Accelerating Birth Timing to Access Cash Transfers? Evidence from Households in Extreme Poverty*

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Abstract

There has been a sustained rise in cash transfer programs to the poor, and burgeoning interest in interventions promoting early childhood development. We draw together these trends to study whether open enrolment interventions targeting cash transfers to pregnant mothers unintentionally induce those not pregnant to accelerate birth timing in order to start receiving cash transfers. Our study context is rural Northern Nigeria, where households have high demand for liquidity because the majority reside in extreme poverty, are credit constrained, and are reliant on volatile earnings streams from agriculture. Our evidence comes from a four-year evaluation of an intervention providing high-valued unconditional cash transfers to pregnant mothers, with open enrolment into the program. We examine how this impacts pregnancy timing and birth spacing among 1700 women not pregnant at baseline. We document relatively weak distortionary impacts on pregnancy timing and birth spacing. The reasons are women retain full control over the use of cash transfers, they have available productive investment opportunities in their own businesses, and they choose to invest in those rather than transfer cash to husbands. This constellation of factors allows women to internalize the marginal benefits and marginal costs of accelerating birth timing, and so place a brake on the incentives households otherwise have to accelerate birth timing. On external validity, we draw together 45 DHS surveys to classify countries into those more or less likely to see distortionary effects on birth timing from open enrolment interventions targeting cash transfers to pregnant mothers. *JEL: I15, O15.*

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1 Introduction

A profound change in the development policy landscape has been the increased provision of cash transfers directly to the poor. 119 low-income countries have now implemented unconditional cash transfer programs, and a further 52 have established forms of conditional cash transfer programs [Handa *et al.* 2017]. This trend extends to Sub Saharan Africa, where 40 countries had implemented unconditional cash transfer programs by 2014, double the number in 2010 [De Groot *et al.* 2017]. A body of evidence shows cash transfers reduce poverty through multiple channels related to education, health and nutrition, savings and investment [Bastagli *et al.* 2018].

Equally stark has been the growing recognition that 250 million children in low- and middle-income countries are at risk of not meeting their developmental potential because of inadequate nutrition and stimulation [Black *et al.* 2017], and such deprivation has consequences throughout the life cycle [Almond and Currie 2011]. This has resulted in a growth of interventions promoting early childhood development (ECD), including in Sub Saharan Africa [Carneiro *et al.* 2021]. Such interventions have been shown to generate private, social and intergenerational returns [Gertler *et al.* 2014, Conti *et al.* 2016, Heckman and Karapakula 2019, Doyle 2020, Carneiro *et al.* 2021].

These trends are intertwining, with cash transfers increasingly being targeted to households with newborn children with the aim of improving human capital accumulation in early life. A standard feature of such interventions is that eligibility hinges on whether a woman within the household is pregnant, or has a very young child. We study whether the offer of cash transfers as part of an open enrolment ECD intervention distorts one aspect of fertility, birth timing, in households that have no pregnant women in them.[1] The question is relevant because of a basic tension in open enrolment interventions: economically disadvantaged households to which cash transfers are targeted are also those households with high demand for short term liquidity, providing them incentives to accelerate birth timing in order to gain transfers. In so doing, households deviate away from their optimal fertility paths, shorten birth spacing potentially worsening child and maternal outcomes, and at least partially undoing some of the intended effects of the intervention.

Our study context is rural households in Jigawa and Zamfara states in North West Nigeria. In this region households lack basic information related to antenatal practices and infant nutrition, and the majority of young children are at risk of not reaching their development potential: 60% are stunted. The majority of households also face extreme economic destitution: 72% of our sample resides in extreme poverty, so below the $1.90/day threshold. A combination of extreme poverty, volatile earnings streams from agriculture, imperfect credit markets that prevent resources being shifted across time, and frequent man-made and natural aggregate shocks that villages in this region are subject to, all provide households with high demand for short term liquidity. Household demand for liquidity has been experimentally documented in other low-income contexts, including

[1] The intervention we study could potentially impact the number of children born for a subset of families. However, as we discuss below, the fertility incentives it provides affect the timing more than the quantity of births.

in response to lean seasons and fluctuating income streams from agriculture [Casaburi and Willis 2018, Casaburi and Macchiavello 2019, Fink *et al.* 2020, Mobarak *et al.* 2021]. We build on these insights by linking such mechanisms to the design of open enrolment programs providing cash transfers as part of interventions targeting early childhood development.

Our evidence comes from a long term randomized control trial to evaluate an open enrolment intervention offering high-valued cash transfers to pregnant women, that aims to foster early childhood development. The intervention is randomized across 210 villages. We track a sample of 1700 households with women not pregnant in them at baseline, explicitly collected to address our research question. The intervention comprises a bundle of information to mothers and fathers on recommended practices related to antenatal practices and infant feeding, and unconditional cash transfers. Transfers are paid directly to mothers conditional on their pregnancy being verified, so while the child is *in utero*, each month until the child turns 24-months old. The value of the unconditional cash transfer is US$22 per month. Their monthly value is substantial, corresponding to 100% of women's baseline monthly earnings or 27% of monthly food expenditures. The cumulative resources available to enrolled households is $500, akin to a big push on the scale as asset transfer policies [Banerjee *et al.* 2015, Bandiera *et al.* 2017]. The fact that women know transfers will be provided monthly over the first 24 months of the child's life provides them with a more stable flow of resources than available from agriculture or other labor activities: the transfers almost act as a *de facto* temporary basic income for pregnant mothers.

The ECD program design is standard in that transfers are provided for one child – the first one born after the program starts. Enrolment into the program was announced to be open for four years from baseline. Hence given households' demand for short term liquidity, this program structure provides households economic incentives to accelerate birth timing (a *tempo* effect), enrol into the program and start receiving cash transfers. The program design gives weaker incentives to increase total fertility (a *quantum* effect). We track households from baseline, a two-year midline and four-year endline. Our study timeline is thus long enough to detect responses among two types of household: (i) those that accelerate birth timing to start receiving transfers earlier than their pre-intervention planned fertility path; (ii) those that decide to bring forward birth timing into the four-year period of open enrolment, because they otherwise lose access to the high-valued cash transfers altogether.

The marginal costs of accelerating birth timing are borne largely by mothers and children. In our sample women are aged 24 and have 4 children aged below 18 and resident with them. Average birth spacing is 33 months, that remains well above a biological lower threshold. Hence it is feasible for birth timing to be accelerated in response to the offer of cash transfers. However, as birth spacing starts close to the WHO recommendation of 33 months, a body of medical evidence suggests that even small reductions can place children at higher risks of mortality and undernutrition, and mothers at higher risk of birth complications [DaVanzo *et al.* 2004, WHO 2005, Pimentel *et al.* 2020, Damtie *et al.* 2021]. Moreover, if husbands have some say over birth timing and are less informed of risks of maternal mortality and morbidity compared to their wives [Ashraf *et al.* 2022], this further tilts the balance towards endogenously accelerating birth timing in order for their wives to begin receipt of cash transfers.[2]

Our core finding, using survival analysis, is a positive but muted treatment effect on fertility dynamics over the four-year study window. In our full sample, we find no statistically significant evidence of accelerated birth timing, changes in birth spacing, or the total number of children born among treated households relative to controls. We provide evidence to rule out various explanations for why average effects are muted by: (i) accounting for differential likelihoods to give birth during our study period; (ii) examining whether the intervention increases the use of methods to delay pregnancy; (iii) documenting that households understand there is a limited period of open enrolment, with those giving birth outside of that window losing access to cash transfers forever; (iv) examining whether accelerated birth timing is concentrated among households at the start of their fertility cycle, or among non-polygamous households (because in polygamous households at least one wife would become eligible relatively quickly). While our data and sample size do allow us to find evidence of accelerated birth timing among specific subsets of households, the broad pattern of evidence suggests most households do not accelerate birth timing in order to start receiving the cash transfers.

To understand what causes muted responses in birth timing despite the economic circumstances of households, we unpack who in the household makes decisions over birth timing, and over how to spend cash transfers from the intervention. We find that while nearly no women have access to family planning and men thus have relatively more say over birth timing, the reverse is true for the control of the cash transfers received. In over 75% of households, women report being able to decide alone over how the unconditional cash transfer is spent. How wives choose to spend the cash transfer *ex post* shapes the *ex ante* incentives for husbands and wives to accelerate birth timing. To understand these incentives, we map how the marginal benefits from cash transfers distribute over: (i) outcomes benefiting husbands; (ii) outcomes benefiting wives; (iii) common gains to both spouses.

We find wives choose to transfer little of the cash transfer to husbands. There are no impacts on husband's labor supply, investment in the husband's business, or their overall earnings. In sharp contrast, wives use their agency over how to spend the cash transfers to significantly increase their own labor force participation, and drive forward significant increases in their self-employment related to rearing livestock and petty trading (there are 26% and 37% increases in these forms of self-employment by endline, respectively). These increases in labor supply and self-employment

[2] The WHO recommends a minimum birth interval of 33 months to maximize health benefits for mothers and newborns [WHO 2005]. Spacing a minimum of two years reduces infant mortality by 50% [DaVanzo *et al.* 2004]. Short birth spacing has been linked with adverse pregnancy and childbirth outcomes such as low birth weight, preterm birth, congenital anomalies autism, small size for gestational age, neonatal, infant and child mortality. Women with short birth spacing are at high risk of developing hypertensive disorders of pregnancy, anemia, third-trimester bleeding, premature rupture of membranes and puerperal endometritis [Damtie *et al.* 2021].

are matched by increased investments into business inputs for women's self-employment – for example, women's ownership of livestock increases by 16%. By endline, the resulting effect on women's monthly earnings is an increase of $17 (a 78% rise over the baseline level in controls).

There are of course also common gains from cash transfers, shared and valued across household members. We document such gains in: (i) food expenditure; (ii) food security; (iii) savings; (iv) early child outcomes. Spouses value these gains, but how these common gains accrue is determined through womens' choices of how to spend the cash transfers, and so they internalize the marginal benefits of such shared gains against the marginal costs borne by them if birth timing is accelerated. The fact that birth timing is not shifted forward to receive these gains earlier suggests the marginal costs of bringing forward birth timing outweigh the marginal benefits from doing so. As we document muted responses in birth timing even among households that give birth outside the window of open enrolment, this suggests these marginal costs, that are borne by women and children, are valued at least as high as the present value of the cumulative lost cash transfers from the intervention, that is close to $500.

In short, in our study context muted impacts on birth timing from the offer of high-valued cash transfers can be explained through a combination of factors: (i) women entirely retain control of the cash transfers from the intervention, even though men have considerable say over birth timing; (ii) women have available productive investment opportunities in their own businesses; (iii) women choose to transfer few resources to husbands and invest in their own businesses. This constellation of factors means women are able to apply a brake on the economic incentives husbands might have to accelerate birth timing, even among households residing in extreme poverty and in the presence of the offer of high-valued cash transfers.

Given these factors driving our main finding, we use cross-country data to speculate on external validity. We draw together 45 DHS surveys to shed light on potential distortionary effects on birth timing when cash transfers are targeted to pregnant mothers. This analysis provides new insights on which other countries have the same constellation of factors related to women retaining control over resources and having productive employment/investment opportunities, as in our context. These include Nigeria as a whole, Ghana, Gambia, Uganda and Burkina Faso. All else equal, we might expect muted consequences on birth timing from similar open enrolment interventions offering high-valued cash transfers to pregnant women in those contexts. At the other extreme, in contexts such as Ethiopia, Mozambique and Afghanistan, women lack agency and labor market opportunities. These are settings where our results suggest more caution when cash transfers are targeted to pregnant women to promote ECD.

Our analysis contributes to the burgeoning literature evaluating early childhood interventions, a more established literature on cash transfers, and a long-standing body of work examining household decision making over fertility.

In relation to ECD-related cash transfers programs, earlier work has understandably focused on impacts on child anthropometric and health outcomes [Sridhar and Duffield 2006, Manley *et* *al.* 2013, Levere *et al.* 2016, Fernald *et al.* 2017, Ahmed *et al.* 2019]. Far less is known about distortionary effects on birth timing among initially non-eligible households for programs with open enrolment. This is partly because studies have combined samples of pregnant and non-pregnant women and simply not considered distortions in birth timing [Maluccio and Flores 2004, Levere *et al.* 2016, Fernald *et al.* 2017, Ahmed *et al.* 2019, Field and Maffioli 2021].

On the wider literature on cash transfers, eligibility is sometimes tied to compliance with activities related to the education of older children, health or work. In some cases, benefit amounts have been linked to the number of children, but usually only for older school-aged children. There is a body of work examining impacts of such non-ECD related cash transfers on fertility. This continues a long history in economics studying income effects on fertility, stemming back to Malthus [1890]. Work using experimental research designs has largely focused on *quantum* fertility responses to cash transfers to shed light on income versus substitution effects (or child quantity versus quality trade-offs). Much of this evidence has been generated from conditional cash transfer programs in Latin America [Stecklov *et al.* 2007, Arenas *et al.* 2015, Palermo *et al.* 2016, Handa *et al.* 2017, Baird *et al.* 2019, Parker and Ryu 2023]. Our analysis is distinct from this body of work: rather than examining *quantum* effects on total fertility, we use survival analysis to study whether cash transfers conditional on pregnancy as part of an open enrolment ECD intervention induce endogenous *tempo* responses in birth timing among those initially not eligible.[3]

We go beyond the analyses of cash transfers on total fertility in earlier work, and offer a way to reconcile findings across contexts. Our approach adds to a nascent literature emphasizing how the nature of intrahousehold decision making determines fertility [Rasul 2008, Ashraf *et al.* 2014, Doepke and Kindermann 2019, Rossi 2019, Ashraf *et al.* 2022] – we add to this by showing such decision making processes also shape (unintended) fertility responses to common policy interventions. We provide granular detail on how cash transfers are spent *ex post*, and this helps understand the *ex ante* private incentives of spouses to accelerate birth timing. This reveals a constellation of factors related to household decision making and women's investment opportunities that allow women to put a brake on distortionary changes in birth timing despite the demand for short term liquidity, and helps explain why the same intervention can have different impacts on birth timing in other contexts. We are thus able to explain the results found by some earlier studies in other contexts in Sub-Saharan Africa [Palermo *et al.* 2016, Handa *et al.* 2017].[4]

[3] One exception is Todd *et al.* [2012] who also use survival analysis to estimate the impact of cash transfers on birth timing. They do so in the context of a standard non-ECD conditional cash transfer intervention in Nicaragua, the *Red de Proteccion*, where transfers are provided conditional on compliance with education, nutritional and health requirements (including increased access to contraceptives), and participation in other program activities. They find the intervention decreased the hazard of birth and increased birth spacing.

[4] There is also an established literature from high-income countries on how cash benefits – including childcare support, tax credits and paid leave – impact fertility. This literature has studied the impacts of tax incentives on fertility [Moffitt 1998, Rosenzweig 1999, Baughman and Dickert-Conlin 2003], those of the wider benefits system on fertility [Hotz *et al.* 1997, Grogger and Bronars 2001, Laroque and Salanie 2004, Milligan 2005, Cohen *et al.* 2013, González 2013, Gonzales and Trommlerova 2023, Aizer *et al.* 2024], and how parental leave impacts fertility [Lalive and Zweimüller 2009, Malkova 2018]. Many of these studies focus on completed fertility, are identified from

Finally, this work relates to our earlier analysis using data from the same project. In Carneiro *et al.* [2021] we evaluated the same intervention tracking 3700 women already verified to be pregnant at baseline. Among that sample, by construction, there is no endogenous fertility response to the intervention. The focus was to document impacts on children *in utero* at baseline. We found persistent and large improvements in child outcomes and estimate an IRR to the program of at least 10% under plausible assumptions. Hence, the program is cost effective among the cross section of women that are exogenously pregnant at baseline. This paper focuses on an entirely different sample of 1700 not pregnant at baseline, to understand endogenous responses in birth timing to the offer of cash transfers. We later use our results to revisit Carneiro *et al.* [2021] though the lens of household decision making, and to understand whether and how our earlier results on cost effectiveness extend to the far larger cohort of women not pregnant at baseline, and thus understand the population-wide returns to the intervention.

Section 2 describes the intervention and data. Section 3 presents results on the timing of pregnancy, birth spacing, total fertility and maternal health. Section 4 explains muted distortionary impacts on the timing of pregnancy by examining household decision making over fertility and how cash transfers are spent. Section 5 revisits Carneiro *et al.* [2021], discusses external validity and implications for the design of ECD policies targeting cash transfers to households with a verified pregnancy. Section 6 concludes. Additional results are in the Appendix.

2 Intervention and Data

2.1 Intervention

The intervention we study is called the Child Development Grant Programme (CDGP). It is a village level intervention, providing information to mothers and fathers on recommended practices related to pregnancy and infant feeding, and unconditional cash transfers to mothers once they are verified to be pregnant. Verification uses an on-the-spot urine test in the presence of a female community volunteer [Sharp *et al.* 2018]. Eligibility is conditional only on verified pregnancy: any household can then enrol onto the program and start receiving cash transfers. Enrolment into the program was announced to be open for four years: any household having a verified pregnancy during that period could receive monthly cash transfers until the child turns 24 months old.[5]

natural experiments exploiting cross-jurisdiction variation in taxes/benefits, and sometimes the marginal financial incentive is hard to isolate (or needs to be simulated). Our research design and data allows us to improve on all three margins.

[5] In rural Nigeria, communities are normally subdivided into traditional wards, that represent separate clusters of households. In cases where communities were too large to serve as sampling units, we randomly selected one ward. In cases where a sampled community had less than 200 households, we merged it with the neighboring community. We refer to these sampling units as villages.

