Do Cash Transfers Promoting Early Childhood Development Have Unintended Consequences on Fertility?*

Pedro Carneiro Lucy Kraftman Imran Rasul Molly Scott†

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Abstract

There has been a stark rise in direct cash transfer programs to the poor, and in policy interest in interventions targeting outcomes in early childhood. We draw these trends together to study whether interventions offering cash transfers to pregnant mothers to promote early childhood development, unintentionally induce those not pregnant to bring forward birth timing in order to become eligible for the receipt of such transfers. We do so in the context of rural Northern Nigeria, where the majority of households reside in extreme poverty, are credit constrained and food insecure, and frequently experience aggregate shocks. Contraceptives are unavailable and men largely drive fertility choices, yet the costs of bringing forward birth timing mostly operate through worse maternal and child health. We present evidence from a randomized control trial evaluating an intervention offering high-valued and long-lasting unconditional cash transfers to pregnant mothers. We examine how this impacted fertility dynamics among 1700 households in which women were not pregnant at baseline, tracked over four years from the intervention initiation. We document precise null impacts on the timing of births, total births and composition of households becoming pregnant over our study period. The reason is a combination of three factors: (i) women retain full control over the use of additional resources they bring into their household; (ii) women have available productive investment opportunities in their own businesses; (iii) they choose to transfer few resources to husbands. Hence ultimately men have weak private incentives to alter birth timing. Based on this constellation of factors, we use DHS surveys to classify low-income countries into those that are more or less likely to see fertility consequences when cash transfers for early childhood development are targeted to pregnant mothers. *JEL Classification I15, O15.*

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†Carneiro: UCL, IFS, CEMMAP, FAIR-NHH, p.carneiro@ucl.ac.uk; Kraftman: IFS, lucy.kraftman@ifs.org.uk; Rasul: UCL and IFS, i.rasul@ucl.ac.uk; Scott: National Centre for Social Research, molly.scott@natcen.ac.uk.

1 Introduction

One of the most prominent changes in the landscape of development policy has been the increased use of direct cash transfers to the poor. 119 low-income countries have now implemented some unconditional cash transfer program, and a further 52 have established 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]. Cash transfers to the poor are likely to become further entrenched as such policies have been a leading response to the COVID-19 crisis in the developing world. An established body of evidence shows cash transfers are an effective means by which to reduce poverty and increase household welfare through multiple channels including those related to education, health and nutrition, savings and investment [Bastagli *et al.* 2018].

Equally impressive has been the rise in policy maker interest in promoting human capital development in early life. There has been 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 in early life [Black *et al.* 2017], and such deprivation in early life has consequences throughout the life cycle [Almond and Currie 2011]. Evaluations from across settings – including in Sub Saharan Africa – show interventions to promote human capital development in early life can generate large private, social and intergenerational returns.[1]

These powerful trends are now intertwining, with the provision of cash transfers increasingly being used to promote early childhood development. Common features of such interventions are that resources are provided directly to mothers, and that eligibility hinges on whether a woman within the household is identified to be pregnant, or to have a very young child. We study whether and how the availability of such programs impact households without pregnant women in them and so not initially eligible for the cash transfers. More precisely, in a context of extreme economic destitution, we study whether the offer of cash transfers promoting early childhood development induces endogenous fertility responses among not pregnant households, in order to gain access to the resources on offer and ease some of the economic hardships they would otherwise face.

We do so by presenting evidence from a randomized control trial evaluation of program providing extremely high-valued cash transfers to promote early child development, in a context of extreme poverty (two states in Northern Nigeria). The program is known as the Child Development Grant Program (CDGP). It provides any household in which a women is verified to be pregnant (via a urine test) a bundle of: (i) information to mothers and fathers on recommended practices related to pregnancy and infant feeding; (ii) unconditional cash transfers, paid directly to mothers conditional on their pregnancy being verified, so while the child is *in utero,* each month

[1] Impacts of interventions in early life have been found on cognitive development and health [Campbell *et al.* 2014, Conti *et al.* 2016, Attanasio *et al.* 2019, Doyle 2019, Carneiro *et al.* 2021, Field and Maffioli 2021, Justino *et al.* 2021], schooling and labor market productivity [Hoddinott *et al.* 2008, Gertler *et al.* 2014] and across generations [Heckman and Karapakula 2019].

until the child turns 24-months old. Targeting children *in utero* reflects the belief that providing resources to households in the critical window of the first 1000 days of life generate higher returns on child outcomes than those targeting children later in life. Transfers are provided for one child, not later borns, although there is no requirement for the pregnancy to occur closer to baseline. Enrolment into the program was announced to be in open for four years from baseline, providing a narrow window for non pregnant households to endogenously accelerate birth timing within.

The value of the unconditional cash transfer is US$22 per month. However benchmarked, this is a substantial amount, corresponding to 85% of women's baseline monthly earnings or 26% of monthly food expenditures. The cumulative resource flow available to eligible households amounts to over $500, akin to a big push on the same scale as asset transfer policies [Banerjee *et al.* 2015, Bandiera *et al.* 2017]. Moreover, the fact that women know transfers will be provided monthly over this period of the child's life provides them with a more stable flow of resources than is available from labor activities: the transfers almost act as a *de facto* temporary basic income for pregnant mothers.

Understanding fertility responses among those not immediately eligible is important because in low-income contexts even small changes in birth timing and total fertility can have large and persistent impacts on the welfare of children and mothers, through well-documented risks to maternal and child health [DaVanzo *et al.* 2004, WHO 2005, Pimentel *et al.* 2020, Damtie *et al.* 2021].[2] Our study context – Jigawa and Zamfara states in Northern Nigeria – is an area of intense economic destitution and any unintended consequence on fertility might by especially costly. As Figure A1 shows, this region is yet to experience the demographic transition, with total fertility rates close to six and so well above the average in Sub Saharan Africa (Panel A); birth spacing is 30 months (Panel B) with around 30% of births being spaced less than 24 months apart (Panel C), a threshold marker for detrimental impacts for mothers and children [WHO 2005]; contraceptive usage is very limited, with fewer than 10% of married women in Jigawa and Zamfara states in Northern Nigeria reporting using any form of contraception (Panel D).

These features are reflected in our evaluation sample, that is purposefully collected in order to answer our core research question. Our sample comprises households not immediately eligible for the program because the main women in them is not pregnant when the program is initiated. At baseline, 85% of our sample households live in extreme poverty – so below the $1.90/day threshold. Infant mortality rates are 90 per 1000 children, and the vast majority of women entirely lack access to contraceptives. Both factors result in high fertility rates – at baseline, women in our sample

[2] The WHO recommends a minimum birth interval of 33 months or more to ensure the maximum health benefits for mothers and newborns [WHO 2005]. Spacing the child for a minimum of two years reduces infant mortality by 50% [DaVanzo *et al.* 2004]. Short birth spacing has been linked with different adverse pregnancy and childbirth outcomes such as low birth weight, preterm birth, congenital anomalies autism, small size for gestational age, and neonatal, infant and child mortality. Moreover, 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 aged 24 and have 4 children on average, yet are far from completing their fertility cycle. In this region the agricultural cycle includes a lean season in which households face food shortages, and have to resort to extreme coping strategies. Households have access to highly imperfect credit markets that limit household's ability to smooth consumption, and our sample villages are subject to frequent aggregate shocks. Finally state capacity is limited, so even though the intervention we study is delivered by NGOs and announced to have a fixed window of enrolment, its introduction might not signify permanent changes in the social assistance on offer.

All these features of the environment create economic forces pushing in the same direction, to provide households high-powered incentives to accelerate the timing of births in order to become eligible for cash transfers, or to have an additional child if their fertility cycle was already complete.

The marginal benefit of doing so is that such resources might then be used to ameliorate economic pressures, as well as being used to invest in the human capital of young children. The marginal cost of doing so is that by reducing birth spacing, there are risks to maternal and child health. These marginal benefits and marginal costs are spread slightly differ across household members: husbands, wives and children.

Furthermore, we note that Nigeria is a patriarchal society where bride prices are still 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. These features of our study context are well documented by work in demography and gender studies [Caldwell and Caldwell 1987, 1988, Odimegwu and Adenini 2014, 2015]. If men are less informed about the maternal risks of shifting birth timing or total fertility than their wives [Ashraf *et al.* 2020a], yet have some say over fertility, the imbalance between spouses in terms of who drives fertility decisions and who bears the marginal costs of shifting birth timing can further tilt the balance towards households bringing forward the timing of births in order to gain access to these resources.

Our research design and data collection allow us to present evidence to causally link multiple margins of fertility-related responses to the offer of cash transfers conditional on pregnancy, and link them to these marginal benefits and marginal costs across household members.

Our evaluation covers households in 210 villages: two thirds of them are randomly assigned to treatment, where the intervention is rolled out from baseline. In earlier work, Carneiro *et al.* [2021], we evaluated the 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. For the children that were *in utero* at baseline, we find persistent and large positive impacts on child anthropometrics, such as their height-for-age Z-scores, and statistically and economically significant reductions in the incidence of child illness, improvements in child nutrition, deworming and vaccination rates. We estimate an IRR to the program, in terms of child outcomes, of at least 10% under plausible assumptions. Hence the program is cost effective among the cross section of women that happen to be pregnant at baseline. In this paper we shed light on whether this extends to the far larger cohort of women not pregnant at baseline.

Relative to Carneiro *et al.* [2021] we focus on an entirely separate sample of 1700 women that were not pregnant at baseline. We shed light on endogenous fertility responses to the intervention offer of cash transfers by tracking this sample of women not pregnant at baseline at two- and four-years post intervention.

Our four year study timeline is thus long enough to consider behavioral responses among households: (i) that decide to shift forward their birth timing in order to start receiving transfers earlier than they otherwise would have if they maintained the fertility path they had planned pre-intervention; (ii) towards the end of the study period and lifetime of enrolment into the program, who otherwise risk losing access to the high-valued cash transfers altogether. On (i), households demand for short term liquidity has been experimentally documented in other low-income contexts, including 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 bring this issue to study of cash transfers targeting pregnant mothers, whereby households can gain liquidity to ease economic hardships and uncertainty, but only conditional on women in them becoming pregnant. On (ii) we note that in other studies where large big push resource injections are offered to households, take-up rates are close to 100%. We extend this to a context in which cumulative resources of around $500 gain be gained by households if they shift birth timing into the four-year window of open enrolment into the program.

Results First, we document precise null impacts on the fertility dynamics of women not pregnant at baseline over the study period. This is both in terms of overall fertility (a *quantum* effect) and the exact timing of births as identified from survival analysis (a *tempo* effect). On the number of children born, these null effects are precise, the 95% confidence interval on our treatment effect rules out a magnitude larger than .094 between baseline and the two-year midline, and larger than .066 between midline and the four-year endline. We also find very limited impacts on the composition of households that become pregnant during our four-year study period.

Second, for women not pregnant at baseline, but who gave birth during our study period, the intervention still leads to improvements in child outcomes. For example, at midline: (i) children's height-for-age Z-scores (HAZ) improve by $.26\sigma$; (ii) there is a reduced incidence of stunting of 10.7pp, corresponding to an 22% reduction. Stunting is the best measure of cumulative effects of chronic nutritional deprivation, and is therefore a key indicator of long-term well-being. On health, an index of health outcomes improves by $.16\sigma$ at midline, and this improvement is sustained at endline, where the index rises by $.25\sigma$ over controls.

Overall, we show the trajectory of child development is very similar between the children of mothers that were not pregnant at baseline and only become eligible through pregnancy, and those that were pregnant at baseline and so automatically eligible at baseline, as we previously analyzed in Carneiro *et al.* [2021]. We also show there are no detrimental impacts on maternal health from the offer of cash transfers. These results reinforce the finding that any endogenous fertility responses to the offer of cash transfers by households not immediately eligible for the intervention is second order.

Our third set of results explain what causes the null effects on fertility dynamics, despite strong economic incentives households face in this setting to become eligible for the cash transfers by bringing forward the timing of births, or to alter the quantity of births for those families who were close to the end of their planned fertility. To do so, we reiterate the wedge between wives – who primarily bear the marginal costs of shifts in birth timing or total fertility – and their husbands, who largely drive fertility decisions given the absence of contraceptives. We thus dig deeper to examine the role of husbands.

We show the null fertility impacts can be explained through a combination of three factors: (i) there is separate spheres decision making in these households [Lundberg and Pollak 1993, Browning *et al.* 2010], so women entirely retain control of any resources they bring into the household (such as earnings), including how additional resources such as 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) few resources leak to husbands for their private gain, except via household public goods of higher food consumption and savings in the longer run.

These factors combine to imply husbands have weak *private* incentive to change fertility dynamics in response to the program, all else equal, despite the strong decision making rights husbands have over fertility in this context.

We underpin this interpretation of our null results using within-sample variation in fertility responses between women that were not pregnant at baseline and: (i) report having full autonomy over how to spend any exogenous increase in resources they bring into the household; (ii) are self-employed; (iii) have opportunities for business investment (either because they own productive livestock or have other business assets at baseline), versus women for whom none of these three conditions are satisfied at baseline. We view the husbands of the former group of women as having relatively weaker incentives to bring forward birth timing, and the latter as having husbands with relatively stronger incentives to do so – either because they can appropriate the cash, or their wife willingly transfers resources to them because she lacks investment opportunities of her own.

We continue to find null fertility impacts among the former group of households. In contrast, we find that for the latter group, households are 9.2pp significantly more likely to have any children born between baseline and midline (corresponding to a 16% increase over comparable households in controls). As is intuitive, this increase in fertility for husbands with stronger incentives occurs within the first two years of open enrolment into the program. Testing for differences across households where husbands have weaker and stronger incentives, the differential impact on the number of children born between baseline and midline is statistically different ($p = .091$).

We rule out further alternative explanations for the null impacts on the timing of fertility unrelated to any specific role of husbands. For example, we consider alternatives related to birth spacing and the demand for additional children. We show that birth spacing – while low – is not close to its biological lower bound [WHO 2005]. It is feasible for the vast majority of households to bring forward birth timing if they choose to. We also show the results are not driven by women close to the end of their fertility cycle and so perhaps with little desire for additional children. We do so by documenting null impacts for households at different stages of the fertility cycle.

We also rule out that households expect the offer of cash transfers from the intervention to be available for the foreseeable future. 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. At its initiation, the program is announced to be in place for four years. This is reinforced by a further announcement – close to our four-year endline – that the program will close further enrolment. Households that have not become pregnant at that stage risk losing receipt of cash transfers altogether. We find no spike in births even towards the end of our evaluation period. At the four-year endline, 9.4% of households in treated villages that have a non-pregnant woman in them at baseline, still have not had a child by endline (and this does not differ to controls where it is 9.9%).

At a final stage of analysis we take the implications of the mechanisms driving our main results to wider cross-country data to speculate on the external validity of our findings, and their implications for the next generation of interventions using cash transfers to promote early childhood development. More precisely, we draw together DHS surveys from across Sub Saharan Africa to shed light on other low-income settings that are more or less likely to see unintended fertility consequences when substantial cash transfers promoting early childhood development are offered to pregnant mothers. We establish this typology based on this constellation of factors related to household decision making and investment opportunities available to women revealed by the evidence from the randomized control trial.

This analysis provides new insights on which are the other countries where the same constellation of factors related to women retaining control over resources and having productive employment/investment opportunities, come together as in our sample. These include Nigeria as a whole, Ghana, Gambia, Uganda and Burkino Faso. All else equal, we might expect muted (unintended) consequences on fertility dynamics from the offer of high-valued cash transfers to pregnant women in those contexts. At the other extreme, contexts such as Ethiopia and Mozambique, women lack agency and labor market opportunities. These are locations where our results suggest more caution when using cash transfers to promote early childhood development.

Contribution Our analysis spans three literatures and provides contributions to each: on the design of cash transfer policies in low-income settings, on cash transfers for early childhood development, and on the design of social assistance and household fertility, a literature hitherto mostly based in high-income countries with advanced welfare benefit programs.

On cash transfer policies, we build on work examining potential unintended consequences of the income effects provided by cash transfers. Prominent examples studied are disincentive effects on labor supply [Blattman *et al.* 2014, the studies cited in Banerjee *et al.* 2017, Banerjee *et al.* 2020] or price distortions [Egger *et al.* 2019, Attanasio and Pastorino 2020, Cunha *et al.* 2019, Filmer *et al.* 2021]. Most of these concerns have been shown not be founded, at least outside of particular settings. Our approach ties concerns arising through income effects on eligibles, to the hitherto separate literature on the distortionary effects of conditionality in transfer policies [Bryan *et al.* 2021 and references therein]. Conditionality has been shown to impact behaviors of non-eligibles, especially in the context of interventions in Latin America where conditionality is linked to (un)employment status [Garganta and Gasparini 2015]. We bridge these literatures by providing novel evidence on a new and important margin of unintended consequence, relevant when eligibility for transfers hinges on being pregnant or with young children: fertility.

In so doing, we reveal a critical constellation of factors related to household decision making and women's investment opportunities that eliminate any such unintended consequences. Our approach adds to a nascent literature in economics that has emphasized the role of intrahousehold decision making over fertility, and our granular experimental evidence helps provide insights on the distinct roles that husbands and wives play in such decisions [Rasul 2008, Ashraf *et al.* 2014, Doepke and Kindermann 2019, Rossi 2019, Ashraf *et al.* 2020a]. We thus extend the standard concern raised that transfers to households might crowd in/out informal transfers they receive, to the notion that the nature of intrahousehold decision making and economic opportunities available are also critical to understand the aggregate impacts of social assistance programs to households.

