Should Cash Transfers Be Conditional?

Conditionality, Preventive Care, and Health Outcomes[1]

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Abstract

We study a Conditional Cash Transfer program in which the cash transfers to the mother only depends on the fulfilment of the national preventive visit schedule by her children born before she registered in the program. We estimate that preventive visits of children born after the mother registered in the program are 50% lower because they are excluded from the conditionality requirement. Using the same variation, we also show that attendance to preventive care improves children's health.

1. Introduction

Conditional Cash Transfer (CCT) programs have become increasingly popular tools to foster human capital accumulation and reduce poverty in developing countries. According to Fiszbein and Schady (2009), CCT programs have been introduced in at least twenty-eight countries by 2009, covering between 20 and 40 percent of the population of these countries. While Latin American countries were the first ones in implementing these programs, they are being currently expanded in Africa and Asia. CCT programs have an education or health component, or both. The education component provides cash transfers to mothers if her school -age children are attending school. Typically, the health component implies that mothers receive cash transfers if their pre-school children are up to date with preventive health care visits. [2] In these preventive care visits, children's height and weight are measured, and the mother receives information on the nutritional status of her child, advice on nutrition and hygiene, and possibly nutritional supplements or medicines.

CCT programs are complex interventions that jointly increase mother's income, the relative price of school attendance, health information, and the relative cost of access to health care services. Many evaluations exist of such CCT programs (among many others see Schultz 2004, Gertler 2004, Behrman and Hoddinot 2005, Attanasio et al., 2005, Janvry, 2006; Barham and Maluccio 2009) but they typically do not provide estimates of the impact of individual components of the program, which is key to understand what elements are important for the overall impact observed and which might be less essential and, if costly, could be eliminated. In particular, in most available studies, the role of the conditionalities is not identified despite the fact that their role has received considerable attention in the recent policy debate.

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[2] In a broader sense a CCT program is any program that provides cash if a condition is fulfilled. Under this broader definition, CCT programs also include interventions that provide cash transfers to individuals who remain HIV-negative (Kholer and Thornton, 2012) or programs that provide cash transfers to mother conditional on skilled birth attendance (Mazumdar et al., 2012 and Powell-Jackson and Hanson 2012). In this paper, we use the CCT term to refer to programs which were modelled after Bolsa Escola in Brazil and PROGRESA in México (Bourguignon et al. 2003; Skoufias and Parker, 2001).

From a theoretical point of view, conditionalities change the net cost of certain activities, typically related to investment in human capital. This change in relative prices will compound possible income effects that are triggered by the transfer itself. It is interesting to note that, as the program has different components, the same households might be receiving transfers for some activities that it would undertake regardless of its presence (such as enrolment in primary school of its youngest children) while considering whether to undertake a different activity (such as enrolment in secondary school of older children) that it might not undertake except for the conditionality that would trigger a payment. Therefore the same household will be affected by a change in income that is effectively unconditional and a change in the relative price of some specific investments activities that might be triggered through a substitution effect.

CCT programs can also affect behaviour through channels different from income and substitution effects. CCT programs might help to overcome procrastination, especially in the case of preventive care as the program gives a schedule of preventive visits that children must fulfil by a given deadline. They can also raise the salience of the behaviour that they are incentivizing as well as endorse it (Benhassine et al, 2013). For instance, households might update their beliefs on the benefits of a given activity if the government is promoting it. Interestingly, comparing the effects of a pure unconditional cash transfer program with a CCT one would not allow one to tease out how important salience/endorsement effects would be *vis a vis* income and substitution effects: as we discuss below, our exercise can be informative about this distinction.

In the policy debate, different opinions exist on the role of conditionalities. One possible argument is that conditionalities introduce un-necessary distortions. One view could be that if cash transfers are deemed desirable, either for redistributive purposes, or to alleviate liquidity constraints or similar imperfections, conditionalities would not be required as households would allocate the grants to their most efficient use. An alternative view is that conditionalities effectively promote investments in some activities that should be subsidised, either because of positive externalities, or because of the failure from the part of the parents to recognize the long run returns to such activities (Das, Do, and Özler, 2005; de Janvry and Sadoulet, 2005). Some commentators have also argued that conditionalities provide a

political justification that allows the political success and survival of CCTs which might be an important redistribution tool (Gelbach and Pritchett, 2002).

However, the conditionalities also come at some cost. They increase the cost of running the program. For instance, conditionality related cost amount to 24% of the total costs (excluding transfers) of PROGRESA (Caldés, Coady and Maluccio, 2006). Even more importantly, the conditionality might get on the way of relaxing credit constraints because some of the poorest households will face high costs of fulfilling the conditionality requirements (Benhassine et al, 2013). This would be the case if poverty and the costs of fulfilling the conditionality requirement are positively correlated. For instance, some of the poorest households might live far away from the health centre and have higher transportation costs. Also, poorer households will have more children and they will need to go to the health centre more often, but the cash transfer amount is usually independent of the number of children. At the cost of higher program complexity, some of these issues could be tackled by tailoring eligibility rules and transfer amounts to individual circumstances, but these could distort behaviours and create perverse incentives (de Janvry and Sadoulet 2006).

We focus on *Familias en Acción* (*FeA*), the CCT program implemented by the Colombian government since 2002. Our identification strategy exploits a rule of the program that was put in place at its inception. In particular, children in treated municipalities who were born after the family's registration date (FRD) to *FeA* were not subject to the conditionality requirement, and the family would still receive the nutritional cash transfer as long as all under 7 years old children born before the FRD fulfilled the conditionality requirements. In other words, whether a mother received the nutritional cash transfer depended only on whether her children born before the FRD complied with the conditionality, and hence the number of preventive visits of children born after the FRD was completely irrelevant for the cash transfer. Moreover, the nutritional cash transfer is a lump sum, which does not depend on the number of children under 7 years old who were born before the FRD (as long as there is at least one).

This eligibility rule interacts with another important feature of the program: the number of health centre visits younger children had to attend to fulfil the conditionality requirements is

larger than those that older children had to attend. For instance, children younger than 12 months had to attend five visits a year, while children older than 2 are only requested to have two visits per year. Hence, even if young children born after the FRD are taken to preventive care visits with their older siblings, they will have less preventive visits than a child who was born before the FRD and fulfils the conditionality requirements. We also note that our identification strategy provides a lower bound estimate of the effect of the conditionality as some children born after the FRD might get preventive care visits only because their older siblings are getting them (and they are taken along with them).