Information Information messages are tailored to the context and were developed by our intervention partners: Save the Children (SC) and Action Against Hunger (AAH). Panel A of Table A1 shows the messages disseminated. These relate to pre-natal, per-natal and post-natal behaviors. The program does not provide information related to the costs of shortened birth spacing, nor on methods to delay pregnancy. Panel B details the channels through which messages are delivered. Information is publicly disseminated in treated villages and so is non-excludable. Households therefore face no incentive to accelerate birth timing to acquire information on pregnancy and infant feeding from the program. To confirm this, Figure A1 shows recall rates at midline for the eight messages provided, so two years after the program started. The top panel shows recall rates for: (i) women pregnant at baseline; (ii) women that became pregnant between baseline and midline; (iii) women not pregnant at baseline and still not pregnant by midline. The bottom panel repeats the analysis for husbands. For all messages, knowledge improves with the intervention but there are no significant differences in recall between the three groups. This is in line with the timing of pregnancy being unrelated to the receipt of information from the intervention.

Cash Transfers This is in sharp contrast to the receipt of cash transfers: these can *only* begin to be given once pregnancy is verified. The value of the unconditional cash transfer – US$22 per month (at the PPP exchange rate in August 2014) – was calibrated by our intervention partners to correspond to the cost of a diverse household diet (not accounting for any crowd out of existing food expenditures). The monthly value of the transfer is substantial, corresponding to 12% of household monthly earnings, 100% of women's monthly earnings, or 27% of monthly food expenditures. Since transfers are provided monthly from when a pregnancy is verified until the child turns 24 months old, the cumulative value of transfers can be more than $500.

Transfers are given directly to women. Women are eligible to receive transfers for only one child – the child *in utero* when eligibility is established. For polygamous households, multiple wives in the same household can be eligible.[6]

[6] In the case of maternal mortality, payments would still be disbursed to a female caregiver of the child. In the case of child mortality, the woman remains eligible for a later child. Cash transfers were delivered by payment agents who visited villages monthly, using thumbprints to identify the correct eligible women, and transferring cash directly to them. Key challenges lay around ensuring security of payments and predictability of when transfers would occur. A target payment date of the 19th of each month was chosen (to avoid coinciding with state government payments and when local banks face liquidity issues). Beneficiaries collected payments from a fixed location (pay points), located within 5km of each village. The decision to centralize pay points (rather than have one per village) was made both for security reasons and to coordinate CDGP activities, such as pregnancy testing and information messaging. 95% of payments were made within 10 days of target [Visram *et al.* 2018]. On payment days, pay agents adopted a first-come-first served policy. Delays sometimes occurred on payment days if pay agents ran out of cash reserves and had to restock. Overall though, given the context, the cash transfer component of the intervention operated largely as intended.

2.2 Data Collection

Study Timeline Our evaluation covers 210 villages in two states in North West Nigeria: Zamfara and Jigawa. Pre-intervention, we conducted a village census covering $38,803$ women aged 12-49 in all 210 study villages. Households are almost entirely of Hausa ethnicity and Muslim religion, and are structured around a male household head. Nearly all are married, with 40% being in a polygamous relationship. The census identified households with a women aged 12-49 not pregnant at baseline, who could register for the program at any point of open enrolment in the next four years. Figure 1 shows the study timeline from June 2014. Our baseline survey took place from August to October 2014, our midline survey was conducted in October/November 2016, and the endline survey took place from August to October 2018.

Given the logistical challenges of operating in these states, cash transfers began being disseminated in August 2014, a few months after open enrolment started and information began being provided in treated villages. Just before the end of our study period it was announced enrolment would close from April 2019. Our study timeline is thus long enough to estimate behavioral responses among households that accelerate their birth timing to: (i) start receiving transfers earlier relative to the counterfactual in which they would have given birth later in the window of open enrolment; (ii) have their pregnancy verified during open enrolment, relative to the counterfactual in which they would have become pregnant after the window of open enrolment and lose access to cash transfers forever.

Surveys and Sampling We drew a sample of 26 women per village, interviewed separately from their husband on modules covering pregnancy, antenatal care and infant nutrition, labor activities, asset ownership/business investment, consumption and savings. We use this to build a detailed picture of how cash transfers are utilized within households to generate private gains to each spouse, as well as provide benefits common to all members. This helps pin down how the marginal benefits of the cash transfers are distributed across wives, husbands and children.

Among not pregnant women identified in our census, we selected those most likely to give birth in the two years after baseline using a prediction model based on the 2013 Nigeria DHS survey. Panel A of Figure A2 shows the distribution of predicted probabilities. Panel B shows the *ex post* likelihood of actually giving birth by midline. We find a weak gradient between the predicted and actual likelihood of giving birth. As such, the sample is more representative of all not pregnant women at baseline, and thus we only exploit the predicted probabilities for robustness checks.[7]

Our baseline sample covers 1743 not pregnant women and their husbands. We implemented a mother-child survey to collect outcomes for the first child born after baseline. We refer to this as the ‘target’ child: this is the only child for whom cash transfers are provided. At baseline we

[7] We predict the likelihood of becoming pregnant using the covariates common with our census: age, time since last birth, household size, number of children aged below five and TV ownership.

also collect information about a randomly selected child aged 0-60 months – providing a sample of 1565 older siblings of the target child. Among our sample of not pregnant women at baseline: (i) 36% had no target child by midline; (ii) 64% had one additional child. We obtained data on 973 (1330) target children at midline (endline).

Randomization and Attrition Villages were randomly assigned to a control group or two treatment arms. Treatment arms varied only in the intensity of information delivered, as described in Table A1. The cash transfer component of the intervention is identical in both treatment arms and so for this study, we combine them throughout. We divided villages into three tranches, with random assignment of villages taking place within each tranche.

We focus on the cash transfer component as potentially shifting fertility dynamics among those not immediately eligible for the intervention. We however recognize the information component could potentially alter perceived costs and benefits of having another child. This could cause birth timing to be delayed or accelerated. We examine this possibility by exploiting the experimental variation in the intensity of information delivered across treatment arms.

By the four-year endline, 20% of women had attrited. Table A2 shows attrition to be: (i) uncorrelated to treatment; (ii) almost perfectly predicted by whether the village is insecure at endline (and thus enumerators were unable to travel there and interview *any* households). In villages that were always secure, only 8% of women attrit by endline; (iii) there is no evidence of differential attrition in treated villages by baseline characteristics of women or their households (Column 3): the p-value on the joint significance of these interactions is .221. Columns 4 to 6 show similar levels and correlates of attrition for husbands, the older sibling of the target child (tracked from baseline to midline), and the target child (tracked from midline to endline).[8]

Awareness of the Intervention Table 1 documents knowledge wives and their husbands have about the intervention. As our sample covers households with a not pregnant woman in them at baseline, these households are not immediately eligible for the cash transfers. Despite this, we see that two years after the program starts, spouses in treated villages are nearly all aware of the CDGP (Panel A), and the vast majority are aware of the eligibility criteria (Panel B). Those in controls are also aware of the intervention, but to a lesser extent.

Panel C shows actual enrolment rates for the cash transfer component of the CDGP using administrative records. In treated villages, 74% of households with women that were not pregnant at baseline, had a verified pregnancy and had started receiving payments by midline; 88% had enrolled by endline. We also note a small degree of enrolment in control villages by endline (12%): this is likely due to cross-village registrations and implementation errors. On the timing and

[8]At midline, enumerators were unable to visit 18 villages due to security risks, and this rose to 28 villages at endline. Village insecurity is uncorrelated to treatment but relates to man made shocks to villages such as curfews, violence, or migration into the village.

intensity of payments we see that women start receiving cash transfers in their sixth or seventh month of pregnancy, 59% receive their first transfer sometime during pregnancy. By endline, women have received on average 23 payments, of cumulative value $430.

Balance Table 2 shows that the samples are balanced between treatment and controls on characteristics of households, women, husbands, and the older sibling of the target child. Panel A highlights the economic vulnerability of our sample: 72% of controls reside live in extreme poverty, below the $1.90/day global threshold. They also suffer food insecurity, with 17% reporting not having enough food at some point during the year. Monthly food expenditure is $83, representing 41% of all household expenditure. The majority of women are engaged in some labor market activity. Their modal activity is to rear/tend household livestock (33%). Women's monthly earnings are around $21, the same value as the monthly cash transfer available from the program. Among men, 81% have farming as their main labor activity, with their earnings corresponding to 88% of total household earnings.

To establish potential gains from the intervention in improving human capital accumulation in early life, Panel B shows characteristics of the target child's older sibling, who (on average) is aged 28 months at baseline. Only half were appropriately breast-fed (this is a dummy indicating age-appropriate breast-feeding according to WHO guidelines [WHO 2008]), reflecting the low levels of parental knowledge on child nutrition practices pre-intervention. Mothers report 29% of children having diarrhoea in the two weeks before baseline. Finally, the low level of resources and knowledge translate into staggering levels of stunting: 59% of old children are stunted (so with a height-for-age Z-score (HAZ) below -2 standard deviations of the WHO defined guidelines [WHO 2009]), and at risk of not reaching their developmental potential. All these pre-intervention outcomes highlight the vulnerability of mothers and newborns to accelerating birth timing further.

2.3 Incentives to Accelerate Birth Timing

Beyond the level of need for resources given their economic destitution, households also have high demand for short term liquidity arising from sources of economic volatility and uncertainty. In combination, these provide households a strong incentive to accelerate birth timing in order to start receipt of the high-valued cash transfers on offer.

To begin detailing these sources of volatility, we first emphasize that the main source of household income is from husband's work in agriculture. This naturally exposes households to seasonal earnings variation. This is further exacerbated by a lean season: in rural North West Nigeria this starts in May and is most severe from June to October. One third of husbands and wives each report there are months of the year where earnings are lower. August is the modal month where earnings from both spouses are reported to be lowest, right in the middle of the lean season.

Table 3 details the seasonal volatility that households are subject to, as measured in controls at midline.[9] Panel A shows the incidence of food shortages by season: while only 4% report not having enough food to east during the first two seasons (from October through to February), this rises to 13% during Rani (March to May) and to 22% during Damuna (the peak of the lean season). The primary reason wives give for this shortage is a lack of money: over 60% report this to be main reason in Rani and the lean season. The availability of food is not a reason for food shortages – rather food prices rise in these months, which given the budget share of food is 40% (Table 2), reduces households' ability to purchase adequate quantities.

Panel B documents the coping strategies households use in response to these seasonal fluctuations in earnings and food purchases. While taking on more work is always an important response, 36% of wives report reducing the number of daily meals during the lean season, 23% reduce the use of sauces/condiments and some gather wild food, further worsening nutritional intake. Assistance from friends/family is an important coping mechanism. Credit market imperfections mean that few households report being able to borrow or rely on savings. Finally, we note that 7% of households report selling off productive assets (livestock) during Rani and the lean season (more do so in anticipation of the start of the lean season), that likely worsens their long run welfare.

The combination of anticipated resource shortages during these months and imperfect credit markets, provides households with a strong demand for short term liquidity – an economic incentive to bring forward the timing of birth to receive a stable flow of cash transfers. Given the substantial value of the monthly transfer – equivalent to 100% of wives earning or more than one quarter of food expenditures – the marginal benefits of so doing are likely immediate and noticeable given the high marginal utility of consumption in these periods.

Finally, our study context is one which state/NGO capacity to deliver programs is limited, and households face frequent aggregate shocks, both natural and man made. We described earlier how village insecurity led to our enumerators being unable to reach some villages. Figure A3 shows that even in the secure villages that we sample, nearly all have been hit by a natural shock in the year prior to midline or endline (crop damage caused by weather or pests, floods and droughts), and the majority have been hit by man made shocks (curfews, violence, or widespread migration into the village). With such background uncertainty, households might perceive a relatively narrow prospect of cash transfers being delivered for the full four year intention originally announced, again providing incentives to bring forward birth timing.

2.4 Feasibility of Accelerating Birth Timing

Given households' incentives to accelerate birth timing, Table 4 presents descriptives to establish the feasibility of doing so. We see that women are on average aged 24 while their husbands are on average aged 41 (where recall that 40% of households are polygamous). Despite their young age, women in our sample have on average 4 children aged below 18 and resident with them. However,

[9] We use the midline data because in that survey we elicited a more detailed set of coping mechanisms.

households still remain far from completing their fertility cycle: our study region is yet to undergo the demographic transition and has among the highest fertility rates on the planet: 2013 DHS data from a comparable sample suggests women in North West Nigeria have on average six surviving children at the end of their fertility cycle.

Average birth spacing between children is 33 months, and does not change much with birth order. Hence birth spacing remains well above a biological lower threshold, that is closer to 12 months: it is thus feasible for birth timing to be accelerated by up to 20 months in response to the offer of cash transfers. To see this further, Panel A of Figure 2 shows, for control women that were not pregnant at baseline, the CDF of birth spacing between their first child born in the study period and their immediately older sibling. There is a wide range of birth spacing: fewer than 10% of households have birth spacing of less than 24 months; at the same time almost 20% of households having a birth spacing of at least 48 months. Thus a notable proportion of households risk losing access to cash transfers forever because their pregnancy could only be verified outside the four-year period of open enrolment.

A second dimension on the feasibility of accelerating birth timing relates to the season in which households might be most in need of additional resources – the lean season. The ability to do so however might be limited by amenorrhea, or a reduced likelihood to become pregnant during the stresses of the lean season. To check for this, Panel B of Figure 2 shows (using DHS data) months of birth for rural Northern states, as well as urban areas as a benchmark. There are seasonal fluctuations in months of birth, but these do not differ between rural Northern states subject to the lean season, and urban areas. Assuming full term births, the months in which the fewest children are conceived in are February/March (and so born in November/December), so outside of the lean season. We thus find little robust evidence on a relationship between the lean season and the feasibility of conception, reinforcing the idea that households could accelerate birth timing in response to the offer of cash transfers to gain resource flows in that part of the year.[10]

2.5 Empirical Method

A natural framework in which to estimate whether the timing of pregnancies is accelerated by the offer of cash transfers is survival analysis [Newman 1983]. Our sample comprises women not pregnant at baseline. Hence 'failure' corresponds to the month of pregnancy of their first child since baseline (an absorbing state), as inferred from the month of birth. This framework captures period-specific probabilities of pregnancy, and accommodates right-censoring of the data. We use a Cox proportional hazard model to estimate the probability that woman i in village v becomes pregnant in period t in which she remains at risk of pregnancy, so conditional on the event not occurring in a previous time period. This is a semi-parametric model, in which the baseline hazard

[10] The data also rules out that high rates of infertility limit responses in terms of birth timing: less than 2% of women have no children at baseline, and less than 1% of women have no children by endline.

is not parameterized, while the effects of other explanatory variables on the hazard of pregnancy are assumed to be proportionally constant in every time period. The model can be written as:

$$h_{iv}(t|\mathbf{x}_{ivt}) = h_0(t)\exp(\beta T_v + \gamma X_{iv,t=0} + \eta_d + \lambda_s), \tag{1}$$

where the baseline hazard $h_0(t)$ is left unspecified, and t corresponds to months since baseline. We scale the baseline hazard by a function of treatment assignment (T_v), baseline controls ($X_{iv,t=0}$), a district (LGA) fixed effect (η_d) and randomization strata (λ_s). We report the hazard corresponding to the coefficient estimate $\widehat{\beta}$, where the null is that this is equal to one and so households in treated and control villages are equally likely to become pregnant in any given time period conditional on not having done so since baseline.[11]

For outcomes of the target child, mothers and the household, we estimate the following ANCOVA specification for impacts by the four-year endline:

$$Y_{ivt} = \gamma_E T_v + \alpha Y_{iv,t=0} + \beta X_{iv,t=0} + \eta_d + \lambda_s + \varepsilon_{ivt}, \tag{2}$$

where Y_{ivt} is the outcome and $X_{iv,t=0}$ is as previously described. ε_{ivt} is clustered by village given this is the level of randomization. For outcomes related to the target child that are not born at baseline, we cannot control for $Y_{iv,t=0} = 0$. For those outcomes we extend the specification to show impacts at midline and endline, and thus test whether impacts accumulate over time.

3 Results

3.1 Pregnancy Timing

We first examine impacts on the timing of pregnancy. Figure 3 shows unconditional Kaplan-Meier (KM) estimates for the likelihood of not becoming pregnant at any given month since baseline. The Kaplan-Meier estimator provides a non-parametric statistic to estimate this survival function. For each month since baseline, the pregnancy probability is calculated as the number of women who have become pregnant divided by the total number of women. This is calculated among women not pregnant at baseline, separately for treatment and control.

In control villages, the KM curve declines smoothly: around 30% of women do not become pregnant before midline, and 20% do not so by endline. This confirms there is scope for birth timing to be accelerated forward by counterfactual households in treated villages. We see that women in treated villages are marginally more likely to become pregnant *earlier* in time, but this effect is small (a Chi-squared test of equality of the curves marginally rejects the null, $p = .086$).

[11] The baseline characteristics of the household and mother in $X_{iv,t=0}$ are the number of children aged 0-2, 3-5, 6-12 and 13-17, the number of adults, the number of adults aged over 60, mother's age, whether she ever attended school, total monthly expenditure, and a dummy for polygamous relationships.

We also see no evidence of any discernible widening between the KM curves towards the end of the period of open enrolment (48 months after baseline) suggesting weak responses among those that increasingly risk losing access to the cumulative value of cash transfers altogether.

Panel A of Table 5 reports the corresponding estimate of (1). We see a positive but statistically insignificant effect of the offer of cash transfers in households bringing forward birth timing: the magnitude implies an 8% increase in the likelihood per period of pregnancy, but this hazard is not different from one between treated and control households.