On cash transfers for early childhood development, the literature has mostly focused on evaluating impacts on child outcomes such as birthweight and anthropometrics [Sridhar and Duffield 2006, Manley *et al.* 2013, Caeyers *et al.* 2016, Levere *et al.* 2016, Fernald *et al.* 2017, Ahmed *et al.* 2019]. Far less is known about the nature of unintended fertility effects on non-targeted households. This is in part because most studies combine pregnant and non-pregnant women or samples are based on the age of children in the household [Maluccio and Flores 2004, Levere *et al.* 2016, Fernald *et al.* 2017, Ahmed *et al.* 2019, Field and Maffioli 2019]. Most broadly, the concern as been raised that the majority of early childhood evaluations lack detailed analysis of maternal outcomes beyond those related to parenting practices [Evans *et al.* 2021]. We bring new and important margins of evidence to this body of work, using a sample purposefully designed to study endogenous fertility responses to the offer of large-scale and long-term cash transfers.

Of course there is an extensive literature examining fertility responses to more general cash transfer programs, where eligibility either does not depend on pregnancy, eligibility is 'closed' and fixed by initial household characteristics, or conditionality depends on factors unrelated to pregnancy (such as work or schooling requirements for school-age children) [Arenas *et al.* 2015, Handa *et al.* 2017, Baird *et al.* 2019]. Our study is distinct from this large body of literature: rather than examine income effects on fertility, a concern stemming back to Malthus [1840], we study whether and how cash transfers that are effectively conditional on pregnancy, induce endogenous responses in fertility timing among those not eligible for the transfers to begin with.

The closest papers to ours based on experimental estimates are Stecklov *et al.* [2007] and Palermo *et al.* [2016]. Stecklov *et al.* [2007] examine unintended fertility impacts of the RPS conditional cash transfer program in Honduras, where conditionality is based on the HAZ of first grade children in the household, and is 'open' in the sense that non-eligibles can become eligible for the cash if they later meet the conditionality criteria. They find positive impacts on fertility of between 2 and 4pp, on those initially non-eligible, two years after the program initiation. Palermo *et al.* [2016] study a child grant program in Zambia – designed to target early childhood development – over a four year horizon. In contrast, they find null impacts on fertility.

We go far beyond the analyses of Stecklov *et al.* [2007] and Palermo *et al.* [2016], that both focus primarily on overall fertility. We study both *tempo* and *quantum* margins of fertility, child and maternal outcomes, and how cash transfers are actually allocated, across consumption, saving and investment purposes. This allows to paint a rich picture of the marginal costs and benefits of changing birth timing or total fertility in response to offer of cash transfers. We ultimately shed light on the fundamentals of household decision making processes that explain why null fertility responses are found in our context, and the constellation of factors that could lead to different fertility responses in other contexts in Sub Saharan Africa (so explaining the null effects found in Zambia by Palermo *et al.* [2016]).

Finally, on welfare programs and fertility choices, there is an established literature from high-income countries on how cash benefits – including childcare support, tax credits and paid leave – impact fertility. These are some of the most popular pro-natal policies in OECD countries with advanced programs for social assistance. Strands of this literature have used data from the US and other high-income countries to study the impacts of tax incentives on fertility [Moffitt 1998, Rosenzweig 1999, Baughman and Dickert-Conlin 2003, Kearney 2004], the impacts of the wider benefits system on fertility [Hotz *et al.* 1997, Hoynes 1997, Grogger and Bronars 2001, Laroque and Salanie 2004, Keane and Wolpin 2007, Milligan 2005, Kearney 2008, Brewer *et al.* 2012, Cohen *et al.* 2013, González 2013, Aizer *et al.* 2020], and how parental leave impacts fertility [Lalive and Zweimüller 2009, Malkova 2018, Raute 2019]. The evidence is somewhat mixed. Many of these studies focus on completed fertility (the *quantum* effect), they are identified from natural experiments exploiting cross-jurisdiction variation in taxes/benefits, and sometimes the marginal financial incentive is hard to isolate (or needs to be simulated rather than measured). Our research design and primary data collection allows us to improve on all three margins in our work.

Section 2 describes our context, intervention and data. Section 3 presents results on the number and timing of births, and selection of households into pregnancy. Section 4 considers the marginal costs of accelerated fertility, embodied in impacts on child and maternal outcomes. Section 5 explains the null impacts on fertility dynamics by examining the pattern of marginal benefits across household members. Section 6 speculates on the external validity of our findings. Section 7 concludes. Additional results are in the Appendix.

2 Intervention

2.1 Context and Program Design

Context Our evaluation sample covers 210 villages in two states in North West Nigeria: Zamfara and Jigawa. Households are almost entirely of Hausa ethnicity and Muslim religion, and are structured around a male household head. Women are often secluded during daytime but engage in income-generating activities such as petty trading or rearing livestock. Hence it is commonplace for women to be generating resources flows into the household.

Information The intervention we study is called the Child Development Grant Programme (CDGP) and is provided at the village level.[3] The CDGP intervention comprises a bundle of: (i) information to mothers and fathers on recommended practices related to pregnancy and infant feeding; (ii) unconditional cash transfers to mothers once they are verified to be pregnant, using an on-the-spot urine test in the presence of a female community volunteer [Sharp *et al.* 2018]. Any household can enrol onto the program and thus receive cash transfers conditional on a verified pregnancy. This targeting criteria is simple, transparent and open in the sense that households can enter the program at any time while the CDGP is taking enrollees.

Information messages are tailored to the context and were developed by our intervention partners: Save the Children (SC) and Action Against Hunger (AAH).[4] Panel A of Table A1 shows the messages disseminated. Panel B details how messages were delivered through various low-intensity channels including posters, radio, Friday preaching/Islamic school teachers, health talks, food demonstrations, and pre-recorded SMS/voice messages. High intensity channels (offered in addition, in a random subset of villages as discussed below) include small group parenting sessions and one-to-one counselling in home visits.

Households face no incentive to change the timing of fertility in order to acquire the information supplied because messages are publicly disseminated in treated villages and so non-excludable. Figure A2 shows recall rates for the eight messages provided, measured at midline, so two years after the program started. The top panel shows recall rates at midline for (i) women pregnant at baseline; (ii) women that became pregnant between baseline and midline; (iii) women not pregnant at midline. The bottom panel repeats this for husbands. We see that for most messages, there are no significant differences in recall between the three groups.

This is in sharp contrast to being able to receive cash transfers offered by the intervention: these can *only* begin to be given to a women once she is verified to be pregnant. For the remainder

[3] In rural Nigeria, communities are normally subdivided into traditional wards, that represent a community subdivision made up of a separate cluster of households. In cases where communities were too large to serve as sampling units, we randomly selected one ward in the community. 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.

[4] The program is implemented in Zamfara by SC, and in Jigawa by AAH. The exact same program is implemented by both NGOs, using common modalities.

of the analysis we therefore focus on the cash transfer component of the intervention as potentially driving unintended consequences on fertility dynamics among those not immediately eligible for the intervention when it starts at baseline.

Cash Transfers 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: at baseline, it corresponds to 12% of household monthly earnings, 85% of women's monthly earnings, or 26% of monthly food expenditures. Given transfers are provided monthly from when a pregnancy is verified until the child turns 24 months old, the cumulative value of transfers can be upwards of $500, corresponding to nearly three months of the combined earnings of husbands and wives.

Once eligibility was established, thumbprints were taken to be used when transfers were disbursed. Women are eligible to receive transfers for one child only – the child *in utero* when eligibility is established. 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 women remain eligible for a later child. Finally, for polygamous households, multiple wives in the same household can be eligible.

Delivery of Cash Transfers The intervention is designed to be scalable within Nigeria and transportable to other contexts with low state capacity [Visram *et al.* 2018]. 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).[5] 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.[6]

[5] It was originally planned for mobile the phones to be used for payments, but this proved infeasible. In practice, phones were used to alert beneficiaries about payment dates.

[6] There are not many reports of beneficiaries raising security concerns when travelling back from pay points. The program leveraged local knowledge in avoiding areas with security concerns, and security assessments were conducted before each payment cycle.

2.2 Data Collection

Study Timeline Figure 1 shows the study timeline from June 2014. In villages where the CDGP was implemented, there was a one week period of intense mobilization, involving local and religious leaders. Given the logistical issues described above, cash transfers began being disseminated in August 2014, some three to four months after registration took place and information provision began. Even with this delay, any non-pregnant women at baseline could thus expect transfers to be received as soon as any later pregnancy was verified.

We conducted a village census covering 38,803 women aged 12-49 in the 210 villages. 83% of them were married, 53% were in polygamous relationships. The census identified households with a pregnant woman, and so immediately eligible for the program, as well as women aged 12-49 who were not pregnant at baseline. 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.[7]

Enrolment into the program was announced to be in place for four years from baseline. Just prior to the end of our study period in 2018 it was formally announced that no new enrolment would take place from April 2019. Our study timeline is thus long enough to consider behavioral responses among households: (i) that decide to shift forward their birth timing in order to start receiving transfers earlier than they otherwise would have if they maintained the fertility path they had planned pre-intervention; (ii) towards the end of the study period and lifetime of enrolment into the CDGP, who otherwise risk losing access to the cash transfers altogether.

Surveys and Sampling We drew a sample of 26 women per village. Each was interviewed separately from their husband on modules covering knowledge related to pregnancy and infant nutrition, consumption, savings, asset ownership/investments, and labor activities. This allows us to build a detailed picture of how cash transfers are utilized, including within household transfers and hence how the marginal benefits of the cash transfers are spread across spouses and children.

Among not pregnant women identified from 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.[8] We selected not pregnant women with the highest predicted probability. Panel A of Figure A3 shows the distribution of predicted probabilities (using a linear probability or probit model). Panel B shows the *ex post* likelihood of actually giving birth by midline. We actually find a very weak gradient between the predicted and actual likelihood of giving birth. As such,

[7] Households are defined as individuals residing in the same dwelling unit with common cooking/eating arrangements. Polygamous husbands can rotate dwellings where they sleep, as wives are not always in the same dwelling. The lean season in rural North West Nigeria runs from March to October. This coincides with the baseline and endline surveys, but this timing does not differ between treatment and control villages.

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

the sample is more representative of all not pregnant women at baseline, and thus we only exploit the underlying predicted probabilities for some robustness checks.

Our baseline sample covers 1743 not pregnant women and their husbands. We implemented a mother-child specific survey to collect outcomes for the first child conceived and born after baseline. We refer to this as the 'target' child: this is the child for whom the cash transfer is provided for (until they are 24 months old). At baseline we also collect information about a randomly selected child aged 0-60 months – an older sibling of the target child. Among our sample of not pregnant women at baseline: (i) 44% had no target child by midline; (ii) 56% had one additional child. We surveyed 973 (1330) target children at midline (endline), and 1565 older siblings at baseline.[9]

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 merge these treatment arms throughout. We divided villages into three tranches, with random assignment of villages taking place within each tranche.

By the four-year endline, 20% of women not pregnant at baseline had attrited. Table A2 shows that attrition is: (i) uncorrelated to treatment; (ii) almost perfectly predicted by whether the village is insecure (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 .569. Columns 4 to 6 show similar levels and correlates of attrition for husbands, the older sibling of the target child (that is tracked between baseline and midline), and the target child (that is tracked from midline to endline).[10]

2.3 Descriptives

Baseline Balance Table 1 shows the samples are well balanced between treatment and controls on characteristics of households, women and their husbands, and of the older sibling of the target child. Panel A highlights the economic vulnerability of households: monthly food expenditures are $83 (recall the monthly transfer is $22). 41% of all expenditures are on food. 72% of households 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. The lean season in rural North West Nigeria runs from March to October: this is when food is in short supply and, absent

[9] It is also possible that between midline and endline, another child is born after the target child (their younger sibling). We collected information on these children at endline, but they are not the focus of this study.

[10] At midline, enumerators were unable to visit 18 villages due to security risks, and this rose to 28 villages at endline. Village insecurity is itself not correlated to treatment, but largely relates to various types of man made shock that the village experiences such as curfews, violence, or widespread migration into the village.

financial resources, households have to sometimes resort to extreme coping strategies. All else equal, the combination of anticipated shortages of food in the lean season and highly imperfect credit markets, provides households with an economic incentive to bring forward the timing of birth to start receipt of the high-valued cash transfers provided by the intervention each month. The marginal benefits of so doing are likely immediate and noticeable. Households demand for short term liquidity has been experimentally documented in other low-income contexts, including 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].

Panels B and C show baseline characteristics of women and their husbands. Despite women being age 25 on average, they have 4.6 children aged below 18 and resident with them. Yet they are far from completing their fertility cycle: DHS data suggests women in North West Nigeria have on average six surviving children at the end of their fertility cycle. Almost half our sample are in polygamous marriages with far older husbands, who are on average aged 41. Both spouses have low levels of human capital: 20% of women are literate, 40% of husbands are literate. The majority of women are engaged in some labor market activity (68%). Their modal activity is to rear/tend or sell household livestock (37%). Among men, over 80% have farming household land as their main labor activity.

Panel D shows outcomes for the older sibling of the target child, who is aged 28 months at baseline. Only half of them 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. On health outcomes, mothers report 29% of children having diarrhoea in the two weeks prior to baseline. Birth spacing is short: the average gap between the old child and their immediately older sibling is 29 months, so lower than the WHO recommendation of at least 33 months, but still above a realistic biological lower threshold. Finally, the low level of household resources and knowledge all 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 so at risk of not reaching their developmental potential.

Knowledge of the Intervention and Take-up Table 2 documents knowledge women and their husbands have over the details of the intervention as measured at midline, two years after the intervention started. To reiterate, our sample covers households with a not pregnant woman in them at baseline, and so not immediately eligible for the CDGP at baseline. We see that such women and their husbands in treated villages are nearly all aware of the CDGP, and the vast majority can correctly provide its eligibility criteria.

We next show enrolment rates for the cash transfer component of the CDGP using administrative records. Panel C shows that in treated villages, 74% of households with women that were not pregnant at baseline received payments by midline; 86% had taken up by endline. This reiterates the high fertility rates in this setting, so that the majority of women give birth at least once in the four-year study period. We also note a small degree of take-up in control villages (11%): this is likely due to cross-village registrations and implementation errors. Panel D focuses on the timing and intensity of payments: on average, women start receiving cash transfers in their six 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.

2.4 Empirical Method

We estimate the following specification when considering outcomes of mothers because these are all measured at baseline:

$$Y_{ivt} = \gamma_M T_v.(1 - E_t) + \gamma_E T_v.E_t + \alpha Y_{iv,t=0} + \beta X_{iv,t=0} + \eta_d + \lambda_s + \omega E_t + \varepsilon_{ivt}. \tag{1}$$

Y_{ivt} is the outcome, T_v is a treatment indicator, E_t is an endline wave indicator, $(1 - E_t)$ is a midline wave indicator, $X_{iv,t=0}$ are baseline controls, η_d is a district (LGA) fixed effect, λ_s are randomization strata (the tranches used given rolling enrolment into the program). ε_{ivt} is clustered by village given this is the level of randomization. For outcomes related to the target child who did not exist at baseline, we obviously cannot control for $Y_{iv,t=0} = 0$ (the baseline outcome).[11]

(γ_M, γ_E) are the coefficients of interest: the two- and four-year intent-to-treat impacts of the offer of cash transfers from the CDGP intervention among households in which the women was not pregnant at baseline. In the Appendix we show the robustness of the estimated coefficients of interest, within each survey wave, to multiple hypothesis testing, adjusting p-values using a stepwise testing procedure [Romano and Wolf 2005].

3 Fertility Dynamics

3.1 Contraceptive Use

In our study context, households almost entirely lack access to contraceptives. Taking a comparable sample of women in the same states in Nigeria from the 2013 DHS Nigeria 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. On husbands use of condoms, 54% of women report not being able to ask a partner to use condom. The main circumstance women report being justified in being able to ask their husband to use a condom is

[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, a dummy for polygamous relationships, and the gender of the target child.

if he has an STI (61%).[12]

It was because of this almost non-existent use of contraceptives reported in the DHS data that we decided not to collect any information on contraceptive use in our surveys. We did collect information on knowledge of contraceptives, and the majority of women are aware of forms of contraception. The information messages provided in the CDGP did not relate to family planning and we find no evidence of knowledge over contraceptives being impacted by the intervention.[13]

3.2 Quantum Effects

We begin with the extensive margin measure of whether any child is born, or the number of children born by some fixed time: a *quantum* effect. This is the behavioral margin that much of the literature studying cash benefits and fertility has focused on. The results are in Panel A of Table 3. Column 1 shows the mean outcome in controls between baseline and midline. Columns 2 and 3 show ITT impacts only conditioning on randomization and district strata. We find no significant ITT impact on the likelihood any child is born, or the number of children born, either between baseline and midline, or between midline and endline. In short, the offer of high-valued and long-lasting cash transfers induces no significant changes in the extensive margin of fertility over the four-year study period, among women that were not pregnant at baseline. Columns 4 and 5 show a similar pattern of null results controlling for the full set of baseline covariates.

Recall that at its initiation, the CDGP is announced to be in place for four years. The lack of fertility response between baseline and the two-year midline emphasizes that households expecting to give birth sometime over the four year study period do not adjust their fertility timing to obtain cash transfers earlier than they otherwise would. The lack of fertility response between midline and the four-year endline suggests a null response even among households that risk losing receipt of the cash transfer altogether, given the program is due to end shortly after endline.

These null results are precise. For example on the number of children born, we note the standard error from the conditional model in Column 4 is less than 6% of the standard deviation of the outcome in controls. The 95% confidence intervals rule out a treatment effect on the number of children born larger than .094 between baseline and midline, and larger than .066 between midline and endline.[14]

A concern is that treatment effects might be attenuated by including women that are very unlikely to give birth in our study period. We address this in Table A3 by showing the baseline null result is robust to: (i) weighting the sample based on the likelihood the women was predicted

[12] The Demographic and Health Surveys (DHS) data are nationally representative surveys which are comparable across countries. All female respondents are of child bearing age (15-49). We use the micro-level Nigeria data which has 2400 respondents from Jigawa and Zamfara combined.