The first contribution of this paper is to study the role played by one of the conditionalities often imposed on recipients of a typical CCT: a proportion (or all) of the cash transfer is provided only if young children are up to date with a certain schedule of preventive health care visits. We ask the question: how different will be the preventive care received by a young child if the cash transfer that the mother receives only depends on the older siblings being up to date with preventive check-ups instead of the both the older siblings and the young child himself. Answering this question is important not only in its own right, but also because it provides insights of why some mothers do not take their children to preventive health care visits. If the conditionality is important, it implies that low perceived returns of preventive care (either because benefits are perceived as small or costs are high) must be a reason why some children do not receive preventive care among poor households in Colombia. Alternative reasons include credit constraints and lack of women empowerment, which would be relaxed even with an unconditional cash transfer. Hence, analysing the role of conditionality provides interesting insights on the barriers for preventive health care use in developing countries.

The second contribution of this paper is to estimate the effect of preventive health care visits on child's health. Again, this is not only important in its own right, but it is also crucial to provide an interpretation to the effect of conditionalities. If preventive care is ineffective (possibly because health care is of poor quality), then it would be natural to find a positive effect of the conditionality on the demand for preventive health care (as the benefits of preventive care would be null for the mother and/or child in the absence of the conditionality). However, this increase in preventive care visits would be a complete

deadweight loss, which would be best avoided (and hence an unconditional cash transfer would be better than a conditional one).[3] On the other hand, should we find that preventive care visits improve health status, then the question becomes why parents do not use these services (which are typically free). The answers could be either a failure to recognize the benefits of such an investment (Jensen, 2010) or an excessive net cost, in terms of borrowing or bequest constraints (Martinelli and Parker, 2003) (including the opportunity cost).

Our first finding is that conditionalities are important for preventive care visits. Lack of conditionality reduces by 50 percent the number of preventive care visits that young children attend. Because households were already fulfilling the conditionality requirement for their older children, our estimated effect must be interpreted as the effect of conditionality in a setting where the activities that are being incentivized are already salient and have been received endorsement by the government. This contrasts with the lack of effects of school related conditionality in a setting where schooling has also received endorsement and been made salient (Benhassine et al, 2013).

Our second finding highlights the importance of preventive care for children health. The eligibility rule for the conditionality provides us with an exogenous source of variation in exposure to preventive care visits across children. We exploit this source of variation to assess the causal impact of preventive care on a set of health outcomes. Our results indicate that preventive care improves a composite health indicator that includes measures of child morbidity (symptoms of respiratory disease and diarrhoea) and nutritional status. When we consider the individual components of the index, preventive care only decreases significantly the probability of children being underweight, although all other measures move in the same direction.

Despite their importance, the existing knowledge of the marginal effect of conditionality in conditional cash transfer programs is limited, and especially focused on educational outcomes. Brauw and Hoddinott (2011), using data collected for the Mexico's PROGRESA program, exploit the fact that some beneficiaries eligible for transfers did not receive the

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[3] Unless, of course, the increased demand for preventive health care is matched with an improvement of its quality.

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forms needed to monitor the attendance of their children at school. They find that the absence of these forms reduces the probability that the children attend school, especially among those transitioning to lower secondary school. Schady and Araujo (2008), exploiting differential parental beliefs on the school attendance requirement attached to program, find similar results using data from the Ecuadorian program Bono de Desarrollo Umano. Baird et al. (2011) show experimental evidence on the relative effectiveness of a conditional and unconditional cash transfer program on young girls' human capital investment and family formation in Malawi. Their findings indicate that girls who received the conditional treatment feature higher school attendance rates and improved their test scores, whilst those in the unconditional arm feature substantially lower pregnancy and marriage rates. In light of their results, a conditional cash transfer for young children that switches to unconditional once the girls complete a certain grade would increase human capital formation, while also delaying marriage and reducing early pregnancy. Akresh et al. (2013), using data from a randomized trial in Burkina Faso, show that conditional programs are more effective in improving the school enrolment of children less likely to go to school, such as girls, younger and low ability kids, than unconditional programs

Closer to our paper, Akresh et al. (2012) uses a cluster randomized trial in Burkina Faso and find that unconditional cash transfers did not increase preventive care visits but conditional ones did. Their paper and ours complement each other: although we must deal with the non-random assignment of the program, we report results on how preventive care visits affect child health which allows us to interpret the findings about the importance of the conditionality. In particular, the fact that we find preventive health care to have positive effects on children rules out the explanation that low demand for preventive health care is driven by low returns. It must also be noted that the interpretation of conditionality is different in both papers so we answer somewhat different questions. The effect of conditionality in Akresh et al. (2012) might include effects related to salience/endorsement of preventive care; but our paper is silence on what would happen if the transfer to older siblings were unconditional.

The rest of the paper is organized as follows. In section 2, we describe the operation of the program. In section 3, we briefly describe the data set we use to estimate the impact of the

conditionality. In section 4, we illustrate the identification strategy. Section 5 and 6 report the main empirical results of the paper. Section 7 concludes.

2. Program description

Familias en Acción (FeA) was introduced by the Colombian government between 2001 and 2002, with the intention to alleviate poverty while at the same time fostering human capital accumulation. The program, initially financed with a loan from the World Bank and the Inter-American Development Bank, was modelled after PROGRESA in Mexico. It consists of cash transfers conditional on certain health and education activities to the poorest families living in the municipalities targeted by the program.

In the first four years, 622 out of the 1,098 municipalities in Colombia were deemed eligible to qualify for the program, based on the fulfilment of several criteria: (i) They had less than 100,000 inhabitants and were not a departmental capital; (ii) they had sufficient education and health infrastructures; (iii) they had a bank for delivering secure payments; (iv) the mayoral office had to report a battery of documents to the central government.

Within each qualifying municipality, eligible households were identified as those with children aged from 0 to 17 years old, registered to the lowest level of the SISBEN index as of December 1999.[4] When the program entered a municipality, registration of eligible households took place during an intensive period of one or two months. After that, few families registered until a new wave of registrations occurred, which in most cases took place past our sample period. 87% of eligible households registered in the program.

The education component of the program is a monthly grant for children aged 7-17, conditional on the child attending at least 80 per cent of school lessons. The grant was approximately US$7 for each child in primary school and twice as much for each child attending secondary school. The health component of *FeA* consists of a flat rate monthly cash subsidy (approximately 15 US dollars) given to mothers of children younger than 7 years old

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[4] The SISBEN indicator is computed using a number of different indicators of economic well-being related to poverty. Depending on the value of the index, each household is assigned to one of six levels. SISBEN 1 includes roughly the 20 percent poorest households.

in beneficiary households (the subsidy amount is per mother rather than per child). The receipt of the cash transfer is conditional on fulfilling the nationally recommended schedule of preventive care visits for all her children below 7 years old born before the FRD.[5] According to our data, 76% of our respondents to the survey were aware that registered children had to fulfil the schedule of preventive visits for the mother to receive the health component subsidy.

