associated with the UCT program.

Columns (1) and (2) in Table 12 show the probit regression estimates for indicators for whether a child has visited a health clinic in the past month and whether they have received

Table 11: Means of Immunization Requirement by Cohort and Treatment Status, Main Experiment

	Treatment Status					
Cohort	**CCT Treated**	**CCT Untreated**	**Difference**	**UCT Treated**	**UCT Untreated**	**Difference**
IFLS-4 (2007/2008)	0.72	0.75	0.03	0.69	0.77	0.08
IFLS-5 (2014/2015)	0.63	0.71	0.08	0.7	0.71	0.01
Difference	-0.09	-0.04	-0.05	0.01	-0.06	0.07

Note: Sample includes only respondents from the IFLS-4 (2007/2008) and IFLS-5 (2014/2015), and estimation does not include any controls.

Table 12: Regression Results - Health and Labor

Effect of Programs on Health and Labor Outcomes

VARIABLES	(1) Visited Health Clinic in Past Month	(2) Received Required Immunizations	(3) Hours Worked in Past Week
Post	-0.244	-0.135	-0.0339
	[0.189]	[0.150]	[0.341]
Received CCT	-0.224	0.230	2.155***
	[0.251]	[0.258]	[0.790]
Received UCT	-0.337***	-0.349***	0.0327
	[0.124]	[0.116]	[0.296]
CCT*Post	0.468	-0.320	-0.118
	[0.399]	[0.347]	[1.099]
UCT*Post	0.0944	0.236	0.259
	[0.172]	[0.151]	[0.419]
Observations	2,358	2,358	3,188
R-squared			0.135

Note: All specifications include controls for age, sex, household size, television ownership, the log of monthly income per capita, and year-of-birth fixed effects. Columns (1) and (2) estimate probit regressions, while column (3) estimates a linear regression. Sample includes only respondents from the IFLS-4 (2007/2008) and IFLS-5 (2014/2015). Stars denote significance at the 1, 5, and 10 percent levels based on unadjusted p-values. Robust standard errors clustered at the household level are shown in parentheses.

all of their required immunizations including controls as specified in equation (1) in Section III.C. For the CCT program, the coefficients of the interaction term are 0.468 and -0.32 respectively, with neither significantly different from zero. The marginal effects on the likelihood of each outcome indicate that an individual that received the CCT in the post-treatment sample is 0.070 percentage points more likely to have visited a health clinic in the past month and 0.078 percentage points less likely to have received all of their required immunizations. These results suggest that the CCT program had no impact on either of these outcome variables, despite the fact that both are included within the program's conditions. As above, this is likely due to the fact that conditions were not enforced until at least 2010, and so households would continue to receive the program despite not sending their children to health check-ups or for their immunizations.

The coefficients of the UCT interaction term in columns (1) and (2) are 0.0944 and 0.236 respectively, and while neither result is significantly different from zero, the estimate for whether or not a child has received all of their required immunizations has a fairly low p-value of 0.116. Translating these estimates to their marginal effects on each outcome's probability, I find that a post-treatment individual that received the UCT program is 0.014 percentage points more likely to have visited a health clinic in the past month and 0.058 percentage points more likely to have received all of their required immunizations as compared to a pre-treatment individual that received neither program.

The results for the UCT program are somewhat contradictory, as they indicate that, without clear stipulations for increasing health-seeking behaviors, households do not send their children more frequently to health check-ups yet also are more likely to send them to receive immunizations, thus both supporting and detracting from the necessity of including conditions in cash transfer programs. This is perhaps due to the fact that the suite of immunizations included in my outcome variable are all supposed to be received before the age of 1, and so households, having completed those immunizations early in their children's lives, then bring their children to check-ups less frequently as they grow older.

Due to these contradictory effects, it is unclear whether conditionality is important in increasing health-seeking behaviors. What can be inferred from these results, however, is that, without explicit conditions, households might make incorrect or improper investment decisions due to information asymmetries. For instance, an individual might only believe that a child should go for regular health-checkups in the first few years of its life rather than for their entire childhood, and thus send them less frequently as they grow older. Including conditions would make clear that health check-ups are necessary for a longer amount of time, thus solving the problem of information asymmetry as well as inducing desirable behavior change.

C. Labor

Table 13 shows the raw comparison of averages with no controls for the number of hours a child worked in the past week. I find that the difference-in-difference estimator for the CCT treatment is a 0.12 hour increase in the number of hours a child worked in the past week, and, for the UCT treatment, a 0.3 hour increase in the number of hours worked.

