Families as Drivers of Inequality: Experimental Evidence from an Early Childhood Intervention*

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Abstract

Families shape inequality across individuals, by determining whether initial endowment differences across children are magnified or equalized through the intrahousehold allocation of resources over time. We study the link between early life circumstances, parental investments and child outcomes, over time and across multiple siblings in families in rural Northern Nigeria, where households reside in extreme poverty and sibling rivalry effects can be first order. We do so by evaluating a pre-natal intervention providing information and cash transfers to families triggered by the verified pregnancy of a target child. We track outcomes and child-specific parental inputs across older and younger siblings of the target child in 3600 families over four years. We find that unlike for the target child, stunting outcomes for older siblings do not improve, because they are too old when the intervention begins to gain from it in terms of height. We also document muted gains on height for younger siblings, and show this is because of endogenous responses to the intervention through shorter birth spacing between the target child and younger siblings, labor supply responses of mothers, and fade out of knowledge on specific peri-natal practices. However, on a raft of other outcomes such as health, nutrition and parental inputs more relevant outside the first 1000-days of life window, outcomes significantly shift forward for all siblings. Our results show parents behave as if to equalize inputs across siblings, despite differences in their physical endowments. Calculating the annualized IRR to the intervention based on this fuller set of family impacts, leads them to rise ten-fold over those based on target child outcomes alone. *JEL: I15, O12*.

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1 Introduction

Families can play an important role in determining inequality across individuals. Intrahousehold allocations that parents make across children drive differences in the human capital they acquire from the outset of life. Such differences in development – in terms of physical growth, skills and health, say – have persistent effects on education, labor market success, and lifetime welfare. While intrahousehold allocations have been shown to drive sibling inequalities in high-income contexts where households can better insure against idiosyncratic shocks [Datar *et al.* 2010, Pavan 2016, Daysal and Sominson 2020, Black *et al.* 2021], these issues are even more acute in the most economically deprived environments, when sibling rivalry for resources is first order [Rosenzweig and Schultz 1982, Mordoch 2000].[1]

Parental investments across children can be driven by differences in the initial endowments of children, the production function for human development early in life that can create incentives for parents to reinforce or compensate such initial differences in endowments for a given child [Becker and Tomes 1976, Behrman *et al.* 1982, Cunha *et al.* 2010], and parental objectives related to inequality over their children [Becker and Tomes 1976, Griliches 1979].

We shed light on the how inequality across siblings unfolds within families by examining parental behaviors towards multiple children over time, in the context of an early childhood intervention. The pre-natal intervention we study provides parents with information and resources which could benefit all children, but by targeting one child during their pre-natal period, it creates exogenous variation in one of the key drivers of parental behavior: the initial endowment of the targeted child. We evaluate the intervention combining a four-year randomized control trial with data on child-specific outcomes and parental investments across multiple siblings – the targeted child, an older sibling, and any younger sibling born within our four-year evaluation period.

Our study context – households in rural Northern Nigeria – is one where trade-offs for parents across children and sibling rivalry effects can be first order given the majority reside in extreme poverty. Indeed, existing studies using continent-wide data show that: (i) children born in periods of higher household income enjoy better health and parental investments than their siblings – a result interpreted in terms of households reallocating resources away from the weakest to the fittest offspring to raise the chances of survival in a competitive environment [Berman *et al.* 2021]; (ii) three-quarters of undernourished children are not in the poorest 20% of households, a result partly driven by differential parental investments between children in families across the wealth

[1] Using the Children of the National Longitudinal Survey of Youth from the US, Datar *et al.* [2010] find children who are normal birthweight are more likely to receive parental investments compared to their low-birth weight siblings. Pavan [2016] uses the same data to show differences in parental investments across siblings can account for more than half their gap in cognitive skills. In terms of health shocks, Daysal and Sominson [2020] and Black *et al.* [2021] document sibling spillover effects from having younger siblings that suffer significant health shocks, using registry data from the US and Denmark. All this work builds on long-standing literatures from both high- and low-income settings examining differential human capital accumulation of children within families based on their birth order, gender and the interaction of the two [Jayachandran and Pande 2017].

distribution, as well as idiosyncratic health shocks to children [Brown *et al.* 2019].[2]

The early life intervention we evaluate relaxes informational and resource constraints for parents. The information component comprises the dissemination of messages to mothers and fathers focused on child-related practices and nutrition during the critical phase for their physical development: the first 1000 days of life including *in utero* [Grantham-McGregor *et al.* 2007]. The resource component comprises the provision of unconditional cash transfers. While messages from the intervention are publicly disseminated, to be eligible for receipt of the cash transfers mothers had to be verified to be pregnant via an on-the-spot urine test in the presence of a community volunteer [Sharp *et al.* 2018]. In common with many early childhood interventions, eligibility is thus triggered by pregnancy related to a specific child, whom we label the 'target' child. Women are eligible to receive transfers for one child only – the target child *in utero* when eligibility is first established. Cash transfers are provided monthly to mothers from when the pregnancy is verified until the target child is 24 months old.

Our evaluation tracks 3688 families over four-years, a long enough time frame to observe the emergence of inequalities across children. All families include a woman verified to be pregnant at baseline and so immediately eligible for cash transfers. 70% of households reside in extreme poverty and both spouses have very limited knowledge on child-related practices in the first 1000 days of a child's life. As a result, baseline levels of human capital deprivation are high: 70% of older siblings of the target child are stunted at baseline.

In earlier work [Carneiro *et al.* 2021], we used this sample to focus entirely on outcomes for the target child. We found the intervention to be highly effective from their perspective, leading to large and sustained reductions in their incidence of stunting. Stunting is the best measure of cumulative effects of chronic nutritional deprivation, reflecting an inability to reach linear skeletal growth potential. This is a key indicator of long-term well-being for individuals and is the main measure of physical development we focus on. The intervention also produced large changes in household resources, parental knowledge about health and nutrition practices, and parental behaviors towards the target child.

To understand the deeper role of families as drivers of inequality, in this paper we broaden the focus to shed light on the distributional effects of the intervention across siblings – using the impacts on the target child as a natural benchmark from which to measure the gains/losses to older and younger siblings. To do so we exploit detailed child-specific data, related to the physical development, health, nutrition and parent-child inputs for the target child, their older sibling, and a younger sibling. Such detailed data collection on multiple children from the same family remains rare, even more so when combined with an experimental intervention in a low-income setting. The older sibling is randomly selected from children in the family already aged 0-60 months at baseline.

[2]Berman *et al.* [2021] utilize DHS surveys from across 54 countries, exploiting variation in price of local crops to drive income variation. Brown *et al.* [2019] base their analysis of the poverty of individuals utilizing DHS and LSMS surveys from 30 countries across Sub-Saharan Africa.

A mother-child specific survey is also implemented for a younger sibling born to the same mother after the target child during the period of our evaluation. We consider both the contemporaneous effects of the intervention on older siblings, and intertemporal effects on younger siblings over the four-year post-intervention period. Interpretation of treatment effects on younger siblings are of course filtered through any endogenous responses in terms of fertility, child mortality and birth spacing between the target child and their younger sibling.

Distributional impacts across older and younger siblings are shaped by three forces: (i) the persistence of household knowledge and resource gains arising from the intervention; (ii) the production function for human capital for a given child, that shapes parental incentives to continue providing resources and attention to the target child later in life; (iii) parental objectives related to inequality across their children early in life.

On dynamic knowledge gains, we find little overall evidence that parents recall of recall of messages or actual knowledge of child-related practices in the first 1000 days of life fade-out (the period targeted by intervention messages). For older siblings, such knowledge gains are however largely irrelevant since they are already outside the first 1000 days of life. Given that parents have increased knowledge throughout the pregnancy of the younger sibling (not just from when the pregnancy is verified as for the target child), intervention impacts operating through knowledge gains should be at least as great for the younger sibling. A caveat to this is that there is evidence of fade-out in maternal knowledge related to peri-natal practices, concerning, for example, the importance of immediately breast-feeding a newborn. This could potentially weaken outcomes for the younger sibling if they are especially sensitive to exactly those practices at birth.

We find persistent intervention impacts on resources available to households, even after cash transfers expire. The mechanism driving this is that mothers use cash transfers to increase their labor supply and invest in productive assets, endogenously generating earnings that offset the eventual loss of cash transfers. Persistent resource gains can benefit all children if their outcomes are normal goods. As with knowledge gains, the fact that households have higher resources throughout the pregnancy of the younger sibling suggests impacts operating through resource channels can be at least as great for the younger sibling as for the target child. However, *how* these resources are generated differs across siblings. In comparing outcomes of the target child to those for younger siblings, resource increases for the former stem largely from the receipt of unconditional cash transfers, while for the latter they are driven by endogenous labor supply responses of mothers, so mother's time allocation can vary across siblings. Outcomes of the younger sibling might then be impacted if maternal time is a key input into their human development.[3]

Knowledge gains translate into improved early life practices towards the target child in their

[3] In higher income-settings the relationship between income and child development is unclear [Carneiro and Heckman 2002, Dahl and Lochner 2012, Heckman and Mosso 2014]. Studies documenting the importance of time relative to resources as an input into child development across low- and high-income contexts include Miller and Urdinola [2010], Justino *et al.* [2022] and Agostinelli and Wiswall [2022].

first 1000 days of life. Relative to controls, they are 16% more likely to receive antenatal care, 41% more likely to be born at a health facility, 59% more likely to be breast-fed immediately at birth, and 12% more likely to be exclusively breast-fed for the first six months of life. Given the uncontroversial fact that such practices improve circumstances for children in early life [WHO 2015], the target child thus experiences a large uplift in their human capital endowment and well-being early in life. This is an advantage not given to their older sibling because most of these dimensions of knowledge are irrelevant for them. Such pre-natal interventions thus almost mechanically create intrahousehold inequality between the target child and older siblings on some dimensions of human capital. If parents recognize such differences across children [Adhvaryu and Nyshadham 2014, Breinholt and Conley 2020], these can impact behaviors across children depending on the other two fundamentals: the production function for human capital for a given child, and parental objectives related to inequality over children.

On the production function, if the elasticity of substitution between investments early and later in life is low, parents have an incentive to continue reinforcing investments into the target child, all else equal. A body of structural and reduced form empirical work finds evidence of such reinforcing behaviors (at least when a single dimension of human capital is focused on).[4]

The trade-off parents face in investing in the target child relative to their siblings depends on their objectives, leading them to either reinforce endowment differences [Becker and Tomes 1976], or to act as equalizing agents so their investments compensate for endowment differences across children [Griliches 1979]. If parents have a sole concern for efficiency, then for given level of resources they will equate the marginal returns to investing across children. Such behaviors can stem from evolutionary motives [Stearns 1992, Berman *et al.* 2022], especially in contexts where households are resource deprived, face credit constraints, and child mortality rates are upwards of 10%. At the other extreme, if parents have a sole concern for ensuring equality of outcomes or inputs across children, they will expend the required resources to do so, even if this objective is met at the expense of reducing their own consumption. To date, direct evidence on parental objectives across children remains largely confined to lab-in-field studies [Berry *et al.* 2022, Giannola 2022].[5]

Our main results on distributional impacts across siblings are as follows.

First, for treated older siblings – in contrast to contemporaneous impacts on the target child – height and stunting outcomes do not improve. The reason is they already lie beyond the 1000-day

[4] Reduced form evidence also typically suggests parental investments reinforce initial endowments of target child arising from pre-natal health shocks [Behrman *et al.* 1994, Rosenzweig and Zhang 2009, Almond and Mazumder 2013]. Microfoundations for such behavior are provided by studies estimating the production function for human capital, that suggest dynamic complementarities and self-productivity can be important features of human development early in life [Heckman 2007, Attanasio *et al.* 2022].