[13] More precisely, we asked about knowledge of contraceptive methods (given usage was so low). At baseline 65% of women reported knowing some contraceptive method. Our two-year treatment effect on this is .005 (with a standard error of .024).

[14] We also find no impact of the offer of cash transfers on child mortality in these time periods.

to become pregnant by midline using a probit model; (ii) weighting using OLS weights; (iii) restricting the sample to those with below median OLS weights.

3.3 Tempo Effects

We now use a finer approach to examine *tempo* effects on the exact timing of fertility. Figure 2A 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 time interval, 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.

Note first that in control villages, the KM curve declines smoothly: around 30% of women do not give birth before midline, and 10% do not so by endline. There is scope for birth timing to be nudged forward by comparable households in treated villages, enrol into the program, gain liquidity and thus ease economic hardships. We see that women in treated villages are marginally more likely to give birth *earlier* in time, but this effect is relatively small. To test if two KM curves are different from each other at any given month since baseline, we run an OLS linear probability regression where being pregnant is the outcome for every month from baseline. We shade those months where the estimates differ significantly from each other at the 10% significance level. We test the joint significance of treatment from all these regressions. We report the p-value on the Chi-squared test of equality and reject the null ($p = .053$). The difference in birth timing is most pronounced 24 to 28 months after baseline (so just after the midline survey).

Panel B shows all these results to be robust to conditioning on baseline covariates.[15]

This evidence reconciles with the earlier extensive margin results in Panel A of Table 3: at midline there is no divergence in KM estimates, and at endline, the two survival functions converge again so the overall likelihood of becoming pregnant by these specific times is no different in treatment and control villages. Towards the end of the study period, we again see no evidence of any acceleration into pregnancy, so even among those households at risk of altogether losing access to the cumulative value of cash transfers – over $400 – because the intervention is due to stop new enrolments after four years.

The minor changes in fertility dynamics still have important implications for birth spacing, that as shown earlier in Figure A1 and Table 1, are relatively short to begin with in our study context. Panel B of Table 3 shows, in line with the KM estimates, there is a reduction of about .4 months in birth spacing between the target child and their immediately older sibling, although

[15] To do so, for each month since baseline we calculate the probability of pregnancy for an index individual, where we take the index individual as the one who is at the median of covariates controlled for. The median (rather than the mean) is used as many of these controls are binary. In practice, this fits separate Cox regression models for treated and control groups using the variables we adjust for as covariates. The separately estimated baseline survivor functions are then retrieved.

this is not statistically significant. Birth intervals of less than 24 months are considered to place the child at higher 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]. Focusing on this threshold, we see there is a significant increase of almost 5pp in such short birth spacing (in controls, 12.3% of target child births are within 24 months of their older sibling). So there is some heaping at this extreme threshold due to the responses of 5% of treated households.

The qualitative pattern of results on birth spacing are robust to: (i) conditioning on covariates (right hand side of Table 3); (ii) weighting the sample in alternative ways (Panel B of Table A3).

3.4 Selection Effects

As a final step we examine whether there are changes in the composition of households becoming pregnant over time – a selection effect. This has not been studied much in the earlier literature, but is not ruled out by the earlier null *quantum* effects and muted *tempo* effects: there could be a compositional change in which households become pregnant even if aggregate fertility patterns remain unchanged between treatment and controls.

Column 1 of Table 4 shows baseline characteristics of households, women, their husbands and older sibling of the target child in control villages. We then show treatment effects on that characteristic among woman that became pregnant between: (i) baseline and midline (Column 2); (ii) midline and endline (Column 3). In both time frames and for each and every characteristic – of the household, the woman, her husband, or their older child – we find no evidence of significant selection effects of treatment. Although some of these null impacts are imprecise, in most cases the estimated standard error is less than 15% of the standard deviation of the same characteristic among controls at baseline.

To examine dynamic selection, we check whether KM survival estimates differ along each characteristic, between treated and control households. We take each characteristic and define a dummy equal to one if the household is above the median on that characteristic and zero otherwise (or analogously for dummy variables). We then estimate the difference-in-difference in survival functions. Column 4 in Table 3 reports the p-value on the null that this difference-in-difference is zero, and the sign of this estimate in each case. There are only two characteristics for which we find statistical evidence of dynamic selection effects: (i) whether the woman can read and write at baseline; (ii) whether she is rearing livestock as her primary income generating activity at baseline. The sign of both difference-in-differences is negative, indicating that women that have such characteristics significantly *delay* selection into pregnancy relative to controls.

We return to these results in Section 5, as they relate to the nature of household decision making and investment opportunities available to women, that helps explain why there are muted impacts on fertility dynamics despite the offer of high-valued and long-lasting cash transfers.

4 Child and Mother Outcomes

The results so far address the question on whether the availability of high-valued unconditional cash transfers to promote early childhood development has unintended impacts on fertility dynamics among those not immediately eligible but who can later enrol into the program conditional on a verified pregnancy. We find null impacts of the intervention on the number, timing and selection into birth, despite households having strong economic incentives to accelerate birth timing or to have an additional child. To understand why this is so, we build evidence on the pattern of marginal costs and benefits across household members from offer of cash transfers.

To begin with we examine how the intervention impacts outcomes of the target child and their mother, conditional on any *quantum*, *tempo* and selection effects. We compare outcomes to those in Carneiro *et al.* [2021] that were based on women pregnant at baseline and so that did not endogenously select to enrol into the intervention. This comparison helps confirm whether the muted impacts on fertility dynamics translate into equally sized treatment effects for child outcomes across the two samples, or whether these reveal potentially important unobservables that at least determine selection into fertility (if not the incidence and timing of fertility choices for treated households) and correlate to child and maternal outcomes.

4.1 Child Stunting and Health

We first examine the outcome of 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 3 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 5 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 10.7pp, corresponding to a 22% reduction. Both results are robust to multiple hypothesis testing (Table A4).

At the extreme tail of the distribution, there is no significant reduction in the incidence of extreme stunting. These impacts become less precise at endline, but we cannot reject equality of the two- and four-year impacts on each dimension. This might be so because as documented earlier, changes in fertility dynamics cause significantly shorter birth spacing with a heaping of children within a 24 month birth window of their immediately older sibling (Table 3). This heaping could impact the extreme tail of the distribution of HAZ scores.

Columns 4 to 6 then repeat the analysis but for the target child born to women that were pregnant at baseline. We see a very similar pattern of results, with endline impacts being more precisely estimated in this sample (that is twice as large). Within each survey period, we cannot reject equality of impacts across the two samples.

One concern is that the earlier *tempo* effect results suggested small changes in birth timing (that were not robustly different between treatment and controls). As a result, at midline the target child is 12 months old in control villages, and 11 months old in treated villages; at endline the target child is 35 (34) months old in control (treated) villages. To establish these small differences in age do not drive any of these results for height, Table A5 shows these estimates are robust to non-parametrically controlling for the age of the target child at midline and endline.

Panel B of Table 5 shows treatment effect estimates on health-related outcomes for the target child, again split between those born to women not pregnant at baseline (Columns 1 to 3) and those born to women pregnant at baseline (Columns 4 to 6). We first consider an index of health outcomes made up of two dummy variable components: whether the new child has not been ill in the last month, and whether the target child had diarrhea in the two weeks prior to the survey. Among target children born to women not pregnant at baseline, this index significantly improves by $.16\sigma$ at midline, and by $.21\sigma$ at endline (where this latter result remains robust to multiple hypothesis testing). The remaining rows show impacts on each index component: there is a reduction in illness/injury for new children of 6pp at midline, and this reduction improves slightly to 9.6pp (corresponding to a 15% fall relative to controls) by endline. The incidence of diarrhea among the target child also falls dramatically: at midline there is a reduction of 5.8pp, and this falls further by 8.4pp (corresponding to a 24% fall) at endline.

The right hand side of Panel B shows a very comparable set of effects in the sample of target children born to women that were pregnant at baseline. Again, within each survey period, we cannot reject equality of impacts across the two samples.

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 impacts on children of mothers that were not pregnant at baseline.

4.2 Dynamic Treatment Effects

We noted above that for women not pregnant at baseline, the average age of their target child is 11 (34) months at midline (endline). Outcomes for the target children of women that were pregnant at baseline are measured when they are aged around 24 (48) months at midline and endline respectively. Hence we can collate the estimates across samples to show dynamic treatment effects on children approximately aged 1, 2, 3 and 4 years. Figure 4 shows these dynamic treatment effects for each outcome. We see they are monotonic across these four age bands: the intervention has reduced impacts on HAZ over time, while there are accumulating impacts on child health.

In short, the trajectory of child development is very similar between the children born to women that were, or were not, pregnant at baseline. An immediate implication is that the IRR to the program, in terms of child outcomes, is likely to be similar to that calculated in Carneiro *et al.* [2021] for the sample of households in which women were already verified to be pregnant at baseline. Under plausible assumptions that was calculated to be at least 10%. Given high fertility rates in this setting, these gains accrue not just to the cross section of women that happen to pregnant at baseline, but also to a further 90% of women aged 12-49 at baseline that become pregnant over the four year window of the program.

The only clearly non-monotonic outcome is extreme stunting. Again this might be because of the small share of treated households that change fertility timing to cause a heaping of birth spacing of the target child within a 24 month birth window of their immediately older sibling.

Taken together, the results confirm the muted impacts on fertility dynamics translate into equally sized treatment effects for child outcomes across the two samples. The largely monotonic results across samples help ameliorate the concern that unobservables drive the exact timing and selection of treated households into fertility among those that were not pregnant at baseline.

4.3 Maternal Outcomes

The other key marginal cost of bringing forward birth timing to the offer of unconditional cash transfers is to worsen maternal health. By examining such outcomes we address a broader concern that the majority of early childhood evaluations lack detailed analysis of maternal outcomes beyond those related to parenting practices [Evans *et al.* 2021]. Moreover, it has been documented in a similar setting that husbands might be ill-informed about maternal mortality and morbidity compared to their wives [Ashraf *et al.* 2020a]. If fathers have some say over birth timing and are less aware of these marginal costs of bringing forward births, this could tilt the balance towards such endogenous responses to the offer of cash transfers in our setting.

The results are in Table 6, where Column 1 shows baseline outcomes for women not pregnant in control villages. Columns 2 and 3 show that on weight, height, BMI and an indicator for malnourishment, there is no evidence of worsening health outcomes for mothers, either at the two-year midline or four-year endline. The right hand side of the table replicates findings from Carneiro *et al.* [2021] to show the same outcomes for mothers pregnant at baseline and so exogenously eligible for the cash transfers. We see that the pattern of null impacts is the same in both samples. Within each survey period, we cannot reject equality of impacts across the two samples.

This further underpins the earlier results that there are precise null endogenous fertility responses to the intervention, so that there are no hidden marginal costs in terms of maternal health from any response of non pregnant women to the intervention offer of cash transfers.

5 Explaining the Lack of Fertility Responses

Our study context is one in which the presence of a new intervention offering high-scale and long-lasting unconditional cash transfers to pregnant women, there are strong economic motivations for households to bring forward the timing of births to obtain receipt of those resources, and help alleviate economic hardships faced. Yet we find very muted impacts on total fertility, the timing of births or selection into births. All these results are buttressed by pattern of child and maternal outcomes being in line with those found for mothers already pregnant at baseline and so who could not endogenously enrol into the program.

To begin explaining this null response, we first note that given the near complete absence of contraceptive availability in this region, women have less agency over fertility timing relative to contexts in which contraceptives (especially those controlled by women) are available. Moreover, Nigeria is a patriarchal society where bridal prices are still common, so wives are 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, as is well documented by work in demography and gender studies [Caldwell and Caldwell 1987, 1988, Odimegwu and Adenini 2014, 2015].[16]

Furthermore, the primary marginal costs of accelerating birth timing are likely borne by mothers through risks in pregnancy and to their later health. Hence given the wedge between wives – who primarily bear the marginal costs of shifts in birth timing or total fertility – and their husbands, who largely drive fertility decisions given the absence of contraceptives, we dig deeper into the nature of intrahousehold decision making and women's agency, to establish how the marginal benefits of receiving cash transfers are distributed among household members.

5.1 Decision Making

To begin with, we consider decision making rights over other dimensions beyond fertility. Using a comparable sample as our evaluation sample, the 2013 DHS Nigeria data 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 show there is one key dimension on which wives retain agency: how to

[16] The evidence on the causal impact of a greater availability of contraceptives on fertility is mixed. A small set of experimental studies, discussed in Ashraf *et al.* [2014], provide mixed results: increasing access to contraception is found to have a significant impact on decreasing fertility in four countries: 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 a sample of DHS countries in Sub Saharan Africa. They reach the conclusion that eliminating supply constraints would have limited impacts on contraceptive use, while policies targeting husbands beliefs and preferences could be more effective.

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.

We designed our baseline survey module on household decision to follow the structure of the DHS questions. Our data largely corroborates this sharp contrast in agency women have over earnings versus other dimensions of household decision making. Figure 5 shows data from our baseline survey on household decision making over various dimensions. On each dimension, the top bar shows women's report over who in the household makes decisions, and the bottom bar shows the corresponding report of husbands (where recall, we interviewed spouses separately). In line with DHS data, our survey reveals women have weak decision making rights over major household purchases, which food to grow, and what food to buy. As the top half of Figure 5 shows, along all three dimensions, the majority of women report their husband decides alone (or in consultation with them); very few women ever make these decisions alone. These findings are confirmed in both interviews to the wife and their husband.

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. Recall, that as shown in Table 1, in our sample women have high labor force participation rates, and so the majority will normally be bringing a flow of 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 self-employment for women, versus if the 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 CDGP), to the receipt of NGN1000.

As shown in the lower part of Figure 5, in each of these scenarios: (i) the majority of women reported they would decide *alone* how to spend the additional resources; (ii) this was irrespective of how the additional resources were generated (either through labor earnings, or as a gift to the wife) or the amount of additional earnings; (iii) husband reports were near identical to their wives.

5.1.1 Decision Making over Unconditional Cash Transfers

Building on this insight that both spouses report women controlling any 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 5 was asked at midline in treated villages: we asked each spouse about who actually got to decide how the unconditional cash transfer from the CDGP program was spent. Even more so than the hypothetical positive earnings shocks asked about, in over 70% 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.

Taken together, the results in Figure 5 show household decision making is well characterized within a separate spheres framework [Lundberg and Pollak 1993, Browning *et al.* 2010]: while nearly no women have access to family planning and men thus can have relatively more control over the timing of fertility, the reverse is true for the control of the cash transfers received from the intervention. This creates a wedge between the private returns to the receipt of cash transfer between women and their husbands.

In order to understand exactly what men gain on the margin from the receipt of cash transfers within their household, we document how women use these resources.

5.2 How Unconditional Cash Transfers are Spent

Descriptives Figure 6 presents descriptive evidence on how, at midline and endline, women report spending the unconditional cash transfers. It is again instructive to show comparable results among women that were pregnant at baseline, and so did not endogenously choose to become eligible for cash transfers. Throughout we see little difference between the spending patterns of these two groups. The majority of women report spending the cash on food for the household (that slightly declines between midline and endline), for food for children (that rises between midline and endline as expected as children age), and on assets (that rise between midline and endline).

In the remainder of this Subsection, we present ITT estimates, using the specification described in (1), of how additional resources are spent in terms of consumption, savings, and own business investments. This builds up a granular picture of the marginal gains to husbands from bringing forward the timing of births in order for the additional resources from the intervention to flow into their household. To be clear, husbands might well value gains of improved child outcomes to the exact same extent as their wives. However, given the majority of households give birth at some point over the four year period the intervention is announced to be in place for, these gains would accrue irrespective of whether birth timing was brought forward or the household retained its original fertility path when the program is first announced. Hence we focus on other material gains resulting from the receipt of the cash transfers. Some of these gains will be shared with other household members – such as building a buffer stock of savings, but others will be closer to *private* gains to the husband in terms of expenditures on certain adult goods or investments into the husband's labor activities.

Consumption, Adult Goods and Food Security Table 7 presents treatment effect estimates on how the additional resources are used, focusing first on consumption and savings. The first row on Panel A examines two- and 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 \$26 (31%) over controls at midline, and this is largely sustained at endline where they are \$19 higher than controls. Some of these increases will of course benefit husbands. However, given that household sizes are over seven at baseline (before 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.

The next two rows examine this further by focusing on impacts on the extensive margin of non-food expenditures for male-orientated adult goods: tobacco/cigarettes, and newspapers/magazines. We see that neither type of adult good expenditure increases (indeed expenditures on cigarettes/tobacco might even be less likely to occur in treated households). The next row shows no significant rise in monthly non-food expenditures overall, although these estimates are noisier than for food expenditures.[17]

Panel B then examines outcomes related to food security: we see that at baseline in controls, 30% of households report not having enough food to eat in the past year. In this region the agricultural cycle includes a lean season from March to October in which households face food shortages, and have to resort to extreme coping strategies. We see that in treated households food security improves dramatically by endline, with a 14pp fall in households not reporting having enough food to eat (a result robust to multiple hypothesis testing). 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 cash transfers from the intervention.

Saving As described in Carneiro *et al.* [2021], there are two substantive reasons why the cash transfers can impact outcomes beyond consumption. First, the value of the cash transfer was calibrated by our intervention partners to correspond to the cost of a diverse household diet. However, at baseline controls already spend $83 per month on food suggesting a potential crowd out of resources for other uses. Second, the fact that transfers are provided each month until the target child is 24 months old, provides women with a more stable flow of resources than is available from most labor activities in these rural economies. The magnitude and certainty of transfers opens up the possibility that they are used for saving and investment purposes, over and above impacts on consumption.

We begin to examine these other outcomes in Panel C of Table 7. This presents ITT estimates on household savings. By endline there is a significant rise in the stock of household savings of $56 (22%), equivalent to 2.5 months of cash transfers (a result robust to multiple hypothesis testing). This benefits husbands in allowing their household to build resilience to shocks.