Column (3) in Table 12 shows the linear regression estimate with controls for the number of hours a child has worked in the past week, where I find that the CCT treatment is associated with a child working, on average, 0.118 fewer hours, and that the UCT treatment is associated with a child working, on average, 0.259 more hours, though neither result is significantly different from zero. The difference in the effect of the CCT program as compared to the raw data in Table 13 is noteworthy, and so I again perform a stepwise addition of controls to this linear regression to test which controls are driving this dramatic decrease, the results of which are shown in Table 14.

As above in Section IV.A, it appears that the age-related controls in columns (6) and (7) are the reason for the decrease in the effect size of the CCT program. Similarly, as with years of education, it is reasonable to assume that as children grow older, they will work more hours in a given week, and so the inclusion of these age-related controls is necessary. What is surprising, however, is that the effect of the UCT program on the number of hours worked a child has worked in the past week does not appear to face that large of an upward bias in the raw data as we see a similar magnitude of the coefficient on UCT*Post, namely, 0.3 versus

Table 13: Means of Number of Hours Worked in the Past Week by Cohort and Treatment Status, Main Experiment

	Treatment Status					
Cohort	**CCT Treated**	**CCT Untreated**	**Difference**	**UCT Treated**	**UCT Untreated**	**Difference**
IFLS-4 (2007/2008)	7.72	5.42	-2.3	5.8	5.31	-0.49
IFLS-5 (2014/2015)	7.36	4.94	-2.42	5.58	4.79	-0.79
Difference	-0.36	-0.48	0.12	-0.22	-0.52	0.3

Note: Sample includes only respondents from the IFLS-4 (2007/2008) and IFLS-5 (2014/2015), and estimation does not include any controls.

Table 14: Regression Results - Hours Worked in Past Week - Stepwise Addition of Controls

Stepwise Addition of Controls - Effect of Programs on Hours Worked in Past Week

VARIABLES	(1) Hours Worked in Past Week	(2) Hours Worked in Past Week	(3) Hours Worked in Past Week	(4) Hours Worked in Past Week	(5) Hours Worked in Past Week	(6) Hours Worked in Past Week	(7) Hours Worked in Past Week
Post	-0.592**	-0.539**	-0.476**	-0.364	-0.346	-0.0895	4.239***
	[0.243]	[0.244]	[0.240]	[0.256]	[0.256]	[0.246]	[0.371]
Received CCT	2.136**	2.133**	1.996**	1.983**	1.972**	2.103***	2.155***
	[0.921]	[0.911]	[0.891]	[0.887]	[0.881]	[0.797]	[0.790]
Received UCT	0.344	0.0441	-0.0451	-0.153	-0.127	0.0302	0.0327
	[0.302]	[0.305]	[0.306]	[0.311]	[0.310]	[0.294]	[0.296]
CCT*Post	0.176	0.286	0.348	0.294	0.194	-0.0604	-0.118
	[1.255]	[1.239]	[1.234]	[1.234]	[1.228]	[1.103]	[1.099]
UCT*Post	0.422	0.628	0.636	0.652	0.630	0.255	0.259
	[0.441]	[0.437]	[0.437]	[0.437]	[0.436]	[0.418]	[0.419]
Owns a TV		-1.334***	-1.364***	-1.280***	-1.280***	-1.217***	-1.233***
		[0.369]	[0.364]	[0.367]	[0.364]	[0.344]	[0.343]
Household Size			0.493***	0.450***	0.450***	0.435***	0.442***
			[0.124]	[0.130]	[0.131]	[0.130]	[0.130]
log(Monthly Income per Capita)				-0.170	-0.167	-0.141	-0.142
				[0.104]	[0.104]	[0.098]	[0.099]
Female					0.692***	0.789***	0.801***
					[0.194]	[0.185]	[0.185]
Age						0.659***	
						[0.034]	
Observations	3,188	3,188	3,188	3,188	3,188	3,188	3,188
R-squared	0.013	0.018	0.026	0.027	0.031	0.129	0.135

Note: Column (7) includes year-of-birth fixed effects. Sample includes only respondents from the IFLS-4 (2007/2008) and IFLS-5 (2014/2015). Stars denote significance at the 1, 5, and 10 percent levels based on unadjusted p-values. Robust standard errors clustered at the household level are shown in parentheses.

0.259. It thus appears that children in households that received the UCT program did not increase their working hours as they grew older as much as children in households that received the CCT program did, though the exact reason for why such a phenomenon is seen is unclear and untestable.