[5] Giannola [2022] presents evidence from a survey experiment in India, and finds parental investment decisions across children are driven by efficiency considerations rather than inequality concerns over children's final outcomes. Berry *et al.* [2022] find using lab-in-field experiments across countries that parents are willing to forgo 15-20% of their potential earnings to equalize opportunities across children. Parents in their high income sample (US and UK) display a preference for equalizing expected outcomes, but this is not so for their low-income sample (Malawi).

window when the intervention begins, and such outcomes are harder to shift later in life. However, on a range of other outcomes across which parents can take more decisive actions related to health, nutrition and later life inputs, we find significant advances for older siblings. The kinds of parental input that increase towards the older sibling are not directly messaged as part of the intervention. The magnitude of the impacts is in line with parental objectives being to equalize inputs across the target child and their older sibling, despite differences in their physical development early in life. The evidence is not consistent with the idea that parents focus attention solely on the target child to the detriment of older siblings.

Second, for treated younger siblings, we also observe muted gains on their physical development. On the other hand, across a raft of other outcomes such as health, nutrition and parental inputs relevant outside the 1000-day window, we observe significant gains for the younger sibling. Hence the impacts of the intervention persist to improve the well-being of later born younger siblings. Parents engage in these behaviors towards younger siblings despite them not having the same early improvement in stunting as the target child, mimicking the results on parental behaviors towards the older sibling. Overall, the findings are again in line with a parental objective of equalizing inputs, health and nutrition outcomes across these two children. The evidence is inconsistent with the idea that parents focus attention solely on the target child (and older siblings) to the detriment of younger siblings.

Given parents have increased knowledge throughout the pregnancy of the younger sibling (not just from when the pregnancy is verified as for the target child), it remains a puzzle why the physical development of younger siblings does not shift forward. We show this can be explained through three channels. First, we show in treated households the birth spacing between the target child and their younger sibling reduces, in part because mothers become healthier over time. Around 20% of the difference in height between the target child and their younger sibling can be explained by endogenous responses in birth spacing. Second, although net resources of the household are the same over time, *how* these resources are generated differs across siblings. We document 10% of the difference in height between these siblings can be explained by endogenous labor supply responses of mothers that alter time allocations across siblings over time. Third, we show there is fade-out of maternal knowledge related to peri-natal practices by the four-year endline, especially regarding the practice of immediately breast-feeding a newborn. Examining actual practices towards children early in life, the likelihood of the child being put to breast immediately is significantly lower for the younger sibling than for the target child ($p = .048$). This decline might occur because peri-natal practices have to take place within a very narrow time window at child birth, when mothers might be stressed to begin with (in a context where child mortality is over 10%). The fade-out of such specific peri-natal practices can have persistent dampening effects on physical development early in life.

At a final stage of analysis, we draw together findings across siblings to establish the cost effectiveness of the intervention based on a fuller set of impacts on family welfare than those limited to the target child alone. Many parts of Sub Saharan Africa are yet to undergo the demographic transition, and as in our context, family sizes are upwards of seven. Hence depending on the sign and magnitude of any spillovers of the intervention to multiple children in the family, the returns to such interventions can be severely under or over estimated, relative to the case where the focus is only on the target child.

Our findings highlight that the extent of positive sibling spillovers varies across outcomes. If gains are measured purely via changes in physical development such as height or stunting (as in our earlier work), then the annualized internal rate of return (IRR) to the intervention is the same as when only gains to the target child are accounted for. If we take a more holistic view of human development – and consider gains via health- or nutrition-related outcomes – then given positive impacts across siblings on such dimensions, the IRR to the intervention rises substantially. To illustrate the extent to which this is so, we combine our estimates of the intervention impacts on de-worming rates across siblings, with existing estimates on long run earnings impacts of being de-wormed in early life [Hamory *et al.* 2021]. When impacts across older and younger siblings are factored in, the benefit/cost ratio of the intervention rises three-fold, and the annualized IRR rises ten-fold relative to when only gains to the target child are considered. In short, such interventions can generate far greater returns than currently appreciated if families act as equalizing agents, endogenously responding to such policies by pushing forward the human development of all their children despite inequalities in their physical endowments.[6]

Our work provides new insights on the fundamental role of families in determining inequality across children early in life, by linking early life circumstances, parental investments and human development across multiple children in the same family. We this answer an appeal of Heckman and Francesconi [2016], by providing among the first experimental evidence on the distributional impacts within families of early childhood interventions, drilling down to uncover the origins of inequality across individuals. Our work complements established literatures on the crowd-in/out of public policy by private behaviors within families, that typically does not consider distributional consequences across children. Our work builds on related literatures on sibling spillovers exploiting non-experimental research designs related to endowment shocks [Almond and Mazumder 2013, Adhvaryu and Nyshadham 2014], health shocks later in life [Yi *et al.* 2014], or twin-based studies exploiting plausibly exogenous variation in initial endowments across twins [Bharadwaj *et al.* 2017]. We show how early life interventions can mechanically drive intrahousehold inequality on some dimensions of human capital, but that there are also critical distributional responses of parents that unfold over time to equalize outcomes and inputs across children along other dimensions of human capital.[7]

[6] To the best of our knowledge, the only other similar exercise conducted for early interventions is Bennhoff *et al.* [2022]: they use data from the Perry Preschool Project tracking original participants and their siblings (and their children) to evaluate the long run returns to the intervention, using actual data on participant outcomes. They find spillover benefits to siblings to be imprecisely estimated but large in magnitude.

[7] Evidence based on non-experimental shocks to children's initial endowments or later life health face empirical

The paper is organized as follows. Section 2 describes the intervention, evaluation, and our data on child-specific outcomes and parental inputs. In Section 3, we lay the foundations for how the intervention impacts siblings by documenting impacts on parental knowledge and household resources over time. Section 4 presents the contemporaneous impacts on older siblings while households are in receipt of unconditional cash transfers. Section 5 examines intertemporal impacts on younger siblings when there might be learning/fade out of knowledge and endogenous labor supply responses of mothers to the provision of cash transfers. Section 6 calculates the annualized internal rate of return to the intervention accounting for impacts across siblings. Section 7 discusses directions for future work and implications for the design of early childhood interventions. The Appendix provides additional results and robustness checks.

2 Intervention, Evaluation and Data

2.1 The Child Development Grant Programme

The intervention we study is the Child Development Grant Program (CDGP), a multifaceted program comprising two components: (i) the dissemination of messages to mothers and fathers focused on the first 1000 days of life, related to child practices and nutrition during the pre-, peri- and post-natal periods; (ii) unconditional cash transfers.

Information Panel A of Table A1 shows the eight messages disseminated. These were developed based on an earlier nutritional intervention conducted in the same context by our intervention partners – Save the Children (SC) and Action Against Hunger (AAH) – that identified prevalent and important knowledge gaps among the rural poor. They accord with standard advice on best practices to improve early life outcomes in deprived environments [WHO 2015]. Panel B of Table A1 details how information messages are delivered. Low-intensity channels include posters, radio, Friday preaching/Islamic school teachers, health talks, food demonstrations, and pre-recorded SMS/voice messages. High intensity channels include small group parenting sessions (focusing on

challenges such as any documented positive correlation between endowments at birth and later life investments reflecting a correlation between unobserved investments over time rather than a behavioral response to the endowment, and parental responses might be directly impacted by whatever drives the initial endowment shock. While some of these issues can be addressed better in twin-based studies, such designs are not amenable to explore intertemporal responses and spillovers to later born siblings. The closest experimental evidence relates to nutrition-based interventions [Kazianga *et al.* 2014, Adams *et al.* 2018] or education-based interventions for older children [Barrera-Osorio *et al.* 2011, Ferreira *et al.* 2018]. Nutritional interventions are however typically not targeted to a specific child but can lead to crowd in/out of intrahousehold food allocation across children. As such, these papers focus only on contemporaneous spillovers. On education, Ferreira *et al.* [2018] examine sibling spillovers from a conditional cash transfer program related to middle-school attendance. They find positive effects on eligible children (akin to the target child in our context) but no contemporaneous spillovers (either positive or negative) onto ineligible siblings. In a similar intervention in Colombia, Barrera-Osorio *et al.* [2011] document that the schooling outcomes of not all eligible children were impacted – suggesting parents have preferences over which children to educate – and siblings became more likely to drop out of school and enter the labor market.

nutrition and health practices), and one-to-one counselling in home visits.

While messaging is continuously provided by locally recruited female community volunteers, the food and health demonstrations are delivered monthly in each village, by trained CDGP staff alongside these volunteers. Information is provided via public meetings to which all are invited. This ensures information disseminates beyond currently pregnant women, to women likely to become pregnant in the future, as well as those influential in setting local norms related to child practices and child care such as men and older women.[8]

Unconditional 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 (without accounting for any crowd out of food expenditure or consumption of own produce). The value of the monthly transfer is substantial, corresponding at baseline to 85% of women's monthly earnings, or 26% of household's monthly food expenditure.

Eligibility While information was publicly disseminated, mothers had to meet two criteria to be eligible for the cash transfers: (i) be resident in a CDGP village; (ii) be pregnant, as verified by an on-the-spot urine test in the presence of a community volunteer [Sharp *et al.* 2018]. In common with many early childhood interventions, eligibility is thus triggered by pregnancy related to a specific child, whom we label the 'target' child. The fact that households can start to benefit from the intervention while the targeted child is *in utero* means the intervention can have greater returns – for the target child – than programs starting post-natally [Bhutta *et al.* 2013]. Women are eligible to receive transfers for one child only – the target child *in utero* when eligibility is first established. They do not receive additional transfers for any subsequent child, namely younger siblings of the target child (nor do they receive transfers related to older siblings of the target child). Cash transfers were delivered monthly by payment agents visiting villages. They use thumbprints to identify the correct eligible mother, and transfer cash directly to them.[9]

We might think of this as a labelled cash transfer given it is bundled with information on child-related practices, nutrition, health and sanitation. However, because transfers are provided monthly until the target child is 24 months old, this provides mothers with a far more stable flow of resources than is available from most labor activities in these rural economies. The magnitude

[8]The CDGP is designed to be scalable to other contexts with low state capacity. The day-to-day running of the program is the responsibility of locally-hired community volunteers (CVs). These are of two types: (i) a lead CV (one per village), who is typically relatively high skilled and is further trained in counselling; (ii) nutrition promoter CVs (two per village), who disseminate information and refer mothers to more senior CDGP staff when necessary. The lead CV is paid, while the nutrition promotion CVs receive a stipend to cover transport and meals, and certified training for their role. Administrative records show both types of CV work for around 25 hours/month in line with their being intense information dissemination activities.

[9]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.

and certainty of the flow of transfers – a *de facto* basic income for two years – enables resources to be used for (child-orientated) consumption and investment purposes. This is despite the fact that at no point was it suggested to beneficiaries they should use the transfers to engage in new forms of income generating activity [Carneiro *et al.* 2021].

Take-up As information is publicly disseminated, we focus on take-up rates of the cash transfer component. We do so using CDGP administrative records. These show that in treated villages, over 90% of households with women pregnant at baseline (and so immediately eligible for transfers) received some payments by endline. There is also a small degree of take-up in control villages (12%), due to cross-village registrations and implementation errors. On the timing of payments, on average, women start receiving cash transfers in their final month of pregnancy. 41% receive their first transfer sometime during pregnancy, 14% start receiving them in the month of birth of the target child, and 33% start receiving them post-natally. These administrative records show that by midline mothers have received on average of 24 payments, of cumulative value $477.