Taken together, these results show the gains to husbands from their wife receiving the cash transfer are either relatively small (in terms of food and non-food expenditures), concentrated during the lean season (in terms of food security at endline), or accrue some time into the future

[17] Non-food expenditure is obtained combining the following sources: (i) a 7-day expenditure recall of consumables (e.g. matches, fuel); (ii) a 30-day recall of other items (e.g. toiletries, utensils, household items, health expenditure); (iii) a 12-month recall of major expenses (e.g. school fees, ceremony costs, remittances); (iv) expenditure on durables using a 12-month recall of expenditure on assets the household owns (e.g. TV set, wheelbarrow, mattress). The top 1% of total expenditure amounts are trimmed.

(in terms of accumulated savings by endline).

Labor Activities and Investment into Own Businesses Cash transfers can also be used to advance the labor activities of wives and husbands. We narrow in on impacts specific to each partner to shed light on the private gains to men from their wife being in receipt of cash transfers, beyond the impacts on food expenditure, food security and savings shown above.

These results are shown in Table 8. Panel A first focuses on labor activities each spouse is engaged in. Women's labor force participation rates are high to begin with (72% at baseline in controls). For treated women this rises by a further 9pp by endline (a result robust to multiple hypothesis testing). The right hand side shows much smaller impacts on husband's labor supply.

On the types of labor activity engaged in, at baseline the most common activities for women are being self-employed in rearing livestock or petty trading. For women, by endline we see significant increases in any form of self-employment, and in petty trading activities. These are large magnitudes of impact of engagement in these forms of self-employment, corresponding to 23% and 32% increases respectively. It is perhaps unsurprising that these impacts are not immediate or measured at the two-year midline given our sample of women are not pregnant at baseline, but the majority give birth to the target child by midline (Figure 2).

The right hand side of Table 8 shows no corresponding impact on the labor activities of husbands: they are mostly engaged in farming their own land and the incidence of this does not change post-intervention.

In Panel B we examine investments into the self-employment business of wives and their husbands separately. Given women's main form of self-employment in this context, we consider business investment and livestock ownership. We see that both types of productive investment significantly increase by endline. On inputs into women's businesses, these increase significantly by $20/month at endline. We see no corresponding increase in expenditures on inputs for the husband's business. Regarding livestock ownership, women's ownership of any animal increases by 6.9pp (9%) at midline, and by 10.2pp (13%) 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.[18]

The final row of Table 8 combines all 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 $17.3 (corresponding to a 19% 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, either at midline or by endline.

[18] As in Carneiro *et al.* [2021] we note that in this setting livestock are owned by an individual (not a household). Examining livestock ownership of women that were not-pregnant at baseline we find: (i) significant increases in household ownership of livestock are largely driven by livestock owned by treated women (and not another household member); (ii) the ITT point estimate on them owning any given animal is higher at endline than midline.

In summary, there are sharply divergent gains across spouses in terms of their labor activities, own business assets, and earnings as a result of the household becoming eligible for the cash transfers from the CDGP intervention. These cash transfers are provided directly to women, and then data suggests women retain the agency to spend these resources to push forward transformative changes in their labor supply, self-employment and the accumulation of productive assets for their own businesses.

This pattern of results on the use of cash transfers is also borne out in the parallel qualitative analysis of the CDGP conducted in our study context [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 transfers. The largest recorded in the qualitative sample was NGN1100, so less than one third of one month's transfer.

A growing literature on micro-entrepreneurship in developing countries has shown that male but not female-operated enterprises benefit from unconditional cash transfers. A number of explanations have been put forward for this: (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]. None of these appear to apply in our context because our evidence suggests women critically: (i) retain control of resources; (ii) have profitable business investments to undertake; (iii) opt not to transfer resources to their husbands for their consumption of adult goods or own business investments.

5.3 Separate Spheres

A granular picture emerges where the private gains to husbands from bringing forward the timing of births are limited to any share of food consumption that accrues to them, and enabling their households to be more food secure and build a buffer stock of savings by the four-year endline. They do not gain in terms of expenditures on adult goods, their labor activities or associated business investments. In short, the muted response to the offer of high-valued and long-lasting cash transfers can follow from the fact that ultimately, men have weak private material incentives to shift forward the timing of births. This is despite households residing in extreme poverty, and so having short term need for financial liquidity and longer term needs for economic resources.

These patterns of behavior are inconsistent with a unitary model of household decision making, and more closely mimic separate spheres decision making between spouses. However, we make no claim as to whether households reach the frontier of their production possibility set. For example, in households where the marginal return to the husband's own business investment was higher than for their wife, on face value it would seem that household surplus would have been maximized if the wife transferred resources to him to make those investments. However, this calculation implicitly holds constant family size. If husbands anticipate such *ex post* transfers, then in treated villages they have greater *ex ante* incentives to endogenously bring forward birth timing, and it is thus unclear whether this leaves wives better off overall given they bear the cost in terms of maternal health as a result of shortened birth spacing.

We reiterate that in this context, women's labor force participation rates are high, with most women engaged in self-employment activities, and many having productive investments they can make in their own business. This links back to the earlier results on dynamic selection (Table 4) where we found evidence that wives that significantly *delayed* selection into pregnancy in treated villages relative to controls had two characteristics: (i) they could read and write; (ii) they were rearing livestock as a primary income generating activity at baseline. Both results are consistent with those women having productive investments available to them (either because they are skilled or want to build their livestock asset base). Hence, all else equal, in these households, the private incentive of husband's to bring forward the timing of fertility is relatively weak.

We underpin this interpretation by considering within-sample variation in fertility responses. We consider women that at baseline have a constellation of three characteristics: (i) they report having full autonomy over how to spend any exogenous increase in resources they bring into the household; (ii) they are self-employed; (iii) and, they have opportunities for business investment (either because they own livestock or have business assets). We consider households in which, at baseline, wives have all three of these characteristics versus households in which wives do not have all three conditions satisfied. Under separate spheres household decision making, we view the former group of households as ones in which husbands have relatively weaker incentives to bring forward birth timing, and the latter as ones in which husbands have relatively stronger incentives to do so – either because they can appropriate the resources from the CDGP intervention (condition (i)), or their wife transfers resources to them because she lacks investment opportunities of her own (conditions (ii) and (iii)). With this classification, 414 (815) households are ones in which the husband has weak (strong) incentives. Naturally those households classified as having stronger incentives are more heterogenous because they include those for whom none, one or two of conditions (i) to (iii) are satisfied.

The results on *quantum* effects on fertility in these subsamples are shown in Panel A of Table 9. Columns 1 to 3 show that in households in which the husband has weaker incentives, we continue to find null impacts on whether any child is born between baseline and the two-year midline, and between midline and the four-year endline. Similarly, we continue to find no statistically significant impact on the number of children born over these time periods. These null impacts remain precise. For example on the number of children born, we note the standard error from Column 3 is less than 13% of the standard deviation of the outcome in controls. The 95% confidence intervals rule out a treatment effect on the number of children born larger than .151 between baseline and midline, and larger than .176 between midline and endline.

These results are in contrast to those for households in which the husband faces relatively stronger incentives to bring forward the timing of births, shown in Columns 4 to 6. We see that among this group, treated households are 9.2pp significantly more likely to have any children born between baseline and midline (corresponding to a 16% increase over comparable households in controls), and to have .08 more children born over the same period. As is intuitive, this increase in fertility occurs within the first two years of the program being introduced.

Pooling specifications and testing for differences across households where husbands have weaker and stronger incentives, the differential impact on the number of children born between baseline and midline is statistically different ($p = .091$).

On the *tempo* effects or exact timing of births, Figure 7 shows unconditional Kaplan-Meier (KM) estimates for the likelihood of not becoming pregnant at any given month since baseline, split by these subsamples. Panel A shows that among those with relatively weaker incentives, there is no differential pattern of birth timing between treatment and controls in any month since baseline. In contrast, Panel B shows the timing of fertility among those with relatively stronger incentives to bring forward birth timing, in treatment and control villages. We see that those in treated villages significantly bring forward birth timing, with the most pronounced effects occurring around the two-year midline, and just prior to the end of the intervention at endline (where we shade in gray the months since baseline where the KM curves significantly differ).

Completing the analysis of *tempo* effects, Panel B of Table 9 shows that within the subsample of households where husbands have weaker incentives to bring forward birth timing,we also see null impacts on birth spacing consistent with our core findings. For those with stronger incentives, we do not however see any significant reductions in birth spacing between the target child and older sibling suggesting that even within this group those that endogenously respond to the offer of cash transfers do so in a way so that any marginal costs of bringing forward birth timing do not worsen child or maternal outcomes.

5.4 Alternative Explanations

We consider four alternative explanations for the null impacts on fertility. Each is unrelated to separate spheres household decision making and thus any distinct role for husbands and wives.

Birth Spacing Households might be unable to bring forward birth timing if they are already close to the lower biological bound on birth spacing. As shown in Table 1, at baseline our sample households are characterized by having high fertility rates and short birth intervals. However, we also see that the median birth spacing between the new child and their immediately older sibling is 31 months, so it remains feasible for households to shorten further the birth interval between the target child and their older sibling. Households are also not at the end of their fertility cycle. As Table 1 shows, the average number of children at baseline is four, while DHS data reported in Figure A1 suggests total fertility rates are close to six in this region. To probe this further, Columns 1 and 2 of Table A6 show the estimated incentive effects when we split the sample into women with above/below the median number of children at baseline: we find a very similar pattern of null impacts on the number and timing of births in both.

Polygamy 40% of our sample households are in polygamous marriages (Table 1). Null effects might then be driven by husbands having small returns for bringing forward birth timing for any given wife. Two factors mitigate against this interpretation. First, in polygamous marriages any wife that is pregnant is eligible for the CDGP transfers (not just one wife). Second, Columns 3 and 4 of Table A6 estimate effects on fertility by polygamous and non-polygamous households: we find very similar (and equally muted) responses in fertility dynamics.

Policy Certainty A third explanation is that households expect the offer of cash transfers from the intervention to be available for the foreseeable future. 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. Two pieces of evidence mitigate against this interpretation.

First, this is an environment in which state or NGO capacity to reliably deliver programs is generally limited, and households face huge uncertainties from aggregate shocks, both natural and man made. This applies to all five LGAs in the states of Zamfara and Jigawa in our evaluation sample. We described earlier how village insecurity led to our enumerators being unable to reach some villages. Figure A4 shows that even in the secure villages that form our sample, nearly all have been hit by a natural shock in the year prior to midline or endline (such as crop damage caused by weather or pests, floods and droughts), and with the majority having been hit by a man made shock (such as 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 the timing of births.

Second, the CDGP intervention did not last forever and this was known to households. As the timeline in Figure 1 shows, the program stopped enrolling pregnant mothers in April 2019, around six months after our endline survey (women who were verified to be pregnant before that deadline would still be eligible for the full sequence of monthly cash transfers until their child turned 24 months old). This announcement was made just before our endline survey began to be rolled out, allowing us to measure whether households were aware of the termination. Although knowledge was far from perfect, we find that at endline, among women in treated villages: (i) 68% correctly knew how long they could expect to receive cash transfers for; (ii) 67% correctly stated the year in which the CDGP would end; (iii) 59% heard this information directly from community volunteers charged to disseminate the information component of the intervention.

Moreover, the KM estimates in Figure 2 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, 9.4% of households in treated villages that have a non-pregnant woman in them at baseline, still have not had a child by endline (and this does not differ to controls where it is 9.9%).[19]

Time Lags A final explanation for the null 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 some lag between when a household might first decide to respond to the offer of cash transfers and their actual receipt. Null fertility impacts could then result because this time lag causes any marginal benefit to bring forward birth timing to be discounted. However, the fact that as shown in Table 9 there does exist a subset of households that do respond on the fertility margin suggests such time lags are not sufficient to explain our core results in the sample overall.

As a further check on this, we consider responses among households with above/below median savings at baseline, as a potential proxy for those with higher/lower discount rates: the results in Columns 9 to 12 of Table A6 show both sets of households have largely null response in terms of total fertility (Panel A). Those with low savings at baseline appear to be among those contributing to a heaping of birth spacing below 24 months for the target child (Panel B).

We return to this issue of time lags below when considering policy implications of our findings.

6 Discussion

We conclude by discussing the external validity of our findings, and their policy implications for the next generation of interventions designed to use cash transfers to promote human capital development in early childhood in low-income contexts.

6.1 External Validity

The null effect on fertility dynamics in our study can be explained by a constellation of three factors in our context: (i) there is separate spheres decision making, 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 make intrahousehold transfers to husbands. These factors combine to imply husbands have little *private* incentive to change fertility dynamics in response to the program.

[19] We also note that the value of the cash transfer increased from NGN3500 to NGN4000 from January 2017. Again we see no evidence from Figure 2 that this marginal increase in financial incentives changed fertility dynamics.

The external validity of our findings hinge on how peculiar this constellation of factors is. We investigate this by drawing together DHS surveys from 45 middle and low-income settings to document the prevalence of these factors, as closely as can be measured in a large cross-country sample (and combining rural and urban samples). In doing so, we 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 offered to pregnant mothers. The results are in Figure 8.

Panel A examines the prevalence of separate spheres 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 being above/below each mean. Countries with separate spheres decision making as in the CDGP sample 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.

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 scatter on separate spheres decision making, we use the same color coding as in Panel A. Those 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 (9 out of 16) have separate spheres decision making (orange dots). These include Nigeria, but also Ghana, Gambia, Uganda and Burkino Faso. It is in such countries where the same constellation of factors come together as in our evaluation 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 fertility consequences will be stronger in countries where fertility and spending 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, women can best internalize any cost of bringing forward birth timing, they retain control of cash transfers and have productive investments they can make related to employment opportunities.

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 at least remains the option of husbands opting to invest cash transfers in the employment prospects of their wife. These countries include Burundi, Liberia and Tanzania. For those in the South West quadrant of Panel B, women lack 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 African, these countries include Afghanistan and Tajikistan. These are locations where our results suggest the most caution when using cash transfers to promote early childhood development.[20]

6.2 Policy Design

We end by discussing design issues to consider in the next generation of interventions using cash transfers to promote child outcomes in early life. We consider design margins related to targeting, eligibility and component bundling.

Targeting It matters *who* in the household cash transfers are provided to. Most conditional cash transfer programs in developing countries explicitly target payments to women [Fiszbein and Schady 2009]. A few studies have experimentally varied the identity of recipients in the household, with mixed findings: while some studies suggest targeting women can change consumption levels and bundles [Armand *et al.* 2020], other studies find less impact [Benhassine *et al.* 2015, Haushofer and Shapiro 2016]. Our results suggest targeting resources to women rather than husbands will be especially important for avoiding unintended fertility impacts in contexts where women control the use of any additional earnings and have productive investment opportunities available to them.

Eligibility The purpose of eligibility being linked to pregnancy is so that benefits start accruing to the child as early as possible – even while they are *in utero* as in the CDGP. However, in contexts where unintended fertility consequences might occur, policy makers could ease the concern by further separating in time when fertility decisions are made and when payments start to be received. This leads to a policy trade-off: 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, but at the same time could prevent a rush to bring forward births, and this force moves to improve children's initial outcomes such as birthweight, as well as impacting maternal health.

On component bundling, consider first information plus cash designs. In our context information was provided as a public good. Hence there was no reason to change fertility dynamics in order for households to gain from that component of the program. However, designs where information components are more targeting to specific households – say because of the desire to provide tailored information to mothers – do potentially create a second incentive for households to bring forward birth timing. Again, policy makers face a trade-off between improving child outcomes through more household-specific information delivery, versus worsening child outcomes through accelerated fertility and shorter birth intervals.

[20] Our analysis of DHS data also 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.

Component Bundling It might be natural to think of also providing family planning to women, or to empower them through the provision of bargaining and negotiation skills [Ashraf 2009, Ashraf *et al.* 2014, 2020b]. However, it is unclear what impacts this would have for the timing of births relative to the counterfactual of existing decision making rights. It might break the delicate balance of separate spheres decision making in some contexts, but in other contexts, it would allow women to internalize the costs and benefits of bringing forward the timing of births, while retaining control over earnings streams.

7 Conclusion

Huge reductions in global poverty have been achieved over the last three decades, at least until the global COVID-19 pandemic. Some part is due to the increased use of direct transfers to the poor. Such 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 aim. We have provided granular evidence on any causal distortionary effect that such conditionality – based on verified pregnancy – provides on fertility behaviors. In a context where households face extreme economic hardships, we show such unintended consequences are avoided – even with the offer of high-valued cash transfers – where there exists a constellation of three factors: (i) women retain full control over the use of transfers they bring into their household; (ii) women have available productive investment opportunities in their own businesses; (iii) they choose to transfer few resources to husbands.

Given these forces, that are not unique to our context, an important implication is that the Child Development Grant Program we study has widespread benefits across households: both the cross section of those exogenously enrolled into the program because they contain a pregnant woman at baseline, and the far larger group of households that endogenously enrol through pregnancy over the lifetime of the intervention. By setting out a precise mechanism driving the lack of distortionary effects, we are able to speculate on parts of the developing world where similar programs can be expected to generate high population-wide returns, and also settings where more caution needs to be taken in the design of cash transfers promoting early childhood development.

Finally, our analysis highlights that in studying interactions between households and governments and the aggregate effects of social assistance, it is critical to understand how spouses interact with each other, and the nature of household decision making. This goes beyond the standard concern raised that transfers to households might crowd in/out informal transfers received, and can have far reaching consequences for other economic policies and the design of social assistance programs and the welfare state more broadly.