While neither result in column (3) of Table 12 is significant, the signs of the coefficients tell an interesting story regarding the unintended effects of conditionality. That individuals who received the CCT worked fewer hours might be explained by the fact that they are indeed spending more time on their schoolwork, in line with the increases in exam scores amongst those treated by the CCT in Section IV.A. The increase in the number of hours worked amongst the UCT group is more surprising, but one possible reason might be that households

that receive the UCT payments then use them towards procuring or upkeeping productive assets, such as a small business or land for agriculture, which would then be worked in part by that household's children. In a study of productive asset grants as part of a child labor elimination program in the Philippines, Edmonds and Theoharides (2020) find that households generally rely upon family members to work the asset and thus that these grants in actuality led to increases in child labor. A similar phenomenon may be occurring here, though I lack the data to investigate this hypothesis.

Considered together, this suggests that without carefully formed conditions for cash transfer programs that require households to prefer long-term investment decisions (such as in education or health), households might instead allocate their resources towards relatively more short-term investments such as working productive assets. This highlights the importance of conditionality in inducing behavior change towards those considered desirable by the government rather than letting households decide for themselves where to best allocate their resources, since that might then lead to increases in child labor as seen here even though we also see increases in education.

D. Restricted Sample

The results from the analysis of the restricted sample of panel data is shown in Table 15. Within years of education in column (1), I find that the CCT program is associated with a child receiving, on average, 0.0154 fewer years of education, not significantly different from zero, and that the UCT program is associated with a child receiving, on average, 0.142 fewer years of education, with this result significant at the 10% level. Despite the fact that this is panel data, the failures of condition enforcement as described in the CCT program would still be reflected in the estimated impact, thus potentially leading to the near-zero effect seen here. The negative impact of the UCT program may be driven by the fact that the analysis of the restricted sample now considers only variation within households, suggesting that children in

Table 15: Regression Results, Restricted Sample

Effect of Programs on Outcomes - Restricted Sample

VARIABLES	(1) Years of Education	(2) Sufficient Education by Age	(3) Attended ECED Program	(4) Visited Health Clinic in Past Month	(5) Hours Worked in Past Week
Post	0.341	-0.00415	-0.281**	-0.403**	0.760
	[0.440]	[0.012]	[0.115]	[0.159]	[11.389]
CCT*Post	-0.0154	0.0625**	-0.0346	0.105	4.980*
	[0.207]	[0.027]	[0.024]	[0.144]	[2.998]
UCT*Post	-0.142*	-0.0584**	-0.0154	0.0445	0.442
	[0.075]	[0.029]	[0.032]	[0.052]	[1.633]
Observations	856	856	856	352	162
R-squared	0.992	0.024	0.026	0.155	0.362
Number of hhid	462	462	462	192	87

Note: All specifications include controls for sex and household fixed effects, and all specifications less column (2) also include controls for age and year-of-birth fixed effects. Columns (1) and (5) estimate linear regression models while columns (2) to (4) estimate linear probability models. Sample includes only respondents from the IFLS-4 (2007/2008) and IFLS-5 (2014/2015) that appear in both datasets. Stars denote significance at the 1, 5, and 10 percent levels. Robust standard errors clustered at the household level are shown in parentheses.

households that received the UCT program then either stopped attending school or had to repeat a year of school at some point. This is in line with the linear probability model results for the sufficient education by age outcome in column (2), which show that the CCT program increased the likelihood of having a sufficient number of years of education for an individual's age by 6.25 percentage points and that the UCT program decreased that same likelihood by 5.84 percentage points, both of which are significant at the 5% level. This second result then indicates that the CCT program might actually be associated with a positive effect on education despite the lack of significance in column (1).

Column (3) shows that both programs had a negative effect on the probability of attending ECED programs in early childhood. The CCT program decreased the likelihood of attending ECED programs for a post-treatment individual that received the treatment by 3.46 percentage points, significant at the 15% level, while the UCT program decreased that same likelihood by 1.54 percentage points, not significantly different from zero. This again suggests

that specific and clear conditions are necessary in order to induce certain behavior changes, and that my hypothesized positive externality effect is not in play here.

Neither of the results in column (4) are significantly different from zero, and these magnitudes show that the CCT program led to a 10.5 percentage point increase in the likelihood of a child visiting a health clinic in the past month, and that the UCT program led to a similar 4.45 percentage point increase in that likelihood. While these results are not significant, that they are positive and the magnitude of the CCT effect is larger than UCT effect suggest that the inclusion of conditions might lead to a more positive impact on certain behavioral changes, thus underscoring the importance of including conditionality in cash transfer programs.