2.2 Evaluation

Our evaluation covers 210 villages in two states in North West Nigeria: Zamfara and Jigawa. One third of villages were randomly assigned to a control group, and the others divided evenly into two treatment arms where the CDGP was implemented. Treatment arms varied only in the intensity of information delivered, as described in Table A1. The low-intensity channels of information delivery and cash transfer components of the intervention are identical in both treatment arms and so we combine treatments throughout. To ensure the program could feasibly disseminate information and cash transfers to women while they were still pregnant with the identified target child, we divided villages into three tranches (strata), with random assignment of villages taking place within each.[10]

2.3 Timeline and Data Collection

Figure 1 shows the timeline of activities from June 2014. Treated villages underwent a one week period of mobilization involving local and religious leaders. The low-intensity information channels of the CDGP serve as a continuation of the village mobilization. Cash transfers were disseminated from August 2014 onwards. We initially conducted a census covering $38,803$ women aged 12-49 in

[10] The CDGP is implemented in Zamfara by SC, and in Jigawa by AAH. The exact same program is implemented by both NGOs, using common modalities. The evaluation takes place in five LGAs (districts) in these two states: Anka, Tsafe in Zamfara, and, Buji, Gagarawa and Kiri Kasama in Jigawa. 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.

the 210 villages. This allows us identify households with a pregnant woman, and so immediately eligible for cash transfers.[11] Our baseline survey took place from August to October 2014, our two-year midline survey was conducted in October/November 2016, and the four-year endline survey took place from August to October 2018.[12]

Sample We use the census to draw a baseline sample of all 3688 women verified to be pregnant in villages in our evaluation, and their husbands. By construction, this sample avoids issues of endogenous selection into pregnancy due to the availability of cash transfers. Each spouse is interviewed separately at each survey wave. Survey modules to spouses included those on consumption, savings/borrowing, asset ownership/investments, and labor activities. These allow us to build a rich picture of dynamic endogenous labor supply responses of mothers and fathers to the resources provided by the intervention. Impacts on spousal earnings and time allocations can impact outcomes for all children beyond the target child. Other survey modules for spouses related to the information components of the program. At midline and endline, we asked each parent about their recall of the messages provided, and related questions to measure their actual knowledge of child practices during pre-, peri- and post-natal periods.

Baseline Balance In our study context, households are almost entirely of Hausa ethnicity and Muslim religion. Table 1 shows other characteristics of households and spouses at baseline, confirming balance across treatment and controls. Panel A shows that there are on average 7 individuals per household, with their being 4.5 children present – so lots of scope for parents to generate or minimize inequality across individual children. Household monthly food expenditures are \$85 (whereas the monthly CDGP transfer is \$22). Around 40% of monthly expenditures are on food. Pre-intervention, households are severely resource constrained: 72% of them live in extreme poverty, below the \$1.90/day global threshold.

Panels B and C show baseline characteristics of pregnant women and their husbands. Despite mothers being aged 26 on average, they have 4.6 children alive, aged below 18, and resident with them. Hence the vast majority of target children have older siblings at baseline. Around half of the mothers are in polygamous marriages with older husbands (who are on average aged 43). Both spouses have low levels of human capital, with 20% of mothers being literate, while 40% of husbands are literate. The majority of women engage in income-generating activities, and

[11] 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.

[12] Two other issues related to the timeline are of note. First, the lean season in rural North West Nigeria runs from March to October: this is when food is in short supply and households have sometimes to resort to extreme coping strategies. This coincides with the baseline and endline surveys, but this timing does not differ between treatment and control villages. Second, just before the end of our study period it was announced enrolment into the CDGP would close from April 2019. Households in treated villages are nearly all aware of the CDGP intervention, and the vast majority are aware of the eligibility criteria. This is unsurprising given the salience of the program in this region, and the large potential improvements from child well-being that can result from enrolment.

they retain control over income streams they generate. The main labor activity for women is to rear/tend or sell household livestock: 36% are engaged in such work. Among men, over 80% have farming household land as their main labor activity.

Finally, both spouses display limited knowledge of practices related to children early in life. For example, only 15% of mothers (20% of fathers) believe a health facility is the best place to give birth, and only 14% of mothers (13% of fathers) report a newborn child should be exclusively breast-fed during the first six months of life. This practice is especially important in this context where water quality is poor – the alternative to breast-feeding is to provide newborns with water, that likely severely increases their likelihood of illness in early life.

Attrition By the four-year endline, 23% of women pregnant at baseline had attrited. Columns 1 and 2 of Table A2 show 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) – indeed, in villages that were always secure, only 8% of pregnant 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 the interaction between treatment and baseline characteristics (household demographics and village insecurity) is .373; (iv) the same is true for correlates of attrition of husbands from baseline to endline (Column 4).[13]

2.4 Child Specific Data

Sample Our analysis is built on the collection of detailed information on child-specific outcomes and parental inputs. This enables us to evidence distributional intrahousehold impacts of the intervention across siblings. We implemented mother-child specific surveys to collect information for the target child and multiple siblings, on their physical development, health, nutrition and parental inputs targeted towards them. All siblings are biological children of the same mother in the household.

To understand the data structure across siblings we first note that in our core sample of 3688 women verified to be pregnant at baseline, by the two-year midline: (i) 83% had one new child; (ii) 12% had more than one new child; (iii) 5% had no new children by midline (due to miscarriage or child mortality). For the target child (that is in *in utero* at baseline and the one for whom cash transfers are provided), we track information on 2719 of them at midline.[14]

At baseline we also fielded a mother-child questionnaire to collect information about a randomly

[13] At midline, enumerators were unable to visit 18 villages due to security risks, rising to 28 villages at endline. Village insecurity is uncorrelated to treatment, but relates to various types of man made shocks such as curfews, violence, or widespread migration into the village.

[14] For the 12% of women that had more than one child since baseline, we randomly selected one of their children aged 0-2 at midline to be designated as the target child.

selected child in the family already aged 0-60 months. This is an older sibling of the target child, and we surveyed 2597 of them at baseline.

Finally, it is possible that between midline and endline another child is born to the same mother after the target child (their younger sibling). At endline we implemented a mother-child questionnaire to also collect information on these younger siblings. We surveyed 1886 younger siblings at the four-year endline.

Figure A1 describes our data structure, showing box-whisker plots for the age of the target child, their older and younger sibling, by survey wave. Each shows the 10th percentile, interquartile range, median and 90th percentiles of the age distribution (in months) of that child. At baseline, the *in utero* target child is estimated to be -4 months from birth. At midline the median target child is aged 21 months, and at endline they are aged 44 months. Older siblings are on average 37 months old (and their median age is 36 months) at baseline, and are tracked to midline when they are aged around five years. The younger sibling of the target child is observed only at the four-year endline, when their median age is 13 months.

Physical Development The outcomes we focus on for each child relate to their height and stunting. We use height-for-age Z-scores (HAZ) and a standard measure of stunting – HAZ scores below two standard deviations of international norms [WHO 2009]. Stunting is the best measure of cumulative effects of chronic nutritional deprivation, reflecting an inability to reach linear skeletal growth potential. It is a key indicator of long-term well-being for individuals. To minimize measurement error, this information was collected by a dedicated anthropometric enumerator in each survey wave.

Health and Nutrition We consider two health related outcomes for each child: whether the child has not been ill in the last month, and whether they have had diarrhoea in the two weeks prior to the survey date. On nutrition, we consider the dietary diversity of foods consumed, again *specifically by* each child. We do so using an overall index of the dietary diversity measuring the number of food groups a child is fed. This is obtained using a 24-hour food recall module administered to the mother or main carer for each child. Data on the food consumed by each child in the day prior to the interview from waking up to bedtime is recorded, with ingredients of each meal coded into seven food group categories.[15]

[15] To map from meals to food groups, enumerators first listed the dishes consumed by the child in the 24-hour recall module (excluding drinks – these were captured separately in the liquids recall module), and then coded up the individual ingredients used in each dish as reported by caregivers. Although in theory this ingredient list can be very long, in practice the dishes consumed did not vary a lot. The ingredients were then mapped to food groups. These food groups are: (i) grains, roots and tubers; (ii) legumes and nuts; (iii) dairy products; (iv) flesh foods; (v) eggs; (vi) vitamin-A rich fruits and vegetables; (vii) other fruits and vegetables.

Age Profiles of Child Outcomes Figure A2 shows age profiles for these outcomes, using data from older siblings at baseline in treatment and control groups. Given our focus is on the distributional impacts of the intervention across siblings, we overlay each Panel with two shaded regions to highlight the age of siblings when impacts on these outcomes are estimated for them: (i) the red shaded region covers the 10th to 90th percentile of age for older siblings at midline; (ii) the blue shade region covers the 10th to 90th percentile of age for younger siblings at endline. There is substantial age variation across all the outcomes displayed here.

Panel A shows that HAZ scores follow a standard U-shaped profile, a commonly observed phenomenon in low-income settings referred to as 'growth faltering'. Early in life HAZ scores lie below -1.5σ, so children have poor initial conditions in terms of physical human capital accumulation relative to international standards. The fact that older siblings appear stunted at birth further highlights the potentially high returns to a pre-natal intervention for the target child. HAZ scores decline further as children age – with the average older sibling having a HAZ below -2σ and being stunted. HAZ scores then plateau between 24 and 40 months, at which point children catch up slightly relative to the international benchmark.

Panel B shows a slight negative gradient in the incidence of illness among children as they age, and Panel C shows a positive gradient between dietary diversity and age.

Baseline Balance Table 2 shows baseline balance on characteristics of older siblings. Panel A shows that older siblings are 36 months old on average, and birth spacing between all sibling pairs is around 26 months. Older siblings are at risk of not reaching their developmental potential: Panel B shows that 69% of them are stunted at baseline. On health and nutrition outcomes, Panel C shows that mothers report half of them being ill or injured in the month before baseline, almost of a third of them are reported to have had diarrhoea in the two weeks prior to baseline, and their diet is limited, with their meals comprising only two to three food groups.

Attrition Column 5 in Table A2 shows attrition for the older sibling. Between midline and endline, 20% of older siblings had attrited (a similar proportion as mothers). Attrition is uncorrelated to treatment, and almost perfectly predicted by village insecurity at endline. We find no evidence of differential attrition of older siblings in treated villages by baseline characteristics.

Given large family sizes in our study context, to build confidence that we track outcomes for the same child over time – either the target child or their older sibling – we consider the following validation exercise based on reported ages of children across survey waves, and the month in which surveys were fielded. For the target child we consider the difference between: (i) their age as reported by their mother at endline; (ii) their age as reported by their mother at midline and then adding the time between interview months from midline to endline. We find that 88% of target children have an age gap in these two estimates of zero or one month. Repeating the exercise for older siblings between baseline and midline we find that 84 % have an age gap of zero or one month. Hence while this suggests there can be some slight measurement error in reported ages of children, the validation exercise provides confidence that we successfully track the same set of children within the family over time.

Parental Behaviors We ask about two sets of parental behaviors towards children. First, we collect information on practices actually followed by mothers around the time of birth, towards the target child, and towards their younger sibling. These cover behaviors during the pre-, peri- and postnatal periods and map closely to the kinds of messaging provided by the intervention and relevant for the first 1000 days of a child's life. Second, we ask about parental behaviors that are more relevant later in a child's life. These measure parental behaviors towards each child on survey date – when all surveyed older siblings at midline, and target children at endline, are beyond the first 1000 days of life (Figure A1). These data allow us to gauge whether parents engage in reinforcing behaviors towards children later in life depending on whether the intervention also changed practices towards them in the first 1000 days of life. On child-specific inputs for the target child and their siblings, we asked: (i) whether conditional on a child having diarrhoea, treatment was sought and treatments administered; (ii) whether the child had been given de-worming medication. For the target child and their younger sibling we also asked about the types of vaccinations given. These second set of behaviors are not subjects in the information campaign, and therefore they are not directly linked to knowledge provided by CDGP. For the target child and their older sibling, we also collected information about maternal time spent with them.

To build confidence these child-specific measures of development, health and parental inputs contain valuable information to study inequalities across siblings, we examine how much within-household variation there is in each. We run the following regression for sibling s in household h using survey wave t:

$$Y^s_{ht} = \sum\nolimits_j \pi_j [Dage^s_t \in j] + \alpha_h + \epsilon_{ht}, \tag{1}$$

where Y^s_{ht} is the relevant measure, we control for the age of sibling s using 6-month bins (so $Dage^s_t = 1$ if the age of sibling s is in age-bin j at survey wave t), and household fixed effects (α_h) – because we observe any given outcome for at least two siblings in the household. Given we use multiple survey waves, we estimate this only for controls and report $1 - R^2$ for each regression, namely the within-household variation left unexplained once we account for unobserved heterogeneity across households and sibling ages. The results are in Table A3, where in Column 1 we pool observations for the target child and their older sibling at midline, in Column 2 we pool observations for the target child at midline and their younger sibling at endline, and in Column 3 we pool observations across all siblings at midline and endline.