The preventive care schedule, shown in Table 1, prescribes more frequent visits for younger children. In particular, children aged less than 12 months must attend 5 visits per year. Children between 12 and 24 months are supposed to attend 3 visits, while children older than 2 are only required to have 2 visits per year. Consequently, if a young child born after the FRD is only taken to preventive care whenever an older child (born before the FRD, and hence subject to the conditionality) is taken, the visits of the young child will not be sufficient to make the young child comply with the due preventive care visits. This is key for the interpretation of our findings.

During the preventive care visits, a nurse assesses the child's psychomotor development, weights and measures the child, provides nutrition advice and reviews the child's compliance with the vaccination schedule. Moreover, children attending these visits are given iron supplements and de-worming drugs. Hence, the preventive care visits could improve health through two different channels: the possible diet improvements following the nutritional advice received during the visits (Penny et al. 2005 and Santos et al. 2001), and the intake of iron supplements and de-worming drugs. There is some evidence that iron is needed for psychomotor development and resistance to infections (Walker et al 2005, Oppenheimer 2001), and hence iron supplementation may improve growth and reduce morbidity (Lind et al 2004, Baqui et al. 2003, Angeles et al. 1993). De-worming may also reduce morbidity and increase weight (Sur et al. 2005). Hence, we can expect both morbidity and malnutrition to improve, especially as they interact and reinforce each other (Schorling et al 1990, Moore et al 2001).

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[5] The program also encourages that mothers attend talks on nutrition, hygiene, and contraception. Unlike in PROGRESA in Mexico, attendance to these talks is not part of the conditions that must be fulfilled to receive the cash subsidy.

To ensure compliance with the preventive care conditions, medical personnel receive stamps from the municipality to be stuck on a form only when all children less than 7 year-olds who were born before the FRD attended the due visits. The payment of the cash subsidy is arranged every two months, after confirming compliance with the various conditionalities through the relevant forms for all kids subject to the conditionality. The subsidy is temporarily suspended when one child does not receive a visit without a justified reason in the period before the subsidy is paid. It is definitively suspended if the child does not receive one or more visits for four non-consecutive two-month periods or three consecutive ones, and when there are no more children born before the FRD who are below 7 years old in the household. Only when the impact evaluation results were presented to the government (hence once all the data used in this paper had been collected), the eligibility rule was extended to all children below 7 years old, independently of whether or not they had been born before the FRD.

3. Data

We use data collected in the three waves of FeA evaluation for our study. The first wave was collected during the summer 2002; the second wave between July and November 2003, and the third wave took place between December 2005 and March 2006. Treatment municipalities are a stratified random sample of the 622 municipalities where the programme was implemented by the second wave. Implementation of the program was staggered. The program had started in 26 municipalities of the sample before the first wave took place, and it started in 31 municipalities between the first and second wave. By the third wave, the program had been implemented in all treatment municipalities. The FeA dataset also include a sample of municipalities where the program was not implemented (comparison municipalities), which were selected to be as similar as possible to the treatment municipalities in terms of population size, percentage of urbanisation and a composite index of the municipality infrastructure and services. We do not use the comparison municipalities in this paper as our identification strategy can be implemented using the treatment municipalities alone.

The extensive household questionnaires that were used for the evaluation surveys collect a large amount of information on the household socio-demographic structure, children's health and other pieces of information which are essential for this study such as children's date of birth and the FRD. Regarding children's health, the respondent (in most cases the child's mother) is asked whether the child suffered from diarrhoea as well as symptoms of acute respiratory infection in the 15 days previous to the interview. For every child below 7 years old, the mother is also asked for the number of preventive visits in the last twelve months. Each child is also weighed and measured at the same time or very soon after the household survey took place (once per household survey round).

As indicated in section 2, the program could improve both nutritional status and morbidity. Hence, we consider both types of measures. The morbidity indicators include whether the child suffered from diarrhoea or symptoms of respiratory infections. Regarding nutritional indicators, we consider the three most common ones: stunting, underweight and wasting. A child is said to be stunted (underweight) if the height-for-age (weight-for-age) Z-score is smaller than -2. [6] Because the proportion of children being wasted is very low (1.8% of our sample), we use instead whether a child is at risk of being wasted, which is defined as whether the weight-for-height z-score is smaller than -1. While stunting is due to accumulated nutritional deficits, wasting is affected by short-run nutritional insults and underweight is a composite indicator of stunting and wasting (Martorell and Habicht, 1986).

Given that the preventive visits could improve both morbidity and nutritional status, we collapse all five health indicators mentioned above in a summary index. This does not only deal with the problem of multiple inference (Kling *et al.* 2007; Romano and Wolf 2005, Liebman *et al.* 2004), but it improves power by averaging over different health measures, which are correlated among them. In particular, we use the index proposed by Anderson (2008), which consists of a weighted average of the standardized values of the five health measures (previously, the health measures have been re-defined so that 1 indicates good health and 0 bad health). The weights are derived from the variance-covariance matrix of the

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[6] Z-scores are computed as the difference between the variable of interest (height for example) and the median value for the same variable for children of the reference population with the same age (or height in the case of weight for height) and gender, divided by the standard deviation of the reference population for the same age (or height) and gender.

health measures, with the aim of maximizing the informational content of the index: less weight is given to health measures, which are highly correlated with each other.

4. Identification Strategy

Our identification strategy relies on the gradual rollout of the program which meant that even if two children were born in the same month-year, they could be subject to the conditionality requirement or not depending on what municipality they were born. Children already born at the time when the household registered to the FeA programme were subject to the conditionality requirement. Children born after the FRD were not registered in the program and thus were not subject to the conditionality. Therefore, the mother would still receive the cash transfer if the children born *before* the FRD complied with the conditionality. This eligibility rule interacts with another important feature of the program mentioned above: to fulfil the conditionality requirements, younger children must attend more preventive care visits than older children (see Table 1 for the schedule of preventive visits).

We define the FRD for all eligible households in a treatment municipality as the median of the registration dates of all the households of that municipality.[7] We define FRD at the municipality rather than household level because of two reasons. First, registration in the program was mainly driven by the program expansion: whenever the program arrived to a municipality, it would start registration of all the eligible households. Second, household specific FRD might depend on unobservable characteristics which might render the household specific FRD endogenous. For instance, households that delay registration might be those that invest less in their children anyway.