Column (5) tells a different story from the labor analysis of the full sample in Section IV.C above. While I still find that the UCT program increased the number of hours worked in the past week by 0.442 hours for an individual in the post-treatment group who received that program, with this result not significantly different from zero, I instead find that the CCT program also increased the number of hours worked in the past week by 4.98 hours, significant at the 10% level. This is a substantial increase in the number of hours worked and is opposite to the negative effect found previously. This might be explained by the fact that since the children in this set of panel data naturally grew older between the two datasets, they would work for more hours the older they get, although the age-related controls should have accounted for this. However, the fact that I find such a positive increase from the CCT program suggests that perhaps children are substituting away from education in favor of labor, or that a similar phenomenon as documented by Edmonds and Theoharides (2020) is at play. Since this now considers only variation within households, the positive effect seen here may also be driven by the fact that older children often take up more responsibilities within the household, some of which may be related to labor or providing for the household in some capacity.

V. Discussion and Conclusion

A. Limitations

The most significant limitation of this analysis, as described throughout this paper, has to do with the data on which my investigation is based. Due to the structure of the survey, respondents who were aged 15 years or older were asked different questions from children aged below 15 years. Since my analysis deals solely with children and given the different time periods in which my data was fielded, numerous children were aged out of my dataset and then, when resurveyed as adults, were asked a different set of questions. As such, I am unable to track these respondents across my outcome variables. While the number of children across the three datasets, i.e., the total sample size for each dataset, is similar due to younger children joining in while older children were aged out, this presents a serious challenge as I had to resort to comparing means of different groups of respondents as was done in my main analysis. Reducing my sample to panel data of only those respondents present in both pre- and post-treatment datasets then severely limits my sample size. However, because of the fact that my analysis only covers children below 15 years of age, it is not affected by the expansion of compulsory schooling in 2013 to include senior secondary school as described in Section II.B since only those 16 years or older would attend senior secondary school.

Another limitation has to do with the structure of the programs themselves. Due to mistargeting of funds, neither program showed sharp income or expenses cutoffs between those who received the programs and those who didn't, as shown in Figures 2 and 3 in Section III.E, where theoretically and based on the design of each program a cutoff should have been present. This meant that receipt of each program had to be determined solely off of the survey indicators for each program, where ideally, I would be able to use income or some other measure of wealth to determine program eligibility. Doing so would have expanded the treatment group in

my earlier datasets since I would not have to rely on only those households that appear in the later dataset.

Similarly, but affecting only the CCT program, the lack of condition enforcement poses a difficult problem for interpretation of my results. More information on condition enforcement is needed in order to determine the role that conditionality truly plays. That conditions were not always enforced also makes it more difficult to parse out the actual role of conditionality since I am unable to determine when exactly households were meeting the stipulated conditions. Despite this limitation, I am still able to infer some of the impact of conditionality, thus partially elucidating its role in cash transfer programs as was the goal of this study.

B. External Validity

Indonesia provides a unique setting for a case study for a number of reasons. It is one of the most populous countries in the world and thus also has one of the largest education systems in the world. It is made up of over 18,000 islands and thus there exists a high amount of cultural heterogeneity within the country. It is also one of the most rapidly developing countries in the developing world and so is poised to advance into a crucial player in the global economy within the next decade. Due to these distinctive qualities, it is difficult to say how much of any result found in Indonesia can be extrapolated to other countries and the rest of the world. On the one hand, similar programs might yield different results in smaller or more homogeneous economies, thus limiting the external validity of this analysis. Furthermore, these programs were instituted either in response to or soon after negative economic shocks, namely rising oil prices in 2005 as well as any effects of the 2008 financial crisis, and thus my results may have been affected by households' responses and adjustments to these shocks. On the other hand, I believe the general takeaways will still hold: the need for clear, specific, and well-

crafted conditions in order to induce desirable behavior change and a cognizance of any unintended consequences, whether related to labor or otherwise, of such cash transfer programs.

C. Policy Relevance

With both CCTs and UCTs becoming increasingly popular tools to tackle poverty in developing countries, governments should strive to understand as best as possible the mechanisms through which they work in order to ensure that the aims of such programs are met and that any unintended and, in particular, negative consequences are limited. The first question governments should ask of themselves is whether or not to include conditions in their cash transfer programs. The current literature, as outlined in Section II.A, suggests that UCTs, by nature of not requiring households to meet any stipulations, can reach a larger group of households, but that, generally, CCTs are more effective at improving conditioned outcomes, and so governments should decide whether they are prioritizing effects or outreach when choosing what type of program to implement. For the purposes of providing concrete recommendations, I assume that governments will prioritize effects and thus choose to implement a CCT, and so I identify three takeaways from my analysis that provide useful guidelines for the implementation of conditions in cash transfer programs, namely: the strict and thorough enforcement of conditions, the careful construction of explicit conditions, and, where the first two takeaways are too difficult or unrealistic to achieve, the usefulness in and of itself of labeling a cash transfer program with some condition.