We observe considerable within family variation in outcomes and parental inputs. On physical development, Panel A shows that 30% of the variation in HAZ across siblings given their age, arises within families. Panel B shows between 20% and 40% of the variation in the incidence of illness, diarrhoea and dietary diversity occurs across siblings within families. Panel C shows substantial variation within families in health related behaviors towards children, and Panel D shows at least 23% of the variation in early life practices employed by mothers towards the target child and their younger sibling occurs within families.

These findings show the kinds of parental inputs we measure are not best considered simply as pure public goods or club investments that necessarily are the same across all children. They are child-specific, in part because they are costly in terms of time and resources, but also because they occur towards children at different ages, and hence are made in different time periods, over which family circumstances can change dramatically. This is especially so in our study context where households are subject to frequent idiosyncratic and aggregate shocks [Carneiro *et al.* 2021].

3 Impacts on Knowledge and Resources

How the intervention shifts outcomes across siblings rests on its dynamic impacts on parental knowledge and household resources. Following Carneiro *et al.* [2021], we document two- and four-year intent-to-treat estimates on both sets of outcomes using the following specification for outcome Y for parent (household) i in village v in period t:

$$Y_{ivt} = \gamma_M T_v.(1 - E_t) + \gamma_E T_v.E_t + \eta_d + \lambda_s + \omega E_t + \varepsilon_{ivt}, \quad (2)$$

where T_v is an indicator for treatment, E_t is a dummy for the four-year endline, η_d are district fixed effects, and λ_s are randomization strata. We allow the error term ε_{ivt} to be clustered by village. The coefficients of interest are the two- and four-year impacts of the intervention (γ_M, γ_E).

3.1 Parental Recall and Knowledge

We consider the dynamic impacts on mothers and fathers related to the informational component of the intervention. We first document impacts on each parent's recall of the messages delivered by the CDGP. The results are in Table 3, for mothers and fathers separately. In Panel A we consider the share of all messages recalled. Panels B to D show recall of each message related to pre-, peri-, and post-natal practices (replicating the detail on messages from Table A1).

We expect control parents to have little recall of messages, because they are not directly exposed to any messaging campaign. Control means on recall for most messages are close to zero. Panel A shows that at the two-year midline, treated mothers recall a quarter of messages. Their recall fades slightly by the four-year endline ($p = .017$) but recall rates are relatively similar over time (.256 vs .226). From Panels B to D we see there is no evidence of a strong memory fade out for five of the eight messages for mothers. The right hand side of Table 2 documents a very similar pattern for fathers, with recall of the same five messages being sustained over time.[16]

In Table 4, we examine dynamic impacts on actual knowledge of child-related practices – again for mothers and fathers separately. While these dimensions of knowledge correlate to the messages provided, we aim to probe parent's ability to apply the messaging to real life interactions with their children. In Panel A we construct an index of all seven components of knowledge, following the approach of Anderson [2008]. The index is normalized in the control group (for each wave) so that impacts can be interpreted as effect sizes. In Panels B to D we show impacts on knowledge on each of seven dimensions covering pre-, peri-, and post-natal practices.

Panel A shows that at the two-year midline, mothers' overall knowledge index of child-related practices increases by $.573\sigma$ and this effect does not fade out over time: the four-year impact on the knowledge index is not significantly different at $.552\sigma$ ($p = .719$). The results help rule out that treated parents only focus attention on child-related practices when in receipt of unconditional cash transfers. Examining specific dimensions of knowledge, there is no fade out for four of seven components. For pre-natal practices, agreement with the statement that 'the best place to give birth is at a health facility' increases significantly over time ($p = .055$). On the other hand, for two of the three peri-natal practices (breast-feeding immediately and colostrum being good), we see fade-out of knowledge by the four-year endline.

Knowledge impacts on fathers are generally smaller than for mothers, but again we see little evidence of knowledge fade-out: either in terms of the overall index, and six of the seven components either remain stable or significantly increase over time.[17]

3.1.1 Implications for Impacts Across Siblings

Figure 2 summarizes how these knowledge gains translate into actual early life practices towards the target child in their first 1000 days of life, as reported by mothers at midline. We estimate treatment effects for each practice using a specification analogous to (2) but only based on data at the two-year midline. We see that treated households are significantly more likely to engage in each practice for the target child. Across pre-, peri- and post-natal periods, the impacts are sizeable relative to target children in control households. As a second benchmark and to gauge time trends in these practices, we also collected baseline data on pre-natal practices that parents report engaging in towards the older sibling when pregnant with them: 42% of mothers received ante-natal care for the older sibling, only 8% of older siblings were born at a health facility.

Given the motivation underlying why these messages were delivered by our intervention partners – that they represent practices enhancing child development and where knowledge deficits

[16] This reinforces that information is provided as a public good in villages, and disseminates beyond just pregnant mothers. We also note a small degree of information spillover to controls (as expected given that radio messaging is used), but for each message rates of recall are orders of magnitude higher for treated households.

[17] We also note improving rates of knowledge among controls, reflecting that knowledge spillovers might be greater than spillovers of intervention messages directly, given cross-village contacts.

existed pre-intervention – the widespread behavioral responses to the intervention shown in Figure 2 imply the target child has a large uplift in their well-being early in life. This is an advantage not given to their older sibling because most messages and dimensions of knowledge are irrelevant for them given they are on average already 36 months old at baseline. Moreover, if inputs into children later in life are complementary to those early in life, this provides incentives to parents to continue making reinforcing investments towards the target child. Depending on parental objectives across all their children, these channels can shift attention and resources towards the target child.

For any younger sibling born during our four-year post-intervention evaluation, the fact that overall knowledge does not fade out suggests any mediating role of the intervention through knowledge gains should lead to similar outcomes for younger siblings as for the target child. Indeed, given parents have such knowledge throughout the pregnancy of the younger sibling (not just from the moment when the pregnancy is verified as for the target child), intervention impacts operating through knowledge gains should be at least as great for the younger sibling.

A caveat to this is that there is fade out in the impacts of the intervention on maternal knowledge related to specific peri-natal practices, including immediately breast-feeding a newborn (as Table 4 showed). This could potentially weaken impacts on outcomes for the younger sibling if they are sensitive to exactly those practices where maternal knowledge fades out. We later examine this issue directly by estimating impacts on actual child practices that parents display towards the younger sibling.

3.2 Household Resources

We next consider dynamic impacts of the resource component of the intervention. To understand how these can potentially have differential impacts across siblings, we briefly replicate findings from Carneiro *et al.* [2021] on the two- and four-year intent-to-treat effects on the labor supply and earnings of each parent, productive investments, and net household resources. The two-year impacts correspond to when cash transfers from the intervention are close to expiring (given the median target child is 21 months old then). The four-year impacts trace through any persistent changes in endogenously generated resources resulting from the intervention.

The results in Table 5 show that there are long run changes in household's net resources (Panel D), persisting at least four-years post-intervention, so well after mothers are no longer in receipt of the unconditional cash transfer. Panel A shows a mechanism for this is the endogenous increase in labor supply of treated mothers. By the four-year endline, treated women's labor market participation rises by 11pp (in a context where womens' participation rates are high to begin with). This increase is driven by self-employment – such as petty trading and livestock rearing – that are their main income generating activities. Engagement in self-employment increases by 6.3pp at the two-year midline, by 12.8pp at the four-year endline, with this increase over time being statistically significant ($p = .013$). Given the labor activities women engage in, in Panel B we show impacts on women's livestock ownership. We see effects on such productive investments being undertaken by women, which again increase between two and four-years post intervention ($p = .014$). Panel C combines information on changes in labor activities to construct a (noisy) measure of monthly earnings from all forms of employment. By endline women's earnings increase by \$21 (corresponding to a 45% increase over controls at endline).

The right hand side of Table 5 shows little evidence that cash transfers given directly to mothers affect the labor supply and earning of husbands.[18]

Panel D draws together all changes in resource flows at the household level to derive an implied change in the net resources available to treated families relative to controls. The imputed value of net resources is calculated as spousal earnings + savings − borrowing + CDGP transfer, where each element is computed as a monthly flow at survey date.[19] We see an increase in net resources available to treated households: the magnitude of this increase is \$43 at midline, more than double the value of the cash transfer itself. In other words, the intervention induces behavioral responses of mothers that endogenously generate like-for-like resource flows to families. This increase is sustained at endline because the loss of cash transfers is offset both by the increase in mother's earnings, and the steady accrual of net savings to the household.

3.2.1 Implications for Impacts Across Siblings

The positive and persistent resource shock can benefit all children if their outcomes are normal goods. As with knowledge gains, the fact that households have higher resources throughout the pregnancy of the younger sibling (not just from the moment when the pregnancy is verified as for the target child), suggests impacts operating through resource channels can be at least as large for the younger sibling than for the target child. However, *how* these resources are generated differs across siblings. In comparing outcomes at midline for the target child to those for their younger sibling at endline, resources increases for the former stem largely from the receipt of unconditional cash transfers, while for the latter they are driven by endogenous labor supply responses of mothers (but not for fathers), that also mean time allocations of mothers can alter across siblings. Outcomes of the younger sibling might be impacted if maternal time is a key input into the production of their human capital.[20]

[18] In line with such non-expropriation of resources by husbands, at midline we asked who usually decides how to spend the CDGP transfer: nearly 75% of women, and 75% of husbands, reported the wife alone decided.

[19] As saving and borrowing are measured as stocks, we convert these into monthly flows assuming they accumulate at a constant rate between survey waves.

[20] The fact the mothers' earnings increase specifically through their increased engagement in livestock rearing is also important. Livestock ownership is critical in this environment because: (i) it produces an earnings stream all year round from the sale of animal produce such as milk and eggs, thus reducing earnings volatility for women; (ii) animal produce can also be consumed at home. Such protein-rich foods can, if consumed at critical ages early in life, drive physical growth. This can increase developmental outcomes for younger siblings to at least the same extent as for the target child and their older sibling.

4 Older Siblings

4.1 Empirical Method

We first consider intervention impacts on older siblings as evaluated at the two-year midline, so over the period when mothers are receiving cash transfers. For sibling s we estimate this using the following specification:

$$Y^s_{ivt} = \gamma^s_t T_v + \eta_d + \lambda_s + \varepsilon^s_{ivt}, \tag{3}$$

where s refers either to the older sibling (OS) or the target child (TC), and the midline survey wave is used (so $t = M$). Standard errors are clustered by village. We examine differential impacts across siblings by first presenting SURE estimates of the null that $\gamma^{TC}_M = \gamma^{OS}_M$. Given siblings are of different ages and there are age profiles in outcomes (Figure A2), control means can differ between siblings. Denoting these control means as $\overline{Y}^{TC,C}_M$ and $\overline{Y}^{OS,C}_M$, to make precise how these ITT estimates translate into proportional impacts across siblings (and therefore taking into account their age differences), we also report the following statistic: $\widehat{\tau}^{OS} = \frac{\widehat{\gamma}^{OS}_M / \overline{Y}^{OS,C}_M}{\widehat{\gamma}^{TC}_M / \overline{Y}^{TC,C}_M}$, calculating standard errors using the delta method and testing against the null that $\tau^{OS} = 1$.

4.2 Results

Physical Development The results are in Table 6. Panel A focuses on outcomes related to the physical development of the target child and their older sibling. Column 1 shows the control mean at midline ($\overline{Y}^{TC,C}_M$), and Column 2 reports the ITT estimate for the target child ($\widehat{\gamma}^{TC}_M$). Treated target children have significantly higher height-for-age Z-scores (HAZ) at midline than controls – the magnitude of the impact is $.217\sigma$. This shift occurs at the left tail of the distribution for height, so rates of stunting fall by 5.7pp (corresponding to a 9% fall relative to controls). The pre-natal intervention is highly effective for target children on this key indicator of long run well-being [Carneiro *et al.* 2021].