We constrain the sample to children younger than 36 months. This is because 98% of children born after FRD have less than 36 months during the entire period of the three waves; hence we prefer to constrain the sample of children born before the FRD also to be younger than 36 months. 66% of the children were born before the municipality FRD. Because of the natural aging of the children and because the sample in the first wave was representative of

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[7] Conceptually, the first registration date within a municipality seems more appropriate. However, we can see obvious coding mistakes in the FRD dates of most municipalities. Hence, our choice of the median because registration tended to be concentrated in intensive waves when the program would attempt to register anyone eligible. In the Appendix, we show that our results are generally robust to use the 10th or 20th percentile, although the results with the 10th percentile are noisier.

households with children aged 0 to 17 years of age, the third wave includes considerably fewer young children than the first two (see Table 2 which also reports the distribution of children per wave). Table 3 shows that 40% of households have both children born before and after the FRD, with the remaining 60% only have children born before the FRD. Note that we exclude households who only have children born after FRD because they would not be eligible to receive the *FeA* health component subsidy.

In order to graphically preview our results, we compute the difference (in intervals of 30 days) between the municipality FRD and the children's date of birth, and build 30-days interval dummy variables for each such interval. Then, we regress the number of preventive visits over month-year of birth dummies, municipality-wave dummies, and the 30-days interval dummies. Figure 1 shows the coefficients on those 30-days interval dummies. Children to the right of the FRD line are those born after the FRD and hence those not subject to the conditionality. As it is clear from the left panel, the coefficients decline sharply to the right of the FRD line, indicating that lack of conditionality reduces by around 0.55 the number of preventive care visits. Although the coefficients on the individual dummies are not statistically different from each other, our formal analysis in section 5 shows that the average of the coefficients to the left of the FRD is significantly different from the average of the coefficients to the right at 1% level.

In Figure 2, we repeat the same exercise as in the left panel but the dependent variable is the health index computed as we indicated in section 3. Here we also see a sharp drop for some of the 30-days interval dummies, but not all of them, indicating that the estimates will be nosier. This is expected as they are the corresponding second stage estimates of the left panel ones.

5. Does conditionality affect the number of preventive visits?

We implement our identification strategy estimating the following linear equation:

$$Y_{ijt} = \alpha * After\_FRD_{ij} + T_t + \lambda_j + \lambda_j * T_t + \mu_i + \delta X_{it} + \varepsilon_{ijt}, \qquad (1)$$

where the dependent variable Y_{ijt} is the total number of preventive care visits the child i resident in municipality j has received by time t, $After\_FRD_{ij}$ is an indicator variable that takes value of 1 if child i was born after the municipality j FRD, and 0 if born before the municipality j FRD. T_t indicates survey wave dummies (t=1,2,3), λ_j are municipalities fixed effects, μ_i are month-year of birth dummies and X_{it} a set of individual and household characteristics. The error term, ε_{ijt}, is possibly correlated between individuals living in the same municipality but uncorrelated between individuals living in different municipalities. The coefficient of interest is α, which estimates the effect of not being subject to the conditionality on the number of preventive care visits that children receive in treated municipalities.

Regression (1) makes explicit the identification strategy that we discussed in the previous section. Because we condition on month-year of birth dummies, μ_i, $After\_FRD_{ij}$ is identified from children who were born in the same month-year but some were born in municipalities where the FRD is before that month-year while others were born in municipalities where the FRD is later. The gradual expansion of the program is responsible for the variation in FRD across municipalities.

Table 4 reports estimates of regression (1). In Column 1, we include in X_{it} only age in year dummies . In column 2, we include the full set of controls in X_{it}: dummies for child age in years, household size, logarithm of birth order, gender, maternal and paternal educational dummies, number of siblings in the 0-6, 7-13, 14-17 age groups, and rural area residence. Both column 1 and 2 give very similar results: the lack of conditionality reduces the number of preventive visits in around 0.6 of a visit (a 50% decrease over the mean), and this is statistically significant at 1%. The effect that we estimate is smaller but not too different from that of Akresh et al. (2012), who find that children not subject to the conditionality received 64% less visits than children subject to the conditionality in Burkina Faso.

Preventive care check-ups are free and other costs of taking children to preventive care are probably independent of the number of children taken in a given visit (i.e. transportation costs and opportunity cost of time). Hence, the decrease in the number of preventive visits for children born after the FRD should be smaller if there are some older siblings who are subject to the conditionality requirement. This is because the younger sibling can be taken to preventive care at the same time as some of the older siblings are taken at probably zero additional cost. Hence, the effect of the lack of conditionality should be decreasing in the number of siblings subject to conditionality. The larger the number of siblings subject to conditionality, the more opportunities there are for taking the younger sibling to preventive care (because the number of recommended preventive visits vary by age, it is unlikely that all the older siblings will go at the same time). The last column of Table 4 shows suggestive evidence, although not statistically significant at conventional levels: that the number siblings who are subject to the conditionality mitigate the negative effect of the lack of conditionality.

Next, we provide direct evidence that the *FeA* transfers that households received do not differ according to whether a child is born before or after the municipality FRD. To show this, we estimate the same regression as (1) but using the last *FeA* transfer received by the household where the child lives (including zero if they haven't received a transfer). As expected, the estimate is small (8,805 is 8% of the average *FeA* transfer) and not statistically significant (see Table 5). We go further and test whether there are differences in the distribution of transfers by using as dependent variables in regression (1) a dummy variable on whether the household received a positive *FeA* transfer, as well as dummies for whether the household received a transfer higher than the 50th, 75th, and 90th percentiles of the distribution of the last transfer.[8] Again, we do not find significant differences.[9]

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[8] Note that the 25th percentile is zero so the results are the same as whether or not mothers have received a positive transfer.

[9] The number of observations is smaller than in the previous table because there are 31 municipalities that had not started to receive payments by the time the first wave was collected. As nobody could receive payments in these municipalities, we code them as missing in the first wave instead of zero.

6. Do preventive care visits improve child health outcomes?

As indicated in section 2, both child morbidity and nutritional status could improve following the intake of iron supplements and de-worming drugs, which are given to the children during the preventive visits, as well as the possible improvement in diet following the information given to the caregiver during the visit. In this section, we exploit the exogenous variation across children born before/after FRD in their exposure to preventive care visits, to assess the causal impact of preventive care in a summary health index, as well as on the measures that comprise it: whether a child suffers from diarrhoea, whether a child suffer from symptoms of respiratory infections, as well as whether the child is stunted, underweighted, and is at risk of wasting.

Before correcting the endogeneity of preventive visits, Table 6 reports the OLS estimates of the health outcome variables over the number of preventive visits over the same control variables as regression (1) except $After\_FRD_{ij}$. According to the OLS estimates, the prevalence of respiratory problems increases with the number of visits. This might be an indication of extremely low quality of preventive care, or more plausibly of negative self-selection into preventive care by those individuals who have worse health conditions. For instance, it could be that an important share of preventive care visits happen as the child goes to the health centre for curative care.