3.2 Birth Spacing

Panel B in Table 5 examines if these small nudges in pregnancy timing still have measurable impacts on birth spacing. In line with the KM and survival estimates, there is no significant change in birth spacing between the target child and their immediately older sibling. Given the precision of the treatment effect estimate on birth spacing, a reduction by more than 1.2 months (or 4% of the control mean) could have been detected. Birth intervals of less than 24 months are considered to place the child at the highest risk of mortality and undernutrition, and mothers with those intervals are at a higher risk of birth complications [Pimentel *et al.* 2020, Damtie *et al.* 2021]. We see there is an increase of 2.5pp in such short birth spacing. This magnitude is of economic significance given that in controls, 8.8% of target child births are within 24 months of their older sibling. However, again this impact is not statistically different from zero.

Panel C tests whether the season of birth of the target child is impacted: we find no evidence that the target child is significantly more likely to be born during the severest part of the lean season (Damuna), when households might be most constrained because of a fall in agricultural incomes, a reduction in food availability (and hence the consequent rise in food prices), and hence additional resources highly valued on the margin.

3.3 Total Fertility

In response to the offer of cash transfers to pregnant women, households have incentives to accelerate birth timing (a *tempo* effect) rather than increase fertility overall (a *quantum* effect). For completeness, in Panel D we follow the earlier literature studying the impact of non-ECD cash transfers on total fertility and examine the extensive margin of whether any child is born, or the number of children born by endline [Stecklov *et al.* 2007, Palermo *et al.* 2016, Handa *et al.* 2017]. We find no significant impact on the likelihood any child is born, or the number of children born between baseline and endline. On the likelihood that any child is born, we note that in controls 78% of households had an additional child by endline. Of the 22% of treated households that could have responded on this margin, the estimate suggests a treatment effect of 1.8pp.

3.4 Mother's Health

The marginal cost of bringing forward birth timing includes worsening maternal health, especially in a population where birth spacing is already low and households reside in extreme poverty. We examine this in Table 6, where Column 1 shows midline outcomes for women not pregnant in controls. Column 2 shows that for maternal weight, height, BMI, and measures of malnourishment, there is no evidence of worsening health outcomes at the four-year endline. This underpins the finding of muted endogenous responses in birth timing to the intervention.

3.5 Robustness

Our study context is one in which households face strong economic incentives to accelerate birth timing, such behavioral responses are feasible, yet the results show statistically insignificant impacts of the intervention on the timing of pregnancy, birth spacing and total fertility. This is so even among those households that risk giving birth outside the period of open enrolment and losing access to the high cumulative value of cash transfers altogether. As detailed in the Appendix, we probe various explanations for these muted distortionary effects: (i) treatment effects might be attenuated by including women that are very unlikely to give birth in our study period; (ii) the impacts on accelerated birth timing might be concentrated among households closer to the start of their fertility cycle, or among non-polygamous households (because in polygamous households at least one wife would become eligible relatively quickly); (iii) time lags between when pregnancies are verified and cash transfers begin lead to the resource injections to be discounted; (iv) the intervention might increase contraceptive use or other methods to delay pregnancy; (v) households expect the offer of cash transfers to be available for the foreseeable future and do not understand there is a limited period of open enrolment.

To summarize, for the various subsamples of households considered, we continue to find muted responses in terms of pregnancy timing, season of birth or total fertility. However, we find evidence that households closer to the end of their fertility cycle in treated villages are significantly more likely to shorten birth spacing to below the 24 month threshold relative to controls. So while our data and sample size do allow us to find evidence of accelerated birth timing among specific subsets of households, the broad pattern of evidence suggests most households do not accelerate birth timing in order to start receiving the cash transfers.

3.6 Role of Information

We have focused on economic incentives to accelerate birth timing in order to gain access to the high-valued unconditional cash transfers from the intervention. However, the information component of the intervention might impact decisions over birth timing. As Table A1 shows, the information provided does not relate to birth spacing (or family planning), but the messages might still alter the perceived marginal costs and benefits of having an additional child. This could cause births to be brought forward or pushed back. If the latter, this offsets incentives to accelerate birth timing in order to gain receipt of the cash transfers, and could provide an alternative explanation for muted distortionary impacts on birth timing overall.

In the Appendix, we examine the possibility by exploiting the fact that treated villages were randomly assigned to two treatment arms varying the intensity of information delivered, as described in Table A1. The cash transfer component of the intervention is identical in both treatment arms. In line with our main results, we find similar null impacts on each margin irrespective of the intensity of information provided by the intervention.

4 Explaining Muted Responses

We unpack household decision making to understand what leads to muted responses in terms of accelerated birth timing to the offer of high-valued and long-lasting unconditional cash transfers.

4.1 Decision Making

Fertility and Contraceptive Use Nigeria is a patriarchal society where bride prices are common, with wives often perceived as being purchased by their husbands. This leads to decisions about reproduction residing primarily in the hands of the husband and his family, a fact well documented by work in demography and gender studies [Caldwell and Caldwell 1987, Odimegwu and Adenini 2014]. This matters because husband and wife's fertility preferences differ – both in terms of the total number of children desired, and when they want them. In a comparable sample of households from the 2013 Nigeria DHS data: (i) 67% of husbands desire a greater total number of children than their wives; (ii) 50% of husbands want additional children within the next two years, while this falls to 33% for wives.

Women's ability to influence the timing of pregnancies is further limited by the fact that in our study area, households lack access to contraceptives. 96% of women report never using any method to delay pregnancy, or to avoid getting pregnant. This mostly reflects a lack of supply rather than a lack of demand, as knowledge of contraceptives is more widespread. For example, in our sample, 80% of women reported knowing that injective contraceptives could be used to delay pregnancy, 76% reported oral contraceptives could be used.[12,13]

[12] The remoteness of villages further restricts access to health facilities. In our sample: (i) the majority of households report walking travel times over an hour to the nearest health facility; (ii) 58% of women report never having visited health facilities in the six months prior to baseline for reasons other than antenatal care.

[13] A small set of experimental studies, discussed in Ashraf *et al.* [2014], provide mixed results on on the impact of greater contraceptive availability on fertility: increasing access is found to decrease fertility in Ghana, Tanzania, Bangladesh and Colombia. No impact is evident in Ethiopia, Indonesia, Uganda and Zambia. Miller *et al.* [2021] use a structural model to estimate impacts on contraceptive use of eliminating supply constraints using data from Mozambique. They conclude that eliminating supply constraints would have limited impacts on contraceptive use,

This all implies imply husbands play an important role in determining the timing of fertility, and so can influence responses in birth timing to the offer of cash transfers from the intervention. Moreover, if husbands are less informed of risks of maternal mortality and morbidity compared to their wives [Ashraf *et al.* 2022], this further tilts the balance towards endogenously accelerating birth timing in order for their wives to begin receipt of cash transfers. However, to understand their incentives to do so, we now document decision making rights over a wider range of dimensions including control over resources.

Control Over Resources Figure 4 shows data on decision making in our sample at baseline. On each dimension, the top bar shows wives report over who makes related decisions, and the bottom bar shows the corresponding report of husbands (where spouses are interviewed separately). This reveals that, like decisions over birth timing, women also have weak decision making rights over major household purchases, which food to grow, and what food to buy.

However, we additionally asked a series of vignette questions on who would have decision making rights over any new flow of resources that the wife generated. As shown in Table 1, women have high labor force participation, so the majority bring earnings into the household. Our vignettes varied: (i) the source of women's earnings, contrasting between labor market earnings obtained through selling snacks – a common form of female self-employment – versus if money were received as a gift; (ii) the amount of monthly earnings gained, contrasting NGN3500 (to match the value of monthly cash transfers from the intervention), to the receipt of NGN1000.

The lower part of Figure 4 shows that in each scenario: (i) almost a majority of women reported they would decide *alone* how to spend the additional resources; (ii) this is irrespective of how the additional resources were generated or the amount of additional earnings; (iii) husband reports were near identical to their wives.[14]

Who Decides How Unconditional Cash Transfers Are Spent? Building on this insight that wives control additional resources they bring to the household, we next zoom in to consider who in the household decides how to spend the actual cash transfers from the intervention. The question reported in the bottom set of bars in Figure 4 was asked at midline in treated villages: we

while policies targeting women's beliefs about the risk of pregnancy absent contraception, and husbands' fertility preferences and their approval of contraceptive methods would be more effective.

[14]We designed our baseline survey module on household decision making to follow the structure of the DHS questions. The DHS data corroborates our finding of a sharp contrast in agency women have over earnings versus other dimensions of household decision making. Specifically, the DHS data also suggests husbands have decisive decision making rights over many outcomes affecting their wives beyond fertility – including health care, the purchase of large household items, and being able to visit family members. For example, when asked how decisions are made on these dimensions, the share of women that report their husband decides *alone* is, 91% for the health care of the woman, 93% for the purchase of large household items, and 75% for visits to family members. However, the DHS data also show there is one key dimension on which wives retain agency: how to spend their earnings. In sharp contrast to other dimensions of decision making, over 90% report of women report they *alone* choose how to spend money they bring into the household.

asked each spouse about who actually got to decide how the unconditional cash transfer was spent. In over 75% of households, women report being able to decide alone over how the unconditional cash transfer is spent. This is entirely corroborated with what husbands themselves report.

4.2 How Are Cash Transfers Spent?

How wives then choose to spend the cash transfer *ex post* determines *ex ante* incentives for husbands to accelerate birth timing in treated villages in order for their wife to be eligible for receipt of the cash transfers. We map out in granular detail how the marginal benefits at four-years post-intervention are distributed across spouses. We do so by presenting ITT estimates using (2) over three types of outcome: (i) private benefits to the husband; (ii) private benefits to the wife; (iii) common benefits across spouses.

4.2.1 Private Benefits to Spouses

Labor Activities The unconditional cash transfer can also be used to advance the labor activities of wives and husbands. Panel A of Table 7 focuses on the extensive margin of labor activities each spouse is engaged in. Women's labor force participation rates are high to begin with (69% at baseline in controls). For treated women this rises by a further 9pp by endline. On the types of labor activity engaged in, at baseline the most common activities for women are self-employment geared towards rearing livestock or petty trading. By endline we see significant increases in women's self-employment, and petty trading activities. These are economically large magnitudes of impact, corresponding to 26% and 37% increases respectively in these forms of self-employment.

The right hand side of Panel A shows no corresponding impact on the labor activities of husbands. There are no impacts on the extensive margin of husband's labor supply – their modal activity is farming their land and the incidence does not change post-intervention. Nor does the incidence of self-employment among husbands.

Business Investment and Earnings In Panel B we examine how cash transfers are used by wives to invest in their business and that of their husband. Given women's main form of self-employment, we consider business inputs and livestock ownership. Both types of productive investment significantly increase by endline. On inputs into women's businesses, these increase by $21/month at endline. Women's ownership of any animal increases by 9pp (16%) at endline. Livestock ownership is critical in this economic environment because it raises mean earnings for women from the sale of animal produce such as milk and eggs, and it produces an earnings stream all year round thus reducing women's volatility of earnings.[15]

[15] As in Carneiro *et al.* [2021] we note livestock are owned by individuals (not a household). Examining livestock ownership of women that were not-pregnant at baseline we find significant increases in women's ownership of

We see no corresponding increase in expenditures on inputs for husband's businesses.

Panel C combines the information on changes in labor activity to construct a (noisy) measure of total monthly earnings from all forms of employment, for each spouse. We see that by endline, women's earnings increase by $17 (corresponding to a 78% rise over the baseline level in control villages). In line with all the earlier results, we see no statistically significant impacts on earnings of husbands by endline.

This pattern of results on the use of cash transfers is also borne out in a parallel qualitative analysis of the intervention conducted by Sharp *et al.* [2018]. This shows women invested into small-scale home-based activities such as petty trade, food processing and sale, small livestock rearing, and services to other women (such as hairdressing or pounding grain). The qualitative analysis also finds that while beneficiaries do give cash gifts out of their transfer to their husbands, these are small and one-off. The largest recorded in the qualitative sample was NGN1100, so less than one third of one month's transfer.

It is not obvious the marginal return to investing in a wife's business is always higher than investing in the husband's labor activities. If not, household surplus would be maximized if the wife transferred some of the cash transfer to him. However, this calculation implicitly holds constant birth timing. If husbands anticipate such *ex post* intrahousehold transfers, then in treated villages they have greater *ex ante* incentives to accelerate birth timing. Given the potential costs from shortening birth spacing that are solely borne by wives and newborns, it is unclear whether this leaves wives better off overall. Our pattern of results is not in line with a unitary model of the household where spouses can compensate each other and seek to maximize their joint surplus.[16]

4.2.2 Common Benefits

There are of course also gains from cash transfers that are shared and valued across household members. We document these common gains in terms of: (i) food expenditure; (ii) food security; (iii) savings; (iv) early child outcomes of the target child. These results are in Table 8 and described further in the Appendix. To summarize: (i) monthly food expenditures rise by $20 (24%) over controls by endline; (ii) food security improves dramatically by endline, with a 12pp fall in households not reporting having enough food to eat (relative to 17% in controls at baseline); (iii) there is a significant rise in the stock of household savings of $47 (22%), equivalent to 2.5 months of cash transfers; (iv) early life outcomes for the target child such as the incidence of

livestock (and not another household member).

[16]A growing literature on micro-entrepreneurship in developing countries has shown that male but not female-operated enterprises benefit from unconditional cash transfers. Explanations put forward for this include: (i) women are subject to expropriation by husbands [de Mel *et al.* 2009, Jakiela and Ozier 2016]; (ii) women are less committed to grow their enterprises or are more impatient [Fafchamps *et al.* 2014]; (iii) women sort into less profitable sectors because of unequal labor market access/preference for flexibility [Bernhardt *et al.* 2019]. Our evidence instead suggests that in our context, women retain control of resources, have profitable business investments to undertake, and opt not to transfer resources to their husbands for their consumption or business investments.

stunting, improve, and child health significantly improves by endline.

Spouses value these gains, especially as some of these ease impacts of the economic volatility households are exposed to. How these common gains accrue is determined through the choices of how women spend cash transfers, and so factor in any costs borne by them if birth timing is accelerated. Hence even if women lack access to contraception and have less say in the timing of births, how they *ex post* allocate resources from the intervention provides men with weak *ex ante* incentives to try and accelerate birth timing. The fact that birth timing is not shifted forward to receive these gains earlier suggests the marginal costs of bringing forward birth timing outweigh the marginal benefits from doing so – despite households residing in extreme poverty, and having short term demand for liquidity in many months of the year. The fact that we document muted responses in birth timing even among households that give birth outside the window of open enrolment suggests these marginal costs that are borne by women and children, are valued at least as high as the present value of the cumulative lost cash transfers from the intervention, that is upwards of $400.

5 Discussion

5.1 Revisiting Carneiro et al. [2021]

It is useful to relate our findings to our earlier work using data from the same intervention. In Carneiro *et al.* [2021] we evaluated the CDGP intervention tracking 3700 women already verified to be pregnant at baseline. Among that sample, by construction, there are no endogenous responses in birth timing to the offer of cash transfers. The focus of that work was to document impacts on children *in utero* at baseline. The impacts documented in the current analysis related to child outcomes, private and common benefits re-affirm our earlier findings. However, among households not pregnant at baseline, these results can be interpreted in a new light, as aiding an understanding for the lack of distortionary responses to the offer of cash transfers among those not initially eligible.

This is critical for understanding the population-wide cost effectiveness of the intervention. In Carneiro *et al.* [2021] we estimated an IRR to the program of at least 10% under plausible assumptions, among the cross section of women that happened to be pregnant at baseline. This established the cost effectiveness of the program had it been a closed enrolment design – where eligibility is fixed at baseline by pregnancy status. Given the program is actually designed with a four-year window of open enrolment (in common with many cash transfer programs), the current results help establish whether the program remains cost effective when factoring in potentially distortionary responses among the far larger cohort of households without a pregnant women not pregnant at baseline. The lack of distortionary impacts on fertility dynamics, the similar impacts found for economic outcomes, and the fact that gains on child outcomes follow similar trajectories across the samples of households that are and are not pregnant at baseline, all point to the previously documented IRR gains to be population-wide, and not only applicable to the smaller cohort households that happen to be pregnant when the program is launched.[17]

5.2 External Validity

In our study context, the muted effect on accelerated birth timing can be explained by a constellation of three factors: (i) women retain control of resources they bring into the household, including how cash transfers from the intervention are spent; (ii) women have high labor force participation rates and have productive investment opportunities available to them through their own businesses; (iii) women prefer to invest in these opportunities rather than transfer part of the cash to husbands. These factors combine to imply husbands have weak *ex ante* incentives to accelerate birth timing in response to the offer of cash transfers to pregnant women, while women can better internalize the marginal benefits and costs from doing so.

The external validity of our findings hinge on how peculiar this constellation of factors is. We investigate this by pooling DHS surveys from 45 middle and low-income settings to document the prevalence of these factors across societies, as closely as can be measured in a cross-country sample (and combining rural and urban households). We thus shed light on which developing country contexts are more or less likely to see unintended fertility consequences when substantial cash transfers for early childhood development are targeted to pregnant mothers.

The results are in Figure 5. Panel A examines the first two relevant dimensions of household decision making: it shows a scatter plot of the share of married women that report using any form of contraception, against the share of women that report being able to decide how to spend their own earnings. The vertical and horizontal lines represent the means of the variable stated on each axis, splitting the figure into quadrants based on countries being above/below each mean. Countries with separate spheres decision making as in our study context lie in the South East quadrant (orange dots). This is not uncommon: Nigeria lies within a cluster of such countries, that also includes Ghana, Niger, Uganda and the Gambia for example. This helps explain the findings of Palermo *et al.* [2016], who study a child grant program in Zambia and also find null impacts on fertility. As Panel A shows, Zambia is a context where a high share of women report using contraceptives and so can exert more control over the timing of births. As such they are better able to internalize any marginal cost of accelerating birth timing.