To see whether similar gains accrue to older siblings, Column 3 shows the control mean for them at midline ($\overline{Y}^{OS,C}_M$), and Column 4 reports the ITT estimate for treated older siblings ($\widehat{\gamma}^{OS}_M$). We see that neither the HAZ score nor stunting rates for older siblings are shifted by the intervention. We reject the null of equality of impacts for HAZ across siblings ($p = .006$), and proportionate impacts across siblings are significantly lower than one (Column 6). Given the target children is placed on a different trajectory of height early in life relative to their older sibling, the intervention increases intrahousehold inequality in height between these siblings relative to the same sibling gap in control families.

This null result is however not altogether surprising. As Figure A2 shows, an explanation for the lack of impact on the height of older siblings is that when treated households start to gain from the intervention, older siblings are already outside the 1000-day window. Outside of this period, it is generally argued that growth faltering is harder to slow down or reverse [Victora *et al.* 2010]. While this is a strong justification for targeting children in the pre-natal period, such interventions almost mechanically risk increasing inequality across siblings on some margins, because siblings are of different ages at the outset of the program.

Health and Nutrition Panel B examines health and nutrition outcomes across siblings, that by their nature are easier for parents to shift at later ages than height (as the age profiles in Figure A2 also suggest). On these margins we see statistically significant and positive impacts on outcomes for the older sibling. Their incidence of diarrhoea falls by 4.6pp by midline, corresponding to a 23% fall relative to controls, and their dietary diversity significantly increases by 6% over older siblings in controls. We cannot reject the null of equality of these impacts across siblings, although in proportionate terms, two of the three outcomes have $\widehat{\tau}^{OS} \simeq .5$, and for dietary diversity we reject the null that it is equal to one.[21]

Parental Behaviors Panel C shows impacts on parental behaviors, as measured at the two-year midline when both children are beyond the first 1000 days of life. We see increases in parental inputs towards the older sibling, as well as for the target child. Specifically, for the older sibling, parents are significantly more likely to seek advice if the child has diarrhoea, and to treat them with an effective solution in this setting – oral rehydration salts (ORS). Finally, treated older siblings are 10.9pp more likely to be de-wormed than older siblings in controls, corresponding to a 51% increase over de-worming rates among controls. Across all margins of parental inputs towards children, we cannot reject the null of equality of these impacts across siblings, and $\widehat{\tau}^{OS}$ is not statistically different to one (and close in point estimate) for all three margins. In Table A4 we show this pattern of results to be similar by gender of the target child and their older sibling.[22]

Maternal Time Allocation In Table A5 we examine maternal time allocations towards the target child and their older sibling. Panel A shows the total time spent with either sibling does not change. Hence any endogenous change in mothers labor supply in response to the intervention

[21] We note an imbalance at baseline among the incidence of diarrhoea among older siblings (Table 2). To examine the robustness of the results to this imbalance we also estimate a specification analogous to (3) that additionally controls for the outcome at baseline. The results are almost unchanged: the ITT estimate for the older sibling is then $-.042$ with standard error .019.

[22] We have explored impacts on school attendance for older siblings, who by midline are transitioning into formal schooling. We find no significant treatment effect on enrolment rates on older siblings (where baseline enrolment rates among controls are .342). We also note that although our focus is on contemporaneous impacts on the older sibling, another comparison that can be made is between the target child and their older sibling at comparable points of their life cycle – comparing impacts on older siblings at midline (when they are on average five years old) relative to impacts at endline for the target child (when their median age is 44 months). We find impacts related to height, stunting, health, nutrition and parental inputs towards the target child to be sustained over time – and indeed most point estimates are larger in absolute. Hence gaps in outcomes between the target child and their older sibling when they are both older and at the same stage of the life cycle, follow the same broad pattern of findings on contemporaneous impacts across siblings shown.

in the first two years post-intervention (Table 5) does not crowd out time spent with the target child or their older sibling. This is to be expected given mother's labor activities increase because of their engagement in livestock rearing, an activity that can take place at home and simultaneous with looking after children.

Panel B shows results specific to the time mothers report spending playing with each child. Treated families allocating more time to playing with the target child than in controls: the likelihood of mothers spending 0-2 hours per day playing with the target child significantly falls by 6.4pp relative to controls, and this is almost fully offset with an increase of 5.3pp in the likelihood of mothers reporting to spend 5 or more hours playing with the target child. Similar increases in time allocations spent playing with older children are also observed – although these are slightly more muted than for the target child. In particular, the likelihood of spending 0-2 hours per day playing with the older sibling significantly falls by 3.4pp relative to controls, and this is offset with an increase of 2.7pp in the likelihood of mothers reporting to spend 2-5 more hours playing with the older sibling. Given that mothers spend more time playing with both children it is likely that at least part of this increase concerns activities that she can do simultaneously with both siblings.

4.3 Interpretation of Impacts Across Siblings

The pattern of evidence is inconsistent with the idea that in response to the pre-natal intervention triggered by the verified pregnancy of the target child, parents focus attention on the target child to the detriment of older siblings. For the target child, our evidence is in line with much of the earlier literature, suggesting parents engage in reinforcing positive behaviors towards the target child early in life and as they exit the first 1000 days of life – but this does not occur to the detriment of older siblings on margins that parents can more readily influence such as health, nutrition, and parental inputs. The kinds of parental input that increase towards the target child and their older sibling are not directly emphasized to households as part of the intervention. The sign and magnitude of impacts on older siblings are due to a combination of three mechanisms: (i) the production function for human capital early in life; (ii) increased resource or knowledge gains through the intervention; (iii) parental objectives across children.

On (i), dynamic complementarities in the production of human capital for the target child can cause parents to have greater incentives to continue investing in the same child, given they have a better start in life through improved pre-, peri- and post-natal practices towards them. However, the fact that later life practices also increase towards older siblings, despite them not having the same uplift in early life as the target child, suggests dynamic complementarities cannot explain changes in parental behavior towards older siblings.

On (ii), resource gains can relax constraints on making investments in any child. To understand whether this can fully explain impacts across siblings, we consider spillover effects onto other households in treated villages that receive the same publicly provided and non-excludable information messages from the intervention, but are never in receipt of cash transfers because they are neither pregnant at baseline, nor do they have a child over the four-year period of enrolment into the program. While this is clearly a select sample of households, the impacts on older siblings in such never eligible households remain informative. The results in Table A6 show that in never eligible households at midline: (i) maternal recall of messages and knowledge gains related to early life practices are similar to those for mothers in our main sample (Panels A and B); (ii) older siblings in these households have no gains in physical development (Panel C); (iii) health and nutrition practices shift forward in a comparable way to those for older children in our main sample (Panel D); (iv) de-worming rates also increase by comparable amounts as those for older children in our main sample (Panel E). These results suggest changes in behavior stemming from informational gains – rather than resource uplifts – can help explain dimensions of improvement in parental practices towards older siblings. In particular, the provision of information related to child-related practices in the first 1000 days of life leads to changes in parental behavior towards siblings later in life, even if such practices are not part of the intervention messaging, and older siblings have not had the same uplift in measures of physical development.

The evidence in Table 6 on the proportional impacts across treated siblings can be further understood if we consider parental objectives across children. More precisely, the results are in line with parents equalizing inputs across children (where $\widehat{\tau}^{OS}$ is close to one for all three margins in Panel D of Table 6), rather than equalize outcomes across their children in terms of health or nutrition (where $\widehat{\tau}^{OS}$ is closer to .5 for two of three margins).

5 Younger Siblings

5.1 Fertility, Child Mortality and Birth Spacing

We now consider impacts on younger siblings of the target child, measured at the four-year endline. Interpretation of treatment effects on younger siblings are of course filtered through any endogenous responses in terms of fertility and child mortality, both of which can impact family size at endline, as well as birth spacing between the target child and their younger sibling.

Table 7 presents estimates on these margins, using a specification analogous to (2) on household outcomes, but only using endline data. Panel A examines the extensive margin of whether any child is born over the evaluation period, and the number of children born by the four-year endline. We note first that among controls, 84% of families had an additional child during the evaluation period. Given such high fertility rates, it is not surprising we find no significant treatment effect on the likelihood any child is born, or the number of children born between baseline and endline. On child mortality, the intervention does little to reduce these by endline for any child born between baseline and endline ($\widehat{\gamma}_E = -.004$). One reason child mortality is so high in this context is due to low quality water supply, and the intervention does nothing to change this. As a result of these null impacts on fertility and mortality, there are no significant differences in household size between treated and controls at endline: both households have on average of 9 members by then.

Panel B examines impacts on birth spacing between the target child and their younger sibling. We find that in treated families, the younger sibling is born around three quarters of a month earlier than younger siblings in controls ($\widehat{\gamma}_E = -.773$), and this effect is statistically significant at the 5% level. The mean (standard deviation) age of younger siblings in controls at endline is 11.8 (6.2) months, so the reduction in birth spacing corresponds to an effect size of $.773/5.83 = .13\sigma$. To probe this further, we examine the likelihood birth spacing falls below 24 months. Such intervals are considered to place newborns at the highest risk of undernutrition, and mothers with those intervals are at higher risk of birth complications [Pimentel *et al.* 2020, Damtie *et al.* 2021]. In controls, 5.6% of younger siblings are born within 24 months of the target child. We see an increase of 3.4pp in treated families in such short birth spacing.[23]

The intervention can cause these unintended reductions in birth spacing through its two components: (i) information from the intervention reduces perceived costs of having children; (ii) cash transfers from the intervention lead mothers to be healthier, or increase the desire to complete fertility cycles sooner. Given our focus, we do not attempt to disentangle these explanations but note that in relation to information, none of the intervention messaging related to birth spacing or encouraged families to ensure adequate birth spacing. In terms of resource-related channels, we find some evidence of maternal nutrition improves over time, and food expenditures rise significantly at midline and endline [Carneiro *et al.* 2021].[24]

5.2 Empirical Method

We estimate intervention impacts on younger siblings at the four-year endline, so well after the receipt of cash transfers have expired. Outcomes for younger siblings can be impacted through persistent gains in knowledge and practices generated by the information component of the intervention (Tables 3 and 4), and any sustained endogenous increase in net household resources generated by the earlier receipt of cash transfers (Table 5). To measure outcomes across siblings at the most comparable point of their life cycle, we estimate impacts on the target child using data from midline as before. We estimate specification (3) for younger siblings ($s = YS$) at endline (so $t = M$), but also controlling for the age of younger siblings in the 6-month age bins given the treatment effects on birth spacing between the target child and their younger sibling. We examine

[23] The results in Table 7 are robust to controlling for baseline characteristics of the household and mother (and estimated treatment effects remain quantitatively very similar and are estimated with the same precision).

[24] We note that at baseline, there is a negative gradient between total household expenditure and average birth spacing, and a positive gradient with the total number of children aged below 18. On maternal health we examined outcomes related to weight, height, BMI and various indicators for malnourishment. We found no robust evidence of treatment effects on these health outcomes for mothers either at the two-year midline or at the four-year endline. However, comparing two- and four-year impacts, there is evidence of a statistically significant increase in mid-upper arm circumference of mothers ($p = .039$) suggesting gradually improving nutrition over time.

differential impacts across siblings by presenting SURE estimates of the null that $\gamma_M^{TC} = \gamma_E^{YS}$, and to make precise how these absolute impacts translate into proportional impacts across siblings, for each outcome we also report the following statistic $\widehat{\tau}^{YS} = \frac{(\widehat{\gamma}_E^{YS})/\overline{Y}_E^{YS,C}}{\widehat{\gamma}_M^{TC}/\overline{Y}_M^{TC,C}}$, and again test against the null that $\tau^{YS} = 1$.