To estimate the effect of not being subject to the conditionality requirement, we use OLS to estimate regression (1) but using the health variables as dependent variables, instead of the number of visits. These results are the reduced form of our model and relate health outcomes to conditionalities. The estimates, reported in Table 7, shows that not being subject to the conditionality reduces the value of the composite health index (higher values of the index indicate better health), at 5% level of significance. When we consider the individual components of the index, they all go in the same direction: not being subject to the conditionality reduces health; but the only significant one on its own at 10% level is the probability of the child being underweight, which increases by 7.2 percentage points. Here it is clear that the index allow us to improve power by averaging over individual outcomes measures, which are all affected in the same direction.

Finally, to estimate the effect of preventive care on health, we use $After\_FRD_{ij}$ as an instrument for the number of preventive care visits, Y_{ijt}, in a regression in which the dependent variable, a health measure, is regressed over Y_{ijt}, municipality-wave dummies, month-year of birth dummies and the same covariates as those of regression (1), except of course $After\_FRD_{ij}$. Table 8 reports the Two-Stage Least Square estimates. Consistent with our previous findings, we find that preventive care visits improve health (increase the value of the composite health index) at 5% level. The estimates of the individual components of the index they all go in the expected direction: more preventive visits decrease morbidity and the likelihood of poor nutritional status independently of the measure used. However, each single estimate is too imprecise to be any one of them statistically significant at 5%.

Our results pose a puzzle on the demand for preventive health care. The question is: if the returns to this type of investment are positive, why is conditionality necessary to induce the beneficiaries of *FeA* to use them? One possibility, of course, is that these households are not aware of the returns. The second is that attendance to these visits implies costs for these households that reduce its demand. These costs could be related to opportunity costs.

7. Conclusions

In this paper, we have studied the effect of the conditionality in conditional cash transfer programs with a health component, using data from the Colombian *Familias en Acción* program. Despite conditional cash transfer programs being widely implemented in developing countries, little is known on the effects of the single components, the cash subsidy and the conditionality requirement.

Exploiting exogeneity in eligibility rules, we focus on the effects that excluding a child from the conditionality requirement would have on the child's preventive care and health status. Our results show that children who are not subject to the conditionality get 0.6 preventive care visit less than children who are. We also find that this feeds through to the child's health status, which decreases. Ultimately, this means that preventive care visits as delivered in rural villages in Colombia improve child's health.

Figure 1 – Preventive care visits and distance from the Family Registration Date (FRD)
measured in 30 day intervals
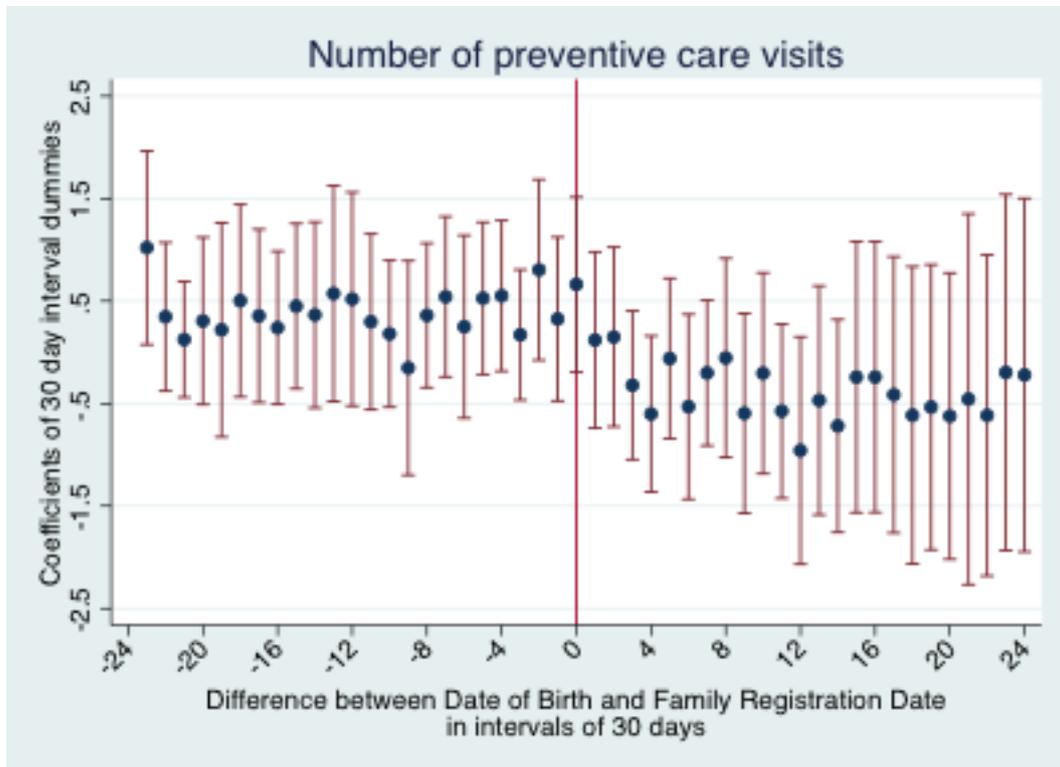
Notes: We run a regression of the number of preventive care visits on a set of control variables and a set of dummy variables d^j, $j=-27,...,55$, where $d^j=1$ if $30*(j-1)$ days <date of birth-FRD≤$30*j$ days. In this way d^j measures the difference between the child's date of birth and the FRD in intervals of 30 days. The dummy variable d^{24} is omitted from the regression (reference group). Other controls are: municipalities fixed effects, survey time dummies, the interaction between municipalities fixed effects and survey dummies, and months of birth dummies. The graph plots the coefficients of the 30 day interval dummies, d^j for $j=-23,...,24$ and their 95% confidence intervals computed using standard errors clustered at the municipality level.

