Alternative Cash Transfer Delivery Mechanisms: Impacts on Illness and Health Clinic Utilization in Burkina Faso*

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Abstract

We conducted a unique randomized experiment to estimate the impact of alternative cash transfer delivery mechanisms on household demand for health services in rural Burkina Faso. The two-year pilot program randomly distributed cash transfers that were either conditional or unconditional and were given to either mothers or fathers. Families under the conditional cash transfer schemes were required to obtain regular preventative care at local health clinics (growth monitoring and vaccinations) for all children under 60 months old. There were no such requirements under the unconditional programs. We find that conditional cash transfers have a significant impact (relative to unconditional cash transfers) on reducing the probability a child was ill during the preceding month, and conditional on being sick the child was more likely to visit the health clinic and was subsequently sick for fewer days. Transfers given to mothers or fathers showed similar magnitude beneficial impacts on improving child health.

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1. Introduction

Poor health is widespread among children in low-income countries. Often, such ill health is coupled with poor access to health care, either because of supply side or demand side constraints. These poor health outcomes negatively affect physical growth and cognitive development, with potential long-term consequences. For example, children who are in poor health are less likely to enroll in school, or enter school later, and when they enroll they tend to perform worse than children in good health (Moock and Leslie 1986; Brooker et al. 1999; Glewwe et al. 2001). This in turn affects productivity later in life (Dasgupta 1993; Strauss and Thomas 1995; Schultz 2001). It is estimated that illness incidence and other health risks prevent approximately 200 million children in low income countries from reaching their full potential in cognitive development (Grantham-McGregor et al. 2007). Obviously, identifying mechanisms which could improve health outcomes among children will have large payoffs, not only at the individual level, but also in term of overall economic progress as well.

Conditional cash transfer (CCT) programs have become one of the most popular social sector interventions in developing countries.[1] While the program design details vary, all programs transfer resources to poor households conditional on the household taking active measures to increase the health and human capital of their children (enrolling their children in school, taking them for regular health care visits, and receiving necessary immunizations). In making transfers conditional, this type of intervention seeks to encourage human capital accumulation and break a vicious cycle in which poverty is transmitted across generations. In

[1] A growing number of countries, in particular in Latin America, but also in Asia have implemented such programs. As of 2009, the following countries in Latin America have implemented CCT programs: Argentina, Bolivia, Brazil, Chile, Columbia, Costa Rica, Dominican Republic, Ecuador, El Salvador, Guatemala, Honduras, Jamaica, Mexico, Nicaragua, Panama, Paraguay, and Peru, while these Asian countries have started CCT programs: Bangladesh, Cambodia, India, Indonesia, Pakistan, Philippines, and Turkey (Fiszbein and Schady 2009). In Africa, two CCT pilot programs (in South Africa and Kenya) have been implemented, but both focus exclusively on orphans and HIV households and neither has yet been rigorously evaluated.

addition, these programs attempt to provide poor households with a consumption floor and to improve a household's asset base and income generating potential. Most CCT programs transfer funds to the mother rather than the father, with the hope that mothers use the funds more wisely.

The CCT programs contrast with unconditional cash transfer (UCT) programs, which do not impose conditionality constraints. CCT programs represent a "top-down" approach in which an outside authority decides what is best for poor children and provides incentives to their parents to achieve these objectives. On the other hand, UCT programs assume that parents, once an income constraint is removed, are in a better position to make appropriate decisions regarding their child's health and education. Also, CCT programs are significantly more costly to administer per recipient than UCT programs, due to the expenses associated with monitoring that the conditions are met. Unconditional cash transfers act through increased income, so that health outcomes should improve as long as the income elasticity of demand for health is positive (Parker and Wong, 1998; Wagstaff and van Doorslaer, 2000). In addition to pure income effects, conditional cash transfers also seek to alter household behaviors by conditioning the stipend on specific behaviors (Fiszbein and Schady, 2009).

There is credible evidence that both transfer schemes can substantially improve child health outcomes (for CCTs in Colombia (Attanasio et al. 2005); Ecuador (Paxson and Schady 2008); Nicaragua (Maluccio and Flores 2005; Macours, Schady, and Vakis 2008); and for UCTs in South Africa (Case, Hosegood, and Lund 2005; Duflo 2003)). As a result many developing countries, including Burkina Faso, have adopted (or are contemplating) cash transfer programs.

In this paper, we present evidence of the health impacts from a unique cash transfer pilot program in rural Burkina Faso, the Nahouri Social Protection Program (NSPP). The NSPP incorporated a random experimental design to evaluate the relative effectiveness of the following

four social protection programs targeting poor households in rural Burkina Faso: conditional cash transfers given to fathers, conditional cash transfers given to mothers, unconditional cash transfers given to fathers, and unconditional cash transfers given to mothers. Like many cash transfer programs in low income countries, the NSPP aims to alleviate short-term poverty while providing households with incentives to invest in long-term human capital growth, especially in child health and education. Our evaluation focuses on health outcomes for children 0 to 5 years old, in particular on the incidence of illness in the 30 days preceding the survey, utilization of health services, and how long a child remains ill for each illness episode.

Burkina Faso offers a unique setting for exploring the effects of cash transfers on rural children's health outcomes. Even by African standards, child health outcomes in Burkina Faso are considered to be poor. In 2003, 38 percent of children under 5 experienced some form of fever or respiratory infection in the two weeks preceding the national Demographic and Health Survey. Among children who had been ill, only 30 percent received any care from a health professional (INSD/ORC Macro, 2004). Hence, not only was the incidence of illness high but utilization of health services was low. Therefore, identifying policy interventions that would reduce the incidence of illness and/or improve utilization of health care services would be valuable to policy makers.

We find that young children in families that received conditional cash transfers are 49 to 59 percent less likely to have been sick during the month before the survey and conditional on being ill were 23 to 29 percent more likely to have used a health clinic compared to comparable children in control villages. We find similar magnitude beneficial impacts for children in families where the mother or father received the cash transfers, indicating that at least for health clinic utilization, the cash transfer recipient's gender is not a critical factor influencing outcomes.

Our experimental design has several distinctive features differentiating it from previous studies. First, we randomly test conditional and unconditional cash transfers in the same context. While there are studies treating the respective merits and disadvantages of each transfer scheme, studies that use a randomized control trial to compare the two schemes in the same environment are extremely rare (Fiszbein and Schady 2009). We are able to provide new insights into whether the determining factor driving impacts is the income effect or the conditionality. If the income effect is the determining factor, then we expect conditional and unconditional cash transfers to have the same impact. Second, unlike many cash transfer interventions which paid the transfers to mothers, we randomly selected mothers or fathers to receive the payments. Hence we contribute to the long-standing question on the effects of resources controlled by the mother or the father on child human capital. If resources are completely pooled within the household, then transfers to mothers or fathers would have exactly the same effect.

The most closely related evaluation project (Baird, McIntosh, and Ozler 2011) examines the impact of conditional and unconditional cash transfers on adolescent girls' schooling in Malawi.[2] However, the cash transfer program we evaluate is significantly different in at least two ways, in addition to differences in geographic context between southern Africa (with high HIV/AIDS rates) and western Africa (with much lower rates). First, the cash transfer program evaluated in this paper is focused on health outcomes for young children under age five, while the Malawi project was concerned with educational and marriage outcomes of adolescent girls. Second, our program design also includes a gender dimension, both in the fact that transfers were randomly given either to the child's father or mother, and that we are able to examine impacts of cash transfers on boys as well as girls, both of which were absent in the Malawi evaluation.

[2] We are aware of one other study in progress that also examines the impact of conditionality on educational outcomes in Morocco.

From a policy-making perspective, we also address the feasibility of conditional cash transfer in the sub-Saharan African context. Since CCT programs rely on a certain level of administrative capacity (the ability to target households, plan meetings to notify households of their obligations and rights, monitor household compliance and conditionality, and transfer funds to families), there is a debate on whether these programs, which have been successful in Latin America, can be successfully implemented by African central or local governments (Samson 2006; Schubert and Slater 2006; Szekely 2006; Freelander 2007).[3]

The remainder of the paper is organized as follows. In Section 2, we review the relevant literature about cash transfers. Section 3 describes the experimental design of the cash transfer pilot program and presents some descriptive statistics. In Section 4, we discuss our empirical identification strategy and Section 5 presents the main results. Section 6 concludes.