Panel B shows a scatter plot of employment rates for married women (that includes self-employment) against the share of women that report being able to decide how to spend their earnings. To link to the earlier scatter plot, we use the same color coding as in Panel A. Those

[17] A third case is where enrolment is permanently open. In such cases in the new steady state, it would be important for future work to explore additional dimensions of household response. These can relate to endogenous changes in household structure arising from husbands taking on additional wives, or households marrying off daughters early.

countries in the North East quadrant have higher than average female employment rates and women control how to spend their earnings. Somewhat surprisingly, most countries in this quadrant (8 out of 14) have separate spheres decision making (orange dots). These include Nigeria, but also Ghana, Uganda and Burkina Faso. It is in such countries where the same constellation of factors come together as in our sample, and, all else equal, we might then expect relatively muted (unintended) consequences on fertility dynamics from the offer of high-valued and long-lasting cash transfers to pregnant women.

Concerns over unintended distortionary fertility responses will be stronger in countries where fertility and resource allocation decisions are in the hands of the same spouse: in Panel A these are in the North East quadrant (women control both, brown dots) and South West quadrant (husbands control both, green dots). Panel B shows that among countries where women control both decisions, nearly all have higher than average female employment rates too. In these contexts, if women internalize the cost of bringing forward birth timing, they retain control of cash transfers and have productive investments they can make related to employment opportunities. A testable implication in these countries for future work is that impacts on birth timing from the offer of cash transfers will depend on the extent to which women understand and internalize the marginal costs of bringing forward birth timing.

Among countries where husbands control both decisions (green dots), there is far more variation in employment rates for women. Among those in the North West quadrant of Panel B, there remains the possibility that husbands invest cash transfers in the employment prospects of their wives. These countries include Burundi, Liberia and Tanzania. For those in the South West quadrant of Panel B, women have fewer labor market opportunities and so might well lack even the possibility of using cash transfers to make productive investments. These countries include Ethiopia and Mozambique. Outside of Sub-Saharan Africa, these countries include Afghanistan and Tajikistan. These are contexts where our results suggest the most caution for ECD-related interventions targeting high valued cash transfers to pregnant mothers.

5.3 Policy Design

Our findings have implications for the design of the next generation of interventions using cash transfers to promote early childhood development. We consider three margins of design: targeting, eligibility, and component bundling.

Targeting It can matter *who* in the household cash transfers are provided to. Most programs in developing countries target payments to women [Fiszbein and Schady 2009]. A few studies have experimentally varied the gender of recipients. While some suggest targeting women can change consumption levels and bundles [Armand *et al.* 2020], others find less impact [Benhassine *et al.* 2015, Haushofer and Shapiro 2016, Almas *et al.* 2021]. The lack of gendered impacts could be due to spouses having similar preferences or the fact that one spouse always takes control of the resources. Our results suggest targeting resources to women rather than husbands will be especially important for avoiding unintended distortionary effects on fertility in contexts where women retain control the use of cash transfers and have available productive investment opportunities.

Eligibility The purpose of linking eligibility to verified pregnancy is so that benefits from cash transfers start impacting the child as early as possible – even while they are *in utero*. However, in contexts where unintended consequences on birth timing are more likely to occur, the concern can be eased by separating further the time between when fertility decisions are made and when payments start to be received. This leads to a trade-off for open enrolment programs: (i) having payments start later (perhaps when the child is born or sometime later within the first 1000 days of life) will lower potential impacts on child outcomes for those that are eligible; (ii) yet this separation in timing would weaken distortionary impacts on birth timing (and hence mitigate adverse consequences for maternal and child health) among those initially not eligible.

Component Bundling The CDGP follows a standard design in bundling information and cash components to foster human capital accumulation in early life. For this program information was provided as a public good and related to basic practices, so there is little incentive to change birth timing to access information. In alternative designs where information is far more tailored to the specific needs of individual households, a second incentive to alter birth timing might be created. Again, policy makers face a trade-off between improving child outcomes through more household-specific information delivery, versus worsening child outcomes through accelerated birth timing and shorter birth intervals.

It might be natural to think of bundling such interventions with family planning, or empowering women through the provision of bargaining and negotiation skills [Ashraf 2009, Ashraf *et al.* 2014, 2020]. However, it is unclear what impacts this would have on the timing of births. Our results provide a word of caution to such multi-faceted approaches because they might break the delicate balance of household decision making in some contexts. However in other contexts such bundling would allow women to internalize the costs and benefits of bringing forward births, while retaining control over earnings streams.

6 Conclusion

Huge reductions in global poverty have been achieved over the last three decades. Some part is due to the increased use of direct cash transfers to the poor. Such cash transfers have been a key policy response to the pandemic in low-income countries, and as such seem destined to become entrenched as a policy instrument in the developing world. As the spread of these programs coincides with renewed policy interest in fostering human capital accumulation in early life, policy makers are increasingly targeting cash transfers to pregnant mothers or those with young children, with this objective.

An inherent tension for such policies is that exactly those households that are most in need of assistance to help newborns reach their developmental potential in early life are also with the greatest demand for short term liquidity given their economic circumstances. Hence the general concern arises that using cash transfers to promote early childhood development by essentially conditioning receipt on pregnancy can induce distortionary responses in birth timing in open enrolment programs. While unintended consequences of cash transfers have previously focused on disincentive effects on labor supply or price distortions, we consider a different, important and irreversible margin for ECD-related programs involving cash transfers in which eligibility is conditioned on pregnancy: birth timing and birth spacing.

In a context where households face extreme economic volatility and women lack access to contraceptives, we show how women are still largely able to put a brake on economic incentives to bring forward birth timing and thus avoid such unintended consequences are – even with the offer of high-valued and long-lasting cash transfers. This occurs because of a constellation of factors: (i) women retain control of the cash transfers, even though men have considerable say over birth timing; (ii) women have available productive investment opportunities in their own businesses; (iii) women choose to transfer few resources to husbands. By setting out the reasons behind the lack of distortionary effects in our study context, we are able to speculate on parts of the developing world where similar programs can be expected to generate high population-wide rates of return, and also settings where more caution needs to be taken when targeting cash transfers to households with verified pregnancies to promote early childhood development.

A Appendix

A.1 Robustness Checks

Sample Composition Treatment effects might be attenuated by including those very unlikely to give birth in our study period. We address this in Table A3 by showing the baseline results are robust to: (i) weighting the sample based on the likelihood the women was predicted to become pregnant by midline using a probit model; (ii) weighting using OLS weights. In both cases, we continue to find largely null impacts on pregnancy timing, birth spacing, season of birth and total fertility from the offer of cash transfers.

Households Already At End of Their Fertility Cycle We noted earlier that the average number of children in the household at baseline is four, while DHS data suggests total fertility rates are close to six in this region. To check whether accelerated birth timing is concentrated among households earlier in their fertility cycle, Columns 1 and 2 of Table A4 show treatment effect estimates on each margin of fertility response when we split the sample into women with above/below the median number of children at baseline. In both subsamples we largely find a similar pattern of muted impacts on the timing of pregnancies and total fertility. Among those households later in their fertility cycle we do see an increased likelihood of birth spacing being reduced so that the target child is born within 24 months of their older sibling.

Polygamy 40% of our sample are in polygamous marriages. In polygamous marriages, any wife that is pregnant is eligible for the cash transfer. Hence in polygamous households the desire to bring forward birth timing is muted because at least one wife might naturally become eligible for the cash transfers when having a pregnancy verified at the originally planned time. To check for this, Columns 3 and 4 of Table A4 show treatment effect estimates on each margin of fertility response when we split the sample by polygamous and non-polygamous households. We find similarly muted responses on all fertility outcomes, and on most margins we cannot rule out equal (and similarly muted) responses between polygamous and non-polygamous households.

Time Lags A fourth explanation for muted fertility impacts relates to the fact that women only become eligible to receive cash transfers once their pregnancy is verified via a urine test. This usually occurs in the second or third trimester of pregnancy. Hence there is obviously a lag between when a household might first decide to respond to the offer of cash transfers and their actual receipt. Muted fertility impacts could result because this time lag causes any marginal benefit to bring forward birth timing to be discounted. As a potential check on this, we consider responses among households with above/below median savings at baseline, as a proxy for those with lower/higher discount rates. The results in Columns 5 and 6 of Table A6 show both sets of households have largely null response in terms of pregnancy timing, birth spacing and fertility. However we again note a significant increase in shortened birth spacing to below 24 months among households with below median savings.

Contraceptive Use A further explanation is that the intervention causes households to start using contraception or other methods to delay fertility, offsetting any incentive to accelerate birth timing. Factors mitigating this concern are that the information messages provided do not relate to family planning (Table A1), and there is little availability of contraceptives in the study area. Taking a comparable sample of women in the same states in Nigeria from the 2013 DHS data, collected a year prior to our baseline, shows 98% of women reporting not using *any* contraceptive method, 96% report never using any method to delay pregnancy or to avoid getting pregnant. Moreover, in our sample, we find no evidence of contraceptive knowledge being impacted by the intervention.[18] Finally, we note the other main method by which to delay pregnancy is continued

[18] More precisely, we asked about knowledge of contraceptive methods (given usage was so low). At baseline 58% of women reported knowing some contraceptive method. Our two-year treatment effect on this is .000 (with

breast-feeding. At baseline, only 6% of non-pregnant women reported knowing that exclusive breast-feeding can be used as a form of contraception (thus increasing birth spacing). This falls to less than 1% by midline.

Policy Certainty A final explanation for muted responses is that households expect enrolment to be available for the foreseeable future (and not closing after four years). Given transfers are only available for one child, and fertility rates are high to begin with, households might be secure in the knowledge that they will eventually receive the transfers without having to adjust their planned fertility path. Three pieces of evidence mitigate against this interpretation.

First, this is an environment in which state or NGO capacity to deliver programs is limited, and households face frequent aggregate shocks, both natural and man made (Figure A3).

Second, it was well understood the intervention had open enrolment for four years. As Figure 1 shows, the program stopped enrolling pregnant mothers in April 2019, around six months after our endline survey. This announcement was made just before our endline survey, allowing us to measure whether households were aware of enrolment closing. We find that at endline, among women in treated villages 68% correctly knew how long they could expect to receive cash transfers for; (ii) 59% heard this information directly from community volunteers charged to disseminate the information component of the intervention.

Finally, the KM estimates in Figure 3 show no spike in births towards the endline survey being triggered by the announcement. Households that have not become pregnant at that stage risk losing cash transfers altogether. At the four-year endline, 13.3% of households in treated villages that had a non-pregnant woman in them at baseline, still had not had a child by endline (and this does not differ to controls where it is 15.4%).

A.2 Role of Information

The information component of the intervention might impact decisions over birth timing if it alters the perceived marginal costs and benefits of having an additional child for those not pregnant at baseline. We examine the possibility exploiting the fact that treated villages were randomly assigned to two treatment arms varying in the intensity of information delivered, as described in Table A1. The high intensity treatment arm includes smaller support groups and one-on-one counselling to mothers tailored to their needs. The cash transfer component of the intervention is identical in both treatment arms.

Figure A4 shows unconditional Kaplan-Meier (KM) estimates for the likelihood of not becoming pregnant at any given month since baseline, for households in each treatment arm and controls. We again see that women in both sets of treated villages are marginally more likely to give birth *earlier* in time, but this effect is small (we cannot reject equality of any pair of KM curves at

a standard error of .024).

conventional levels of significance). We also see no evidence of an uptick in pregnancies towards the end of the period of open enrolment in either treatment arm, again suggesting weak behavioral responses among those that risk losing access to cash transfers forever, irrespective of the intensity of information provided.

Table A5 presents treatment effects from both low- and high-intensity information arms at the four-year endline. To begin with, Panel A examines impacts in the knowledge of wives and husbands, and impacts on ante-natal practices and health behaviors of mothers towards newborns (that refers to the younger sibling of the target child).[19]

We find significant improvements in maternal and paternal knowledge, significant improvements in the index of maternal practices towards newborns, and the index of health related behaviors of mothers towards newborns (where Column 1 shows Control means at endline given some of the outcomes relate to practices towards newborns). Three of the four point estimates are larger for the high-intensity information treatment, with this difference being significant for maternal knowledge ($p = .025$).

Panels B to E show treatment effect estimates on pregnancy timing, birth spacing, season of birth and total fertility by low- and high-intensity treatment arms (where Column 1 shows Control means at baseline). In line with our main results, we find similar null impacts on each margin irrespective of the intensity of information provided by the intervention. For example, in Panel B the hazard of pregnancy is 12pp (5pp) higher in the low (high) intensity treatment arm than in controls, but neither hazard is significantly different from one, nor do they differ from each other ($p = .447$).

A.3 Common Benefits

We document common gains from the receipt of cash transfers in terms of: (i) food expenditure; (ii) food security; (iii) savings; (iv) early child outcomes of the target child.

Food Expenditure Panel A of Table 8 examines four-year impacts on monthly food expenditures (calculated based on a seven-day recall and aggregated over food groups). Food expenditures rise in treated households by $20 (24%) over controls by endline. Some of these increases will of course benefit husbands. However, given that household sizes are over seven at baseline (before

[19] All indices are constructed using the method described in Anderson [2008], so standardized to have mean zero and variance one in controls. The knowledge index (of wives and husbands) includes the following question components: Would you advise to seek a check-up even if the baby is healthy? Is colostrum good for the baby? Should you breastfeed immediately? Where is best place to give birth? Should a baby receive any other liquids on first day? Should you give water to a baby if it is hot out? How long should you exclusively breastfeed for? The practices index for mothers has the following question components: Did the child receive antenetal care? Was the child fed colostrum in the first hour? Was the child put to breast immediately? Was the child born at a health facility? And (if applicable) was the child exclusively breastfed for 6 months? The health behaviors index includes the following question components: Did the child received deworming in the last six months? Did the child receive basic vaccinations?

the birth of the target child), the equivalized share of food expenditure accruing to husbands will likely be a relatively small share of the original value of the cash transfer.

Food Security Panel B examines outcomes related to food security. This is important given the agricultural cycle includes a lean season from March to October when households face food shortages. At baseline in controls, 17% of households report not having enough food to eat in the past year. We see that in treated households food security improves dramatically by endline, with a 12pp fall in households not reporting having enough food to eat. The rows below show these longer run improvements in food security are concentrated in lean season months. These clearly are long run benefits that accrue to all household members from being able to smooth consumption as a result of gaining access to the flow of cash transfers from the intervention, and being less likely to resort to extreme coping strategies.

Saving Panel C presents ITT estimates on household savings. By endline there is a significant rise in the stock of household savings of $47 (22%), equivalent to 2.5 months of cash transfers. This benefits husbands in allowing their household to build resilience to idiosyncratic and aggregate shocks of the types shown in Figure A3.

Child Anthropometrics The final set of common gains from the receipt of cash transfers are improvements in early life outcomes of the target child. As is well documented in the earlier literature, these gains accrue over time and so we consider impacts at midline and endline. To begin with, we examine impacts on height-for-age Z-scores (HAZ) because this relates to stunting: stunting is the best measure of cumulative effects of chronic nutritional deprivation, and is recognized as a key indicator of long-term well-being. To minimize measurement error, data on child height was collected by a dedicated anthropometric enumerator in each survey wave.

Figure A5 shows the distribution of HAZ scores of target children at midline and endline: there is a rightward shift of the distribution between treated and controls in both periods, suggesting large improvements in height for these children born to treated mothers that were not pregnant at baseline. Panel A of Table A6 shows ITT impacts of the intervention on HAZ, stunting and extreme stunting outcomes for the target child. Columns 2 and 3 show that at midline: (i) treated children have a statistically significant increase in their HAZ score by $.26\sigma$; (ii) at the lower tail of the distribution, there is a reduced incidence of stunting of 11pp, corresponding to a 22% reduction. These impacts become less precise at endline, but we cannot reject equality of the two- and four-year impacts on each dimension.

Child Health Panel B of Table A6 shows treatment effect estimates on health-related outcomes for the target child. There is a reduction in illness/injury for new children of 6pp at midline, and this reduction improves slightly to 9.8pp (corresponding to a 16% fall relative to controls) by endline. The incidence of diarrhea among the target child also falls by 8.5pp at endline (corresponding to a 25% reduction), and health behaviors conditional on the child suffering diarrhea also improve by endline. Other outcomes related to child health include the number of vaccinations, whether the target child receives a complete sequence of vaccinations, and whether they are given deworming medicine. All significantly shift forward by endline.

If households were endogenously responding to the offer of cash transfers by bringing forward the timing of births, the most important marginal cost of doing so would be to worsen child outcomes – especially given birth spacing is below WHO recommendations to begin with. These results establish there is no such evidence of such detrimental impacts on children of mothers that were not pregnant at baseline.

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Table 1: Awareness of the Intervention and Enrolment

Sample: Households with non-pregnant women at baseline (N=1743)

	Woman		Husband	
	(1) Control	**(2) Treatment**	**(3) Control**	**(4) Treatment**
Panel A: Existence of the CDGP				
Yes, there is such a program in this village	.205	.947	.218	.956
No, there is not such a program in this village	.791	.048	.769	.044
Panel B: Eligibility Criteria				
Exact Answer	.214	.196	.242	.150
Generally appropriate answer	.622	.699	.591	.663
Inappropriate answer	.031	.037	.046	.038
Don't know	.133	.068	.121	.150
Panel C: Receipt of Any Cash Transfer				
Ever received transfer by midline	.075	.737		
Ever received transfer by endline	.124	.881		
Age of target child (*in utero*) at first payment (months)		-2.54		
		{9.21}		
Transfers received during pregnancy (%)		.591		
Number of cash transfer payments received by endline		23.1		
		{6.89}		
Total amount transferred (US$) by endline		430		
		{132}		

Notes: In Panels A and B, Columns 1 and 2 show the means of control and treatment women's knowledge of the program. Columns 3 and 4 show the corresponding means for husbands. Panel C uses data from the administrative records data on payments. The age of the new child at first payment is derived from the month of pregnancy as reported by mothers pregnant at Baseline. All monetary amounts are converted from Nigerian Naira to PPP US dollars at the 2014 rate.