Figure 2 – Health and distance from the Family Registration Date (FRD) measured in 30 day intervals
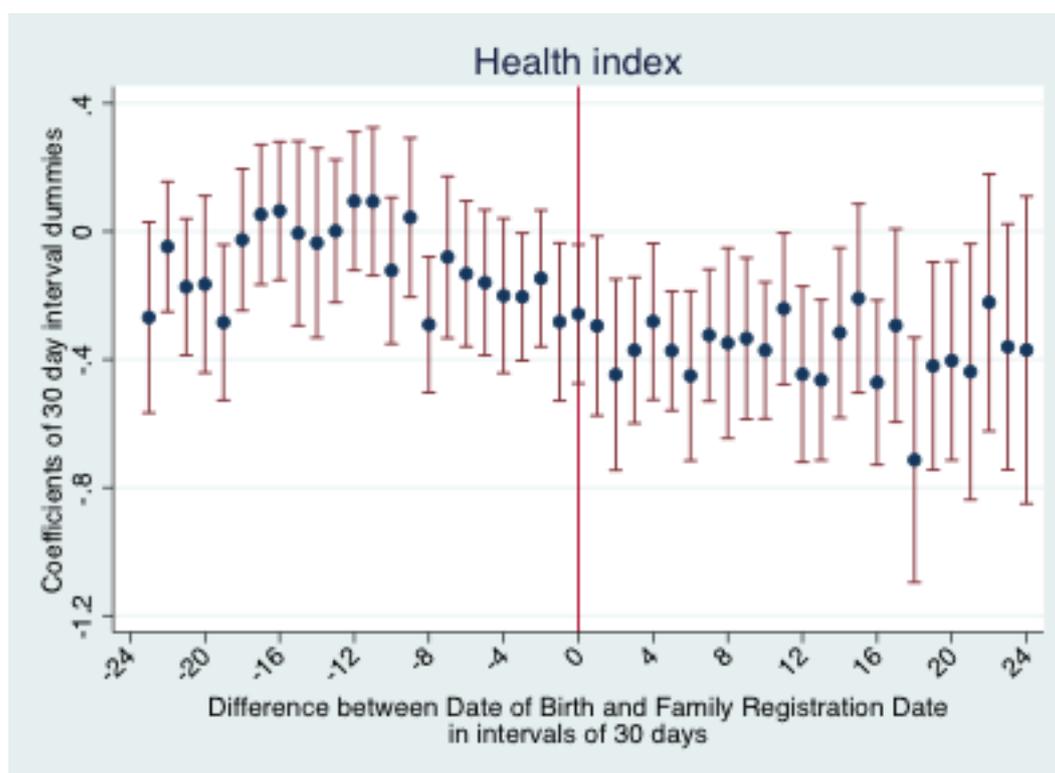
Notes: We run a regression of a health index on a set of control variables and a set of dummy variables d^j, $j=-27,...,55$, where $d^j=1$ if $30*(j-1)$ days <date of birth-FRD$\leq 30*j$ days. In this way d^j measures the difference between the child's date of birth and the FRD in intervals of 30 days. The dummy variable d^{-24} is omitted from the regression (reference group). Other controls are: municipalities fixed effects, survey time dummies, the interaction between municipalities fixed effects and survey dummies, and months of birth dummies. The graph plots the coefficients of the 30 day interval dummies, d^j for $j=-23,...,24$ and their 95% confidence intervals computed using standard errors clustered at the municipality level.

Table 1. Preventive care visits schedule

Age in months	≤12	13-24	25-36	37-60	61-72	73-84
Number of visits	5	3	2	2	2	2

*Not*es: The table shows the schedule of preventive care visits in FeA according to children age in months.

Table 2. Distribution of children by survey and period of birth

	Children born after FRD	Children born before FRD	Total
2001	175	1,577	1,752
2003	591	742	1,333
2005	506	0	506
Total	1,272	2,319	3,591

Notes: The table shows the distribution of children in any period according to whether they were born before or after Family Registration Date to the program.

Table 3. Distribution of households by children birth with respect to FRD, and survey

	Households with children born before and after FRD	Households with children born before FRD	Total
2001	176	1,332	1,508
2003	596	545	1,141
2005	439	0	439
Total	1,211	1,877	3,088

Notes: The table shows the distribution of households with children less than 36 months old according to whether they are have all children or some born before the municipality FeA Family Registration Date.

Table 4. The effect of conditionality on preventive care visits

Dependent variable: Nro of care visits	(1)	(2)	(3)
After_FRD	-0.634***	-0.574***	-0.778***
	[0.152]	[0.151]	[0.228]
Number of siblings born Before_FRD			-0.024
			[0.071]
Number of siblings born Before_FRD*After_FRD			0.118
			[0.080]
Community fixed effects*Survey fixed effects	yes	yes	yes
Cohort and age effects	yes	yes	yes
Individual controls	no	no	yes
Observations	3,591	3,591	3,591
R-squared	0.285	0.308	0.308
Mean dep. Variable		*1.25*	

Notes: This table shows the OLS effect of lack of conditionality (born after Family Registration Date dummy) on the number of preventive care visits. In column 1 we control for municipalities fixed effects, survey time dummies, the interaction between municipalities fixed effects and survey dummies, months of birth dummies, and age in years dummies. In columns 2 and 3 we add individual and households characteristics (gender, logarithm of birth order, family size, maternal and paternal education dummies, number of sibling in the 0-6, 7-13, and 14-17 age groups, rural area). In column 3 we include a control for the number siblings born before FRD, and its interaction with the dummy for being born after FRD. Standard errors clustered at the municipality level in brackets.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5. Differences in subsidy payment and amount across households with and without children born after FRD in treated villages

	FeA amount (pesos)	FeA payment positive	FeA payment greater than 50th pctile	FeA payment greater than 75th pctile	FeA payment greater than 90th pctile
	(1)	(2)	(3)	(4)	(5)
Born after FRD	-8,805.57	0.027	0.009	-0.073	-0.056
	[13,408.124]	[0.053]	[0.064]	[0.049]	[0.036]
Observations	2,641	2,641	2,641	2,641	2,641
R-squared	0.248	0.209	0.256	0.293	0.235
Mean dep var.	*87,336*	*0.78*	*0.59*	*0.29*	*0.12*

Notes: This table shows the OLS relation between households' payment received and having a child born after the Family Registration Date to the program. In the first column the dependent variable is the amount of pesos received in the last *FeA* payment, in the second a dummy for having received a positive payment, in the third a dummy for having received a payment above the 50th percentile of the amount distribution, in the fourth a dummy for the amount being greater than the 75th percentile, in the fifth greater than the 90th percentile. Controls include individual and households characteristics (months and years of birth dummies, age in year dummies, gender, logarithm of birth order, family size, maternal and paternal education dummies, number of sibling in the 0-6, 7-13, and 14-17 age groups, rural area), municipalities fixed effects, survey time dummies and the interaction between municipalities fixed effects and survey dummies.