2. Cash Transfer Programs

Fiszbein and Schady (2009) provide an extensive review of conditional cash transfers, and in different contexts, cash transfers have been shown to improve school attendance, health outcomes, and other socio-economic outcomes. Evaluations of the impact of CCT programs on children's health (which are all from Latin American programs) tend to either focus on final outcomes such as child height and weight or instead focus on intermediate inputs such as nutrition, health status, or the use of preventative health care services. Overall, impact results tend to be somewhat mixed and vary depending on the specific details about the cash transfer program, the age range of the child studied, the evaluation method, and whether the evaluations were measuring short or medium term impacts. Gertler (2004) and Behrman and Hoddinott

[3] The cost of administrative capacity is also significant. Caldes, Coady and Maluccio (2006) estimate that for the CCT program in Honduras, Programa de Asignación Familiar, $0.50 of program costs are spent for every $1 of transfers given to households, while for Nicaragua's CCT program, $0.63 of program costs are spent for every $1 of household transfers. Mexico's CCT program only incurred $0.11 of program costs for every $1 of transfers.

(2005) in Mexico, Attanasio et al. (2005) in Colombia, and Maluccio and Flores (2005) in Nicaragua find significant improvements in children's height due to CCT programs. However, Morris, Olinto et al. (2004) in Brazil, Macours, Schady, and Vakis (2008) in Nicaragua, and Paxson and Schady (2008) in Ecuador do not find significant height improvements. Even focusing just on intermediate inputs still yields mixed results. CCT programs often require the regular use of preventative health care services, but evaluations for Chile (Galasso 2006), Ecuador (Paxson and Schady 2008), and Nicaragua (Maluccio and Flores 2005) show no significant change in the number of visits to the health clinic for preventative reasons. This contrasts with evaluations in Colombia (Attanasio et al. 2005), Honduras (Morris, Flores et al. 2004), Jamaica (Levy and Ohls 2007), and Nicaragua (Macours, Schady, and Vakis 2008).

In contrast to CCT programs, unconditional cash transfers to poor targeted households are less costly (as there is no need to enforce conditionality) and evidence demonstrates their effectiveness. An evaluation by Aguero, Carter, and Woolard (2006) of the South African Child Support Grant, an unconditional cash transfer targeted to families with young children, finds an improvement in children's nutrition. Evaluations of South Africa's pension program (a type of unconditional cash transfer) show improvements in children's health (Case and Deaton 1998; Duflo 2003). However, none of these UCT program evaluations were randomized, which yields the possibility that unobservable factors correlated with receipt of the treatment also influence outcomes. For a UCT program in Ecuador that incorporated an explicit randomized evaluation, results show significant positive improvements for schooling (Araujo and Schady 2006) and nutritional status and cognitive development (Paxson and Schady 2008). However, in the implementation of the UCT program, program administrators stressed the importance of schooling, which led some households to believe that conditionality was part of the cash transfer.

In addition, 42 percent of households in the control group received cash transfers, raising doubts about the randomized evaluation design (Schady and Rosero 2007).

However, even for programs for which rigorous impact evaluation results are available, it is impossible to determine whether the observed impacts on child health are due to the income transfers provided by the program, or whether child health improved because the program forced households to undertake specific behaviors in order to receive the funds. Several recent studies attempt to directly compare CCT and UCT programs. De Janvry and Sadoulet (2005) present theoretical arguments for why a CCT program should have a larger impact on the conditioned outcomes than a UCT program. Kakwani, Soares, and Son (2005) present ex ante simulations arguing that UCT programs will have a small impact in Africa. Finally, for some CCT recipients in Mexico's Progresa program, the conditionality requirements were not enforced and results show educational attainment was reduced (for these pseudo-UCT households) but only for students transitioning from primary to secondary school (de Brauw and Hoddinott 2007).

Most cash transfer programs (conditional or unconditional) pay stipends to mothers. This decision is often explicitly or implicitly motivated by previous research indicating that resources under the mother's control have a stronger positive impact on a child's health and schooling than when those resources are controlled by the father.[4] Non-unitary household theoretical bargaining models predict that cash transfers given to mothers will increase their bargaining power within the household and lead to expenditures that better reflect the mother's preferences (Chiappori 1988; Lundberg and Pollak 1993). However, a significant difficulty in the intrahousehold bargaining literature to empirically prove this point is the potentially endogenous nature of men's

[4] Seminal empirical papers by Schultz (1990) and Thomas (1990) as well as more recent work by Lundberg, Pollak, and Wales (1997), Duflo and Udry (2004), Attanasio and Lechene (2002), and Bobonis (2009) provide empirical evidence supporting this finding. For an overview of the intrahousehold bargaining literature see Strauss and Thomas (1995); Haddad, Hoddinott, and Alderman (1997); and Strauss, Mwabu, and Beegle (2000).

and women's income. By randomly allocating transfers to mothers or fathers, we are able to more convincingly address this shortfall.

3. Burkina Faso Nahouri Social Protection Program

3.1. Program Design

The pilot cash transfer program was conducted in Nahouri province in southern Burkina Faso, located approximately 100 miles from the capital, Ouagadougou. The 75 villages in Nahouri province that each have a primary school were randomly allocated to the following five groups: (i) conditional cash transfers given to the father (CCTF), (ii) conditional cash transfers given to the mother (CCTM), (iii) unconditional cash transfers given to the father (UCTF), (iv) unconditional cash transfers given to the mother (UCTM), and (v) a control group.[5] There were 15 villages in each treatment arm and in the control group, and only poor households were eligible to receive a cash transfer.[6] Once villages were randomly assigned to the five groups defined above, poor households in the treatment villages were randomly assigned to receive that particular type of cash transfer.

In our three rounds of surveys (baseline, one-year follow-up, two-year follow-up) conducted in June 2008, June 2009, and June 2010, we interviewed all poor households that were eligible to receive a transfer in each of the treatment villages and who were randomly selected to receive the transfer. In each of these four groups of 15 villages, we interviewed approximately 540 poor households randomly selected to receive transfers. The control group consisted of 615

[5] Due to the low primary school enrollment rates in Burkina Faso, the program intervention focused exclusively on primary schooling as opposed to also covering secondary schools.

[6] Immediately prior to the intervention and baseline survey, we conducted an extended household census in every village to collect information from each household about household living structure (cement or mud brick walls, metal or straw roof, flooring, access to latrine), household asset ownership (plow, cart, draft animals, motorcycle, radio), whether the head of household ever attended school, whether the household grows cotton, and whether there was a weekly market in the village. This information was combined with a Burkina Faso nationally representative household survey to calculate a predicted consumption level for each household and compare that with the national poverty line to determine whether a household is considered poor or non-poor, and subsequently is eligible to receive the cash transfer.

randomly selected poor households that did not receive a cash transfer in the 15 control villages where no households received cash transfers.

For households randomly assigned to a CCT scheme, for their children under age six, receiving the transfer required quarterly visits to the local health clinic for preventive health care (growth monitoring and vaccinations), while for children age seven to fifteen, receiving the transfer required school enrollment with an attendance rate above 90 percent each quarter. For families randomly assigned to a UCT program, the mother or father received a quarterly stipend for each child, and there were no requirements or conditions linked with receiving the stipend.[7] For each child under age six, in the CCT and UCT programs, the mother or father would receive 4000 FCFA per year, distributed in four quarterly payments. Using the exchange rate during the 2008 baseline of 415 FCFA = $1 USD, the annual transfer would be worth approximately $9.64, which is 9 percent of household per capita expenditures. For each child age 7 to 10 (or in grades 1 to 4), the mother or father would receive 8000 FCFA per year, while for each child age 11 to 15 (or in grades 5 or higher), the mother or father would receive 16,000 FCFA per year, distributed in four quarterly payments. The program design assumes that each of the treatment groups would receive equal amounts of resources per capita over the two-year transfer program period, if households randomly allocated to the conditional cash transfers fully satisfied the conditionality. In practice, because there was not full compliance with conditionality, the households under the UCT programs, on average, received more resources per capita.