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Table 1: 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	
Monthly food expenditure (in $USD)	83.0	78.3	[.889]
	{118}	{116}	
Share of monthly expenditures on food (%)	41.2	39.1	[.206]
	{.278}	{26.5}	
Living on less than $1.90/ day (extreme poverty) (%)	72.1	73.1	[.728]
Did not have enough food in past year (%)	16.7	14.4	[.667]
Panel B: Wife			
Observations	574		
Age (years)	24.4	23.8	[.104]
	{6.57}	{5.89}	
Number of children aged 0-18	4.24	4.20	[.893]
	{3.01}	{2.99}	
Polygamous relationship (%)	40.6	38.7	[.629]
Can read and write at least one language (%)	21.4	23.0	[.734]
Paid/unpaid work in past year (%)	68.6	70.9	[.321]
Rearing/ tending or selling household livestock (%)	36.8	32.8	[.876]
Total monthly earnings (in $USD)	21.2	23.1	[.253]
	{49.9}	{48.4}	
Panel C: Husband			
Observations	453		
Age (years)	41.0	40.4	[.251]
	{8.87}	{8.24}	
Total monthly earnings (in $USD)	161	146	[.776]
	{327}	{359}	
Farming household's land (%)	80.8	80.1	[.732]
Panel D: Older Sibling			
Observations	518	1178	
Age (months)	28.4	26.5	[.624]
	{15.4}	{16.1}	
Exclusively breastfed (%)	50.5	47.5	[.559]
Had diarrhea in past 2 weeks (%)	29.1	30.7	[.793]
Birth spacing from previous child (months)	29.3	28.7	[.322]
	{12.5}	{11.0}	
Stunted (HAZ<-2) (%)	59.1	60.3	[.959]

Notes: Panels A,B, C and D report data from the household surveys. In Panel A, food expenditure is based on 7-day recall for food items. Total expenditure is based on: food expenditure, a 7-day recall for consumable items (e.g. petrol, fuel, phone credit, cigarettes), a 30-day recall for items such as toiletries and clothing and an annual recall for larger items such as dowry, funerals and school expenses as well as durables such as mattress, table motorbike, which we then convert to a monthly expenditure measure. 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. In Panels B and C, 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 D, the 'child put to breast immediately' variable is a dummy for the child having been put to the breast in the first 30 minutes after birth. The appropriately breastfed variable is a dummy indicating age-appropriate breastfeeding according to WHO guidelines [WHO 2008, i.e. exclusive breastfeeding up to the age of 6 months and complementary breastfeeding from 6 to 23 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]. Columns 1 and 2 report the mean (and standard deviation for continuous variables) of the variable in the Control group, 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 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 2: Program Knowledge 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 Program				
Yes, there is such a program in this village	20.5	94.8	21.8	95.6
No, there is not such a program in this village	79.1	4.77	76.9	4.44
Don't know	.40	.49	1.30	0
Panel B: Eligibility Criteria				
Exact Answer	21.4	19.6	24.2	15
Generally appropriate answer	62.2	69.9	59.1	66.3
Inappropriate answer	3.1	3.7	4.5	3.92
Don't know	13.3	6.78	12.1	15
Panel C: Receipt of Any Cash Transfer				
Ever received transfer by midline	7.52	73.7		
Ever received transfer by endline	11.1	86.5		
Panel D: Timing of First Transfer				
Age of new child (*in utero*) at first payment (months)		-2.3		
		{9.53}		
During pregnancy (%)		58.6		
Number of payments 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 for sampled women's knowledge of the program. Columns 3 and 4 show the corresponding means for husbands. Panels C and D use 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. Columns 1 to 2 report the mean (and standard deviation for continuous variables) of the variable in the Control group and the treatment groups respectively. All monetary amounts are converted from Nigerian Naira to PPP US dollars at the 2014 rate.

Table 3: Quantum and Tempo Effects

Sample: Non-pregnant women at baseline (N=1743)

Column 1: Standard deviation in braces

Columns 2-5: Standard errors in parentheses clustered by village

	Unconditional			Conditional	
	(1) Control Mean between Baseline and Midline	(2) ITT Between Baseline and Midline	(3) ITT Between Midline and Endline	(4) ITT Between Baseline and Midline	(5) ITT Between Midline and Endline
Panel A: Fertility					
Any child born (%)	61.8	3.12	.985	2.72	.531
		(3.03)	(2.68)	(2.90)	(2.59)
Number of children born	.715	.041	.010	.038	.008
	{.515}	(.029)	(.031)	(.028)	(.029)
Panel B: Birth Spacing					
Birth spacing between target child and their older sibling (months)	34	-0.395		-.779	
	{11.8}	(.756)		(.738)	
Birth spacing between target child and their older sibling <= 24 months (%)	12.3	4.87**		4.99**	
		(2.06)		(2.03)	

Notes: Significance levels: * (10%), ** (5%), ***(1%). Column 1 shows the mean (and standard deviation for continuous outcomes in braces) value in Control households at Midline, for outcomes since Baseline. Column 2 reports ITT estimates at Midline, and Column 3 reports ITT estimates at Endline. These are estimated using OLS, controlling for just LGA and randomization tranche fixed effects, hence the unconditional specification. Column 4 reports ITT estimates at Midline, and Column 5 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, hence the conditional specification. Standard errors are clustered at the village level throughout.

Table 4: Selection Effect

Sample: Households with non-pregnant women at baseline (N=1743)
Means, standard deviation in braces, p-values in brackets
Columns 2 and 3 present treatment effect estimates in each subsample

	(1) Control, Baseline	(2) Gave Birth Between Baseline and Midline	(3) Gave Birth Between Midline and Endline	(4) Interaction of Treatment and X	
				P-value	Sign
Panel A: Household					
Number of children aged 0-18	4.24	.095	.199	[.328]	POS
	{3.01}	(.228)	(.345)		
Monthly food expenditure ($USD)	83.0	3.82	8.21	[.774]	NEG
	{118}	(8.69)	(11.8)		
Panel B: Women					
Age (years)	24.4	.122	-.029	[.431]	POS
	{6.57}	(.360)	(.544)		
Can read and write at least one language (%)	21.4	.109	3.42	[.060]	NEG**
		(3.20)	(5.16)		
Polygamous relationship (%)	40.6	-1.13	5.79	[.179]	POS
		(3.57)	(5.35)		
Paid/unpaid work in past year (%)	68.6	-1.62	6.61	[.328]	NEG
		(4.03)	(4.87)		
Rearing/ tending or selling household livestock (%)	36.8	-.760	-7.47	[.020]	NEG***
		(3.77)	(5.30)		
Knowledge index	.034	-.064	.040	[.855]	NEG
	{.961}	(.083)	(.106)		
Panel C: Husband					
Age (years)	41.0	-.223	.553	[.329]	POS
	{8.87}	(.655)	(1.03)		
Knowledge index	.029	-.055	.073	[.751]	NEG
	{.978}	(.069)	(.120)		
Panel D: Old Child					
Age (months)	28.4	-.679	.741	[.502]	NEG
	{15.4}	(.990)	(1.87)		
Stunted (HAZ<-2) (%)	59.1	1.78	-4.64	[.163]	NEG
		(3.89)	(6.00)		

Notes: In Panel A, household size is the number of people living in the household with common eating arrangements. Food expenditure is based on 7-day recall for food items. In Panel D, 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. Columns 2 and 3 present the treatment effect at baseline of different subgroups of the population. These are estimated using OLS, controlling for LGA and randomization tranche fixed effects and clustering standard errors at the village level. All monetary amounts are converted from Nigerian Naira to PPP US dollars at the 2014 rate. For binary outcomes, 'Above median' is when the value takes 1 for an individual and 'Below median' is when the value takes 0. Column 4 shows the chi-squared p-value results which test the significance of the coefficient of the interaction term of treatment and the binary outcome for above or below median in a series of OLS regressions for each month since baseline where the outcome is the probability of pregnancy. The same Controls are used as in Columns 2 and 3. The sign column indicates the most common sign of the interaction term in the series of OLS regressions.

Table 5: Target Child Outcomes

Columns 1 and 4: Standard deviation in braces

Columns 2, 3, 5 and 6: Standard errors in parentheses clustered by village

	Not pregnant women at baseline (N=1743)				Pregnant women at baseline (N=3688)					
	(1) Control, Midline	(2) ITT, Midline	(3) ITT, Endline	(2) = (3)	(4) Control, Midline	(5) ITT, Midline	(6) ITT, Endline	(5) = (6)	(2)=(5)	(3)=(6)
Panel A: Anthropometrics										
Height-for-Age (HAZ)	-1.85	.257**	.113	[.247]	-2.46	.198***	.119**	[.236]	[.754]	[.730]
	{1.42}	(.108)	(.089)		{1.30}	(.068)	(.059)			
% Stunted (HAZ < -2)	49.3	-10.7***	-3.91	[.138]	66.2	-5.22**	-4.87*	[.897]	[.257]	[.733]
		(3.65)	(3.69)			(2.43)	(2.55)			
% Severely Stunted (HAZ < -3)	20.7	-3.04	-2.71	[.940]	34.8	-4.77**	-4.21**	[.812]	[.485]	[.573]
		(2.78)	(3.54)			(2.21)	(2.15)			
Panel B: Health										
Child health outcomes index	.099	.161**	.245***	[.420]	.000	.209***	.288***	[.204]	[.536]	[.644]
	{1.04}	(.080)	(.081)		{1.00}	(.052)	(.050)			
Had illness or injury in past 30 days	62.6	-6.00*	-9.64**	[.449]	69.6	-8.53***	-12.0***	[.269]	[.526]	[.586]
		(3.21)	(3.98)			(2.36)	(2.39)			
Had diarrhea in past two weeks	34.3	-5.81*	-8.39**	[.584]	37.8	-6.90***	-9.30***	[.417]	[.669]	[.858]
		(3.53)	(3.75)			(2.21)	(2.36)			

Notes: Significance levels: * (10%), ** (5%), ***(1%). Columns 1 and 4 show the mean (and standard deviation for continuous outcomes) value in Control households at Midline. Columns 2 and 5 report ITT estimates at Midline, and Columns 3 and 6 report 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, and the gender of the new child. 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 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.

Table 6: Maternal Outcomes

Columns 1 and 4: Standard deviation in braces

Columns 2, 3, 5 and 6: Standard errors in parentheses clustered by village

	Not pregnant women at baseline (N=1743)				Pregnant women at baseline (N=3688)					
	(1) Control, Midline	(2) ITT, Midline	(3) ITT, Endline	(2) = (3)	(4) Control, Midline	(5) ITT, Midline	(6) ITT, Endline	(5) = (6)	(2)=(5)	(3)=(6)
Weight (kg)	48.3	0.253	-0.024	[0.411]	49.8	-0.382	-0.412	[0.897]	[.623]	[.670]
	{7.57}	(0.422)	(0.403)		{7.33}	(0.258)	(0.289)			
Height (cm)	156	0.280	0.144	[0.183]	157	-0.044	0.120	[0.077]	[.281]	[.840]
	{5.51}	(0.229)	(0.246)		{5.56}	(0.095)	(0.103)			
BMI	19.8	0.022	-0.052	[0.572]	20.1	-0.137	-0.181	[0.631]	[.746]	[.718]
	{2.72}	(0.130)	(0.123)		{2.63}	(0.103)	(0.114)			
Malnourished	.118	-0.013	-0.024	[0.690]	.0711	0.018	0.004	[0.301]	[.861]	[.390]
		(0.022)	(0.017)			(0.013)	(0.013)			

Notes: Significance levels: * (10%), ** (5%), ***(1%). Columns 1 and 4 show the mean (and standard deviation for continuous outcomes) value in Control households at Midline. Columns 2 and 5 report ITT estimates at Midline, and Columns 3 and 6 report 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, and the gender of the target child. Column's 1-3 report women who are not pregnant at any of the surveys. Column's 4-6 report women who are pregnant at baseline but not pregnant at any of the other surveys. 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 22cm.

Table 7: Consumption, Food Security and Saving

Sample: Households with Non-Pregnant Women at Baseline (N= 1,743)
Standard deviation in braces
Standard errors in parentheses clustered by village

	(1) Control, Midline	(2) ITT, Midline	(3) ITT, Endline	(2) = (3)
Panel A: Expenditure				
Monthly food expenditure	84.2	26.0***	18.8*	[.570]
	{121}	(9.22)	(9.99)	
Any cigarettes or tobacco	2.34	-1.54*	-.940	[.490]
		(.843)	(.572)	
Any newspapers and magazines	.234	.324	.009	[.544]
		(.326)	(.386)	
Monthly non-food expenditure	133	11.3	14.4	[.834]
	{173}	(10.3)	(10.9)	
Panel B: Food Security				
Did not have enough food in past year (%)	.303	-0.041	-0.142***	[0.003]
		(0.030)	(0.032)	
during Kaka (Mid Oct to Dec)	.0355	-0.007	-0.013	[0.698]
		(0.011)	(0.009)	
during Sanyi (Dec to Feb)	.0376	-0.011	-0.021*	[0.486]
		(0.010)	(0.012)	
during Rani (Mar to May)	.134	-0.003	-0.094***	[0.001]
		(0.021)	(0.026)	
during Damuna (Jun to Mid Oct)	.223	-0.051**	-0.132***	[0.011]
		(0.026)	(0.029)	
Panel C: Saving				
Total savings (including in kind)	255	-69.0	55.7**	[.063]
	{668}	(60.7)	(27.0)	

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 Midline, and Column 3 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. Any cigarettes or tobacco and any newspapers and magazines indicate any expenditure on these goods in a 7-day recall. Non-food expenditure is obtained combining the following sources: a 7-day expenditure recall of consumables (e.g. matches, fuel), a 30-day recall of other items (e.g. toiletries, utensils, household items, health expenditure), a 12-month recall of major expenses (e.g. school fees, ceremony costs, remittances); 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.

Table 8: Labor Supply and Self Employment, Business Investment, and Earnings by Spouse

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

Standard deviation in braces

Standard errors in parentheses clustered by village

	Wife				Husband			
	(1) Control, Baseline	(2) ITT, Midline	(3) ITT, Endline	(2) = (3)	(4) Control, Baseline	(5) ITT, Midline	(6) ITT, Endline	(5) = (6)
Panel A: Labor Supply and Self-Employment								
Any work in past year (%)	72.4	2.49	8.94***	[.101]	94.5	.287	.306	[.954]
		(2.80)	(2.59)			(.211)	(.222)	
Has business/self-employed (%)	54.1	1.52	12.5***	[.003]	45.7	-2.97	2.62	[.018]
		(2.90)	(3.05)			(2.31)	(1.96)	
Petty trading (%)	40.3	-.608	13.2***	[.000]	-	-	-	
		(3.15)	(3.44)					
Farming own land (%)	-	-	-		81.5	-.651	.218	[.367]
						(.950)	(.683)	
Panel B: Business Investment								
Monthly expenditure on business inputs	-	-	19.7***				-4.83	
			(5.57)				(4.10)	
Owning any livestock (%)	75.8	6.94***	10.2***	[.260]				
	{42.9}	(2.40)	(2.48)					
Panel C: Earnings								
Total monthly earnings	89.6	1.73	17.3***	[.141]	207	14.0	8.81	[.795]
	{164}	(9.94)	(6.50)		{339}	(18.4)	(11.6)	

Notes: Significance levels: * (10%), ** (5%), ***(1%). Column 1 reports the mean (and standard deviation for continuous outcomes) for control households at Baseline. Column 2 reports ITT estimates at Midline. Column 3 reports ITT estimates at Endline. When there are separate answers for the mother and husband to respond to (e.g. their knowledge) we also report the mean, ITT at Midline and ITT at Endline for the Husband in columns 4-6. 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. The index variables are computed using the methodology in Anderson (2008), and are standardized to have mean zero and variance one in the control group. In Panel B, work activities are defined as any paid or unpaid work, either self-employed or salaried, excluding housework and childcare. Self-employed activities are ones where payments are received directly from the client/customer (e.g. hairdresser working in her own shop) rather than from an employer. Panel C shows total earnings. There are methodological differences in how earnings were measured at Midline 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 are then constructed by summing payments and profits (for self-employed 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.