First, the enforcement of conditions is hugely important to the success of CCTs as it ensures that the desirable behavior changes governments want to induce are actually being undertaken by beneficiary households. Without proper enforcement, there is no guarantee that the children of these households are indeed attending more school or improving their health-seeking behaviors, to take the example of conditions from the PKH, and so the CCT becomes nothing more than an LCT as described by Benhassine et al. (2015). While an LCT itself can

still useful, as will be elaborated on below, such an outcome would mean that governments get only the benefits of an LCT while continuing to incur any costs of the partial enforcement of the CCT, which, as we have seen in Caldés et al. (2006), can be very expensive. In other words, governments would still incur all of the costs of condition enforcement but only receive part of the benefits of including conditions, and so might observe that those partial benefits do not outweigh the costs. Of course, condition enforcement in practice is much more difficult, particularly when beneficiary households are also from more rural parts of the country, and so governments should first assess their own capabilities of condition enforcement before deciding between implementing a CCT or an LCT.

Second, should a government decide to pursue a CCT, conditions must be carefully constructed and be as explicit as possible as to the behavior changes that government is hoping to induce. Governments should not rely on any purported positive externalities of the conditions they do include to take place, as is seen in the lack of an effect on ECED attendance amongst children in this analysis. In this example, an explicit condition should be included that requires young children to attend ECED programs in order for beneficiary households to receive the cash transfer. On the point of unintended consequences as well, conditions should be explicit. The main unintended consequence examined in my analysis is that of child labor. While my analysis showed a decrease in child working hours for the CCT treatment, such a result is not guaranteed, and as such conditions might include stipulations that a child does not work, or, at least, only works in a safe way under parent supervision. The drawback here is that the more specific and numerous conditions become, the harder they will be for households to meet, and thus the problem of denying benefits to households that might need it the most becomes all the more severe. In addition, increasing the number of conditions will also increase the difficulty of enforcing those conditions. Thus, governments should seek to optimize

between specificity in conditions and ease of meeting conditions such that as many households as possible are able to meet the conditions and receive the cash transfers.

Third, where the above two points are untenable, labeling a cash transfer program towards some end, of education or health, for instance, can be more effective than simply implementing a UCT. Within this analysis, the fact that conditions were not enforced for several of the years I am studying but desirable behavior change was partly still seen suggests, in line with Benhassine et al. (2015), that labeling a program towards the goals of education and health may be sufficient to induce those changes. Such an action would also solve the problems of information asymmetries, where households may now learn of the value of certain education- or health-seeking behaviors without the need for campaigns or other similar information dissemination programs. To take the example of ECED programs, households might start to understand the value of early childhood education even if a strict condition of ECED attendance is not enforced, thus achieving in part the goal of a CCT while saving on the costs of condition enforcement. The lack of strict conditions would also mean that more households can qualify for receiving cash transfers, thus improving the outreach of such programs. However, since less is known of the efficacy of LCTs and, within my analysis, I hypothesize that the lack of condition enforcement may have led some of my results to face a downward bias, this action should only be taken as a secondary option in place of a CCT or only after LCT pilot studies have been completed to verify their effectiveness.

D. Conclusion

By comparing the effects of two similar sets of cash transfer programs in Indonesia, one a CCT and the other a UCT, I am able, in this study, to assess the impact and role of conditionality in welfare programs. While my analysis is limited by my data and issues with the implementation of the programs themselves, my results do, in part, illuminate some of this impact. As is previously documented, it does appear that including conditions is more effective

at improving those conditioned outcomes, and that, without carefully constructed conditions, their inclusion might lead to unintended negative consequences. Any addition to the growing body of evidence comparing these two types of welfare programs can only improve the rollout of future programs, which will in turn improve the benefit to the poor households that these programs aim to assist. With an improved understanding of the role of conditionality, governments might be better able to reduce poverty and, in particular, the intergenerational, persistent effects of poverty. As CCTs continue to grow in popularity across the developing world, governments should take their implementation as an opportunity to test the differing efficacies of CCTs as compared to UCTs, perhaps through randomized controlled pilot studies or other similar methods of assessment. Such investigations will be the object of future work.

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