3.2. Descriptive Statistics

Households in the Nahouri region are predominantly subsistence farmers growing sorghum and groundnuts and have mean annual per capita expenditures of approximately $111 USD.

[7] To minimize child fostering in response to the program introduction and reduce any potential sample attrition (see Akresh 2009 for evidence on the relationship between income shocks and child fostering), eligibility for cash transfers was based only on the children who were present in the household at the time of the baseline survey.

Summary statistics in Table 1 show that on average, there are 6.9 members in each household, of whom 1.4 are children under 60 months and 2.3 are children age 5 to 15. Focusing on the children under 60 months that are used in this paper's analysis, 49 percent of them are female and their average age is 28 months old.[8] Only 18 percent of these children's parents (father or mother) have ever been enrolled in school. Eleven percent of these children experienced an illness in the month preceding the survey. Conditional on being sick, 81 percent of them were taken to the health clinic for treatment, as opposed to using a traditional healer or not receiving any treatment, and the children were sick on average 6.8 days for each illness.

In Table 2, we use baseline data to test the balance of the randomization experiment. We first present the mean of the specific variable measured at the baseline for the control group and each of the four treatment arms (abbreviated in the table as follows: CCTF = conditional cash transfers given to fathers; CCTM = conditional cash transfers given to mothers; UCTF = unconditional cash transfers given to fathers; UCTM = unconditional cash transfers given to mothers). In the subsequent columns, we present p-values from a Wald test comparing the treatment arm with control group. Finally, in the last column (under the control group heading), we present the p-value for an F-test of the joint test that the means of the five groups are equal. Results show balance across the dependent variables used in this paper, as well as across characteristics for children, parents, and health clinics.

Household attrition was very low between the baseline and one-year follow-up survey (1.42 percent), and increases slightly when comparing the baseline and two-year follow-up survey (4.62 percent). In Table 3, we explore the relative differences between attritor and non-attritor households. Columns 1 presents means of the household-level characteristics from the

[8] Note that of the 2749 poor eligible surveyed households in the treatment and control villages, only 2131 households are used in the health analysis for this paper, because the remaining households did not have any children under 60 months old.

baseline survey for the sample of households that were followed from the baseline to the two-year follow-up survey (non-attritors). Column 2 presents means for the sample of attritor households, and column 3 presents the average difference in characteristics between attritors and non-attritors, as well as a test of whether the difference is statistically significant. Results suggest that attrition is not likely random, as attritors are more likely to come from smaller households, with fewer adults, are younger, have fewer wives, and are more likely to be female headed households. However, what is more relevant for our analysis is whether attrition differs across treatment and control groups. In columns 4 to 7 of Table 3, we present difference-in-differences regressions for each characteristic comparing the difference between attritors and non-attritors in each specific treatment arm with the same difference between attritors and non-attritors in the control group, and we find few significant differences.

4. Empirical Identification Strategy

The key question we want to answer is whether cash transfers improve the health status of young children in recipient households. We first focus on the impact of transfers on whether a child has been sick in the 30 days preceding the survey. Next, we examine whether, conditional on being sick, the child is more likely to receive medical care at a health clinic. Finally, we measure how much transfers reduce the number of days the child is sick for a given illness episode.

The experimental design provides a strong identification strategy and allows us to attribute any differences in outcome indicators (health status and health seeking behavior) between the treatment and control groups to the impact of the program. Because we have access to baseline and follow-up data and to control for any possible differences across villages in the baseline, we use a difference-in-differences model. We focus on the program effect on the treated households (ATE), and we estimate the following regression:

(1) $$y_{iht} = \beta_0 + \beta_1 T_1 + \beta_2 T_2 + \sum_{j=1}^{4} \alpha_{1j} D_j T_1 + \sum_{j=1}^{4} \alpha_{2j} D_j T_2 + \beta_3 X_{iht} + \nu + \varepsilon_{iht}$$

where y_{iht} is a health related outcome for child i in household h in year t, T_i is a round indicator for the first and second follow-up surveys, D_j is the treatment indicator that takes the value one if a child lives in a household which was randomly selected to receive treatment j and zero otherwise, X_i is a vector of child characteristics (gender and age), ν is a village fixed effect and ε is an error term.[9]

In order to obtain clear comparisons between the different modalities of transfers and to increase statistical power, in the empirical estimations, we pool the treatment arms so that we estimate two distinct specifications of Equation 1 (see Figure 1). First, we consider households that were either randomly selected to receive conditional cash transfers or randomly selected to receive unconditional cash transfers (Panel B of Figure 1). This approach combines into one group the conditional cash transfers given to fathers or mothers and into a second group the unconditional cash transfers given to fathers or mothers. With this specification we focus on whether the conditionality matters and we ignore the intra-household allocation aspects of the design. Second, we consider households that were either randomly assigned to receive the stipends via the mother or via the father (Panel C of Figure 2). This approach combines into one group the conditional and unconditional cash transfers given to fathers and into a second group the conditional and unconditional cash transfers given to mothers. When estimating this specification, we focus on intra-household allocation (whether paying the stipends to the mother or the father leads to different outcomes), and we ignore the role that conditionality might play.

[9] Correlation among the error terms of children in a given village experiencing the same shocks in the baseline or follow-up rounds, combined with the design effect of our village-level before and after treatment, might bias the ordinary least squares standard errors downward, so in all regressions we cluster the standard errors at the village*follow-up level.

Under each specification, the impact of transfer scheme j (j=1, 2) in period t (t=1, 2) is given by α_{tj}, the coefficient of the interaction between the treatment status and the round dummy. Since we randomized at the village level and we control for village fixed effects, the treatment dummies (D_j's) are redundant in Equation 1 and therefore are not included.

5. Empirical Results

In Table 4, we present results from estimating Equation 1 for the three main child health outcomes. Each regression includes village fixed effects as well as controls for the child's gender and year of birth cohort. Columns 1, 2, and 3 focus on the comparison between conditional or unconditional cash transfers, while columns 4, 5, and 6 focus on the comparison between transfers given to fathers or mothers. We find that conditional cash transfers have a larger and generally more statistically significant impact on child health than do unconditional cash transfers. Young children in households that received conditional cash transfers are 6.4 and 5.3 percentage points less likely to have been sick in the month preceding the survey in Rounds 2 and 3, respectively, compared to comparable children in the control villages. This represents declines of 59 percent in the first follow-up survey (Round 2) and 49 percent in the second follow-up survey (Round 3) compared to the baseline level of illness. Relative to the unconditional cash transfers, the impacts on being sick during the preceding month are marginally significantly different for the conditional transfers in Round 2 (p-value testing the equality of means between the Round 2 conditional and unconditional cash transfer is 0.10), but there is no difference in Round 3.

For the regressions that are conditioned on the child being ill, arguably the sub-sample of children who have been sick is not random and hence our approach may introduce selection issues. However, we are able to assess under which conditions the type of selection bias we are

concerned with would not affect our conclusion. First, if we had found that the interventions did not significantly impact the probability of being ill, selection bias would not be a concern. Second, if the interventions reduce the probability of being ill, then children from treatment households would be under-represented in the sub-sample of children who have been ill. In the first case, our estimates would not be biased. In the second case, our estimates will be biased downward, so that if we find a significant impact of the cash transfer on health clinic utilization or illness duration, we are likely underestimating the total impact of the cash transfer.

We find that conditional on being sick during the month before the survey, young children in households that received conditional cash transfers are 23.6 and 18.3 percentage points (29 and 23 percent of the baseline health clinic utilization rates) more likely to have been taken to the health clinic compared to similar children in the control villages. Health clinic utilization is defined in contrast to not receiving any treatment or visiting a traditional healer. Relative to the unconditional cash transfers, receiving conditional cash transfers has a statistically different impact in Round 2 (p-value of the test of equality is 0.05) and a marginally significant different impact in Round 3 (p-value of the test of equality is 0.11). Given children in CCT households are less likely to be sick, meaning that on average they are in better health than the children in control households, the fact that when they are sick they are still more likely to be treated at a health clinic indicates the strength of the conditional cash transfers and likely means that we are underestimating the total impact of conditional cash transfers on child health.