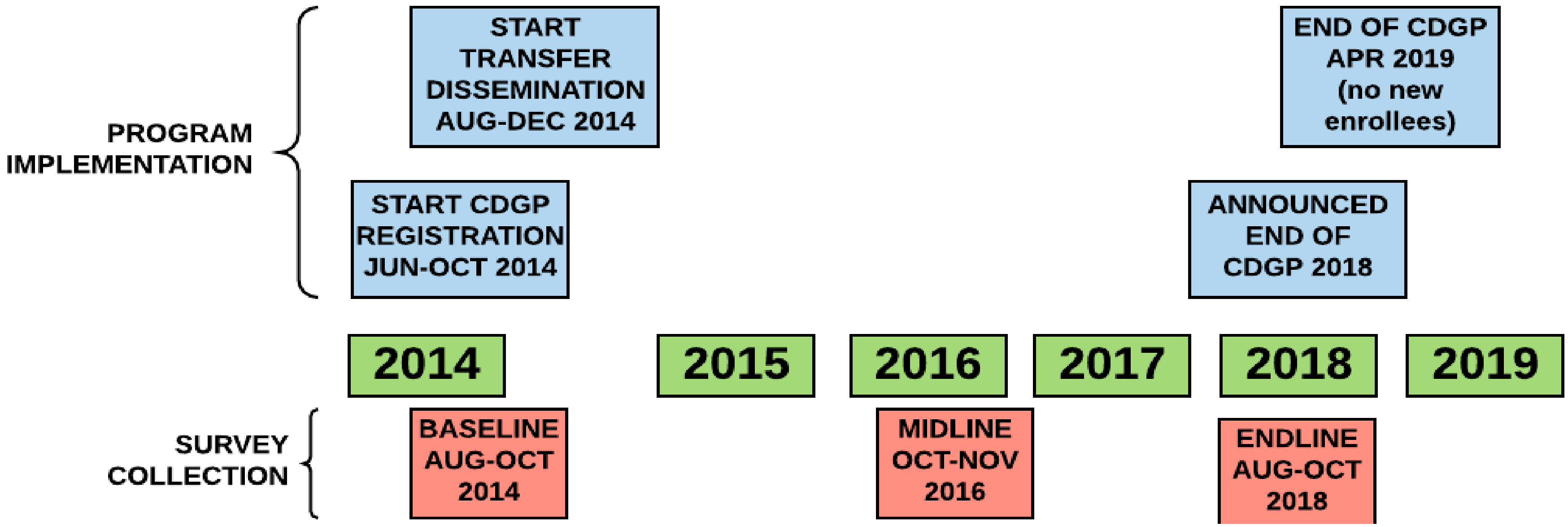
Table 9: Quantum and Tempo Effects by Husband Incentives

Sample: Non-pregnant women at baseline (N=1743)
Column 1, 4: Standard deviation in braces
Columns 2, 3, 5, 6: Standard errors in parentheses clustered by village

	Weaker Incentives for Husband			Stronger Incentives for Husband				
	(1) Control Mean between Baseline and Midline	**(2) ITT Between Baseline and Midline**	**(3) ITT Between Midline and Endline**	**(4) Control Mean between Baseline and Midline**	**(5) ITT Between Baseline and Midline**	**(6) ITT Between Midline and Endline**	**p-value: (2) = (5)**	**p-value: (3) = (6)**
Panel A: Fertility and Child Mortality								
Any child born (%)	68.1	-2.19	-.448	58.5	9.20**	3.84	[.115]	[.131]
	(46.8)	(5.61)	(5.51)	(49.4)	(4.22)	(3.95)		
Number of children born	.735	.033	.046	.706	.080*	.022	[.091]	[.267]
	(.482)	(.059)	(.065)	(.523)	(.046)	(.039)		
Panel B: Birth Spacing								
Birth spacing between target child and their older sibling (months)	35.4	-1.72		34.5	-1.29		[.993]	
	(11.8)	(1.68)		(12.0)	(1.06)			
Birth spacing between target child and their older sibling <= 24 months (%)	29.2	-1.07		28.3	-.568		[.810]	
	(3.93)	(.950)		(6.41)	(.733)			

Notes: Significance levels: * (10%), ** (5%), ***(1%). Column 1 shows the mean (and standard deviation for continuous outcomes in braces) value in Control households at Midline, for outcomes since Baseline. Column 2 reports ITT estimates at Midline, and Column 3 reports ITT estimates at Endline. These are estimated using OLS, controlling for just LGA and randomization tranche fixed effects, hence the unconditional specification. Column 4 reports ITT estimates at Midline, and Column 5 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, hence the conditional specification. Standard errors are clustered at the village level throughout.

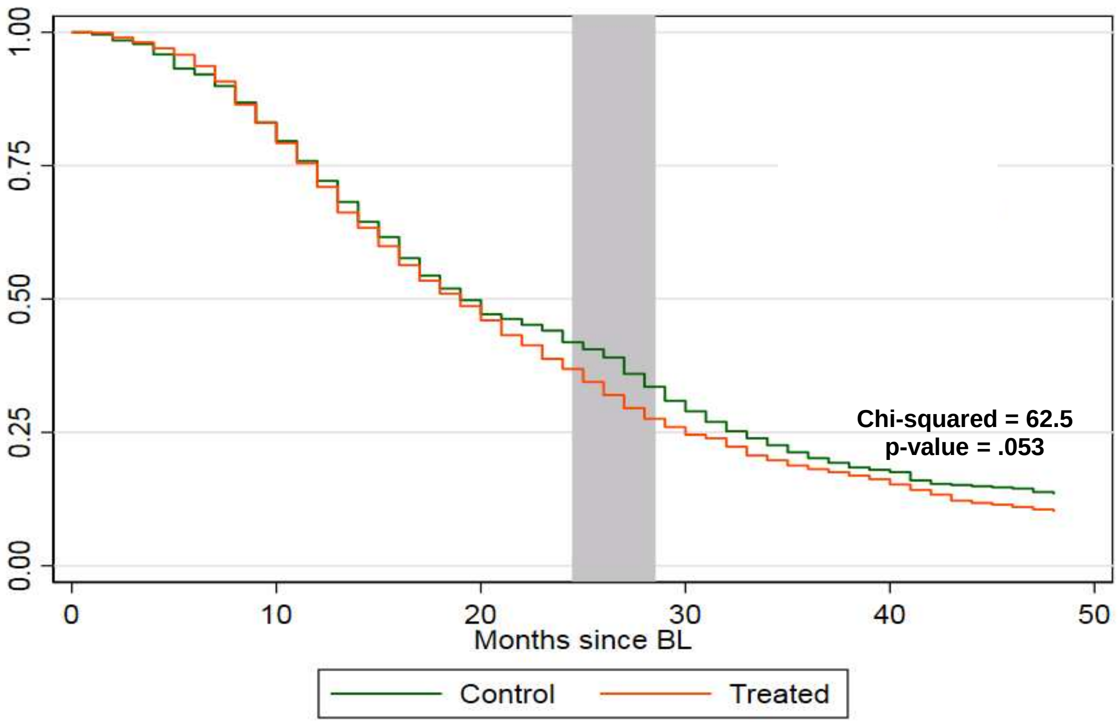
Figure 1: Timeline



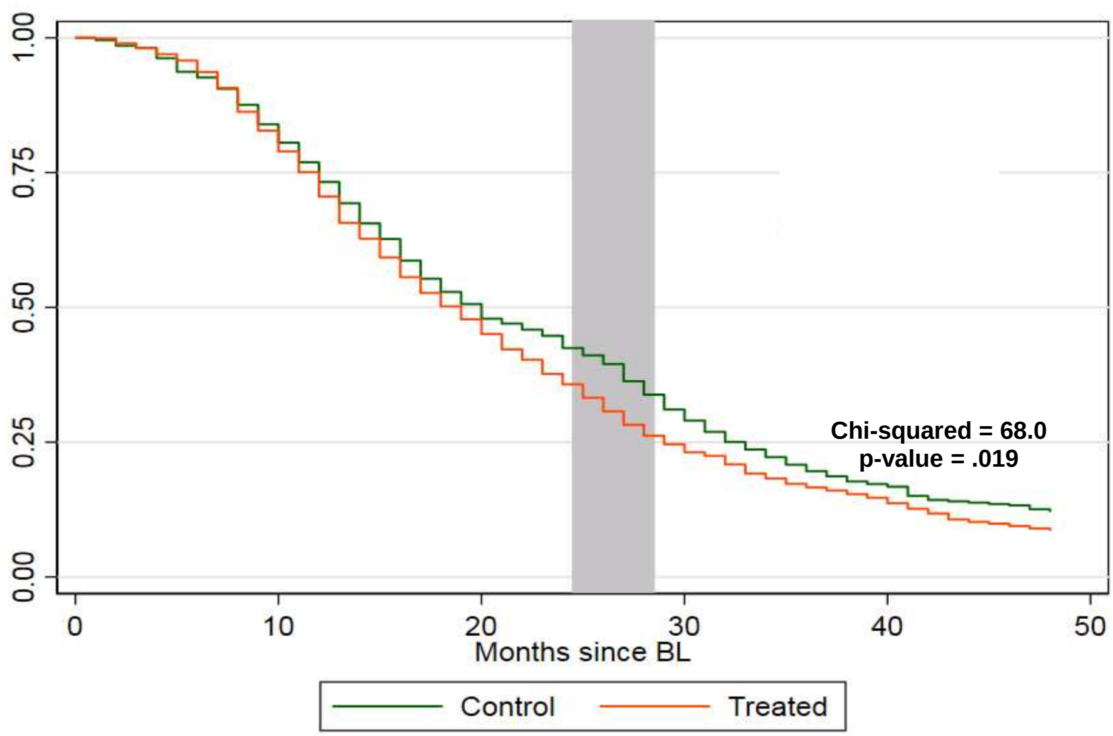
Notes: The top part of the figure shows program implementation: when the registration began, when transfers began, when the program end was announced, and when it stopped enrolling new participants. The central part of the figure shows survey collection timings: when Baseline, Midline and Endline surveys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

Figure 2: Survival Probability of Not Becoming Pregnant

A. Unconditional Survival Probability

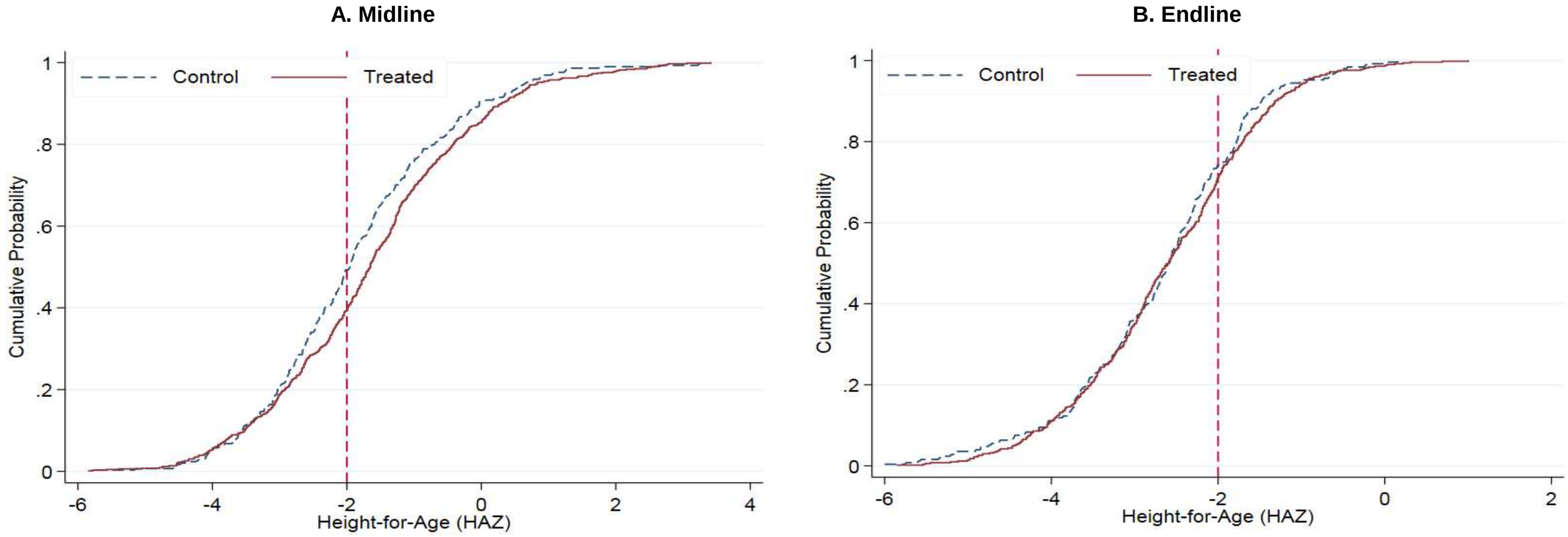


B. Adjusted at the Median for Base Covariates



Note: The number of women at risk are the number who have not become pregnant at the given months since baseline. They grey shaded area shows any individual time period that is significantly different between treatment and control group (at the 5% significance level). The Chi-squared statistic is estimated by running probit regressions of a dummy of pregnancy in each month on treatment. In these regressions we control 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 relationship. The standard errors are clustered at the village level. The p-value reported comes testing the significance on the treatment coefficient in each regression and testing with a Chi-squared statistic with 46 degrees of freedom.

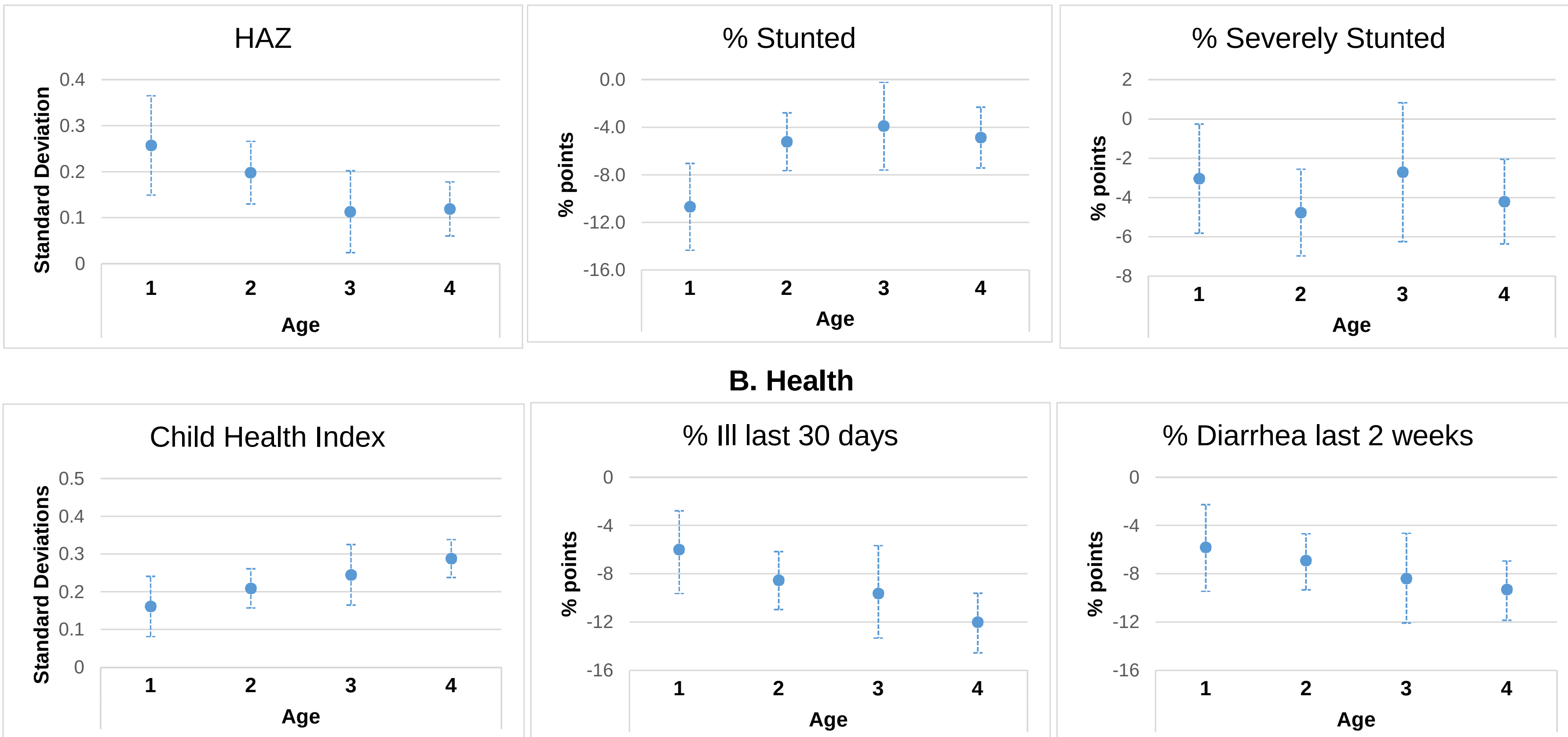
Figure 3: Distributional Impacts on Height-for-Age for the Target Child



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).

Figure 4: Dynamic Treatment Effects

A. Anthropometrics

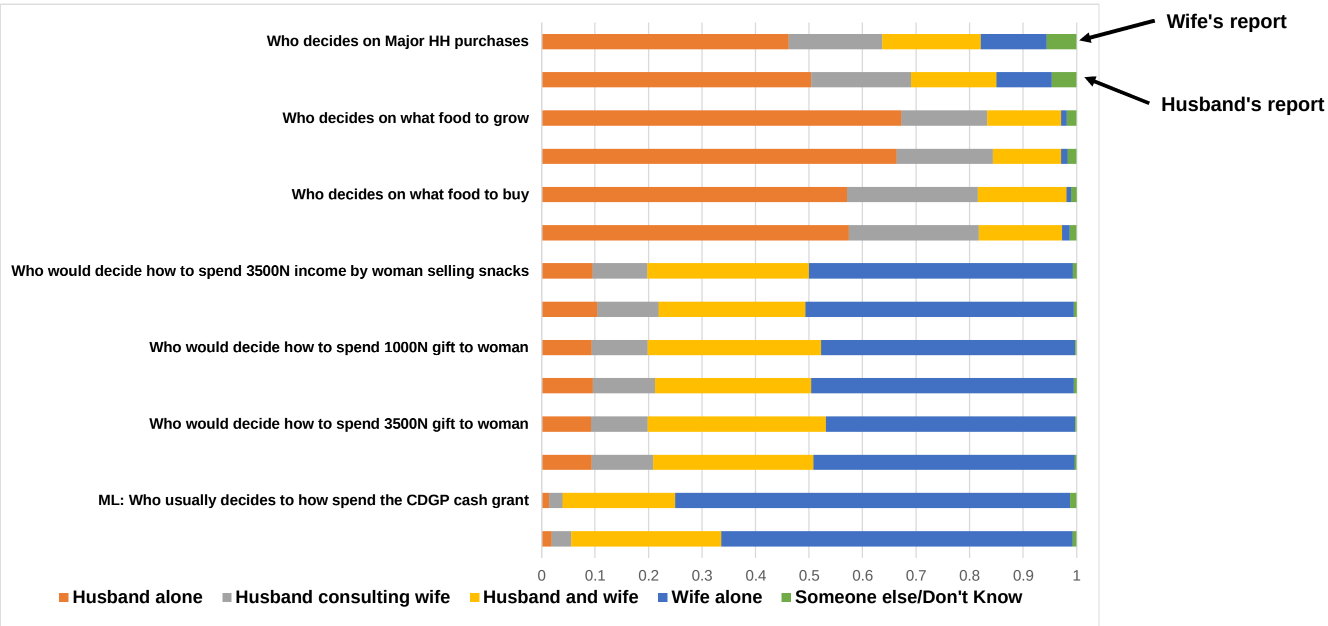


Notes: The age 1 and 3 treatment effects are estimated at midline and endline among the sample of women not pregnant at baseline. The age 2 and 4 treatment effects are estimated at midline and endline among the sample of women pregnant at baseline. These are all estimated using OLS, controlling for LGA 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, and the gender of the new child. Standard errors are clustered at the village level throughout. 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. The 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 5: Bargaining Power

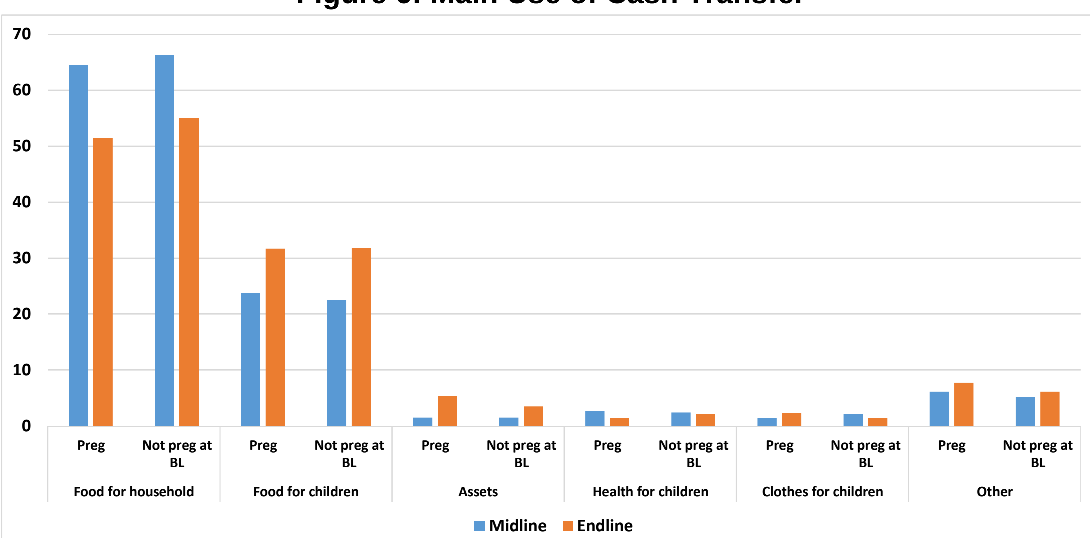
A. Couples Where Wife is Not Pregnant at Baseline (N=1743)

For each question: top bar is wife's report, lower bar is husband's report



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.