Table 2: Baseline Balance

Sample: Households with non-pregnant women at baseline (N=1743)
Means, standard deviation in braces, p-values in brackets

	(1) Control	(2) Treatment	Control = Treatment [p-value]
Panel A: Household			
Observations	574	1169	
Living on less than $1.90/day (extreme poverty)	.721	.731	[.728]
Did not have enough food in past year	.167	.144	[.667]
Monthly food expenditure (in $USD)	83.0	78.3	[.889]
	{118}	{116}	
Share of monthly expenditures on food	.412	.391	[.206]
Wife: paid/unpaid work in past year	.686	.709	[.321]
Wife: rearing/tending livestock	.331	.328	[.876]
Wife: total monthly earnings (in $USD, PPP)	21.2	23.1	[.253]
	{43.2}	{48.4}	
Husband: farming household's land	.808	.801	[.732]
Husband: total monthly earnings (in $USD, PPP)	161	146	[.776]
	{327}	{359}	
Panel B: Older Sibling			
Age (months)	28.4	27.9	[.624]
	{15.4}	{16.2}	
Exclusively breastfed	.505	.497	[.559]
Had diarrhea in past 2 weeks	.291	.284	[.793]
Stunted (HAZ<-2)	.591	.606	[.959]

Notes: In Panel A, food expenditure is based on 7-day recall for food items. Living on less than $1.90 a day indicates if the household is spending less than $1.90 a day according to PPP USD in 2011 terms. This is the World Bank's international poverty line definition for households residing in extreme poverty. Total monthly earnings are the earnings for the husband and wife reported from the past year across all work activities that are carried out for pay. Values above the 99th percentile are set to missing. In Panel B, the 'child exclusively breastfed' variable is a dummy for the child having been exclusively breastfeed up to the age of 6 months. Stunted is a dummy indicating children with height-for-age-z-score (HAZ) under -2 standard deviations of the WHO defined guidelines [WHO 2009]. Column 1 reports the mean (and standard deviation for continuous variables) of the variable in the Control group, and Column 2 reports the same statistics for the low-intensity and high-intensity information treatment arms combined. The p-values on tests of equality across Columns are obtained from an OLS regression, controlling for randomization stratum, LGA, and clustering standard errors at the village level. All monetary amounts are converted from Nigerian Naira to PPP US dollars at the 2014 rate.

Table 3: Seasonality in Food Security and Coping Mechanisms

Sample: Control households with non-pregnant women at midline

	Kaka	**Sanyi**	**Rani**	**Damuna**
	Mid/late October to December	**December to February**	**March to May**	**June to Mid/late October**
Panel A: Food Security				
Proportion without enough food for the hous	.035	.038	.134	.223
Reasons for lack of food (proportion):				
not enough money	.294	.500	.656	.626
small land size	.412	.278	.297	.290
lack of farm inputs	.176	.167	.141	.224
no food in the market	.000	.000	.016	.009
Panel B: Coping Mechanism				
members of household took more work	.471	.333	.313	.364
reduced the number of daily meals	.353	.278	.406	.364
reduced sauce/condiments	.118	.222	.250	.206
gathered/consumed wild food	.000	.000	.063	.019
helped by relatives or friends	.529	.389	.281	.383
borrowed money	.000	.111	.141	.112
relied on savings	.000	.000	.063	.103
sold livestock	.000	.167	.078	.065

Notes: Mothers are asked whether there was always enough food for the household in each season. We report the proportion that says this was not the case at two-year midline. For those mothers who report there was not enough food available in a particular period, they are then asked (for each period) to list reasons for why there was not enough food, and what were the main coping mechanisms. Individuals are not provided with a set of reasons to choose from. Enumerators record the most commonly cited reasons from their conversation with mothers. In most cases they list multiple reasons for not enough food and mechanisms for coping for each mother. In this table we focus on a representative set of reasons/coping mechanisms frequently mentioned by mothers, and report the proportion of mothers enumerating each of them.

Table 4: Feasibility of Accelerating Birth Timing

Sample: Households with non-pregnant women at baseline (N=1743)

Means, standard deviation in braces, p-values in brackets

	(1) Control	(2) Treatment	Control = Treatment [p-value]
Wife: age (years)	24.4	23.8	[.104]
	{6.57}	{5.89}	
Husband: age (years)	41.0	40.4	[.251]
	{8.87}	{8.24}	
Number of children aged 0-18	4.39	4.32	[.823]
	{3.00}	{2.98}	
Birth spacing of second child from first child (months)	32.7	32.4	[.356]
	{10.7}	{10.6}	
Birth spacing of last child from previous child (months)	33.3	33.1	[.367]
	{10.4}	{10.5}	

Notes: We report data from the household surveys. Column 1 reports the mean (and standard deviation for continuous variables) of the variable in the Control group, and Column 2 reports the same statistics for the low-intensity and high-intensity information treatment arms combined. The p-values on tests of equality across Columns are obtained from an OLS regression, controlling for randomization stratum, LGA, and clustering standard errors at the village level.

Table 5: Pregnancy, Birth Spacing and Total Fertility

Sample: Non-pregnant women at baseline (N=1743)
Standard errors in parentheses clustered by village

	(1) Control Mean, Endline	(2) Four Year Impacts
Panel A: Pregnancy Timing		
Hazard Ratio		1.08
		(.090)
Panel B: Birth Spacing		
Birth spacing between target child and their older sibling (months)	35.0	-.002
	{10.8}	(.752)
Birth spacing between target child and their older sibling <= 24 months	.088	.025
		(.017)
Panel C: Season of Birth		
Born during Kaka (Mid Oct to Dec)	.218	-.042
		(.029)
Born during Sanyi (Dec to Feb)	.161	-.033
		(.025)
Born during Rani (Mar to May)	.258	.025
		(.032)
Born during Damuna (Jun to Mid Oct: lean season)	.362	.050
		(.033)
Panel D: Total Fertility		
Any child born between baseline and endline	.782	.018
		(.038)
Number of children born between baseline and endline	1.20	.043
	{.815}	(.065)

Notes: Significance levels: * (10%), ** (5%), ***(1%). In Panel A we report the impact of treatment assignment on the hazard of becoming pregnancy from a Cox Proportional Hazard model, controlling for LGA and randomization tranche fixed effects, baseline characteristics of the household and mother, and months between the baseline and the previous birth. For the remaining panels, Column 1 shows the mean (and standard deviation for continuous outcomes in braces) value in Control households at baseline. Column 2 reports ITT estimates at the four-year endline. These are estimated using OLS, controlling for LGA and randomization tranche fixed effects and baseline characteristics of the household and mother. In all specifications these characteristics are the number children aged 0-2, 3-5, 6-12 and 13-17, the number of adults, the number of adults aged over 60, mother's age, whether she ever attended school, total monthly expenditure and a dummy for polygamous relationships. Standard errors are clustered at the village level throughout.

Table 6: Mother's Health

Sample: Non-pregnant women at baseline (N= 1,743)
Standard deviation in braces
Standard errors in parentheses clustered by village

	(1) Control Mean, Midline	(2) Endline
Weight (kg)	49.6	.070
	{7.98}	(.466)
Height (cm)	156	-.181
	{5.63}	(.338)
BMI	20.3	.082
	{2.87}	(.173)
BMI: Thin	.286	-.015
		(.023)
BMI: Normal	.647	.003
		(.022)
BMI: Overweight	.067	.012
		(.015)
Middle upper arm circumference (MUAC, mm)	249	1.73
	{24.3}	(1.63)
Malnourished	.114	-.015
		(.017)

Notes: Significance levels: * (10%), ** (5%), ***(1%). Column 1 shows the mean (and standard deviation for continuous outcomes in braces) value in Control households. Column 2 reports ITT estimates at Endline. These are estimated using OLS, controlling for LGA and randomization tranche fixed effects and the following baseline characteristics of the household and mother: the number children aged 0-2, 3-5, 6-12 and 13-17, the number of adults, the number of adults aged over 60, mother's age, whether she ever attended school, total monthly expenditure and a dummy for polygamous relationships. Standard errors are clustered at the village level throughout. Body Mass Index (BMI) is calculated as weight divided by the square of body height. Malnourished is defined as a middle upper arm circumference (MUAC) below 220 mm.

Table 7: Private Spousal Benefits

Sample: Households with non-pregnant women at baseline (N= 1,743)

Columns 1 and 3: Standard deviation in braces

Columns 2 and 4: Standard errors in parentheses clustered by village

	Wife		Husband	
	(1) Control, Baseline	**(2) Four Year Impacts**	**(3) Control, Baseline**	**(4) Four Year Impacts**
Panel A: Labor Supply and Self-Employment				
Any work in past year	.686	.092***	.956	-.000
		(.025)		(.001)
Has business/self-employed	.509	.134***	.415	.008
		(.032)		(.037)
Petty trading	.357	.132***	.204	.015
		(.039)		(.032)
Farming own land			.808	-.001
				(.009)
Panel B: Business Investment				
Monthly expenditure on business inputs (USD, PPP)		21.1***		1.94
		(5.81)		(5.75)
Owning any livestock	.559	.090***		
		(.026)		
Panel C: Earnings				
Total monthly earnings + revenues from business (USD, PPP)	21.2	16.5***	161	10.5
	{43.2}	(6.13)	{327}	(15.6)

Notes: Significance levels: * (10%), ** (5%), ***(1%). Columns 1 and 3 report the mean (and standard deviation for continuous outcomes) for control households at Baseline, for wife and husband, respectively. Columns 2 and 4 report ITT estimates at Endline. Each ITT is estimated using OLS, controlling for LGA and randomization tranche fixed effects, and the following Baseline characteristics of the household and mother: the number children aged 0-2, 3-5, 6-12 and 13-17, the number of adults, the number of adults aged over 60, mother’s age, whether she ever attended school, total monthly expenditure, a dummy for polygamous relationships. Standard errors are clustered at the village level throughout. There are methodological differences in how earnings were measured at Baseline and Endline. At Endline, we slightly changed the questionnaire to capture subtler aspects of income generating activities. For activities such as petty trading and small self-operated artisanal activities, we elicited cost of inputs and sales revenue instead of a more generic “last payment received”. Total earnings at endline are then constructed by summing payments and revenues (for self-employed work), while at Baseline they only include payment for work. Values above the 99th percentile are set to missing. All monetary amounts are converted from Nigerian Naira to PPP US dollars at the 2014 rate.

Table 8: Common Benefits

Sample: Households with non-pregnant women at baseline (N= 1,743)

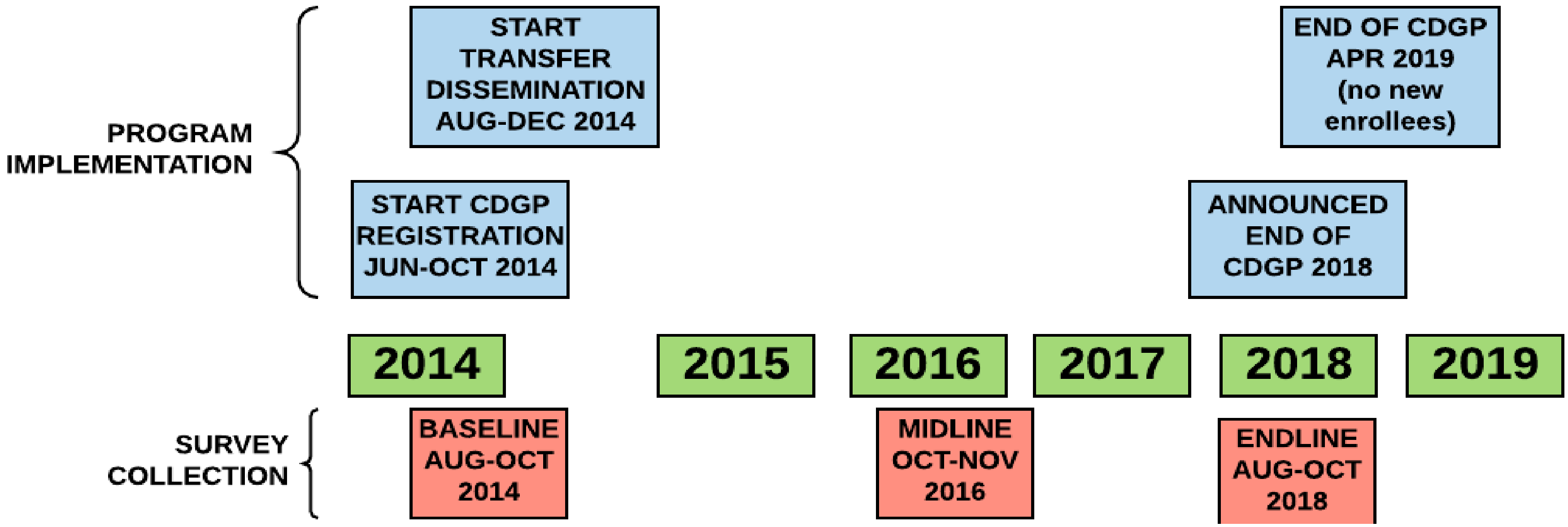
Standard deviation in braces

Standard errors in parentheses clustered by village

	(1) Control, Baseline	(2) Four Year Impacts
Panel A: Expenditure		
Monthly food expenditure	83.0	20.1**
	{118}	(10.2)
Panel B: Food Security		
Did not have enough food in past year (%)	.167	-.118***
		(.034)
during Kaka (Mid Oct to Dec)		-.011
		(.010)
during Sanyi (Dec to Feb)		-.023*
		(.013)
during Rani (Mar to May)		-.089***
		(.026)
during Damuna (Jun to Mid Oct: lean season)		-.109***
		(.030)
Panel C: Savings		
Total savings (including in kind)	218	47.3*
	{614}	(26)

Notes: Significance levels: * (10%), ** (5%), ***(1%). Column 1 shows the mean (and standard deviation for continuous outcomes) values in Control households at Baseline. Column 2 reports ITT estimates at Endline. These are estimated using OLS, controlling for LGA and randomization tranche fixed effects, and the following Baseline characteristics of the household and mother: the number children aged 0-2, 3-5, 6-12 and 13-17, the number of adults, the number of adults aged over 60, mother's age, whether she ever attended school, total monthly expenditure, a dummy for polygamous relationships. Standard errors are clustered at the village level throughout. Food expenditure is obtained using a 7-day expenditure recall of 13 food items. The top 1% of total expenditure amounts are trimmed. All monetary amounts are converted from Nigerian Naira to PPP US dollars at the 2014 rate.