5.3 Results

Physical Development The results are in Panel A of Table 8. To facilitate sibling comparisons, Column 1 again shows the control mean for target children at midline ($\overline{Y}_M^{TC,C}$) and Column 2 reports the same ITT estimate for the target child ($\widehat{\gamma}_M^{TC}$) at midline as earlier). To see whether similar gains accrue to younger siblings, Column 3 shows the control mean for younger siblings at endline ($\overline{Y}_E^{YS,C}$), and Column 4 reports the ITT estimate for treated younger siblings: ($\widehat{\gamma}_E^{YS}$).

Panel A shows that neither the HAZ nor rates of stunting for younger siblings are shifted by the intervention. Testing impacts across the sibling specifications we find significant differences in these effects (p = .038, .005 respectively). The lack of strong impacts on younger siblings physical development is surprising because treated parents have improved knowledge and resources throughout the pregnancy of the younger sibling (not just from the moment when the pregnancy is verified as for the target child). To understand whether this can be explained by parents being focused on the target child by endline, we document impacts on a fuller range of outcomes for the younger sibling.

Health and Nutrition Panel B of Table 8 focuses on health and nutrition outcomes across siblings. We see positive and statistically significant impacts on nearly all outcomes for the younger sibling. Their incidence of illness/injury falls by 8.4pp, corresponding to a 13% fall relative to younger siblings in controls; their incidence of diarrhoea falls by 6.2pp, a 19% fall relative to controls. Finally, their dietary diversity significantly increases by 19% over younger siblings in control. Proportionate impacts on all three margins ($\widehat{\tau}^{YS}$) lie slightly above one and for dietary diversity we can reject the null that $\tau^{YS} = 1$.

Parental Behaviors Panel C shows impacts on child-specific parental behaviors. We see increased positive behaviors towards the younger sibling, with the magnitudes largely in line with those for the target child. Specifically, for the younger sibling, parents are significantly more likely to treat the younger sibling with oral rehydration salts if they have diarrhoea. Treated younger siblings are 11pp more likely to be de-wormed than controls, a 52% increase in de-worming rates. Finally, the number of vaccinations significantly increases for younger siblings, corresponding to a 25% increase in vaccinations over controls. These treatment effects are again no different for younger siblings than the target child with $\widehat{\tau}^{YS}$ being .86 or higher for the three margins that are shifted forward for younger siblings.

5.4 Implications

The results show the impacts of the early childhood intervention persist and lead to improvements in well-being for their later born younger siblings (as measured in terms of health and nutrition outcomes), and specific parent-child behaviors. The pattern of evidence is inconsistent with the idea that parents focus attention exclusively on the target child (and older siblings) to the detriment of younger siblings. The results also rule out that treated parents only focus attention on child-inputs when in receipt of unconditional cash transfers. The documented impacts on younger siblings occur around two years after the receipt of such transfers typically expires.

Parents engage in these behaviors towards younger siblings despite them not having the same uplift in physical development, mimicking the results on parental behaviors towards the older sibling. Overall, as with the comparison over the target child and their older sibling, the findings are again in line with a parental objective of equalizing inputs and health and nutrition outcomes across the target child and their younger sibling. Our results across siblings thus closely match the lab-in-field evidence from Berry *et al.* [2022] that also finds parents in a low-income context (Malawi) act as if to equalize inputs over children.

5.5 Why the Muted Impacts on Development of the Younger Sibling?

It however remains a puzzle why the physical development of younger siblings does not shift forward more strongly. We consider three explanations stemming from different margins of endogenous response to the intervention: birth spacing, maternal labor supply and knowledge fade-out.

Birth Spacing The unintended treatment effect of reduced birth spacing between the target child and their younger sibling could worsen their endowment at birth and offsets any positive impacts of the intervention. To understand the extent to which reduced birth spacing can explain the muted impacts, we need to translate how the treatment effect on birth spacing, denoted $\widehat{\gamma} = -.773$, translates into child outcomes. To illustrate the possibility, we focus on HAZ scores as the outcome and proceed as follows. We first determine the unexplained portion of HAZ for the target child in household i in village v at midline conditional on age, district and strata fixed effects. We denote the resulting residual as $\hat{\omega}^{TC}_{ivM}$. We then estimate the treatment effect on these residuals by regressing $\hat{\omega}^{TC}_{ivM}$ on T_v, obtaining an estimate $\widehat{\beta}^{TC}$. Repeating both steps for the younger sibling at endline we obtain HAZ residuals for this child ($\hat{\omega}^{YS}_{ivE}$) and an analogous treatment effect estimate on HAZ residuals for them ($\widehat{\beta}^{YS}$). Finally, we derive the partial correlation between birth spacing and residualized HAZ for younger siblings among controls using the following regression:

$$\hat{\omega}^{YS}_{ivE} = \psi BS_v + \eta_d + \lambda_s + \epsilon_{ivE}. \tag{4}$$

Putting these elements together, the share of $(\widehat{\beta}^{TC} - \widehat{\beta}^{YS}) = .099$ attributable to changes in birth spacing over these siblings is $\widehat{\psi} \times \widehat{\gamma} = .024 \times (-.773) = -.018$. Hence around 20% of the difference in treatment effects on HAZ across siblings is explained by changes in birth spacing that result from the early childhood intervention. The upper bound of the confidence interval on $\widehat{\psi} \times \widehat{\gamma}$ implies we cannot rule out that 40% of the difference is explained though this channel.[25]

Maternal Labor Supply Households experience sustained resource increases even after cash transfers expire. However, *how* these resources are generated differs across siblings because of endogenous labor supply responses of mothers that mean time allocations of mothers alter across siblings. Given the potential importance of time as an input into physical development of the younger sibling, we examine this explanation in two ways. First, we conduct a similar analysis as above linking birth spacing and HAZ, but for mothers labor supply. Using the extensive margin of labor supply reported in Table 5, there is a negative correlation at endline among controls between maternal labor supply and HAZ of younger siblings. Changes in labor supply can account for $\widehat{\psi} \times \widehat{\gamma} = .107 \times (-.095) = -.010$, so around 10% of the difference in treatment effects on HAZ across siblings (and again the upper bound of the confidence interval on $\widehat{\psi} \times \widehat{\gamma}$ implies we cannot rule out that 40% of the difference is explained though this channel).

Second, we consider how our results vary depending on whether the mother owns livestock at baseline or not because these groups likely differ in how the intervention changes their labor supply and time allocations, all else equal. The results are in Columns 1 and 2 of Table A7. While we find reductions in birth spacing more concentrated in mothers owning livestock, we find null impacts on the physical development of younger siblings for both groups. Second, we consider whether the presence of older sisters might help improve outcomes for younger siblings when mothers increase labor supply. This follows a literature emphasizing the caregiving role of older siblings [Grantham-McGregor *et al.* 2007, Almond and Currie 2011, Qureshi 2017, Jakiela *et al.* 2020]. This might be especially important in low-income contexts where older sisters share childcare responsibilities and can have more formal education than their mothers. We split the sample between younger siblings that have at least one older sister aged 7 to 14 at endline, and those that only have older brothers. The results in Columns 3 and 4 again show muted impacts on physical development in each of these groups.[26]

[25] Juhn *et al.* [2020] explore the link between birth spacing and the cognitive development of older siblings in the framework of a quantity-quality trade-off. Using NLSY79 data they show that cognitive ability of already born children drop after shocks to family size, but only when siblings arrive at younger ages.

[26] The remaining Columns in Table A7 show the shift in birth spacing to be more pronounced when the target child is a girl (Panel A) although we do not rule out that the impacts by gender of the target child are the same ($p = .325$, $p = .733$). In Panel B we focus on the gender of the younger sibling: we see the muted impacts on physical developmental to be similar by gender. A final potential mechanism relates to price effects, whereby cash injections into village economics can potentially impact relative prices over time. While the evidence suggests this does not occur in other contexts [Cunha *et al.* 2019, Attanasio and Pastorino 2020, Egger *et al.* 2022], village remoteness can be a driver of such price changes [Filmer *et al.* 2023]. In our context this channel is partly mitigated because treatment and control villages share local markets, and the intervention causes households to purchase livestock

Knowledge Fade Out We earlier documented that although in aggregate, knowledge gains on child-related practices did not fade out over time, there was fade-out by the four-year endline of maternal knowledge specifically related to the peri-natal practice of immediately breast-feeding a newborn. This could weaken developmental outcomes for the younger sibling if they are sensitive to this behavior at birth. To examine the issue, we estimate treatment effects for child-specific practices that parents report having engaged in towards the target child early in life (as reported at midline), and towards the younger sibling early in their life (as reported at endline). For each practice Y for sibling s in household i in village v at survey wave t (Y^s_{ivt}), we estimate:

$$Y_{ivt} = \gamma_t T_v + \eta_d + \lambda_s + \varepsilon_{ivt}. \quad (5)$$

Standard errors are clustered by village. The ITT estimate for the target child is $\widehat{\gamma}_M$, the ITT estimate for their younger sibling is ($\widehat{\gamma}_E$) and we examine the equality of treatment effects across siblings using SURE.

The results are in Table 9. Panel A shows that for pre-natal practices, treated households are significantly more likely to engage in these practices for the target child and their younger sibling. For both practices – whether the mother received antenatal care while pregnant with the child, or whether the child was born at the health facility – we see the percentage point increase is significantly higher for the younger sibling than for the target child (p = .013, .018 respectively). This is despite the fact that by endline such practices are also slightly more common in controls as information disseminates across communities. For these kinds of prenatal practices, over time families appear to learn-by-doing.

Panel B shows that treated households are also more likely to engage in peri-natal practices for the target child and their younger sibling than controls. However there is more evidence of fade-out in such practices over time and hence across siblings. In particular, the likelihood of the child being put to breast immediately is significantly lower for the younger sibling than for the target child (p = .048). This dimension of fade-out exactly matches the dimension of maternal knowledge that fades out over time (Table 4).

Finally, Panel C confirms no fade-out in terms of exclusively breast-feeding children for the first six months of life: treated families are significantly more likely to engage in this practice for both the target child and their younger sibling.[27]

Overall, engagement in practices early in life across siblings move in opposite directions over time for treated households relative to controls: pre-natal practices increase over time, peri-natal

and so generate a flow of nutrients through home production.

[27] In this context contraceptive availability is highly limited. Hence an important method that could be used to delay pregnancy is continued breast-feeding. However at baseline, only 5% of women reported knowing that exclusive breast-feeding can be used as a form of contraception (thus increasing birth spacing). Although our results show a higher incidence of exclusively breast-feeding the target child for the first six months of their life, this channel cannot explain changes in birth spacing between the target child and their younger sibling. Indeed, the results show this interval falls despite the change in post-natal practices towards the target child.

practices fade, and postnatal practices remain stable. These divergences could be for a number of reasons. It might be that the kinds of pre-natal practice we measure are more memorable, and easier for learning by doing to occur. In contrast, the peri-natal practices we measure have to take place within a very narrow time window at child birth, when mothers might be stressed to begin with – especially given such high rates of child mortality.[28]

Alternatively, it might be that beliefs over the effectiveness of these practices change over time. This is harder to support given the marked improvements seen in the physical development of target children – differences that would be noticeable to parents, and indeed supported by the fact that they engage in reinforcing behaviors towards the target child over time. Moreover, at endline we also asked mothers their beliefs over the positive impacts of some practices, and find these also significantly shift forward.[29]

An implication of not putting newborns to breast immediately after birth is that it increases their exposure to low quality water sources on the first day of life. In our context, only 20% of households at endline have access to potentially safer piped water sources. In the Appendix we exploit this non-experimental source of variation to examine heterogeneous treatment effects on the development of younger siblings to provide suggestive evidence on the potential importance of interactions between peri-natal practices and water quality for child development.