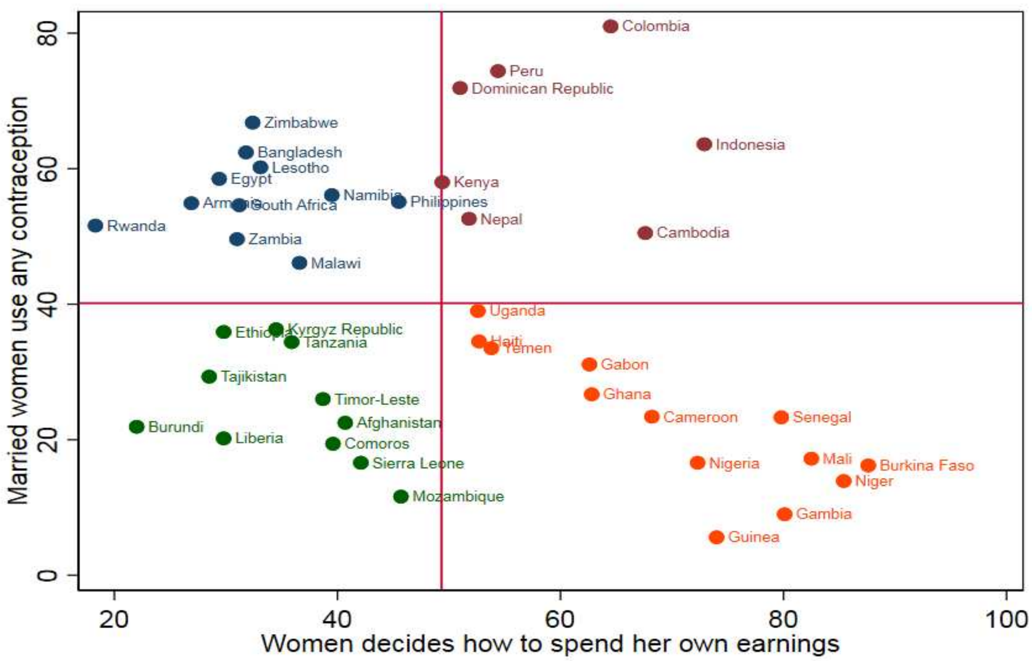
Figure 6: Main Use of Cash Transfer



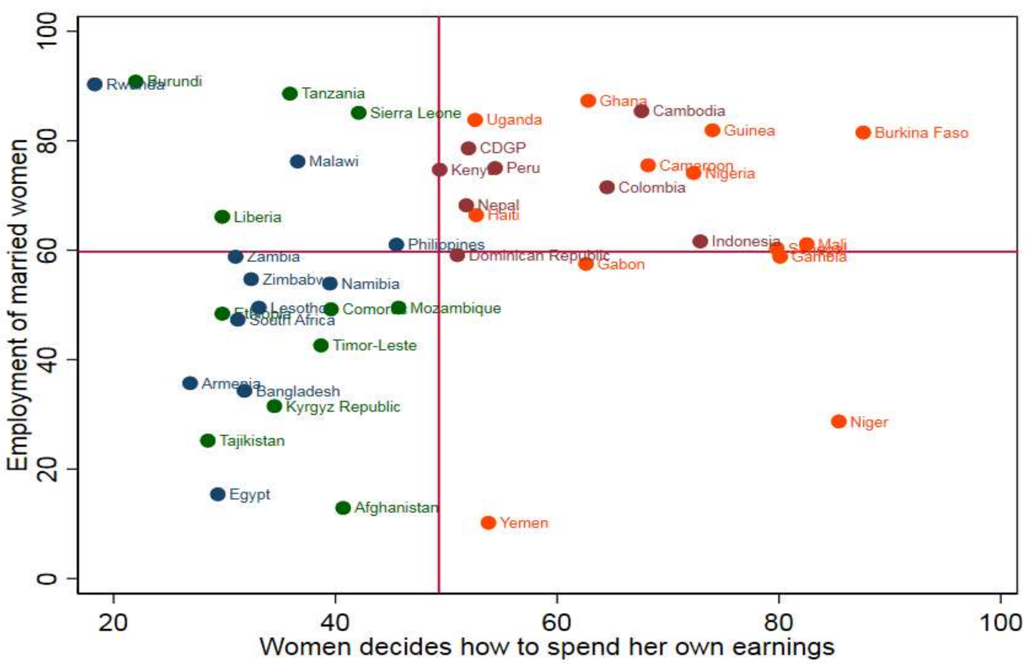
Notes: This figure is based on data from pregnant and non-pregnant women in households. It shows the reports of the main use of the CDGP cash transfer at midline and endline for all the women and the women who were not pregnant at Baseline.

Figure 7: External Validity

A. Separate Spheres Household Decision Making

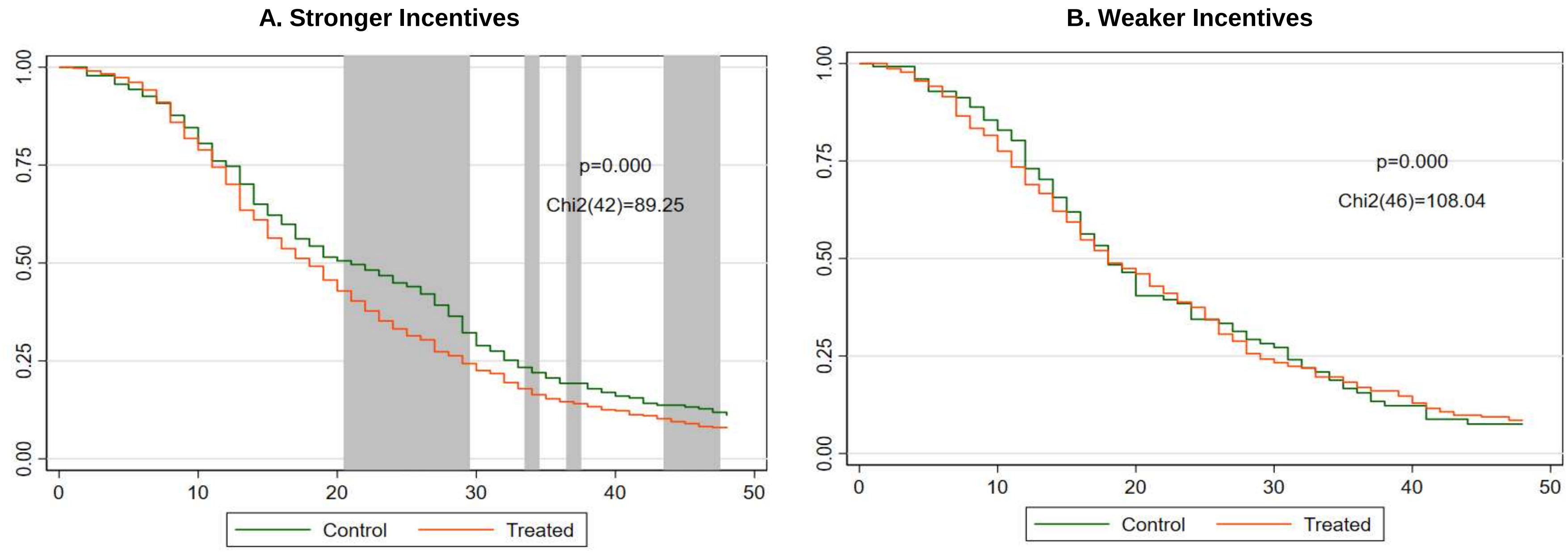


B. Women's Employment and Control of Earnings



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.

Figure 8: Survival Probability of Not Becoming Pregnant For Husbands with Strong and Weak Incentives



Notes: The number of women at risk are the number who have not become pregnant at the given months since baseline. They grey shaded area shows any individual time period that is significantly different between treatment and control group (at the 5% significance level). The Chi-squared statistic is estimated by running probit regressions of a dummy of pregnancy in each month on treatment. In these regressions we control for LGA and randomization tranche fixed effects. The standard errors are clustered at the village level. The p-value reported comes testing the significance on the treatment coefficient in each regression and testing with a Chi-squared statistic with 46 degrees of freedom.

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	.021	.018	.106	0.128	.164	0.055
	(.020)	(.020)	(.134)	(.137)	(.262)	-0.149
Village insecure at midline	.053	.037	.031	.017	.000	.768***
	(.037)	(.036)	(.045)	(.042)		(.027)
Village insecure at endline	.855***	.845***	.878***	.855***		.804***
	(.025)	(.025)	(.036)	(.035)		(.033)
Treated village * Village insecure at midline					-.009	
					(.039)	
Treated village * Village insecure at endline			-.059	-.032		-.111*
			(.032)	(.033)		(.056)
Randomization Strata	Yes	Yes	Yes	Yes	Yes	Yes
Attrition rate	20.4%	20.4%	20.4%	24.7%	23.4%	21.9%
Joint p-value on interactions	-	-	.569	.041	.639	.425
Observations	1743	1743	1743	1565	973	1743

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)

Columns 1, 4: Standard deviation in braces

Columns 2, 3, 5, 6, 7 and 8: Standard errors in parentheses clustered by village

	Weights: Probit			Weights: LPM			Weights: LPM, Below Median	
	(1) Control Mean	(2) ITT Between Baseline and Midline	(3) ITT Between Midline and Endline	(4) Control Mean	(5) ITT Between Baseline and Midline	(6) ITT Between Midline and Endline	(7) ITT Between Baseline and Midline	(8) ITT Between Midline and Endline
Panel A: Fertility								
Any child born (%)	79.8	4.33	1.01	79.9	3.86	.734	1.207	.378
		(3.02)	(2.72)		(2.98)	2.662	(2.85)	(2.64)
Number of children born	.984	.047	.006	.976	.042	.005	.027	.007
		(.031)	(.031)		(.030)	(.030)	(.027)	(.030)
Panel B: Birth Spacing								
Birth spacing between target child and their older sibling (months)	34.2	-.868		34	-.779		34.7	
		(.737)			(.729)			
Birth spacing between target child and their older sibling <= 24 months (%)	11.8	4.52**		14.4	4.58**		10.8	
		(2.01)			(1.93)			

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 weighted control mean), 2 and 3 and the LPM weights are used in column 4 (weighted control mean), 5 and 6. Column 2 and 5 reports ITT estimates at Midline, and Column 3 and 6 reports ITT estimates at Endline. Column 7 and 8 restrict the sample to those who have below median probability of becoming pregnant. All ITT impacts 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. In Panel B we also control for the month in utero of the new child. Standard errors are clustered at the village level throughout. Any child born refers to a child born between Baseline and Midline (Midline and Endline) in Column 2 (3). The age child death outcomes relate to children born between Baseline and Midline.

Table A4: Multiple Hypothesis Testing

Adjusted P-values, Families of Outcomes

		(1) ITT Between BL and ML
Table 3: Quantum and Tempo Effects	***Panel A: Fertility and Child Mortality***	
	Any child born (%)	.563
	Number of children born	.514
	Panel B: Birth Spacing	
	Birth spacing between target child and their older sibling (months)	.563
	Birth spacing between target child and their older sibling <= 24 months (%)	.066

		Not pregnant women at BL		Pregnant women at BL	
		(1) ITT, ML	(2) ITT, EL	(3) ITT, ML	(4) ITT, EL
Table 5: Target Child Outcomes	***Panel A: Anthropometrics***				
	Height-for-Age (HAZ)	.074	.561	.010	.078
	% Stunted (HAZ < -2)	.022	.561	.047	.088
	% Severely Stunted (HAZ < -3)	.390	.561	.047	.088
	Panel B: Health				
	Child health outcomes index	.120	.021	.001	.001
Table 6: Maternal Outcomes	Weight (kg)	.364	.891	.642	.928
	Height (cm)	.567	.214	.648	.543
	BMI	.567	.664	.648	.912
	Malnourished	.580	.664	.642	.928
Table 7: Consumption, Food Security and Saving	***Panel A: Expenditure***				
	Monthly food expenditure	.077	.200		
	Any cigarettes or tobacco	.282	.279		
	Any newspapers and magazines	.676	.774		
	Monthly non-food expenditure	.676	.301		
	Panel B: Food Security				
	Did not have enough food in past year (%)	.676	.002		
	Panel C: Saving				
	Total savings (including in kind)	.676	.084		

		Wife		Husband	
		(1) ITT, ML	(2) ITT, EL	(3) ITT, ML	(4) ITT, EL
Table 8: Labor Supply and Self Employment, Business Investment, and Earnings by Spouse	***Panel A: Labor Supply and Self-Employment***				
	Any work in past year (%)	.626	.002	.973	.843
	Has business/self-employed (%)	.651	.001	.697	.843
	Petty trading (%)	.892	.002		
	Farming own land (%)			.923	.843
	Panel B: Business Investment				
	Monthly expenditure on business inputs		.002		.843
	Owning any livestock (%)	.045	.002		
	Panel C: Earnings				
	Total monthly earnings	.892	.002	.826	.843

		Weaker Incentives for Husband	Stronger Incentives for Husband
		(1) ITT Between BL and ML	(2) ITT Between BL and ML
Table 9: Quantum and Tempo Effects by Husband Incentives	***Panel A: Fertility and Child Mortality***		
	Any child born (%)	.659	.044
	Number of children born	.612	.239
	Panel B: Birth Spacing		
	Birth spacing between target child and their older sibling (months)	.588	.268
	Birth spacing between target child and their older sibling <= 24 months (%)	.004	.268

Notes: We present p-values adjusted for multiple testing, corresponding to the main regression tables. These are computed using the step-down procedure discussed in Romano and Wolf [2016], with 1,000 bootstrap replications. The boxes indicate the outcomes in each panel that are being simultaneously tested.

Table A5: Target Child Outcomes, Age adjusted

Column 1: Standard deviation in braces

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

	Non-pregnant women at baseline (N=1743)				Pregnant women at baseline (N=3688)			
	(1) Control, Midline	(2) ITT, Midline	(3) ITT, Endline	(2) = (3)	(4) Control Mean	(5) ITT, Midline	(6) ITT, Endline	(5) = (6)
Panel A: Anthropometrics								
Height-for-Age (HAZ)	-1.85	.161*	.020	[.263]	-2.46	.133**	.110*	[.702]
	(1.42)	(.086)	(.100)		{1.30}	0.064	0.061	
% Stunted (HAZ < -2)	49.3	-8.67**	-1.66	[.142]	66.2	-2.57	-4.86*	[.389]
		(3.29)	(3.88)			(2.47)	(2.62)	
% Severely Stunted (HAZ < -3)	20.7	-1.73	-1.61	[.977]	34.8	-3.74	-3.91*	[.943]
		(2.54)	(3.60)			(2.43)	(2.43)	

Notes: Significance levels: * (10%), ** (5%), ***(1%). Column 1 shows the mean (and standard deviation for continuous outcomes) value in Control households at Midline. Column 2 reports ITT estimates at Midline, and Column 3 reports ITT estimates at Endline. Columns 4 and 5 control for the age non-parametrically using dummies for different age ranges of the New Child. The age dummies (in months) are: 0-5, 6-11, 12-17, 18-23, 24-30 at Midline and 21-27, 28-33, 34-39, 40-45 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, and the gender of the new child. Standard errors are clustered at the village level throughout. 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.