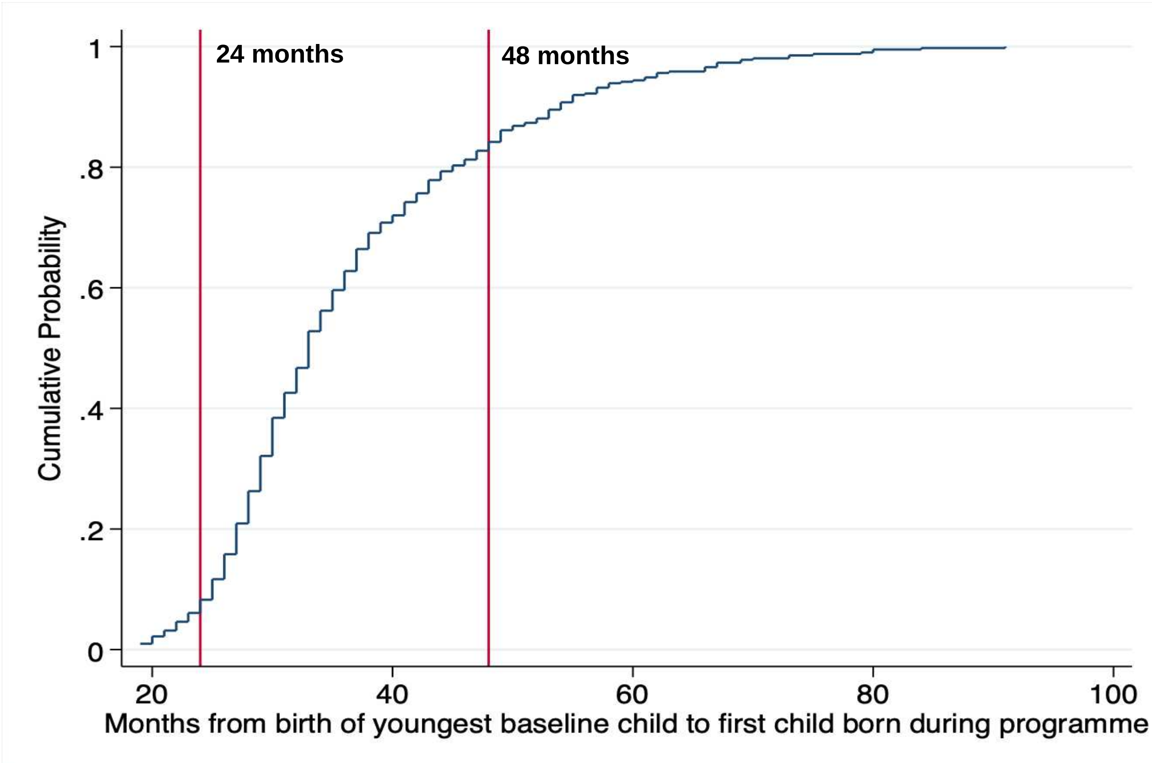
Figure 1: Timeline



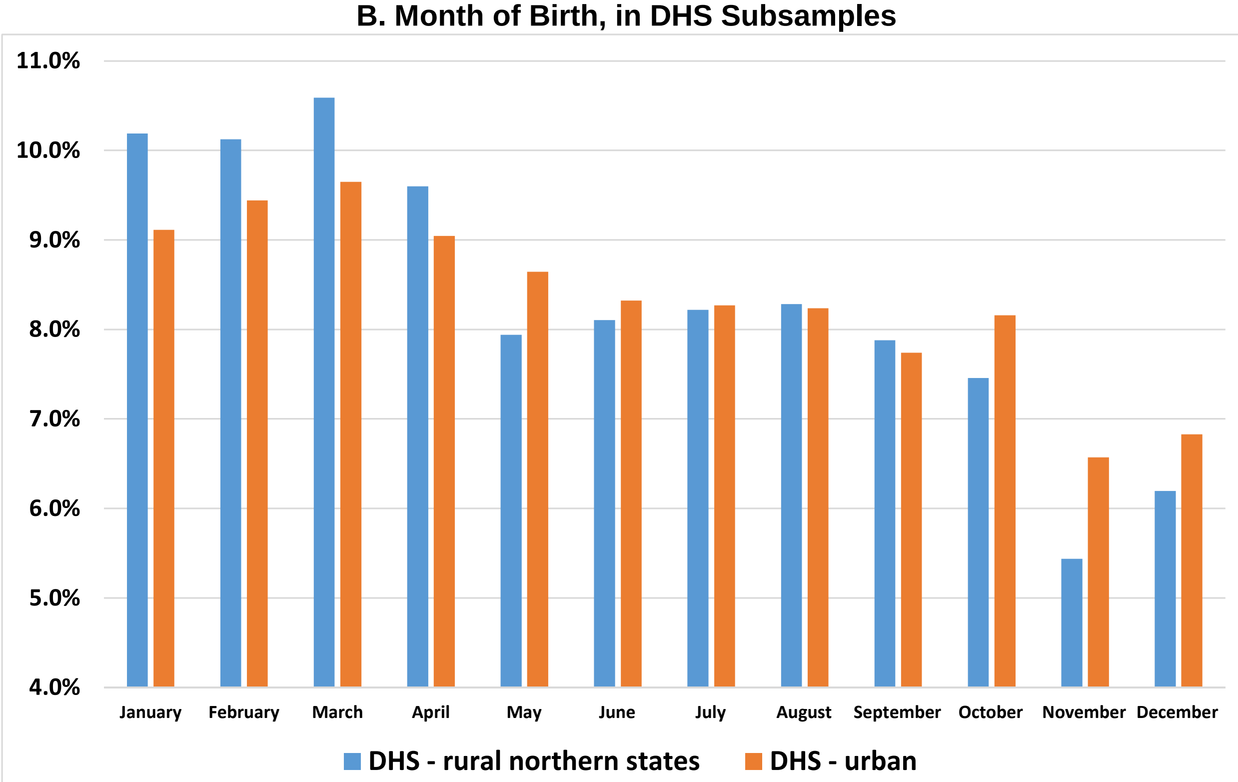
Notes: The top part of the figure shows program implementation: when the registration began, when transfers began, when the program end was announced, and when it stopped enrolling new participants. The central part of the figure shows survey collection timings: when Baseline, Midline and Endline surveys were collected.

Figure 2: Feasibility of Responses in Birth Timing

A. CDF of Birth Spacing Among Women Not Pregnant at Baseline, Controls

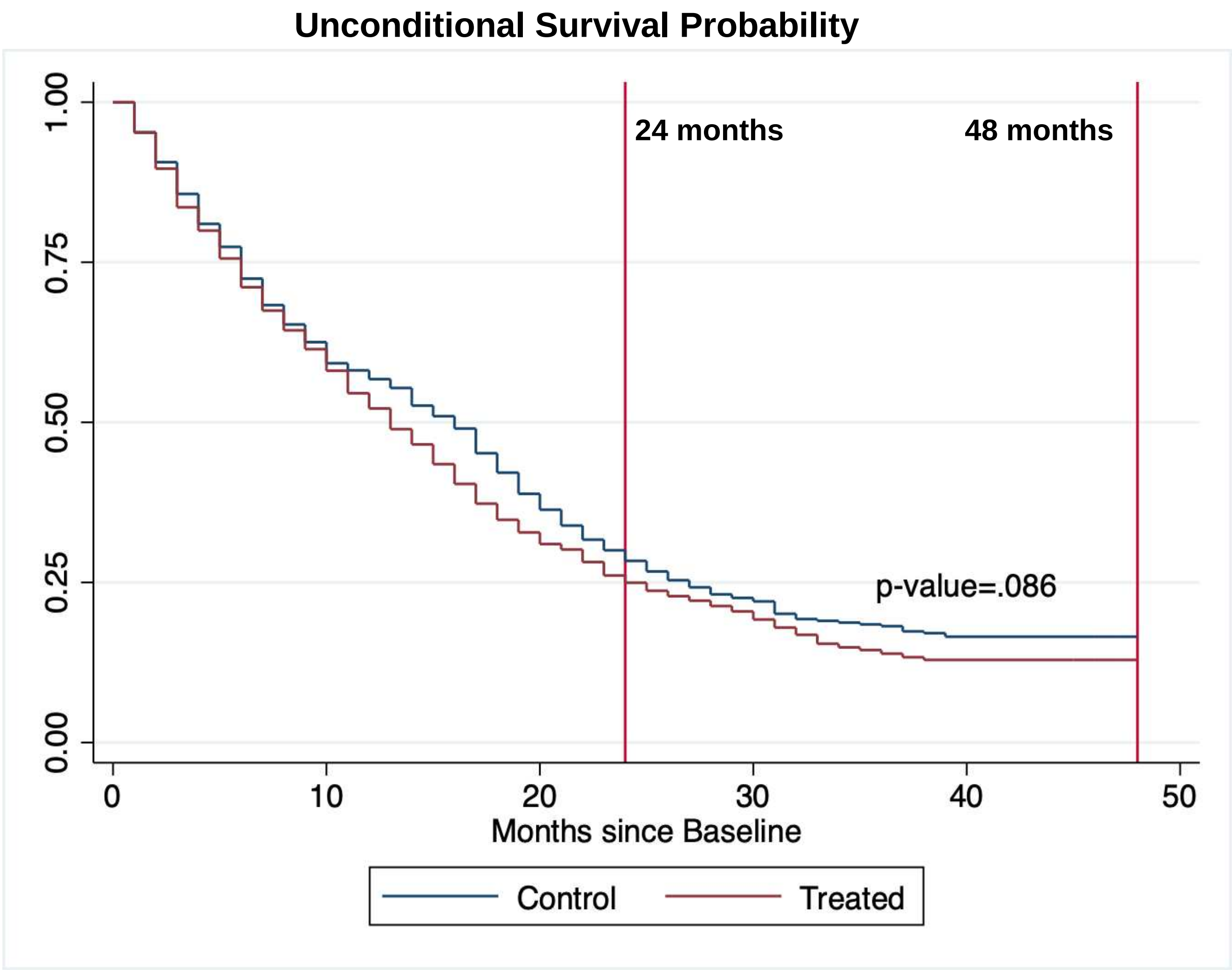


B. Month of Birth, in DHS Subsamples



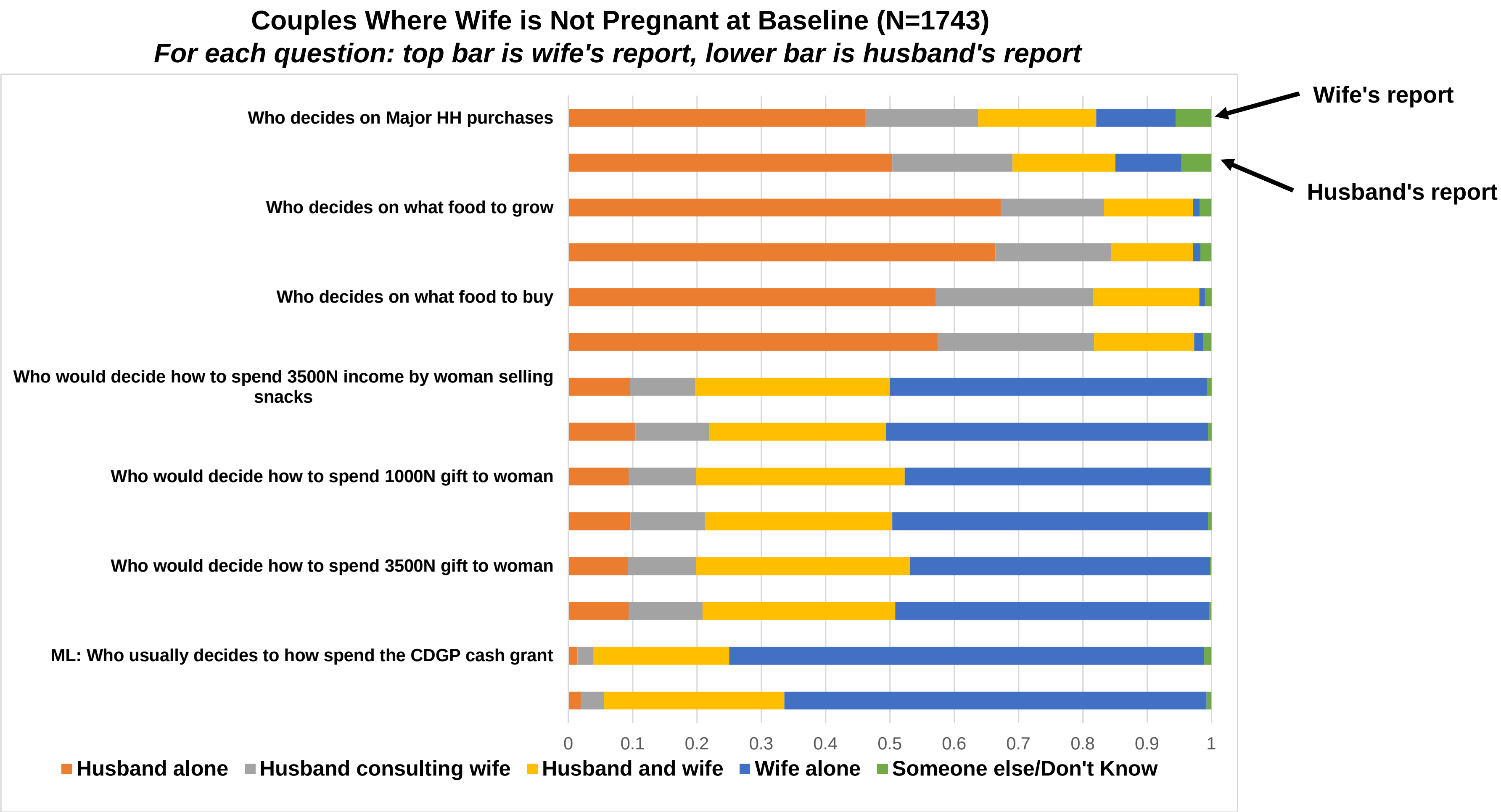
Notes: Figure 2A shows, for control women that were not pregnant at baseline, the CDF of birth spacing between their first child born in the study period and their immediately older sibling. The red lines indicate 24 months and 48 months respectively. Figure 2B shows (using 2013 DHS data from Nigeria) months of birth for rural Northern states, as well as urban areas as a benchmark.

Figure 3: Survival Probability of Not Becoming Pregnant



Note: This plots the unconditional proportion of women who have not become pregnant at each point in time, among those not pregnant at Baseline. Time is measured in months since the Baseline. The red lines indicate 24 months and 48 months respectively. The figure reports the p-value of a Chi-squared test of equality of the two curves.

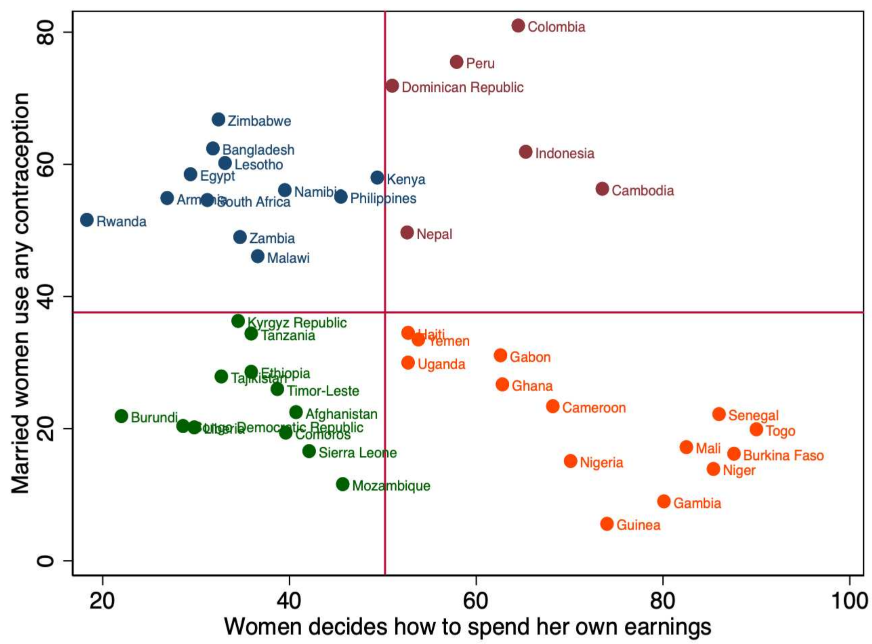
Figure 4: Household Decision Making



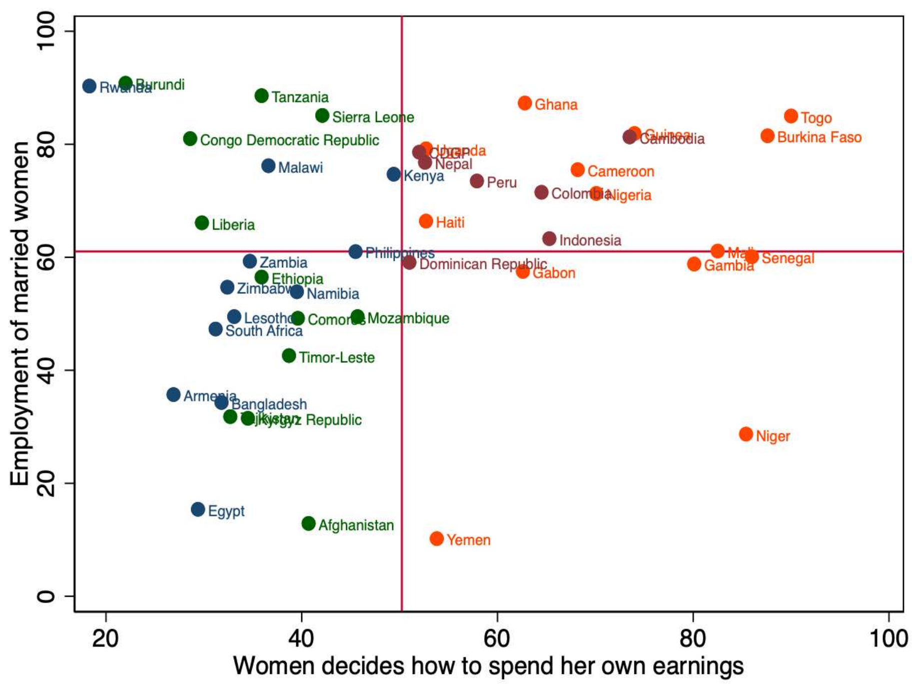
Notes: The main sample for this figure is all households where the women reported to not be pregnant at baseline. The figure shows women and their husbands' responses to questions about bargaining power. Questions labelled 'ML' were asked at Midline. The shading of each area of the bars represents the fraction of respondents giving each answer.

Figure 5: External Validity

A. Separate Spheres Household Decision Making



B. Women's Employment and Control of Earnings



Notes: This figure uses data from multiple Demographic and Health Surveys that cover 45 countries. We take the year nearest to 2013 (the year of baseline for our program) if the country has been surveyed since 2008. Panel (A) plots the proportion of respondents in each country survey who are married and using contraception against the proportion of women who are able to decide how to spend their own earnings. The vertical and horizontal lines represent the means of the variable stated on the relevant axis. We separate each country by being below or above each of the means and this creates the four quadrants depicted by different color points. Panel (B) uses the same color codes from panel (A) and plots employment rates of married women against the proportion of women who are able to decide how to spend their own earnings.

Table A1: Information Components of the Intervention

A. Key Messages

Period	Message	Details
Prenatal	**Attend antenatal care**	Attend antenatal care at least four times during pregnancy.
	Eat one additional meal during pregnancy	Eat one extra small meal or 'snack' (extra food between meals) each day to provide energy and nutrients for you and your growing baby.
Perinatal	**Breastfeed immediately**	Start breast feeding your baby within the first 30 minutes of delivery. Colostrum is good for the baby.
	Breastfeed exclusively	Breastfeed your child exclusively until six months old. Do not give water, tinned milk, or any other food.
Postnatal	**Complementary feeding**	Introduce complimentary foods at six months of age while continuing to breastfeed. Breastfeed on demand and continue until two years of age. Gradually increase food variety as the child gets older.
	Hygiene and sanitation	Wash your hands after going to the toilet, cleaning baby who defecated, before and after feeding baby; wash baby's hands and face before feeding.
	Use health facilities	Take baby to health facility if you notice any of the following: fever, convulsion, refusing to eat, malnutrition, diarrhea.
	Nutritious food	Ensure you buy nutritious foods when you are buying food for your family.

B. Low- and High-intensity Channels of Message Delivery

T1: Low-Intensity	**Information and education posters**	Health and nutrition related posters are affixed in health facilities and village centers.
	Radio jingles / phone-in programs	Jingles are played regularly on local radio channels. Phone-in programs are one-hour shows in which CDGP staff and invited experts talk about one selected topic, and listeners can call in with questions.
	Friday preaching / Islamic school teachers	
	Health talks	Trained health workers come to the village and deliver a session on a selected topic, with the aid of information cards. Any village resident can attend these talks, irrespective of beneficiary status.
	Food demonstrations	CDGP trained staff delivers nutrition education about the benefits of different foods, and demonstrates how to prepare and cook nutritious meals for children and other household members.
	Voice messages	Pre-recorded messages are sent to beneficiaries' program phones to reinforce key messages.
T2: High-Intensity	**Infant and Young Child Feeding (ICYF) support groups**	Groups are formed within communities to support beneficiaries, under the supervision and facilitation of community volunteers and health extension workers. The recommended size is 12-15 people, meeting once a month. They are also offered to men.
	One-on-one counselling	Beneficiaries and their husbands can consult community volunteers on an `as needed' basis to receive specific information and training.