Focusing on illness duration, we observe a similar pattern, showing a drop of 3.6 or 7.2 days in the number of days a child in the CCT household is sick conditional on being ill, although the coefficient for CCT is only statistically significant for the Round 3 interaction term. Likewise, compared to the impact from an UCT, we can only reject equality for the CCT Round

3 interaction term. Summarizing our results, young children ages 0 to 60 months in households that received conditional cash transfers are less likely to become sick in the month before the survey, and conditional on being ill are more likely to use a modern health clinic and have a shorter illness duration than comparable children in the control households that did not receive transfers and compared to children in households that received unconditional cash transfers.[10]

Turning to evaluating the impact of cash transfers given to fathers compared to mothers, in columns 4, 5, and 6, we estimate regressions with the same three health outcomes but with interactions with the round dummies and variables indicating if the cash transfers were given to fathers or to mothers. Overall, results indicate that cash transfers improved child health (less likely to be sick, and conditional on being ill more likely to visit a health clinic and have a shorter illness duration) compared to children in control households, and in general there are not statistically significant differences between giving the transfers to mothers compared to fathers. Looking specifically at the probability a child was sick during the month prior to the survey, there are significant impacts of cash transfers given to fathers in both follow-up rounds and given to mothers in the first follow-up round, and all three of these point estimates are similar in magnitude. We observe similar results for health clinic utilization, which is significantly higher for CTF households in both rounds and CTM households in Round 2 compared to control households, and we cannot reject the equality of the coefficients for cash transfers given to fathers or mothers. Finally, with respect to illness duration, there is some evidence that cash transfers to mothers have a larger impact compared to cash transfers to fathers, but the differential effect is only observed in the first follow-up period.

[10] Results in Table 4 are robust to using illness during the previous year instead of during the previous month as the observation period, but recall error is likely to be reduced in the shorter time period, which is why we focus on that measure in the paper. We also obtain similar results when excluding children ages 0 to 6 months, which is often an exclusive breastfeeding period.

In the next three tables, we examine if there is a heterogeneous impact of cash transfers based on child gender (Table 5), child age (Table 6), or the poverty status of the household (Table 7). In Table 5, we find suggestive evidence that boys benefit more from the cash transfers than girls, although in only a handful of the cases are the boy-girl differences statistically significant. For boys, conditional cash transfers reduce the probability of being sick, increase the likelihood of visiting a health clinic, and reduce the illness duration compared to control households, but not all of the CCT coefficients are statistically significant. Similar health improvements are not observed for girls, where we find almost no statistically significant impacts. Similar gender results are observed when evaluating the cash transfers given to fathers or mothers. For the boys' health clinic utilization, there appears to be a stronger impact of cash transfers to fathers, while the cash transfers given to mothers seems to have a larger impact on the boys' illness duration. For the girls, as in the comparison of CCT and UCT, we find almost no significant impacts, regardless of whether the money was given to mothers or fathers.

Table 6 presents separate results for younger (ages 0 to 23 months) and older (ages 24-59 months) children. We observe statistically significant beneficial impacts for all cash transfers schemes (conditional or unconditional and to mothers or fathers) on reducing the probability of being ill during the previous month for young children, but no significant impacts for older children. For sick children, conditional cash transfers increased the likelihood of using health services for both younger and older children, although the effects are only statistically significant for the older children. This contrasts with the results for the duration of an illness which show significant impacts only for the younger children. Transfers to fathers have a larger impact on health clinic utilization for young children, but not for older children. Overall, transfers of any type improve health status by reducing the likelihood of becoming ill mainly for younger and not

older children, but conditional cash transfers seem to have comparable impacts for younger and older children for health clinic utilization and illness duration.

In Table 7, we find that poor households (defined as those below the median per capita household expenditure level in the baseline survey) benefited the most from the cash transfers in terms of reducing the probability of being ill, but the actual cash transfer delivery mechanism did not matter. Cash transfers whether given as conditional or unconditional or to mothers or fathers achieved comparable impacts on the probability of being sick and the magnitudes of the benefits were large. Despite this, there did not appear to be similar impacts for poor households on health clinic utilization or illness duration conditional on being sick.

In the survey, we also asked whether a child has been sick during the 12 months preceding the survey. A 12 month-recall period gives us the opportunity to explore whether illness incidence and demand for health services vary by season. For instance, households may be less credit constrained in the dry season (after harvest) than in the rainy season when they are still waiting for their crops to mature. Furthermore, the opportunity costs of time for traveling to a distant health care center may be higher during the rainy season.

We investigate how the effects of the cash transfers vary by season in Table 8. Columns 1 to 3 focus on the rainy season, while columns 4 to 6 focus on the dry season. We find that during the rainy season both conditional and unconditional cash transfers have a statistically significant effect on reducing illness incidence. In Round 2, children in households that received conditional cash transfers are 12.7 percentage points less likely to have been sick in the rainy season preceding the survey, whereas children in households that received unconditional cash transfers are 8.8 percentage points less likely to have been sick during the same period, compared to similar children from the control group. In Round 3, conditional and unconditional cash transfers

reduced illness incidence by 7.8 and 9.2 percentage points, respectively, compared to the control group. These results contrast with those for the dry season, where the magnitude of the impact is smaller, and none of the coefficients are statistically significant.

When focusing on the children who were ill and measuring the impacts of transfers on health clinic utilization and illness duration, there are generally significant impacts in the dry season compared to the rainy season, although in many cases we cannot reject the equality of coefficients across seasons. Health clinic utilization and illness duration during the rainy season show almost no statistically significant relationship with cash transfers. This contrasts with those outcomes during the dry season, where conditional on being sick, children in households that received conditional cash transfers are 10 and 19.2 percentage points more likely to be taken to a health center in Rounds 2 and 3, respectively. Unconditional cash transfers increased the likelihood of going to a health center by 10.8 percentage point in Round 3. During the dry season in Round 2, conditional cash transfers reduced illness duration by 9.2 days, while unconditional cash transfers reduced illness duration by 5.9 days.

Similar to the comparison of conditional and unconditional transfers, during the rainy season regardless of delivery mechanism (to fathers or mothers), there were significant impacts on reducing the probability the child was sick. During the Round 2 rainy season, both transfers to fathers and mothers reduced the incidence of illness by 12.2 and 9.1 percentage points, respectively. In Round 3, transfers to fathers reduced illness incidence by 13.5 percentage points. We observe no impact of transfers to mothers or fathers on child illness during the dry season.

Conditional on being sick during the dry season, the transfers to mothers or fathers increased health clinic utilization and reduced illness duration. For children in households where fathers received transfers, health clinic utilization increased by 17.2 percentage points in Round

3. For children in households where mothers received transfers, health clinic utilization increased by 11.5 and 13.1 percentage points in Rounds 2 and 3, respectively. In Round 2, transfers to fathers and mothers reduced illness duration by 7.7 and 7.3 days, respectively. These dry season results contrast with the rainy season where few of the cash transfer impacts are significant.

Overall, the intervention effects differ substantially between the rainy and dry seasons. In the rainy season, the effects of the transfers are manifested essentially through the reduction of illness incidence. Noting that the rainy season coincides with the lean hunger season, it is plausible that food shortages are less pronounced in households that receive transfers, regardless of the delivery mechanism, and children in these households would have adequate nutrition and therefore stronger immune systems. However, the opportunity cost of time is higher during the rainy season as crops are in the field, which might explain why we do not observe any meaningful impact of transfers on utilization of health clinic services. In contrast, during the dry season, food is readily available, so transfers do not influence the incidence of illness. However, with a lower opportunity cost of time, children from treatment households are more likely to be taken to a health center when they are ill compared to children in the control group.

In an environment where the supply of health services is limited, increased supply in the treatment villages could generate the estimated impacts we have uncovered. Because we collected information on the health facilities that households utilize in each village, we can formally test whether supply changed as a consequence of the treatment program. In Table 9, we estimate a regression similar to Equation 1, except that health facilities are the units of observation and our dependent variables are dimensions of the supply of health services.