Table 6. OLS Regression of Health outcomes on Preventive Care Visits

Dependent variable:	Health index	Acute Diarrhoea	Respiratory infections	Stunted	Underweight	Risk of being Wasted
	(1)	(2)	(3)	(4)	(5)	(6)
Preventive care visits	-0.007	0.006	0.018***	0.000	-0.003	-0.004
	[0.006]	[0.005]	[0.005]	[0.006]	[0.004]	[0.004]
Observations	3.221	3.591	3.589	3.275	3.285	3.228
R-squared	0.176	0.102	0.147	0.194	0.131	0.13
Mean dep variable	*0.00*	*0.21*	*0.41*	*0.22*	*0.12*	*0.15*

Notes: This table shows the OLS coefficients of preventive care visits on health outcomes (as indicated in the columns headings). Controls include individual and households characteristics (months and years of birth dummies, age in year dummies, gender, logarithm of birth order, family size, maternal and paternal education dummies, number of sibling in the 0-6, 7-13, and 14-17 age groups, rural area), municipalities fixed effects, survey time dummies and the interaction between municipalities fixed effects and survey dummies. Standard errors clustered at the municipality level in brackets.
*** p<0.01, ** p<0.05, * p<0.1

Table 7. OLS Reduced form Regression of Lack of Conditionality on Health Outcomes

Dependent variable:	Health index	Acute Diarrhoea	Respiratory infections	Stunted	Underweight	Risk of being Wasted
	(1)	(2)	(3)	(4)	(5)	(6)
After_FRD	-0.131**	0.066	0.073	0.054	0.072*	0.026
	[0.051]	[0.040]	[0.044]	[0.037]	[0.037]	[0.038]
Observations	3,221	3,591	3,589	3,275	3,285	3,228
R-squared	0.177	0.103	0.145	0.195	0.133	0.130
Mean dep variable	*0.00*	*0.21*	*0.41*	*0.22*	*0.12*	*0.15*

Notes: This table shows the OLS coefficients of lack of conditionality (born after Family Registration Date dummy) on health outcomes (as indicated in the columns headings). Controls include individual and households characteristics (months and years of birth dummies, age in year dummies, gender, logarithm of birth order, family size, maternal and paternal education dummies, number of sibling in the 0-6, 7-13, and 14-17 age groups, rural area), municipalities fixed effects, survey time dummies and the interaction between municipalities fixed effects and survey dummies. Standard errors clustered at the municipality level in brackets.
*** p<0.01, ** p<0.05, * p<0.1

Table 8. TSLS Regression of Health Outcomes on Preventive Care Visits

Dependent variable:	Health index	Acute Diarrhoea	Respiratory infections	Stunted	Underweight	Risk of being Wasted
	(1)	(2)	(3)	(4)	(5)	(6)
Preventive care visits	0.223**	-0.115	-0.126	-0.092	-0.124*	-0.045
	[0.103]	[0.071]	[0.083]	[0.070]	[0.069]	[0.067]
Observations	3.221	3.591	3.589	3.275	3.285	3.228
F(1,56)	13.77	14.43	14.46	14.1	14.05	13.73
Prob>F	0.0005	0.0004	0.0004	0.0004	0.0004	0.0005
Mean dep variable	*0.00*	*0.21*	*0.41*	*0.22*	*0.12*	*0.15*

Notes: This table shows the coefficients of a separate Two-Stage Least Square regression of a health variable (as indicated in the column heading) on the number of preventive care visits. Controls include individual and households characteristics (months and years of birth dummies, age in year dummies, gender, logarithm of birth order, family size, maternal and paternal education dummies, number of sibling in the 0-6, 7-13, and 14-17 age groups, rural area), municipalities fixed effects, survey time dummies and the interaction between municipalities fixed effects and survey dummies. Preventive care visit is instrumented with the dummy for being born after Family Registration Date. Standard errors clustered at the municipality level in brackets.
*** p<0.01, ** p<0.05, * p<0.1

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Appendix

-Not for Publication-

A1. Descriptive statistics

Definition	Mean	Sd.	Min	Max
Covariates:				
1 if the child is born after median family registration date, 0 otherwise	0.35	0.48	0	1
1 if the child is born after 10th percentile family registration date, 0 otherwise	0.4	0.49	0	1
1 if the child is born after 20th percentile family registration date, 0 otherwise	0.38	0.49	0	1
1 if the child is interviewed in first wave, 0 otherwise	0.49	0.5	0	1
1 if the child is interviewed in second wave, 0 otherwise	0.37	0.48	0	1
1 if the child is interviewed in third wave, 0 otherwise	0.14	0.35	0	1
1 if the child is female, 0 if the child is a male	0.48	0.5	0	1
Child age in years	1.16	0.81	0	2
Family size	6.84	2.36	2	21
Order of the child in the family	4.23	1.84	1	13
Logarithm of order of the child in the family	1.34	0.46	0	2.56
1 if head has below primary education, 0 otherwise	0.66	0.47	0	1
1 if head has below secondary education, 0 otherwise	0.3	0.46	0	1
1 if head has secondary education, 0 otherwise	0.04	0.19	0	1
1 if mother has below primary education, 0 otherwise	0.59	0.49	0	1
1 if mother has below secondary education, 0 otherwise	0.36	0.48	0	1
1 if mother has secondary education, 0 otherwise	0.05	0.22	0	1
Number of siblings in the 0-6 age group	2.44	1	0	7
Number of siblings in the 7-13 age group	1.42	1.2	0	6
Number of siblings in the 14-17 age group	0.39	0.67	0	3
1 if the household lives in the rural part of the municipality, 0 otherwise	0.61	0.49	0	1
Other:				
Number of visits the child received since born until ≤36 months old	1.25	1.7	0	12
Number of siblings subject to the conditionality	1.35	1.09	0	6
health index on morbidity, chronic, global and risk of malnutrition	0	0.54	-1.87	0.57
1 if the child suffers acute diarrhoea in the last 15 days, 0 otherwise	0.21	0.41	0	1
1 if the child suffers acute respiratory infection in the last 15 days	0.41	0.49	0	1
1 if the child is stunted, 0 otherwise	0.22	0.41	0	1
1 if the child is underweight, 0 otherwise	0.12	0.33	0	1
1 if the child is at risk of being wasted	0.15	0.36	0	1
Amount of last payment received from *FeA*	87336	69145.9	0	930000
1 if the household received a positive payment from *FeA*	0.78	0.42	0	1
1 if the household received a payment higher than 25th percentile of the distribution	0.78	0.42	0	1
1 if the household received a payment higher than 50th percentile of the distribution	0.59	0.49	0	1
1 if the household received a payment higher than 75th percentile of the distribution	0.29	0.46	0	1
1 if the household received a payment higher than 90th percentile of the distribution	0.12	0.33	0	1

A2. The effect of conditionality on preventive care visits

Dependent variable: Nro of care visits	(1)	(2)	(3)
After_FRD (**10th percentile**)	-0.495**	-0.475**	-0.514**
	[0.191]	[0.186]	[0.253]
Number of siblings born Before_FRD _10th pctl			0.035
			[0.076]
Number of siblings born Before_FRD*After_FRD_10th pctl			0.027
			[0.091]
Observations	3,591	3,591	3,591
R-squared	0.284	0.307	0.308
After_FRD (**20th percentile**)	-0.608***	-0.549***	-0.698***
	[0.187]	[0.186]	[0.229]
Number of siblings born Before_FRD _20th pctl			0.004
			[0.074]
Number of siblings born Before_FRD*After_FRD_20th pctl			0.082
			[0.094]
Observations	3,591	3,591	3,591
R-squared	0.285	0.308	0.308
Community fixed effects*Survey fixed effects	yes	yes	yes
Cohort and age effects	yes	yes	yes
Individual controls	no	no	yes
Mean dep. Variable		*1.25*	