Notes: Panel A lists the eight key messages around which the behavior change communication component of CDGP was built. Panel B details the channels by which these key messages were delivered to beneficiaries in treated villages.

Table A2: Attrition

Dependent variable: attrit from sample (0/1)
Standard errors in parentheses clustered by village

	Non-Pregnant Woman at Baseline			Husband	Older sibling	Target Child
Period: Baseline to	**(1) Endline**	**(2) Endline**	**(3) Endline**	**(4) Endline**	**(5) Midline**	**(6) Endline**
Treated village	.023	.016	.145	.114	.154	.132
	(.015)	(.016)	(.078)	(.079)	(.106)	(.168)
Village insecure at midline	.051	.053	.100	.095	.863***	
	(.034)	(.034)	(.056)	(.060)	(.033)	
Village insecure at endline	.872***	.875***	.855***	.815***		.874***
	(.023)	(.023)	(.049)	(.053)		(.038)
Treated village * Village insecure at midline			-.097	-.086	-.024	
			(.057)	(.062)	(.037)	
Treated village * Village insecure at endline			.038	.067		-.088*
			(.050)	(.054)		(.035)
Randomization Strata	Yes	Yes	Yes	Yes	Yes	Yes
Attrition rate	.204	.204	.204	.219	.231	.225
Joint p-value on interactions			.221	.335	.259	.030
Observations	1743	1579	1579	1579	1413	888

Notes: Significance levels: * (10%), ** (5%), ***(1%). Each Column presents estimates using a linear probability model where the dependent variable is if the individual subject attrits and the independent variables are a varying set of treatment indicators, baseline covariates and interactions. Attrition takes the value of one if the subject surveyed at baseline (or midline if the target child) was not surveyed at endline (except for attrition of the older sibling, which is measured at midline). The sample in Columns 1 to 3 are women not-pregnant at baseline. In Column 4, the sample is husbands of women who were not pregnant at Baseline. In Column 5, the sample is the older sibling of the target child in households where the woman was not pregnant at baseline. In Column 6, the sample is the target child in households where the woman was not pregnant at baseline. All Columns include treatment status and village insecurity status, at midline and endline. Column 2 adds controls for baseline characteristics of the household and mother: the number children aged 0-2, 3-5, 6-12 and 13-17, the number of adults, the number of adults aged over 60, mother's age, whether she ever attended school, total monthly expenditure, a dummy for polygamous relationships. All other Columns further add interactions between the program indicators and the covariates as well as interaction between security and treatment status. Column 5 does not include insecure at endline as the older sibling is not surveyed then. Column 6 does not include insecure at Midline as the target child is only followed from midline onwards. At the foot of Columns 3 onwards, we report the p-value on the null on the joint hypothesis test that all interaction terms are zero.

Table A3: Fertility Results, Alternative Weighting

Sample: Non-pregnant women at baseline (N=1743)
Standard errors in parentheses clustered by village

	Weights: Probit	Weights: LPM
	(1) Four Year Impacts	**(2) Four Year Impacts**
Panel A: Pregnancy Timing		
Hazard Ratio	1.11	1.10
	(.095)	(.093)
Panel B: Birth Spacing		
Birth spacing between target child and their older sibling (months)	.294	.195
	(.770)	(.761)
Birth spacing between target child and their older sibling <= 24 months	.023	.024
	(.017)	(.018)
Panel C: Season of Birth		
Born during Kaka (Mid Oct to Dec)	-.051*	-.047
	(.031)	(.030)
Born during Sanyi (Dec to Feb)	-.028	-.030
	(.025)	(.025)
Born during Rani (Mar to May)	.027	.030
	(.032)	(.032)
Born during Damuna (Jun to Mid Oct: lean season)	.052	.048
	(.034)	(.034)
Panel D: Total Fertility		
Any child born	.032	.027
	(.039)	(.039)
Number of children born	.048	.043
	(.066)	(.066)

Notes: Significance levels: * (10%), ** (5%), ***(1%). We reweight the sample based on their probability of becoming pregnant in two ways. The likelihood of becoming pregnant was established using a prediction model based on data from the 2013 Nigeria Demographic and Health Survey (NPC and ICF, 2014). The probability of giving birth in the next two years was modelled as a function of woman's age, time since last birth, household size, number of children aged under and over 5 years in household, and TV ownership. The estimated coefficients from a linear probability model on the DHS data were then used to predict pregnancy probability in the CDGP listing data, the Probit weights are used in Column 1, the LPM weights are used in Column 2. In Panel A we report the impact of treatment assignment on the hazard of becoming pregnancy from a Cox Proportional Hazard model, controlling for LGA and randomization tranche fixed effects, baseline characteristics of the household and mother, and months between the baseline and the previous birth. The remaining panels are estimated using OLS, controlling for LGA and randomization tranche fixed effects and baseline characteristics of the household and mother. In all specifications these characteristics are the number children aged 0-2, 3-5, 6-12 and 13-17, the number of adults, the number of adults aged over 60, mother's age, whether she ever attended school, total monthly expenditure and a dummy for polygamous relationships. Standard errors are clustered at the village level throughout.

Table A4: Fertility Results, Robustness Checks

Sample: Non-pregnant women at baseline (N=1743)
Standard errors in parentheses clustered by village

	Below Median Number of Children at Baseline	Above Median Number of Children at Baseline	Polygamous	Non-polygamous	Above median savings at baseline	Below median savings at baseline
	(1) Four Year Impacts	(2) Four Year Impacts	(3) Four Year Impacts	(4) Four Year Impacts	(5) Four Year Impacts	(6) Four Year Impacts
Panel A: Pregnancy Timing						
Hazard Ratio	1.13	1.04	1.18	1.06	1.00	1.05
	(.110)	(.126)	(.149)	(.105)	(.120)	(.111)
Panel B: Birth Spacing						
Birth spacing between target child and their older sibling (months)	-1.06	.773	-.975	.406	.009	-.940
	(1.00)	(.974)	(1.12)	(.952)	(1.14)	(1.02)
Birth spacing between target child and their older sibling <= 24 months	-.000	.051**	.053*	.008	.022	.059**
	(.027)	(.024)	(.027)	(.025)	(.025)	(.027)
Panel C: Season of Birth						
Born during Kaka (Mid Oct to Dec)	-.033	-.071*	-.073	-.026	-.082*	-.031
	(.039)	(.040)	(.047)	(.034)	(.044)	(.046)
Born during Sanyi (Dec to Feb)	-.046	-.014	-.005	-.054	.000	-.068*
	(.035)	(.036)	(.035)	(.034)	(.038)	(.041)
Born during Rani (Mar to May)	.019	.013	.025	.030	.034	.029
	(.044)	(.045)	(.057)	(.039)	(.047)	(.051)
Born during Damuna (Jun to Mid Oct: lean season)	.060	.072	.053	.050	.048	.070
	(.047)	(.054)	(.059)	(.043)	(.052)	(.057)
Panel D: Total Fertility						
Any child born	-.002	.047	.045	.001	.028	.005
	(.041)	(.046)	(.047)	(.041)	(.046)	(.042)
Number of children born	.023	.066	.094	.008	.056	.018
	(.074)	(.079)	(.081)	(.077)	(.078)	(.081)

Notes: Significance levels: * (10%), ** (5%), ***(1%). We split the sample in three ways. Columns 1-2 show a split by the median number of children the mother has had at baseline. Columns 3-4 split the sample by polygamous and non-polygamous households. Columns 5-6 split be above or below median savings at baseline. We report ITT estimates at the four-year endline. In Panel A we report the impact of treatment assignment on the hazard of becoming pregnancy from a Cox Proportional Hazard model, controlling for LGA and randomization tranche fixed effects, baseline characteristics of the household and mother, and months between the baseline and the previous birth. The remaining panels are estimated using OLS, controlling for LGA and randomization tranche fixed effects and baseline characteristics of the household and mother. In all specifications these characteristics are the number children aged 0-2, 3-5, 6-12 and 13-17, the number of adults, the number of adults aged over 60, mother's age, whether she ever attended school, total monthly expenditure and a dummy for polygamous relationships. Standard errors are clustered at the village level throughout.

Table A5: Information, Pregnancy, Birth Spacing and Total Fertility

Sample: Non-pregnant women at baseline (N= 1,743)
Standard deviation in braces
Standard errors in parentheses clustered by village

	(1) Control Mean	(2) Four Year Impacts, T1 (low intensity)	(3) Four Year Impacts, T2 (high intensity)	p-value (2)=(3)
Panel A: Knowledge and Practices				
Wife's knowledge index	0	.431***	.563***	[.025]
	{1.00}	(.074)	(.077)	
Husband's knowledge index	0	.199***	.229***	[.531]
	{1.00}	(.058)	(.055)	
Practices index	0	.492***	.441***	[.648]
	{1.00}	(.100)	(.111)	
Child health behaviours index	0	.425***	.443***	[.870]
	{1.00}	(.098)	(.096)	
Panel B: Pregnancy Timing				
Hazard Ratio		1.12	1.05	[.447]
		(.107)	(.099)	
Panel C: Birth Spacing				
Birth spacing between target child and their older sibling (months)	35.0	-.144	.139	[.742]
	{10.8}	(.888)	(.842)	
Birth spacing between target child and their older sibling <= 24 months	.088	.034	.017	[.446]
		(.021)	(.020)	
Panel D: Season of Birth				
Born during Kaka (Mid Oct to Dec)	.218	-.051	-.034	[.605]
		(.033)	(.033)	
Born during Sanyi (Dec to Feb)	.161	-.029	-.037	[.755]
		(.027)	(.029)	
Born during Rani (Mar to May)	.258	.008	.041	[.359]
		(.036)	(.037)	
Born during Damuna (Jun to Mid Oct: lean season)	.362	.072*	.030	[.322]
		(.038)	(.039)	
Panel E: Total Fertility				
Any child born	.782	.012	.024	[.834]
		(.044)	(.040)	
Number of children born	1.20	.035	.051	[.799]
	{.815}	(.076)	(.070)	

Notes: Significance levels: * (10%), ** (5%), ***(1%). Column 1 shows the mean (and standard deviation for continuous outcomes) value in Control households at endline (Panel A) and at baseline (Panels B to E). Columns 2 and 3 report ITT estimates for each treatment arm (estimated in a common equation), at the four-year endline. In Panel A, the knowledge, practice and health behaviors indices are constructed using the method described in Anderson [2008], so standardized to have mean zero and variance one in controls. The knowledge index (of wives and husbands) includes the following question components: Would you advise to seek a check-up even if the baby is healthy? Is colostrum good for the baby? Should you breastfeed immediately? Where is best place to give birth? Should a baby receive any other liquids on first day? Should you give water to a baby if it is hot out? How long should you exclusively breastfeed for? The practices index for mothers of practices carried out towards the younger sibling of the target child has the following question components: Did the child receive antenetal care? Was the child fed colostrum in the first hour? Was the child put to breast immediately? Was the child born at a health facility? And (if applicable) was the child exclusively breastfed for 6 months? The health behaviors index towards the target child includes the following question components: Did the child receive deworming in the last six months? Did the child receive basic vaccinations? In Panel B we report the impact of treatment assignment on the hazard of becoming pregnant from a Cox Proportional Hazard model, controlling for LGA and randomization tranche fixed effects, baseline characteristics of the household and mother, and months between the baseline and the previous birth. For the remaining panels, Column 1 shows the mean (and standard deviation for continuous outcomes in braces) value in Control households. Column 2 reports ITT estimates at the four-year endline. These are estimated using OLS, controlling for LGA and randomization tranche fixed effects and baseline characteristics of the household and mother. In all specifications these characteristics are the number children aged 0-2, 3-5, 6-12 and 13-17, the number of adults, the number of adults aged over 60, mother's age, whether she ever attended school, total monthly expenditure and a dummy for polygamous relationships. Standard errors are clustered at the village level throughout.

Table A6: Target Child Outcomes

Sample: Households with non-pregnant women at baseline (N= 1,743)

Column 1: Standard deviation in braces

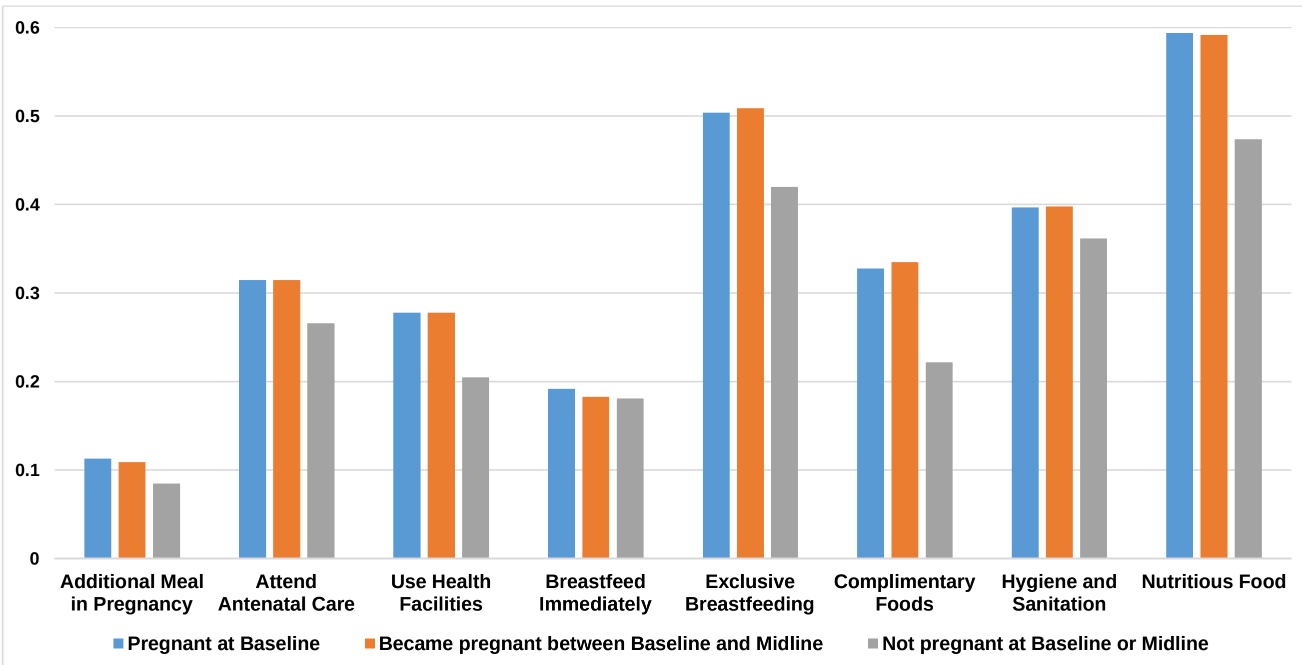
Columns 2 and 3: Standard errors in parentheses clustered by village

	(1) Control, Midline	(2) ITT, Two Year Impacts	(3) ITT, Four Year Impacts	(2) = (3)
Panel A: Anthropometrics				
Height-for-Age (HAZ)	-1.85	.258**	.111	[.232]
	{1.42}	(.109)	(.088)	
Stunted (HAZ < -2)	.493	-.110***	-.042	[.137]
		(.037)	(.037)	
Panel B: Health				
Had illness or injury in past 30 days	.626	-.060*	-.098**	[.423]
		(.032)	(.040)	
Had diarrhea in past two weeks	.343	-.051	-.085**	[.477]
		(.035)	(.037)	
If had diarrhea in past two weeks:				
Anyone sought advice/treatment	.794	.050	-.053	[.146]
		(.047)	(.046)	
Given ORS for diarrhea	.353	.100	.170**	[.496]
		(.061)	(.078)	
Number of vaccinations	.880	.302***	.318***	[.874]
	{1.28}	(.095)	(.149)	
Child given deworming medications in the past 6 months	.155	.022	.180***	[.006]
		(.035)	(.042)	

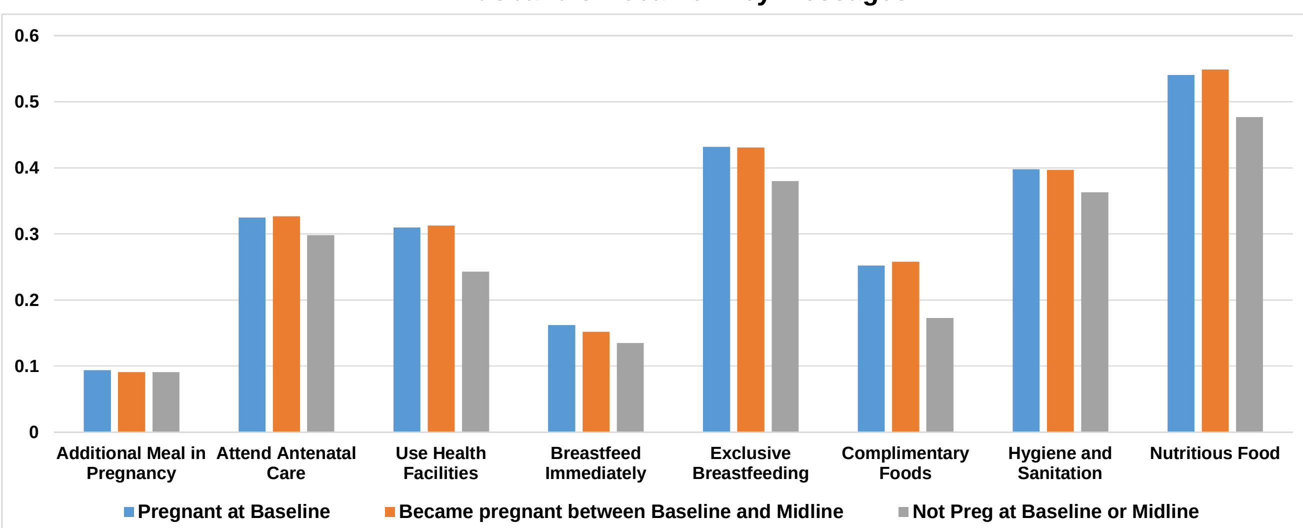
Notes: Significance levels: * (10%), ** (5%), ***(1%). Columns 1shows the mean (and standard deviation for continuous outcomes) value in Control households at Midline. Column 2 reports ITT estimates at the two-year midline, and Column 3 reports ITT estimates at the four-year endline. These are estimated using OLS, controlling for LGA and randomization tranche fixed effects, and the following Baseline characteristics of the household and mother: the number children aged 0-2, 3-5, 6-12 and 13-17, the number of adults, the number of adults aged over 60, mother's age, whether she ever attended school, total monthly expenditure, a dummy for polygamous relationships. Standard errors are clustered at the village level throughout. In Panel A, stunted is a dummy indicating children with height-for-age-z-score (HAZ) under -2 standard deviations of the WHO defined guidelines [WHO 2009]. Severely stunted is a dummy indicating children with height-for-age-z-score (HAZ) under -3 standard deviations of the WHO defined guidelines. In Panel B, the Child Health Outcome Index is constructed as in Anderson [2008], and standardized to have mean zero and variance one in the Control group at Midline. The index includes the following health outcome components: a dummy variable that takes the value of 1 if the child has not been ill in the last month and a dummy variable that takes the value of 1 if the child has not had diarrhea in the past two weeks.