As in the child health tables, in Panel A, we compare conditional and unconditional cash transfers, while in Panel B, we compare transfers to fathers and mothers. The estimation results

convey two main points. First, only about six percent (5 out of 88) of the estimated coefficients are statistically different from zero, indicating that along most dimensions of health supply services that we have information about, the interventions did not introduce any significant changes between treatment and control villages. Particularly, prenatal care, early childhood services, and provision of nutritional supplements did not change. Second, 4 out of 5 of the coefficients that are significant indicate that supply of health services is actually decreasing in treatment villages. Overall, there is no evidence suggesting that the supply of health services differentially improved in treatment villages.

6. Conclusions

This paper presents evidence of the health impacts from a cash transfer pilot program in rural Burkina Faso, the Nahouri Social Protection Program (NSPP). Our evaluation focuses on health outcomes for children 0 to 5 years old, in particular on the incidence of illness in the 30 days preceding the survey, utilization of health services, and how long a child remains ill for each illness episode. The NSPP incorporated a random experimental design to evaluate the relative effectiveness of the following modalities for delivering cash transfers: conditional versus unconditional and transfers to mothers versus fathers. Families under the conditional cash transfer schemes were required to obtain regular preventative care at local health clinics (growth monitoring and vaccinations) for all young children.

Our results indicate that young children in families that received conditional cash transfers are 49 to 59 percent less likely to have been sick during the month before the survey, and conditional on being ill were 23 to 29 percent more likely to have gone to a health clinic than comparable children in the control households that did not receive transfers and compared to children in households that received unconditional cash transfers. We find similar magnitude

beneficial impacts for children in families where the mother or father received the cash transfers, indicating that at least for health clinic utilization, the cash transfer recipient's gender is not a critical factor influencing outcomes.

From a policy perspective, conditional cash transfers appear to have stronger beneficial impacts. This finding should be balanced with the additional administrative and logistical costs implied by imposing and verifying the conditionality. For the outcomes analyzed in this paper - illness and health care utilization conditional on being sick for children under age 5 - the gender of the recipient does not seem to affect critically the impact of the cash transfers on those health outcomes. Such a conclusion, however, needs to be included in a larger analysis of other health and schooling outcomes as well as other age ranges for the children.

Indeed, the NSPP is a broad social protection pilot program that covers all children until age 15 in the eligible households and aims to improve both their schooling and health outcomes. It should be emphasized that while we focus on children under age 5 in the analysis, those children could have benefited from the (larger) transfers received for their older siblings. It is not clear that by only implementing the part of the intervention targeted for young children (0 to 60 months), similar health results would be obtained.

One limitation of our analysis is that the health outcomes analyzed are self-reported by the parents and it is difficult to ascertain the severity of the reported illnesses. In future work, we will investigate more objectively measured child health outcomes such as anthropometrics and infant mortality.

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Table 1: Summary Statistics of Burkina Faso Nahouri Social Protection Program (NSPP) Evaluation Data

Variable	Mean (1)	Standard Deviation (2)
Household Characteristics		
Household size	6.904	3.141
Number of children under 60 months	1.401	0.898
Number of children age 5 to 15 years	2.307	1.737
Proportion either parent ever enrolled	0.175	0.380
Household expenditures per capita (in FCFA)	45880	61062
Child Characteristics		
Child age (in months)	28.43	17.21
Child gender (1=Female)	0.489	0.500
Sick during the last month	0.109	0.311
Health clinic utilization conditional on sick last month	0.812	0.391
Illness duration conditional on sick last month (in days)	6.802	14.550
Number of Households	2131	
Number of Children	4434	

Notes: Household characteristics are based on the 2131 households that were eligible to receive cash transfers (treatment and control groups) and that have children under 60 months old. Child characteristics are based on the 4434 children under 60 months old in these households. Per capita household expenditures are measured in FCFA (415 FCFA=$1). Data source: Nahouri Social Protection Program (NSPP) Evaluation data from 2008-2010.

Table 2: Baseline Means and Randomization Balance

	CCTF		CCTM		UCTF		UCTM		Control	
	Mean	p-value	Mean	p-value	Mean	p-value	Mean	p-value	Mean	p-value
Dependent variables:										
Sick during last month	0.066	0.652	0.080	0.729	0.072	0.898	0.058	0.180	0.078	0.561
Health clinic utilization conditional on sick last month	0.622	0.326	0.696	0.649	0.689	0.713	0.833	0.178	0.750	0.423
Illness duration conditional on sick last month	9.92	0.39	15.61	0.18	6.20	0.35	9.42	0.19	7.25	0.29
Child and Parent Characteristics:										
Child is female	0.490	0.968	0.465	0.241	0.463	0.201	0.472	0.396	0.489	0.464
Child age in months	29.77	0.153	29.45	0.339	29.08	0.665	28.22	0.475	28.35	0.284
Head of household is female	0.137	0.718	0.151	0.910	0.127	0.520	0.102	0.131	0.148	0.466
Proportion either parent ever enrolled	0.155	0.351	0.179	0.764	0.184	0.843	0.200	0.852	0.143	0.834
Number of wives of the father	1.691	0.659	1.701	0.699	1.902	0.408	1.760	0.782	1.735	0.806
Household size	7.936	0.496	7.761	0.756	8.395	0.118	8.208	0.233	7.615	0.473
HH eligible for transfer	0.890	0.789	0.893	0.645	0.896	0.375	0.887	0.951	0.887	0.854
Ethnic group = Kassena	0.596	0.660	0.368	0.297	0.558	0.840	0.697	0.239	0.526	0.219
Ethnic group = Nankana/Farfarse	0.370	0.846	0.478	0.660	0.249	0.333	0.146	0.086*	0.403	0.162
Ethnic group = Mossi	0.023	0.410	0.081	0.733	0.129	0.413	0.097	0.554	0.062	0.129
Ethnic subgroup = Nakomse	0.684	0.369	0.608	0.860	0.659	0.514	0.612	0.823	0.588	0.892
Religion = Muslim	0.220	0.856	0.282	0.210	0.247	0.598	0.260	0.431	0.207	0.765
Religion = Animist	0.482	0.879	0.373	0.307	0.495	0.789	0.436	0.757	0.466	0.663
Religion = Christian	0.289	0.825	0.310	0.941	0.253	0.335	0.243	0.240	0.304	0.685
Village Level Health Clinic Variables										
Villages per health clinic	9.53	0.593	10.57	0.885	9.73	0.782	12.08	0.425	10.31	0.411
Number of days a doctor is present	5.091	0.871	4.455	0.576	4.375	0.582	4.125	0.515	5.250	0.818
Number of nurses	3.467	0.406	3.286	0.352	3.333	0.458	3.500	0.574	4.000	0.863
Childhood nutritional/growth counseling offered	0.867	0.184	0.929	0.370	0.933	0.366	0.692	0.089*	1.000	0.296
Vaccines offered	0.933	0.371	0.929	0.370	0.933	0.366	0.769	0.164	1.000	0.461
Nutritional supplements offered	0.733	0.796	0.929	0.309	0.667	0.545	0.462	0.051*	0.769	0.069*
Health epidemics	0.667	0.231	0.571	0.565	0.538	0.656	0.636	0.370	0.462	0.648
Source of funding for clinic (patient, govt., NGO)	1.200	0.321	1.214	0.383	1.267	0.542	1.692	0.287	1.385	0.127

Notes: The treatment arms are abbreviated as CCTF (conditional cash transfers given to fathers), CCTM (conditional cash transfers given to mothers), UCTF (unconditional cash transfers given to fathers), and UCTM (unconditional cash transfers given to mothers). The p-values are from a Wald test comparing the treatment arm with the control group. In the last column (under the control group heading), the p-values are from an F-test of the joint test that the means of the five groups are equal. Robust standard errors, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%.Data source: Nahouri Social Protection Program (NSPP) Evaluation data from 2008-2010.