Table A6: Quantum and Tempo Effects in Subsamples

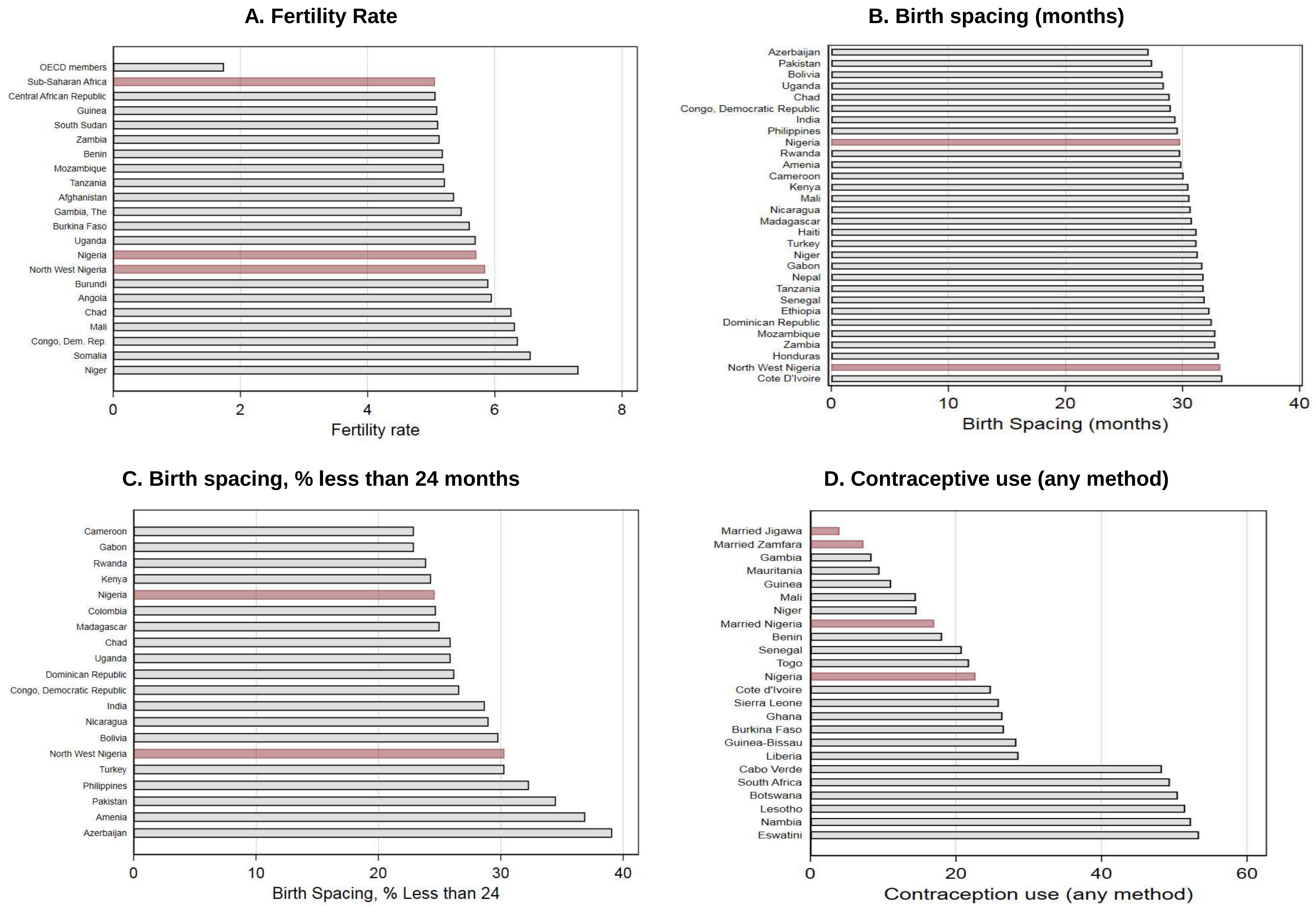
Sample: Non-pregnant women at baseline (N=1743)

All Columns: 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) ITT Between Baseline and Midline	(2) ITT Between Midline and Endline	(3) ITT Between Baseline and Midline	(4) ITT Between Midline and Endline	(5) ITT Between Baseline and Midline	(6) ITT Between Midline and Endline	(7) ITT Between Baseline and Midline	(8) ITT Between Midline and Endline	(9) ITT Between Baseline and Midline	(10) ITT Between Midline and Endline	(11) ITT Between Baseline and Midline	(12) ITT Between Midline and Endline
Panel A: Fertility												
Any child born (%)	.886	-.349	6.48	1.82	3.91	5.03	2.05	1.389	4.67	-2.87	-.211	-.211
	(3.43)	(3.00)	(4.54)	(4.69)	(3.92)	(4.06)	(3.80)	(3.96)	(4.71)	(4.06)	(4.07)	(4.24)
Number of children born	.012	-.027	.096*	.056	.045	.024	.031	.031	.068	-.036	.000	.025
	(.033)	(.038)	(.049)	(.049)	(.042)	(.043)	(.036)	(.038)	(.044)	(.046)	(.046)	(.048)
Panel B: Birth Spacing												
Birth spacing between new child and their older sibling (months)	-.880		.705		.645		-.825		.667		-1.15	
	(.635)		(.720)		(.770)		(.645)		(.773)		(.687)	
Birth spacing between new child and their older sibling <= 24 months (%)	2.27		.190		-.180		2.53		-.375		3.45*	
	(1.62)		(1.84)		(1.86)		(1.78)		(1.77)		(1.83)	

Notes: Significance levels: * (10%), ** (5%), ***(1%). We split the sample in two ways. Columns 1-4 show a split by the median number of children the mother has had at baseline. Columns 5-8 split the sample by polygamous and non-polygamous households. Columns 9-12 split be above or below median savings at baseline. Columns 1, 3, 5, 7, 9 and 11 report ITT estimates at Midline. Columns 2, 4, 6, 8, 10 and 12 reports ITT estimates at Endline. All ITT impacts 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. In Panel B we also control for the month in utero of the new child. Standard errors are clustered at the village level throughout.

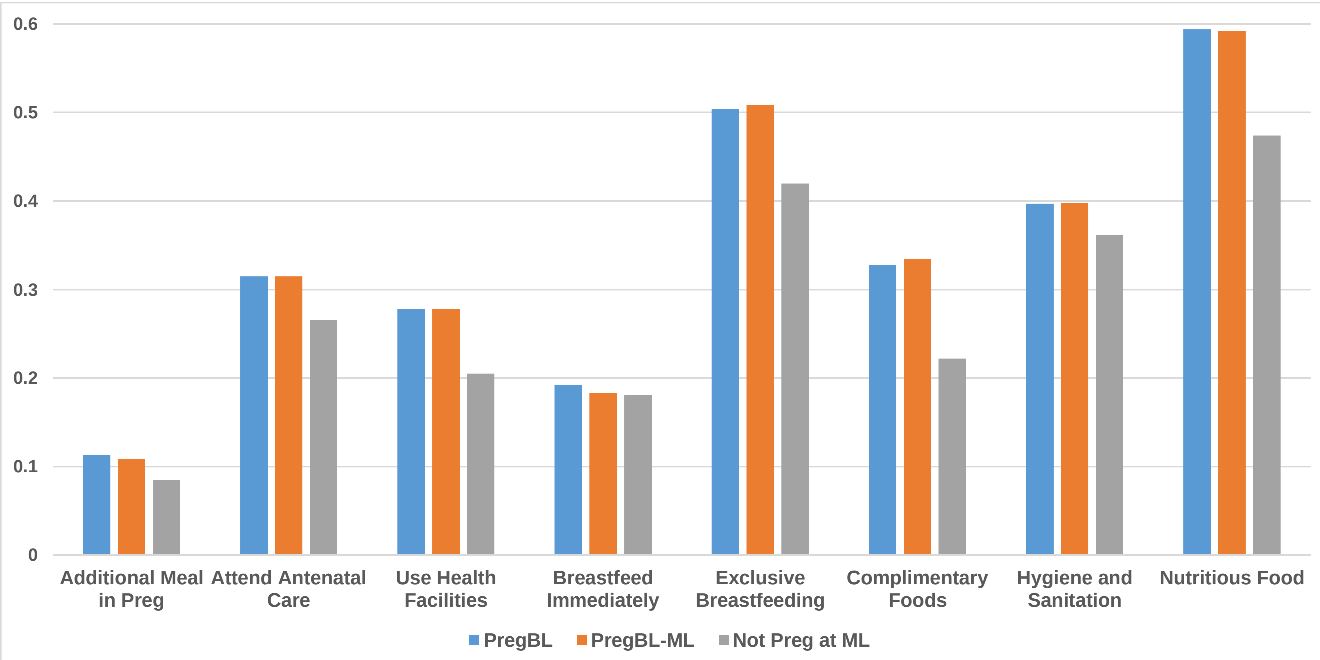
Figure A1: Motivation



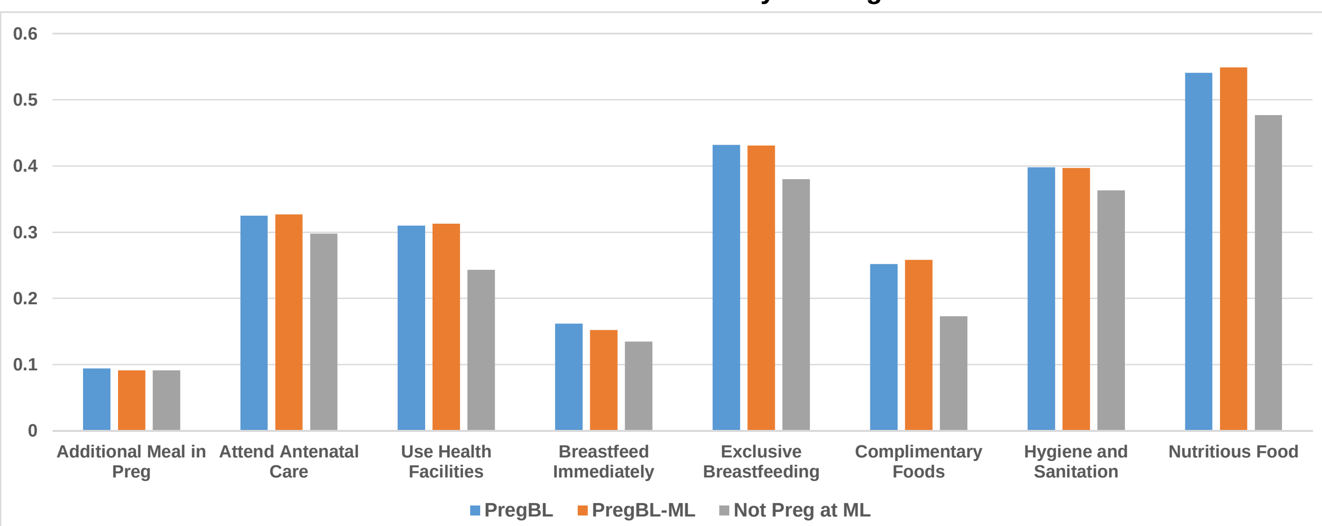
Source for Panel A: World bank fertility rates (births per woman), North West Nigeria statistics comes from the 2013 DHS Survey. We report the 20 countries (19 plus NW Nigeria) with the highest reported fertility rate in 2013. We also report The Sub Saharan African average and OECD member states average. **Source for Panels B and C:** DHS Comparative Reports, Trends in Birth Spacing. For each graph we take any numbers reported more recently than 2000 and show the 20 highest % less than 24 months and 20 lowest median birth spacing. The North West Nigeria statistic comes from the 2013 DHS survey. Panel D: United Nations (2019) Contraceptive Use by Method which uses the most recent DHS wave (Nigeria, 2018). The additional figures in Nigeria come straight from the Nigeria DHS 2018 Survey.

Figure A2: Recall of Key Messages at Midline

A. Wife's Recall of Key Messages

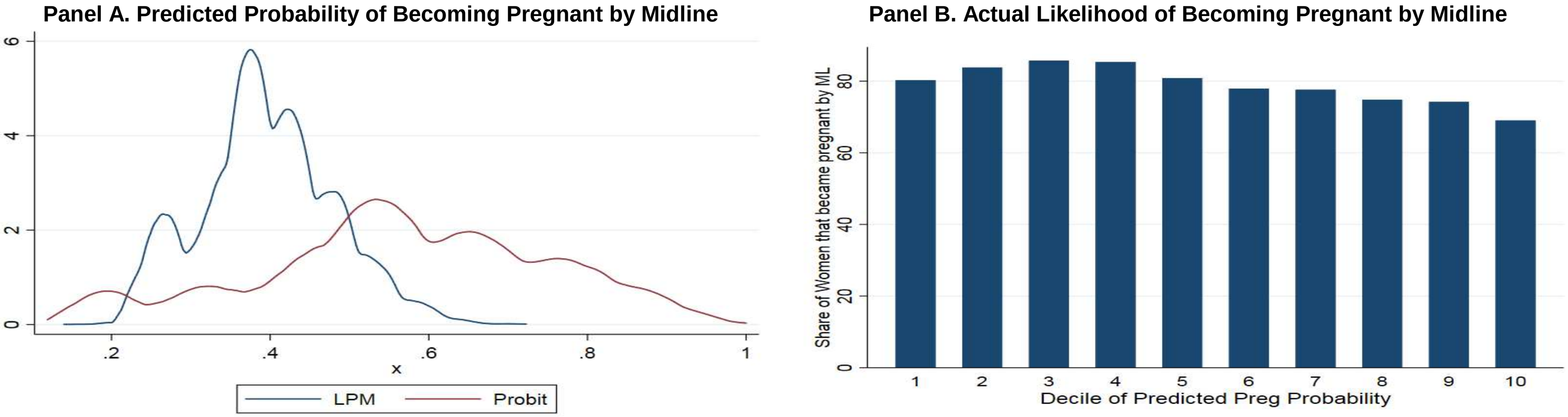


B. Husband's Recall of Key Messages



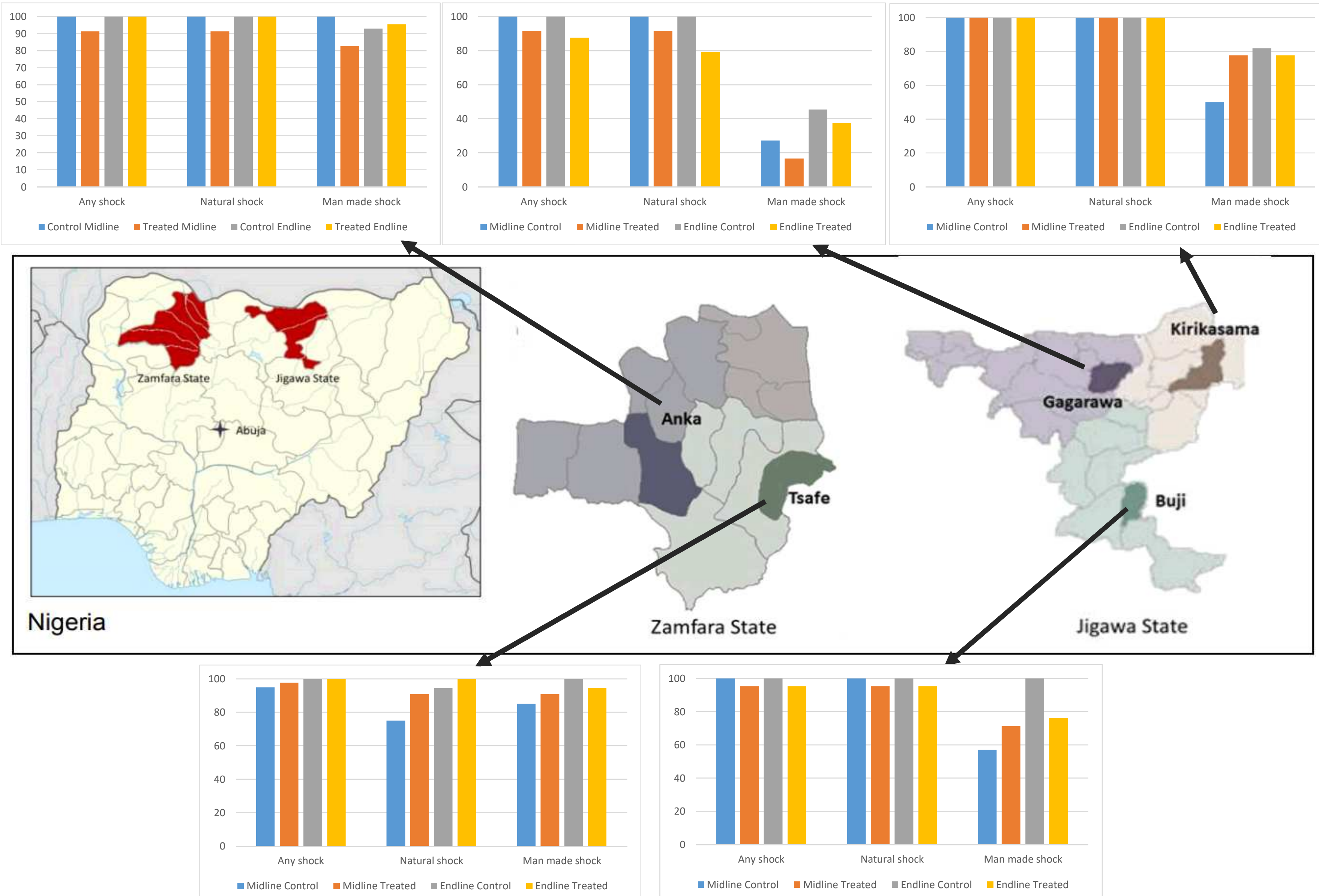
Notes: This figure is based on data from women and their husbands in households. It shows the proportion of pregnant at baseline and not-pregnant at baseline women and husband who recall the eight key messages at Midline. Recall is from any low intensity information channel (posters, radio, food demonstrations 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 A3: Predicted Probability of Becoming Pregnant, Sample of Women Not Pregnant at Baseline



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 A4: Frequency of Village Shocks, by LGA



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.