6 Internal Rate of Return

We draw together findings across siblings to establish the cost effectiveness of the intervention based on a fuller set of impacts on family welfare. As in Yi *et al.* [2015], our results demonstrate the need to consider multiple dimensions of child development because sibling spillovers vary across them. If gains are measured via changes in height or stunting (as in our earlier work), then the annualized internal rate of return (IRR) to the intervention is the same as when only gains to the target child are accounted for. If we broaden dimensions of gains, then the IRR to the intervention can rise substantially. To illustrate the extent to which this is so, we combine our estimates of intervention impacts on de-worming rates across siblings, with estimates on long run earnings

[28] Table A8 examines changes in early life practices towards the younger sibling by mothers ownership of livestock at baseline, the presence of older sisters, and gender of the younger sibling. Panel A shows largely similar rates of pre-natal practices for younger siblings relative to controls, irrespective of whether the mother owns livestock or not, irrespective of the presence of older sisters, or the gender of the younger sibling. The same is mostly true for the post-natal practice of exclusively breast-feeding for six months. On peri-natal practices, we see girls are significantly more likely to be put to breast immediately, but otherwise these behaviors do not significantly differ between households where mothers do or do not own livestock at baseline, or the younger sibling has at least one older sister aged 7 to 14 at endline.

[29] We asked mothers whether they believed exclusive breast-feeding would result in a child being stronger: the treatment effect on this belief is 33pp, relative to 51% of mothers in control holding this belief. We also asked whether they believed exclusive breast-feeding would result in a child being sick less often: the treatment effect on this belief is 25pp, relative to 65% of mothers in control holding this belief.

impacts from de-worming in early life [Hamory *et al.* 2021].[30]

We assume the planner has a 5% discount rate. On program costs we assume: (i) the per beneficiary cost of administering cash transfers are 10% of the actual per beneficiary value of the transfers; (ii) the organization of community volunteers to deliver the information messages amounts to a further 10% of the per beneficiary value of cash transfers. Panel A of Table 10 summarizes the structure of program costs.[31]

In Column 1 we focus on gains arising through de-worming on lifetime earnings for the target child. Panel B shows the NPV of these gains to be relatively small because earnings gains only start to accrue once the child is in the labor market (from age 16), and even though this flow of benefits lasts until they are aged 60, intervention costs are born up front (starting when the child is *in utero*). The gain/cost ratio of the intervention is 1.14, so the program just breaks even and the annualized IRR is .400%. Column 2 adds in impacts across younger and older siblings. Accounting for these intrahousehold distributional impacts, the gain/cost ratio of the intervention rises three-fold to 3.37, and the IRR rises ten-fold to 4.44%. In Column 3 we account for all siblings aged under 5 at baseline. We still assume zero gains to older children and thus ignore potential epidemiological spillovers from de-worming that have been documented in earlier studies [Miguel and Kremer 2004, Ozier 2018]. The IRR to the intervention then rises further to 6.85%, that is 17 times higher than when only de-worming impacts on the target child are accounted for.[32]

[30] We compute the increase in earnings for the target child as follows. First, we note that Hamory *et al.* [2021] estimate the long run impact of de-worming medication on log earnings to be .09, corresponding to a percentage impact of $((\exp(0.09) - 1) \times 100 = 9.42$. At the two-year midline, when target children are on average 19 months old, we estimate the impact on the likelihood of the target child being de-wormed to be .110 for boys and .068 for girls. As Hamory *et al.* [2021] evaluate long-run impacts of de-worming treatment received at primary school ages, we extrapolate de-worming impacts at age 6 combining midline results with intent-to-treat estimates at the four-year endline. At endline, we estimate the impact on the likelihood of the target child being de-wormed to be .116 for boys and .102 for girls. If de-worming impacts continue on this trend, they would be .122 for boys and .136 for girls, when aged 6. Thus the percentage impacts on earnings are 1.15 for boys and 1.28 for girls. Since older siblings are on average 5 years old at midline, we use ITT impacts on de-worming measured at that point in time to evaluate gains for them. These are .105 for boys and .121 for girls. For younger siblings, we only observe de-worming impacts at one point in time, namely four-year endline, which means we cannot extrapolate impacts at primary school age. For this reason, we use the extrapolated impacts at age 6 for target children described above. Second, we derive monthly earnings over the life cycle from our data by regressing monthly earnings on dummies for 10-year age bins, for ages 16 to 60 and by gender. We use this to compute the NPV of the sum of earnings changes across siblings from ages 16 to 60 resulting from the increased likelihood to be de-wormed because of treatment, assuming the percentage impact of de-worming on earnings is constant over the life cycle.

[31] Following Dhaliwal *et al.* [2012] we can view cash transfers as being: (i) a *pure redistribution* of resources from the planner to beneficiaries, and so they involve zero net cost to society; (ii) at the other extreme, we can view them as a *pure cost* solely borne by the planner. Given our aim is to illustrate how intervention returns increase when sibling impacts are accounted for, we focus on the first scenario.

[32] These estimates are likely lower bounds on the true returns because we ignore any pre-labor market gains to children from the intervention. To provide an indicative estimate of what the returns might be once we factor in such benefits to children from age 2 to 16, we consider benefits arising from increased food consumption. More precisely, we use estimated intervention impacts on the per adult equivalent food consumption of children and calculate the NPV of this flow of gains to each sibling from age 2 to 16. We then add this to the NPV of earnings gains from de-worming to construct a measure of total lifetime benefits. The IRR to the program then rises to 20% if we only consider the target child, 71% if we also consider their older and younger siblings, and 106% if all siblings aged below five at baseline are included.

7 Discussion

Inequality across individuals starts at birth or even *in utero*. Families determine whether initial differences across children are magnified or equalized through their actions. This matters because inequalities in place at later stages of childhood have persistent impacts on lifetime welfare. We make progress on understanding the role of families for the origins of inequality across children by linking early life circumstances, parental investments and child outcomes and across multiple children in the same family. We document how parental responses unfold across children and over time, to a pre-natal intervention that creates experimental differences in the initial endowment of the target child relative to their older siblings.

We make two contributions. First, we push forward the study of intrahousehold decision making, by starting to reveal the complex web of parental behaviors across children. We find that even in a context of extreme poverty where sibling rivalry might be strongest, parents still act to equalize dimensions they can influence: they provide similar inputs to all their young children, and health and nutrition outcomes significantly shift forward for all. This occurs despite: (i) mechanical inequalities in physical development being generated because interventions are targeted to a specific child, when older siblings lie outside the critical window of the first 1000 days and are too old to gain on this margin; (ii) unintended inequalities in physical development generated over time for younger siblings, due to endogenous intervention responses related to birth spacing, maternal time allocation, and knowledge fade-out of critical and time-sensitive practices.

Second, we provide among the first experimental evidence on the distributional impacts within families of early childhood interventions. Reducing child health inequalities is one of the UN's Sustainable Development Goals, and interventions to improve the accumulation of human capital early in life hold great promise for the world's poor. Concerns over deficits in human development during the first two years of life has led government and non-governmental agencies to direct cash transfer programs to households with young children. Globally, these have reached between 750mn and 2bn individuals [Bastagli *et al.* 2016, Hoddinott *et al.* 2017]. In Sub Saharan Africa, over a dozen countries are testing multifaceted early childhood interventions combining cash transfers with components to drive behavioral change [Beegle *et al.* 2018]. Ultimately, we establish the cost effectiveness of such interventions based on a fuller set of impacts on family welfare. Based on estimated long run returns to de-worming, the annualized IRR to the intervention rises more than ten-fold once intrahousehold impacts across siblings are accounted for. Our results suggest a need to widen the set of outcomes used to evaluate returns to such interventions beyond those related to physical development, as has been argued elsewhere, to also consider outcomes related to health and cognitive development. This is especially important given the process of economic development can be characterized as one of rising relative returns to brains over brawn, and these outcomes being more closely linked to lifetime welfare [Foster and Rosenzweig 1996].

Future Agenda Our reduced form evidence is in line with parental objectives being to equalize inputs across all children, and equalizing health and nutrition outcomes between the target child and their younger sibling. Throughout we have been careful not to make claims on the actual *returns* to parental inputs. The reason for not doing so is partly that the answer depends on the dimension of child outcome considered – in our context, the physical development of siblings does not shift forward, although measures of health do (with the proportionate impacts on younger siblings being more aligned to those for the target child). More generally, the equalization of parental investments is not sufficient to equalize child outcomes. They might even expand across siblings if the returns to investments increase with endowments, in which case equalization of outcomes across children would only be possible if parental investments overcompensated any initial endowment differences.

Measuring returns requires an altogether different approach of modelling parental objectives across children and parents choosing their inputs optimally given the production function for child development and other constraints. As clearly articulated by Adhvaryu and Nyshadham [2014], parental preferences and parameters of the production function cannot in general be separately identified given only one source of (quasi-) experimental variation. However, our data from multiple siblings over time, combined with additional modelling assumptions, offers the potential to make headway on the issue, and this is a topic we hope to explore in future work.[33]

Policy Implications Our findings suggest two key policy implications. First, given that in our setting parents act as a powerful equalizing force to spread gains from the intervention across children, the returns to the early childhood intervention is higher than would be estimated based on outcomes for the target child alone. These channels of intrahousehold spillovers supplement other arguments for interventions early in life based on productivity gains to the targeted child, arising because the production function for human capital is characterized by dynamic complementarities and self-productivity effects. In higher income settings it is already appreciated that early life interventions are among the social programs with the highest returns, with later remediation of early deficits not being as cost-effective [Hendren and Sprung-Keyser 2020]. Our results suggest the gradient of social returns by age of intervention might be even steeper given intrahousehold sibling spillovers, a mechanism less likely to occur for later-life interventions designed to raise the human capital of adolescents or those in post-compulsory schooling. However, much remains to be understood in terms of whether parents necessarily serve as an equalizing force across settings

[33] A model of parental investments across multiple children could build, for example, on Behrman *et al.* [1982], extending it to allow for knowledge and resources to change, and multiple margins of response to occur (such as labor supply and parental inputs). By making assumptions on the production function across siblings and following the approach of Kline and Walters [2016], who expand the set of instruments available to identify model parameters by interacting exposure to an intervention with baseline household characteristics, thus exploiting the idea it has differential impacts across households, but that such heterogeneity does not affect the productivity of parental inputs or parental responses to child endowments, is one promising avenue by which to simultaneously identify parental preferences and the production function for human development.

or stages of childhood.

Second, our findings have implications for the optimal targeting of early life interventions. They suggest targeting larger families – to maximize contemporaneous spillovers, while also targeting families early in the fertility cycle – to maximize intertemporal spillovers. The trade-off between these is of course slightly tilted towards the second objective given younger siblings benefit for longer. In our setting, intertemporal spillovers are limited partly because knowledge fades out related to specific peri-natal practices. This however is an easy problem to solve with the provision of informational refreshers at the time of each child birth (even if not tied to cash transfers). With such kinds of relatively cheap and scalable design modifications, early life interventions would be more likely to lead to gains in physical development across siblings, making such policies even more powerful in eradicating developmental gaps for the world's poorest children.[34]

A Appendix

Peri-natal Practices and Water Quality To probe further whether the fade-out of peri-natal practices over time can help explain the muted impacts on the physical development of younger siblings, we present more speculative results that exploit non-experimental variation across variations. This follows the intuition that an implication of not putting to breast immediately is that it exposes newborns to low quality water sources on the first day of life. In our context, only 20% of households at endline have access to potentially safer piped water sources.[35]

We use this variation to examine heterogeneous treatment effects on the development of younger siblings. Table A9 first establishes the intervention does not change access to safe drinking water (Panel A), and peri-natal practices towards the younger sibling do not vary across households with and without safe drinking water (Panel B). This is consistent with parents being unaware of the safety of their water sources – a topic not part of the informational messaging of the intervention.[36]

Panel A of Figure A3 presents ITT estimates on our measures of physical development for the younger sibling, allowing these treatment effects to differ across households with and without access to safe drinking water. Treated younger siblings in households without access to safe water have close to zero effects on HAZ and stunting. In contrast, for younger siblings in households with safe water, both outcomes improve relative to controls. There is a statistically significant

[34] Our results also suggest potentially extending the set of information provided to households: such as that related to birth spacing. However this is subject to the caveat that the provision of additional information does not crowd out attention to other messaging provided on child-related practices over the first 1000 days of life. Understanding the optimal information to provide households remains an important topic for future work.