Table 3: Relative Differences Between Attriting and Non-Attriting Households

Variables:	Non-Attritors (n=2622)	Attritors (n=127)	Mean Difference	CCT Diff in Diff	UCT Diff in Diff	CTF Diff in Diff	CTM Diff in Diff
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
HH expenditures per capita (in FCFA)	46036	50228	4192	-4240	-10172	-7525	-10589
	(1165)	(4003)	(5365)	(9396)	(9239)	(10111)	(8669)
# children < 60 months	1.095	1.118	0.023	-0.083	-0.064	-0.169	0.062
	(0.021)	(0.070)	(0.097)	(0.163)	(0.189)	(0.161)	(0.180)
Number of adults	3.123	2.575	-0.548***	-0.005	0.156	-0.014	0.170
	(0.033)	(0.119)	(0.153)	(0.328)	(0.319)	(0.331)	(0.320)
Household size	6.551	5.323	-1.228***	0.524	0.478	0.309	0.848*
	(0.062)	(0.214)	(0.284)	(0.475)	(0.518)	(0.471)	(0.501)
HH head literate	0.157	0.189	0.032	0.007	-0.046	-0.001	-0.031
	(0.007)	(0.035)	(0.033)	(0.078)	(0.094)	(0.079)	(0.093)
HH head age	45.25	42.23	-3.02**	0.122	1.521	-1.935	3.821
	(0.286)	(1.329)	(1.330)	(2.986)	(3.209)	(3.014)	(2.982)
HH head educated	0.157	0.142	-0.015	-0.078	-0.106	-0.020	-0.181**
	(0.007)	(0.031)	(0.033)	(0.071)	(0.075)	(0.060)	(0.086)
HH head gender	1.136	1.189	0.053*	0.092	0.085	0.173*	-0.025
	(0.007)	(0.037)	(0.032)	(0.106)	(0.095)	(0.093)	(0.113)
Polygamous	0.218	0.102	-0.116***	0.049	0.016	0.028	0.053
	(0.008)	(0.027)	(0.037)	(0.062)	(0.064)	(0.063)	(0.063)
Monogamous	0.593	0.042	0.069	-0.137	-0.160	-0.171*	-0.112
	(0.010)	(0.661)	(0.045)	(0.098)	(0.099)	(0.090)	(0.115)
Single	0.189	0.236	0.036	0.088	0.144	0.143	0.059
	(0.008)	(0.038)	(0.047)	(0.101)	(0.094)	(0.094)	(0.109)
Ethnic group=Kassena	0.554	0.591	0.037	-0.063	-0.015	-0.033	-0.009
	(0.010)	(0.044)	(0.045)	(0.153)	(0.152)	(0.148)	(0.161)
Ethnic group=Nankana	0.334	0.260	-0.074*	0.186	0.065	0.092	0.135
	(0.009)	(0.039)	(0.043)	(0.145)	(0.140)	(0.146)	(0.144)
Religion=Muslim	0.231	0.260	0.029	0.124	0.116	0.123	0.113
	(0.008)	(0.039)	(0.038)	(0.107)	(0.111)	(0.104)	(0.115)
Religion=Christian	0.272	0.378	0.106***	0.022	-0.021	0.078	-0.102
	(0.009)	(0.043)	(0.041)	(0.090)	(0.128)	(0.082)	(0.117)
Religion=animist	0.484	0.354	-0.130***	-0.129	-0.100	-0.180*	-0.018
	(0.010)	(0.043)	(0.045)	(0.095)	(0.134)	(0.098)	(0.111)
#wives of HH head's father	2.247	2.214	-0.033	-0.550	-0.246	-0.369	-0.450
	(0.038)	(0.165)	(0.177)	(0.334)	(0.475)	(0.392)	(0.370)
#kids of HH head's father	9.253	8.714	-0.539	-0.208	1.556	0.995	-0.1000
	(0.139)	(0.531)	(0.641)	(1.510)	(1.316)	(1.343)	(1.721)

Notes: *significant at 10%; ** significant at 5%; *** significant at 1%. Column 1 presents means of household-level characteristics from the baseline survey for the sample of households that were followed from the baseline to the two-year follow-up survey (non-attritors). Column 2 presents means for the sample of attritor households, and column 3 presents the average difference in characteristics between attritors and non-attritors. The treatment arms are abbreviated as CCT (conditional cash transfer), UCT (unconditional cash transfer), CTF (cash transfer given to fathers), and CTM (cash transfer given to mothers). Columns 4-7 test for differential impacts of attrition between treatment and control groups. Specifically, for each characteristic, we estimate difference-in-differences regressions comparing attritors and non-attritors for the treatment (CCT, UCT, CTF, CTM) and control groups. Data source: Nahouri Social Protection Program (NSPP) Evaluation data from 2008-2010.

Table 4: Impact of Cash Transfers on Probability of Illness During Last Month and Conditional on Illness Health Clinic Utilization and Illness Duration

Dependent Variable:	Sick during last month	Health clinic utilization conditional on sick during last month	Illness duration conditional on sick during last month	Sick during last month	Health clinic utilization conditional on sick during last month	Illness duration conditional on sick during last month
	(1)	(2)	(3)	(4)	(5)	(6)
CCT * Round 2	-0.064**	0.236***	-3.642			
	[0.027]	[0.077]	[3.174]			
UCT * Round 2	-0.042	0.077	-0.800			
	[0.026]	[0.081]	[1.563]			
CCT * Round 3	-0.053*	0.183**	-7.156**			
	[0.029]	[0.074]	[3.199]			
UCT * Round 3	-0.046	0.064	-0.518			
	[0.028]	[0.083]	[1.519]			
CTF * Round 2				-0.048*	0.176**	0.311
				[0.026]	[0.085]	[2.206]
CTM * Round 2				-0.057**	0.126*	-4.838**
				[0.026]	[0.074]	[2.451]
CTF * Round 3				-0.069**	0.147*	-2.535
				[0.029]	[0.089]	[2.196]
CTM * Round 3				-0.031	0.099	-4.686*
				[0.029]	[0.072]	[2.636]
Round 2	0.084***	-0.028	-0.793	0.084***	-0.028	-0.805
	[0.024]	[0.054]	[1.065]	[0.024]	[0.054]	[1.056]
Round 3	0.110***	0.018	-1.072	0.110***	0.017	-1.077
	[0.026]	[0.058]	[0.904]	[0.026]	[0.058]	[0.897]
Village Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Child Age Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	9156	979	979	9156	979	979
P-value testing equality between CCT and UCT:						
CCT*Rd2 = UCT*Rd2	0.103	0.053	0.382			
CCT*Rd3 = UCT*Rd3	0.652	0.110	0.033			
P-value testing equality between Fathers and Mothers:						
CTF*Rd2=CTM*Rd2				0.526	0.545	0.074
CTF*Rd3=CTM*Rd3				0.014	0.539	0.454

Notes: Robust standard errors in brackets, clustered at the village*follow-up level. *significant at 10%; ** significant at 5%; *** significant at 1%. The treatment arms are abbreviated as CCT (conditional cash transfer), UCT (unconditional cash transfer), CTF (cash transfer given to fathers), and CTM (cash transfer given to mothers). All regressions are restricted to children under 60 months old and include village fixed effects, child age fixed effects, child gender, and round dummies. Data source: Nahouri Social Protection Program (NSPP) Evaluation data from 2008-2010.