Figure A1: Recall of Key Messages at Midline

A. Wife's Recall of Key Messages



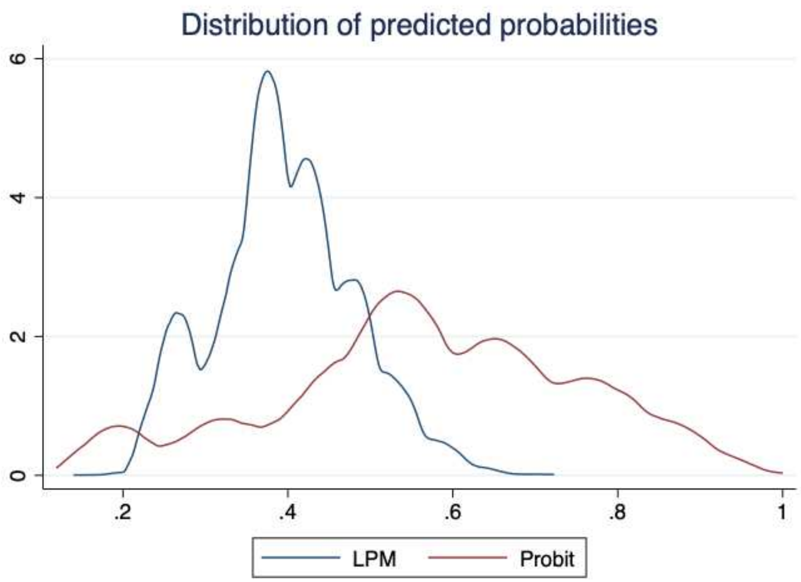
B. Husband's Recall of Key Messages



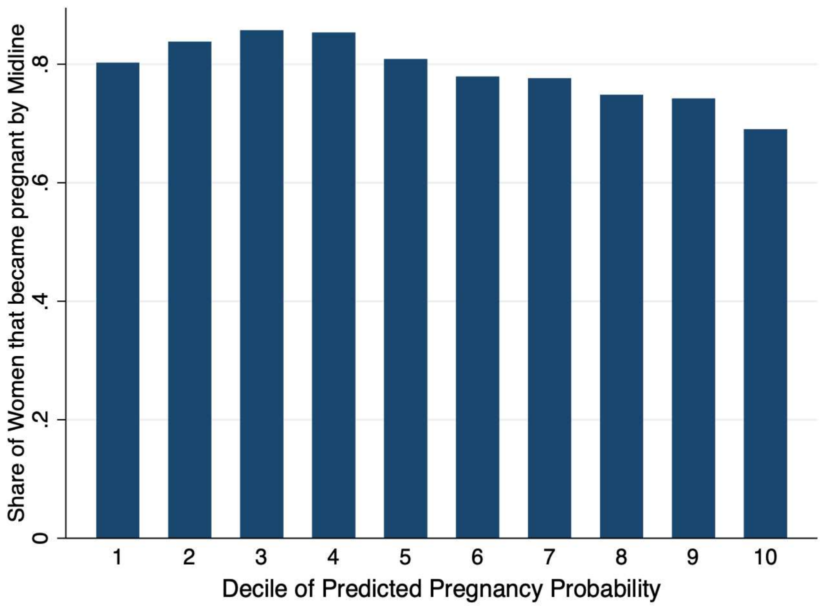
Notes: This figure is based on data from women and their husbands in households. The top panel shows recall rates for (i) women pregnant at baseline; (ii) women that became pregnant between baseline and midline; (iii) women not pregnant at baseline and still not pregnant by midline. The bottom panel repeats this for husbands. Recall is from any low intensity information channel (posters, radio, food demonstrations and health talks). Individuals are asked if they have been exposed to CDGP information from a particular information channel (and we repeat this for each channel). If the individual says yes to this, they are asked what messages do they recall from the information channel. If an individual was not exposed to any information channel, their recall of messages is set to zero.

Figure A2: Predicted Probability of Becoming Pregnant, Sample of Women Not Pregnant at Baseline

Panel A. Predicted Probability of Becoming Pregnant by Midline

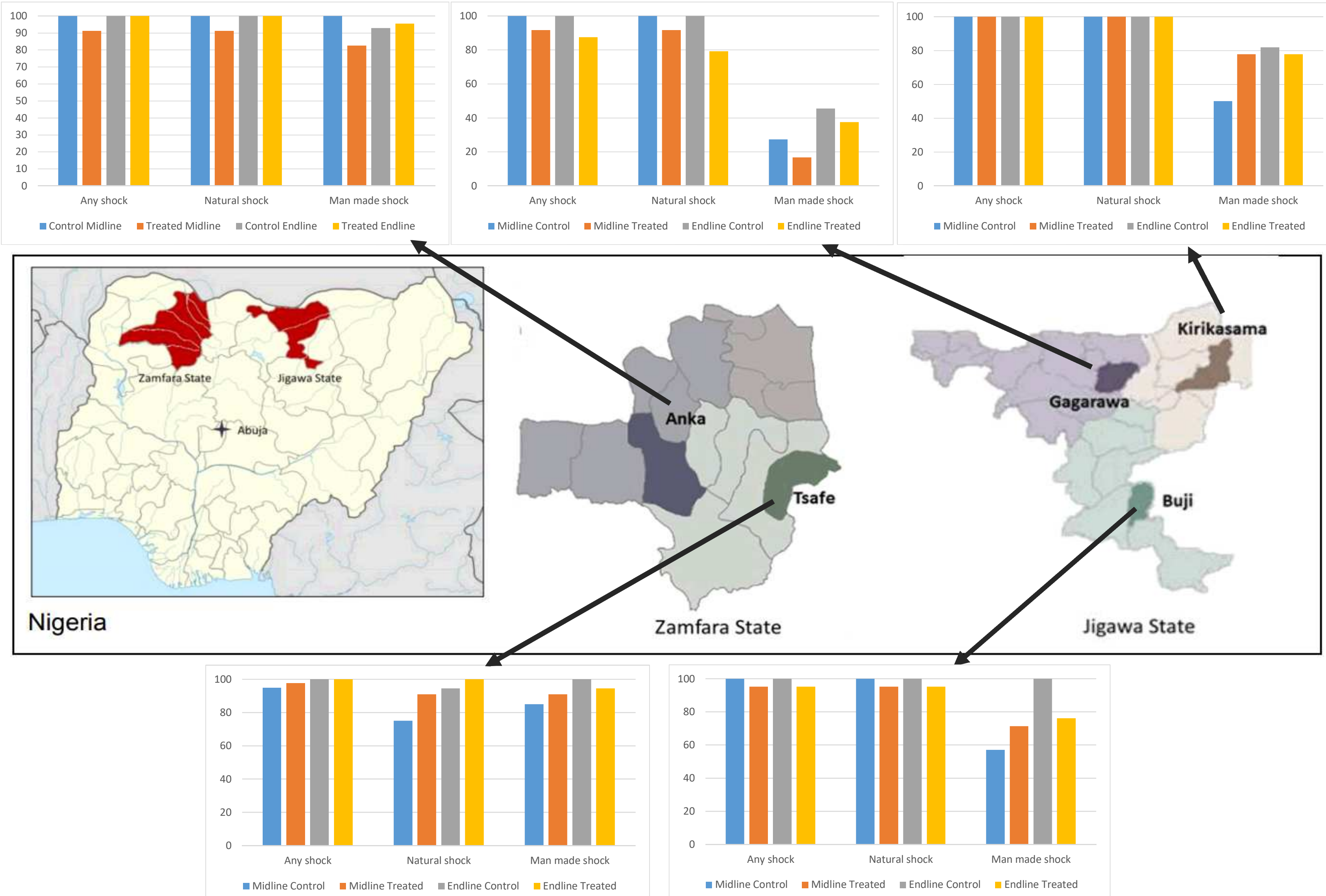


Panel B. Actual Likelihood of Becoming Pregnant by Midline



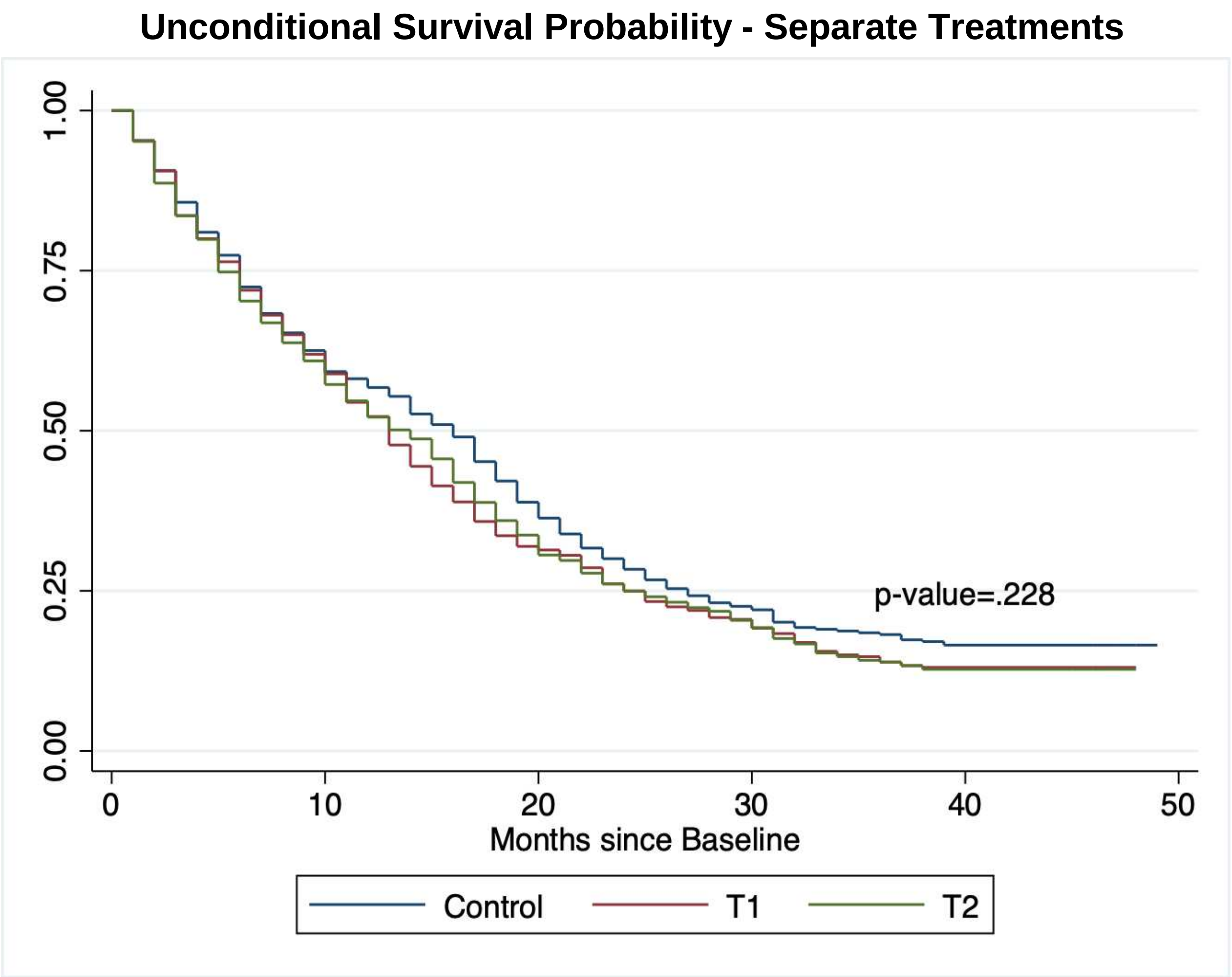
Notes: The likelihood of becoming pregnant was established using a prediction model based on data from the 2013 Nigeria Demographic and Health Survey (NPC and ICF, 2014). The probability of giving birth in the next two years was modelled as a function of woman's age, time since last birth, household size, number of children aged under and over 5 years in household, and TV ownership. The estimated coefficients from a linear probability model on the DHS data were then used to predict pregnancy probability in the CDGP listing data. In Panel A we present the LPM weights which were used in selection and also weights estimated using an alternative Probit specification. In Panel B we present the share of women who actually become pregnant by midline, by decile of predicted probability using the LPM model.

Figure A3: Frequency of Village Shocks, by Local Government Authority



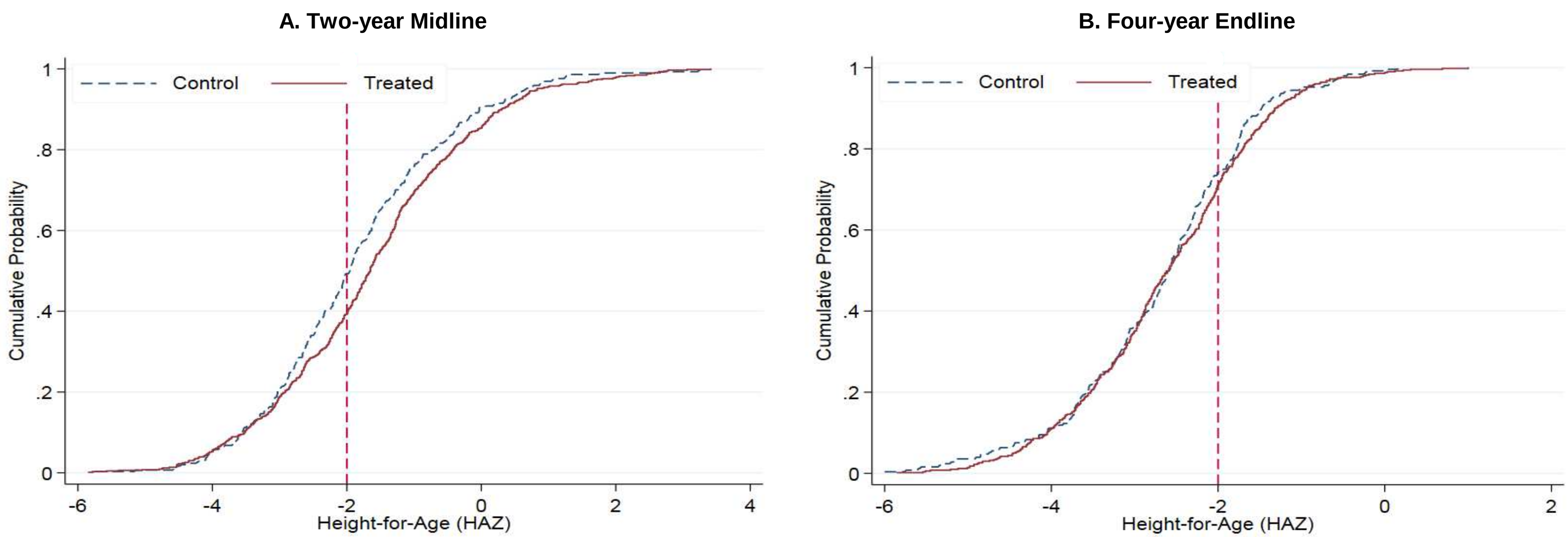
Notes: This figure reports data from the village surveys. A Natural shock in the village in the past year is a dummy equal to one if the village experiences a drought, flood, crop damage by pests or by disease. A Man made shock in the village in the past year is a dummy equal to one if the village experiences curfews, land disputes, violence, widespread migration or cattle rustling.

Figure A4: Survival Probability of Not Becoming Pregnant



Notes: The figure plots the unconditional proportion of women who have not become pregnant at each point in time, among those not pregnant at Baseline, for women in the low intensity treatment group (T1), in the high intensity treatment group (T2) and in the Control group. Time is measured in months since the Baseline. The figure reports the p-value of a Chi-squared test of equality of the three curves.

Figure A5: Distributional Impacts on Height-for-Age for the Target Child



Notes: This shows the cumulative distribution of the HAZ score at Midline (panel A) and Endline (panel B) for the treatment and control group. A score to the left of the red dashed line indicates that the child is stunted (HAZ < -2).