Notes: This table shows the OLS effect of lack of conditionality (born after Family Registration Date dummy) on the number of health care visits. The upper panel shows estimates using the 10th percentile of the FRD, the bottom using the 20th percentile. In column 1 we control for municipalities fixed effects, survey time dummies, the interaction between municipalities fixed effects and survey dummies, months of birth dummies, and age in years dummies. In columns 2 and 3 we add individual and households characteristics (gender, logarithm of birth order, family size, maternal and paternal education dummies, number of sibling in the 0-6, 7-13, and 14-17 age groups, rural area). In column 3 we include a control for the number siblings born before FRD, and its interaction with the dummy for being born after FRD. Standard errors clustered at the municipality level in brackets.

*** p<0.01, ** p<0.05, * p<0.1

A3. Differences in subsidy payment and amount across households with and without children born after FRD in treated villages

	FeA amount (pesos)	FeA payment positive	FeA payment greater than 50th pctile	FeA payment greater than 75th pctile	FeA payment greater than 90th pctile
	(1)	(2)	(3)	(4)	(5)
Born after FRD (10th percentile)	2,257.99	-0.007	-0.013	-0.005	0.022
	[6,507.796]	[0.040]	[0.043]	[0.043]	[0.027]
Observations	2,641	2,641	2,641	2,641	2,641
R-squared	0.248	0.209	0.256	0.292	0.234
Born after FRD (20th percentile)	-8,725.60	-0.061	-0.087	-0.064	-0.031
	[7,640.754]	[0.045]	[0.059]	[0.045]	[0.039]
Observations	2,641	2,641	2,641	2,641	2,641
R-squared	0.248	0.21	0.256	0.293	0.234
Mean dep var.	*87,336*	*0.78*	*0.59*	*0.29*	*0.12*

Notes: This table shows the OLS relation between households' payment received and having a child born after Family Registration Date to the program. The upper panel shows estimates using the 10th percentile of the FRD, the bottom using the 20th percentile. In the first column the dependent variable is the amount of pesos received in the last *FeA* payment, in the second a dummy for having received a positive payment, in the third a dummy for having received a payment above the 25th percentile of the amount distribution, in the fourth a dummy for the amount being greater than the 50th percentile, in the fifth greater than the 75th percentile, and in the last greater than the 90th percentile. Controls include individual and households characteristics (months and years of birth dummies, age in year dummies, gender, logarithm of birth order, family size, maternal and paternal education dummies, number of sibling in the 0-6, 7-13, and 14-17 age groups, rural area), municipalities fixed effects, survey time dummies and the interaction between municipalities fixed effects and survey dummies.

A4. OLS Reduced form Regression of Lack of Conditionality on Health Outcomes

Dependent variable:	Health index	Acute Diarrhoea	Respiratory infections	Stunted	Underweight	Risk of being Wasted
	(1)	(2)	(3)	(4)	(5)	(6)
After_FRD (**10th pctl**)	-0.065	0.046	0.003	0.023	0.069**	0.024
	[0.047]	[0.033]	[0.034]	[0.032]	[0.033]	[0.031]
Observations	3,221	3,591	3,589	3,275	3,285	3,228
R-squared	0.176	0.103	0.144	0.194	0.134	0.13
After_FRD (**20th pctl**)	-0.117***	0.068**	0.049	0.042	0.086***	0.017
	[0.043]	[0.031]	[0.038]	[0.030]	[0.030]	[0.033]
Observations	3,221	3,591	3,589	3,275	3,285	3,228
R-squared	0.177	0.103	0.145	0.195	0.134	0.13
Mean dep variable	*0.00*	*0.21*	*0.41*	*0.22*	*0.12*	*0.15*

Notes: This table shows the OLS coefficients of lack of conditionality (born after Family Registration Date dummy) on health outcomes. The upper panel shows estimates using the 10th percentile of the FRD, the bottom using the 20th percentile. Controls include individual and households characteristics (months and years of birth dummies, age in year dummies, gender, logarithm of birth order, family size, maternal and paternal education dummies, number of sibling in the 0-6, 7-13, and 14-17 age groups, rural area), municipalities fixed effects, survey time dummies and the interaction between municipalities fixed effects and survey dummies. Standard errors clustered at the municipality level in brackets.

*** p<0.01, ** p<0.05, * p<0.1

A5. TSLS Regression of Health Outcomes on Preventive Care Visits

	Health index	Acute Diarrhoea	Respiratory infections	Stunted	Underweight	Risk of being Wasted
	(1)	(2)	(3)	(4)	(5)	(6)
Instrument: After_FRD (10th pctl)						
Preventive care visits	0.137	-0.096	-0.005	-0.049	-0.146	-0.05
	[0.109]	[0.073]	[0.071]	[0.071]	[0.093]	[0.068]
Observations	3,221	3,591	3,589	3,275	3,285	3,228
F(1,56)	7	6.54	6.55	6.59	6.57	6.93
Prob>F	0.0106	0.0133	0.0132	0.013	0.0131	0.0109
Instrument: After_FRD (20th pctl)						
Preventive care visits	0.209*	-0.123*	-0.089	-0.073	-0.151*	-0.031
	[0.112]	[0.070]	[0.078]	[0.062]	[0.077]	[0.061]
Observations	3,221	3,591	3,589	3,275	3,285	3,228
F(1,56)	8.65	8.75	8.76	8.52	8.34	8.63
Prob>F	0.0047	0.0045	0.0045	0.0051	0.0055	0.0048
Mean dep variable	*0.00*	*0.21*	*0.41*	*0.22*	*0.12*	*0.15*

Notes: This table shows the coefficients of a separate Two-Stage Least Square regression of a health variable (as indicated in the column heading) on preventive care. The upper panel shows estimates using the 10th percentile of the FRD, the bottom using the 20th percentile. Controls include individual and households characteristics (months and years of birth dummies, age in year dummies, gender, logarithm of birth order, family size, maternal and paternal education dummies, number of sibling in the 0-6, 7-13, and 14-17 age groups, rural area), municipalities fixed effects, survey time dummies and the interaction between municipalities fixed effects and survey dummies. Preventive care visit is instrumented with the dummy for being born after Family Registration Date. Standard errors clustered at the municipality level in brackets.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$