Table 5: Impact of Cash Transfers on Probability of Illness During Last Month and Conditional on Illness Health Clinic Utilization and Illness Duration, By Gender

	Boys			Girls		
Dependent Variable:	Sick during last month	Health clinic utilization conditional on sick during last month	Illness duration conditional on sick during last month	Sick during last month	Health clinic utilization conditional on sick during last month	Illness duration conditional on sick during last month
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Comparing Conditional or Unconditional Transfers						
CCT * Round 2	-0.063*	0.238**	-6.400	-0.059**	0.235	1.915
	[0.037]	[0.119]	[3.919]	[0.029]	[0.153]	[3.915]
UCT * Round 2	-0.057*	0.014	-2.175	-0.021	0.062	0.647
	[0.034]	[0.112]	[2.198]	[0.030]	[0.160]	[2.015]
CCT * Round 3	-0.048	0.359***	-9.755*	-0.053	0.001	-3.464
	[0.038]	[0.110]	[5.251]	[0.034]	[0.160]	[3.045]
UCT * Round 3	-0.030	0.036	-2.885	-0.056*	-0.008	1.825
	[0.035]	[0.105]	[2.317]	[0.033]	[0.165]	[2.102]
Round Dummies?	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Child Age Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	4682	532	532	4474	447	447
Panel B: Comparing Transfers to Mothers or Fathers						
CTF * Round 2	-0.050	0.177	-2.488	-0.042	0.142	3.741
	[0.034]	[0.114]	[2.887]	[0.029]	[0.173]	[3.604]
CTM * Round 2	-0.070*	0.031	-5.924*	-0.039	0.139	-1.287
	[0.036]	[0.118]	[3.156]	[0.030]	[0.147]	[2.160]
CTF * Round 3	-0.059*	0.217*	-4.262	-0.073**	0.001	-1.826
	[0.036]	[0.112]	[3.622]	[0.033]	[0.179]	[3.333]
CTM * Round 3	-0.019	0.121	-7.455*	-0.039	-0.010	-0.255
	[0.036]	[0.108]	[3.923]	[0.034]	[0.153]	[2.273]
Round Dummies?	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Child Age Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	4682	532	532	4474	447	447

Notes: Robust standard errors in brackets, clustered at the village*follow-up level. *significant at 10%; ** significant at 5%; *** significant at 1%. The treatment arms are abbreviated as CCT (conditional cash transfer), UCT (unconditional cash transfer), CTF (cash transfer given to fathers), and CTM (cash transfer given to mothers). All regressions are restricted to children under 60 months old and include village fixed effects, child age fixed effects, child gender, and round dummies. Data source: Nahouri Social Protection Program (NSPP) Evaluation data from 2008-2010.

Table 6: Impact of Cash Transfers on Probability of Illness During Last Month and Conditional on Illness Health Clinic Utilization and Illness Duration, By Child Age

	Young Children (0-23 Months)			Old Children (24-59 Months)		
Dependent Variable:	Sick during last month	Health clinic utilization conditional on sick during last month	Illness duration conditional on sick during last month	Sick during last month	Health clinic utilization conditional on sick during last month	Illness duration conditional on sick during last month
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Comparing Conditional or Unconditional Transfers						
CCT * Round 2	-0.128***	0.148	-8.084**	-0.026	0.331***	-3.270
	[0.044]	[0.137]	[3.568]	[0.028]	[0.112]	[5.694]
UCT * Round 2	-0.084*	0.180	-2.122	-0.012	-0.024	0.585
	[0.043]	[0.141]	[1.972]	[0.028]	[0.126]	[2.280]
CCT * Round 3	-0.097***	0.115	-7.790**	-0.020	0.233*	-9.109
	[0.035]	[0.108]	[3.300]	[0.038]	[0.133]	[7.039]
UCT * Round 3	-0.090***	0.128	-1.038	-0.011	-0.015	-1.314
	[0.034]	[0.119]	[1.924]	[0.037]	[0.143]	[2.365]
Round Dummies?	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Child Age Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	3659	474	474	5497	505	505
Panel B: Comparing Transfers to Mothers or Fathers						
CTF * Round 2	-0.097**	0.273*	-4.867	-0.016	0.136	2.966
	[0.044]	[0.151]	[3.412]	[0.027]	[0.131]	[2.951]
CTM * Round 2	-0.111***	0.072	-4.943**	-0.022	0.174	-5.921
	[0.043]	[0.123]	[2.180]	[0.029]	[0.112]	[4.807]
CTF * Round 3	-0.120***	0.220*	-5.241	-0.029	0.080	-1.768
	[0.036]	[0.131]	[3.391]	[0.036]	[0.148]	[3.092]
CTM * Round 3	-0.071**	0.050	-3.305	-0.001	0.144	-7.999
	[0.034]	[0.103]	[2.092]	[0.038]	[0.131]	[6.014]
Round Dummies?	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Child Age Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	3659	474	474	5497	505	505

Notes: Robust standard errors in brackets, clustered at the village*follow-up level. *significant at 10%; ** significant at 5%; *** significant at 1%. The treatment arms are abbreviated as CCT (conditional cash transfer), UCT (unconditional cash transfer), CTF (cash transfer given to fathers), and CTM (cash transfer given to mothers). All regressions are restricted to children under 60 months old and include village fixed effects, child age fixed effects, child gender, and round dummies. Data source: Nahouri Social Protection Program (NSPP) Evaluation data from 2008-2010.

Table 7: Impact of Cash Transfers on Probability of Illness During Last Month and Conditional on Illness Health Clinic Utilization and Illness Duration, By Baseline Poverty Status

	Poor Households			Non-poor Households		
Dependent Variable:	Sick during last month	Health clinic utilization conditional on sick during last month	Illness duration conditional on sick during last month	Sick during last month	Health clinic utilization conditional on sick during last month	Illness duration conditional on sick during last month
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Comparing Conditional or Unconditional Transfers						
CCT * Round 2	-0.080***	0.162	0.336	-0.029	0.316**	-10.735
	[0.030]	[0.127]	[1.419]	[0.041]	[0.160]	[8.045]
UCT * Round 2	-0.059**	0.023	-1.424	-0.007	0.058	2.250
	[0.029]	[0.109]	[1.689]	[0.041]	[0.198]	[2.743]
CCT * Round 3	-0.079**	0.268**	0.569	-0.000	0.176	-15.421**
	[0.031]	[0.116]	[1.470]	[0.045]	[0.146]	[7.606]
UCT * Round 3	-0.074**	0.015	-0.036	0.023	0.063	0.697
	[0.030]	[0.110]	[1.759]	[0.045]	[0.167]	[2.314]
Round Dummies?	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Child Age Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	6,258	628	628	2,893	351	351
Panel B: Comparing Transfers to Mothers or Fathers						
CTF * Round 2	-0.060**	0.120	-0.413	-0.026	0.222	0.942
	[0.030]	[0.121]	[1.649]	[0.041]	[0.181]	[9.173]
CTM * Round 2	-0.077**	0.027	-1.304	-0.015	0.247	-12.976*
	[0.030]	[0.108]	[1.730]	[0.040]	[0.166]	[7.779]
CTF * Round 3	-0.102***	0.114	0.178	0.008	0.186	-8.107
	[0.030]	[0.127]	[1.660]	[0.046]	[0.161]	[9.305]
CTM * Round 3	-0.051*	0.095	0.144	0.012	0.120	-11.567*
	[0.031]	[0.104]	[1.705]	[0.045]	[0.149]	[6.203]
Round Dummies?	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Child Age Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	6,258	628	628	2,893	351	351

Notes: Robust standard errors in brackets, clustered at the village*follow-up level. *significant at 10%; ** significant at 5%; *** significant at 1%. The treatment arms are abbreviated as CCT (conditional cash transfer), UCT (unconditional cash transfer), CTF (cash transfer given to fathers), and CTM (cash transfer given to mothers). All regressions are restricted to children under 60 months old and include village fixed effects, child age fixed effects, child gender, and round dummies. Poor households are defined as being below the median household per capita expenditure level in the baseline survey. Data source: Nahouri Social Protection Program (NSPP) Evaluation data from 2008-2010.

Table 8: Impact of Cash Transfers on Probability of Illness During Last Year and Conditional on Illness Health Clinic Utilization and Illness Duration, By Season

Dependent Variable:	Rainy Season			Dry Season		
	Sick during last year	Health clinic utilization conditional on sick during last year	Illness duration conditional on sick during last year	Sick during last year	Health clinic utilization conditional on sick during last year	Illness duration conditional on sick during last year
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Comparing Conditional or Unconditional Transfers						
CCT * Round 2	-0.127***	0.110	0.738	0.008	0.100*	-9.185***
	[0.039]	[0.071]	[4.025]	[0.038]	[0.059]	[3.094]
UCT * Round 2	-0.088**	0.073	2.021	0.010	0.069	-5.897**
	[0.038]	[0.077]	[4.024]	[0.036]	[0.062]	[2.563]
CCT * Round 3	-0.078*	0.129*	2.885	0.026	0.192***	-2.811
	[0.047]	[0.074]	[4.154]	[0.042]	[0.061]	[2.932]
UCT * Round 3	-0.092**	0.129	4.731	0.027	0.108*	0.437
	[0.046]	[0.084]	[4.169]	[0.041]	[0.065]	[2.334]
Round Dummies?	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Child Age Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	7386	1199	1199	7781	1597	1597
Panel B: Comparing Transfers to Mothers or Fathers						
CTF * Round 2	-0.122***	0.091	0.378	0.031	0.061	-7.676***
	[0.040]	[0.075]	[4.028]	[0.038]	[0.059]	[2.789]
CTM * Round 2	-0.091**	0.082	2.927	-0.013	0.115*	-7.279**
	[0.036]	[0.073]	[3.986]	[0.035]	[0.061]	[2.918]
CTF * Round 3	-0.135***	0.164**	3.466	-0.014	0.172***	-0.781
	[0.048]	[0.083]	[4.176]	[0.042]	[0.066]	[2.685]
CTM * Round 3	-0.037	0.093	4.628	0.064	0.131**	-1.361
	[0.044]	[0.073]	[4.146]	[0.040]	[0.062]	[2.593]
Round Dummies?	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Child Age Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	7386	1199	1199	7781	1597	1597

Notes: Robust standard errors in brackets, clustered at the village*follow-up level. *significant at 10%; ** significant at 5%; *** significant at 1%. The treatment arms are abbreviated as CCT (conditional cash transfer), UCT (unconditional cash transfer), CTF (cash transfer given to fathers), and CTM (cash transfer given to mothers). All regressions are restricted to children under 60 months old and include village fixed effects, child age fixed effects, child gender, and round dummies. Data source: Nahouri Social Protection Program (NSPP) Evaluation data from 2008-2010.

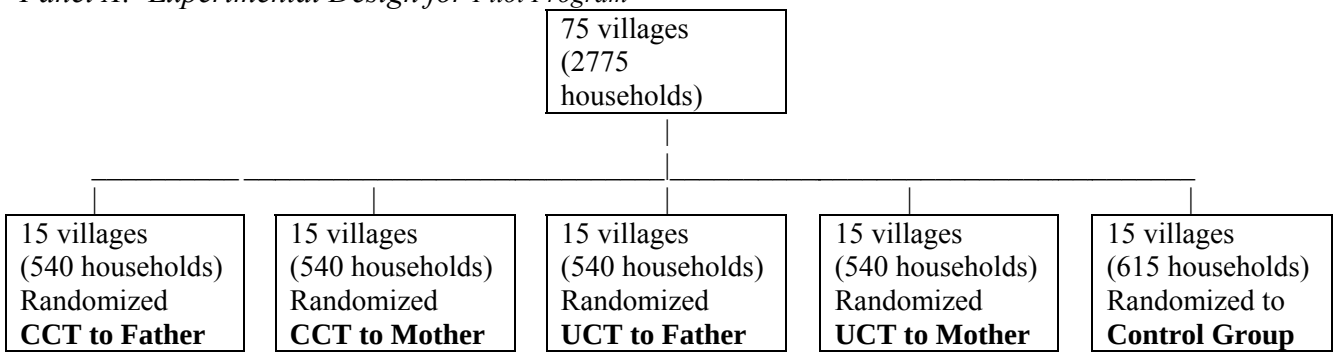
Table 9: Impact of Cash Transfers on Health Clinic Supply-Side Characteristics

Dependent Variable:	#villages using health clinic (1)	Prenatal care services (2)	Early child services (3)	Vaccine services (4)	#days doctor works there (5)	#nurses (6)	Nutrition supplement provision (7)	Who pays for services (8)	Who pays for prenatal services (9)	Who pays for baby weighing services (10)	Who pays for vaccine services (11)
Panel A: Comparing Conditional or Unconditional Transfers											
CCT * Round 2	-1.007	-0.005	-0.005	0.129	-0.059	-1.107	0.026	-0.043	0.154	-0.000	-0.000
	[1.566]	[0.066]	[0.083]	[0.083]	[0.568]	[0.859]	[0.143]	[0.149]	[0.181]	[0.153]	[0.155]
UCT * Round 2	-1.040	-0.010	-0.049	0.085	-0.120	-1.120	-0.097	-0.040	-0.000	-0.000	-0.000
	[1.599]	[0.066]	[0.084]	[0.084]	[0.576]	[0.877]	[0.145]	[0.152]	[0.191]	[0.161]	[0.163]
CCT * Round 3	-3.000*	0.003	-0.037	-0.038	0.080	-1.151	0.017	-0.093	0.000	0.000	-0.000
	[1.663]	[0.069]	[0.088]	[0.088]	[0.595]	[0.906]	[0.151]	[0.161]	[0.186]	[0.157]	[0.159]
UCT * Round 3	-1.282	0.077	0.000	0.000	0.006	-1.892**	-0.131	-0.083	0.077	0.000	0.037
	[1.652]	[0.069]	[0.088]	[0.087]	[0.595]	[0.906]	[0.151]	[0.164]	[0.186]	[0.157]	[0.158]
Round Dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	204	207	207	206	180	204	207	201	164	163	164
Panel B: Comparing Transfers to Mothers or Fathers											
CTF * Round 2	-1.745	0.032	0.032	0.166**	-0.076	-1.241	-0.005	-0.124	0.080	0.000	0.000
	[1.548]	[0.065]	[0.082]	[0.082]	[0.560]	[0.854]	[0.139]	[0.147]	[0.183]	[0.154]	[0.156]
CTM * Round 2	-0.150	-0.053	-0.093	0.040	-0.104	-0.958	-0.067	0.058	0.095	-0.000	0.000
	[1.602]	[0.066]	[0.084]	[0.084]	[0.585]	[0.884]	[0.143]	[0.152]	[0.189]	[0.159]	[0.161]
CTF * Round 3	-3.282*	-0.003	-0.040	-0.040	-0.006	-1.837**	0.142	-0.080	-0.000	0.000	0.042
	[1.676]	[0.070]	[0.088]	[0.088]	[0.603]	[0.918]	[0.150]	[0.165]	[0.190]	[0.160]	[0.161]
CTM * Round 3	-1.167	0.077	0.000	0.000	0.085	-1.249	-0.228	-0.095	0.069	0.000	-0.000
	[1.624]	[0.068]	[0.086]	[0.086]	[0.588]	[0.896]	[0.146]	[0.159]	[0.183]	[0.154]	[0.156]
Round Dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	204	207	207	206	180	204	207	201	164	163	164

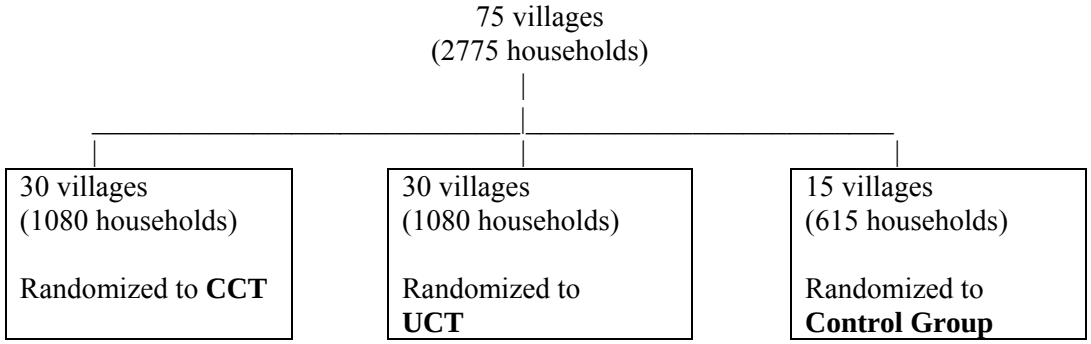
Notes: Robust standard errors in brackets, clustered at the village*follow-up level. *significant at 10%; ** significant at 5%; *** significant at 1%. The treatment arms are abbreviated as CCT (conditional cash transfer), UCT (unconditional cash transfer), CTF (cash transfer given to fathers), and CTM (cash transfer given to mothers). All regressions include village fixed effects and round dummies. Data source: Nahouri Social Protection Program (NSPP) Evaluation data from 2008-2010.

Figure 1: Summary of Treatment and Control Group Randomization Plan

Panel A: Experimental Design for Pilot Program



Panel B: Conditional Transfers versus Unconditional Transfers Comparison



Panel C Transfers to Fathers versus Transfers to Mothers Comparison