[35] A household is defined to have access to safe water if they report their main source of drinking water to be piped water into their structure (14%), or a public tap or standpipe (6%). The primary unsafe sources include boreholes/ tubewells (32%), unprotected dug wells (29%), and surface water (8%).

[36] On ante-natal practices towards the younger sibling, those without access to safe water are significantly more likely to receive antenatal care, but are significantly less likely to give birth at a health facility. There is no difference in their prevalence of exclusive breast-feeding across these two groups of household.

difference between treatment impacts on stunting ($p = .054$). Moreover, the magnitude of the treatment effects for those with safe drinking water are similar to those for target children: HAZ scores increase by $.22\sigma$ and stunting rates fall by 6.1pp.

Of course cross sectional heterogeneity in access to safe drinking water might reflect other differences across households – such as their wealth. We check this concern by conducting the same exercise for target children at midline. Panel B shows no evidence that access to safe drinking water drives their outcomes – so there is no complementarity between responses to the intervention and impacts on the physical development of the target child. In each case, the magnitude of the treatment effect is not statistically different between those with and without safe water, and the magnitudes are in line with those for younger siblings with access to safe drinking water. The results for younger siblings are hence not easily explained by access to safe drinking simply reflecting time invariant differences across households, such as their wealth, because this characteristic only matters for the physical developmental of the younger sibling at endline, and not for the target child at midline.

The results hint at there being important interactions between peri-natal practices and water quality for child development – in line with a body of work across disciplines [Checkley *et al.* 2008, Petri *et al.* 2008, Ashraf *et al.* 2021, Dupas *et al.* 2023, Kremer *et al.* 2023, Weaver *et al.* 2023]. If this were the *only* explanation for muted impacts on stunting for example, the results would imply the 4pp difference in treatment effects of stunting across siblings (Table 8) can be explained by the 6pp fall in the likelihood of immediately breast-feeding the newborn younger sibling (Table 9). At the extreme, this suggests that in the presence of unsafe water, small reductions in some peri-natal practices can wipe out positive impacts of other practices across pre- and post-natal periods. An alternative interpretation of the findings is that reductions in peri-natal practices, that take place in very narrow windows and at potentially stressful times just after child birth (in a context where child mortality is very high), might be a marker for inattentive parents, that also respond more slowly at other moments of a child's early life. The cumulative effects of such inattention could then result in stagnation in a child's physical development, but such inattentiveness does not hold back less time critical parental behaviors towards the younger sibling related to health and nutrition inputs.[37]

[37] Kremer *et al.* [2023] present a meta-analysis of 15 RCTs used to evaluate interventions to improve water quality and prevent diarrhoea in young children in low- and middle-income countries. They find such interventions lead to 25% reduction in the odds of under-5 mortality (from any cause). Other related studies include Ashraf *et al.* [2021], who document in urban Zambia that the availability of piped water (arising from daily supply outage shocks) leads to improved child health, Dupas *et al.* [2023] who show the availability of water purification improves child health in rural Malawi, and Weaver *et al.* [2023] who find that for an intervention in India targeting pregnant mothers with cash transfers during the first two years of a child's life, anthropometric indicators improve only in areas with low rates of open defecation – indicating the importance of sanitation. This is all in line with medical research that shows mechanisms linking poor sanitation and diminished physical development of children include the malabsorption of nutrients due to intestinal disease, loss of nutrients due to diarrhea, and energy expended in fighting disease [Checkley *et al.* 2008, Petri *et al.* 2008].

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Table 1: Baseline Balance

Sample: Households with pregnant women at baseline (N=3688)
Means, standard deviation in braces, p-values in brackets

	(1) Control	(2) Treatment	(1) = (2)
Panel A: Household			
Observations	1186	2502	
Household size	7.70	7.49	[.399]
	{4.33}	{4.32}	
Number of children aged 0-18	4.63	4.53	[.657]
	{3.25}	{3.30}	
Monthly food expenditure (in $USD)	84.9	85.2	[.670]
	{121}	{124}	
Living on less than $1.90/ day (extreme poverty)	.722	.717	[.729]
Polygamous relationship	.491	.487	[.818]
Panel B: Mother			
Observations	1186	2502	
Age (years)	25.5	25.2	[.459]
	{6.82}	{6.85}	
Can read and write at least one language	.191	.213	[.322]
Paid/unpaid work in past year	.743	.700	[.308]
Total monthly earnings (in $USD)	25.6	24.5	[.554]
	{49.9}	{44.8}	
Thinks health facility is best place to give birth	.156	.153	[.477]
Thinks should breastfeed exclusively for six month	.135	.161	[.291]
Panel C: Father			
Observations	952	1828	
Age (years)	43.0	42.2	[.117]
	{9.12}	{9.35}	
Thinks health facility is best place to give birth	.207	.199	[.408]
Thinks should breastfeed exclusively for six month	.128	.133	[.909]

Notes: The sample is based on households in which a woman was pregnant at baseline. In Panel A, household size is the number of people living in the household with common eating arrangements. Food expenditure is based on a 7-day recall for food items. Living on less than $1.90 a day indicates if the household is spending less than $1.90 a day according to PPP USD in 2011 terms. This is the World Bank's international poverty line definition for households residing in extreme poverty. In Panel B, total monthly earnings are the earnings for the wife reported from the past year across all work activities that are conducted for pay. Values above the 99th percentile are set to missing. In Panels B and C, the 'health facility is best place to give birth' and 'should breastfeed exclusively for six months' refer to parental beliefs about these practices. Columns 1 to 2 report the mean (and standard deviation for continuous variables in braces) of the variable in control and treated villages respectively. The p-values on tests of equality across Columns 1 and 2 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: Baseline Balance, Older Sibling Characteristics

Sample: Households with pregnant women at baseline (N=3688)
Means, standard deviation in braces, p-values in brackets

	(1) Control	(2) Treatment	(1) = (2)
Panel A: Age			
Observations	844	1753	
Age (months)	36.4	36.6	[.751]
	{11.5}	{11.7}	
Average birth spacing	26.1	26.0	[.614]
	{9.47}	{9.64}	
Panel B: Physical Development			
Stunted (HAZ<-2)	.690	.674	[.290]
Panel C: Health and Nutrition			
Had illness or injury in past 30 days	.502	.465	[.173]
Had diarrhoea in past two weeks	.327	.274	[.005]
Dietary diversity	2.80	2.74	[.375]
	{.961}	{.962}	

Notes: The sample is based on older siblings of the target child in households in which a woman was pregnant at baseline. In Panel A, we report average birth spacing across all children present in the household at baseline, in months. Values of birth spacing below the 1st percentile are set to missing. Panel B reports anthropometrics of the older sibling. Stunted is a dummy indicating children with height-for-age-z-score (HAZ) two standard deviations or more below the WHO defined guidelines [WHO 2009]. Panel C reports behaviors towards the older sibling. The dietary diversity index is obtained from a 24-hour food recall module administered to the child's mother or main carer. Each meal consumed in the day before the interview from waking up to bedtime is recorded, and each ingredient is coded into categories. The index sums the number of food groups the child has received from the following seven food groups: 1. Grains, roots and tubers, 2. Legumes and nuts, 3. Dairy products, 4. Flesh foods, 5. Eggs, 6. Vitamin-A rich fruits and vegetables, 7. Other fruits and vegetables. A minimum dietary diversity indicates consuming at least four food groups. Columns 1 to 2 report the mean (and standard deviation for continuous variables in braces) of the variable in control and treated households. 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.

Table 3: Recall of Intervention Messages

Sample: Households with pregnant women at baseline (N=3688)

ITT Estimates, standard errors in parentheses clustered by village

	Mother					Father				
	(1) Control Mean, Midline	(2) Midline	(3) Control mean, Endline	(4) Endline	(2) = (4)	(5) Control Mean, Midline	(6) Midline	(7) Control mean, Endline	(8) Endline	(6) = (8)
A. Share of All Messages Recalled	.039	.256***	.028	.226***	[.017]	.051	.230***	.042	.208***	[.048]
		(.017)		(.015)			(.016)		(.014)	
B. Prenatal Practices										
Attend antenatal care	.028	.184***	.019	.160***	[.110]	.045	.159***	.035	.144***	[.230]
Attend antenatal care at least four times during pregnancy.		(.017)		(.015)			(.017)		(.014)	
Eat one additional meal during pregnancy	.012	.087***	.006	.087***	[.924]	.017	.075***	.009	.077***	[.879]
Eat one extra small meal or 'snack' (extra food between meals) each day to provide energy and nutrients for you and your growing baby.		(.010)		(.009)			(.010)		(.009)	
C. Perinatal Practices										
Breastfeed immediately	.021	.155***	.016	.151***	[.770]	.024	.142***	.019	.133***	[.496]
Start breast feeding your baby within the first 30 minutes of delivery. Colostrum is good for the baby.		(.015)		(.013)			(.015)		(.012)	
Breastfeed exclusively	.044	.382***	.035	.328***	[.005]	.044	.346***	.037	.299***	[.005]
Breastfeed your child exclusively until six months old. Do not give water, tinned milk, or any other food.		(.025)		(.022)			(.023)		(.020)	
D. Postnatal Practices										
Complementary feeding	.037	.276***	.024	.239***	[.041]	.043	.238***	.027	.204***	[.055]
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.		(.022)		(.018)			(.021)		(.016)	
Hygiene and sanitation	.065	.308***	.053	.298***	[.560]	.093	.286***	.088	.284***	[.903]
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.		(.022)		(.021)			(.021)		(.021)	
Use health facilities	.043	.166***	.024	.163***	[.833]	.067	.153***	.056	.161***	[.586]
Take baby to health facility if you notice any of the following: fever, convulsion, refusing to eat, malnutrition, diarrhea.		(.018)		(.015)			(.019)		(.017)	
Nutritious food	.060	.489***	.045	.383***	[.000]	.079	.440***	.064	.359***	[.000]
Ensure you buy nutritious foods when you are buying food for your family.		(.028)		(.024)			(.027)		(.022)	

Notes: Significance levels: *(10%), **(5%), ***(1%). The table shows recall of specific messages delivered during the CDGP intervention, from attending village health talks, participating to meetings, or having individual meetings with community volunteers. The sample is based on women who were pregnant at baseline and their husbands. Panel A shows the share of all messages recalled by the woman and her husband (from a total of eight messages). Panels B to D show recall of individual messages. Columns 1 and 3 report the mean for mothers in control villages at the two-year midline and four-year endline. Columns 2 and 4 report ITT estimates for mothers at midline and endline respectively. We report p-values testing for equality between Columns 2 and 4. Columns 5 and 7 report the mean for husbands in control villages at midline and endline respectively. Columns 6 and 8 report ITT estimates for husbands at midline and endline respectively. The final column reports p-values testing for equality between Columns 6 and 8. Each ITT is estimated using OLS, controlling for LGA, randomization tranche and wave fixed effects, pooling across midline and endline survey waves and interacting treatment status with wave fixed effects. Standard errors are clustered at the village level throughout.

Table 4: Knowledge

Sample: Households with pregnant women at baseline (N=3688)

ITT estimates, standard deviation in braces, standard errors in parentheses clustered by village

	Mother					Father				
	(1) Control Mean, Midline	(2) Midline	(3) Control mean, Endline	(4) Endline	(2) = (4)	(5) Control Mean, Midline	(6) Midline	(7) Control mean, Endline	(8) Endline	(6) = (8)
A: Knowledge of Child Practices										
Knowledge index	0	.573***	0	.552***	[.719]	0	.274***	0	.243***	[.517]
	{1}	(.067)	{1}	(.062)		{1}	(.047)	{1}	(.048)	
B. Prenatal Practices										
Go for a check-up, even if healthy	.832	.080***	.915	.056***	[.192]	.890	.047**	.938	.022	[.243]
		(.021)		(.013)			(.019)		(.014)	
Best place to give birth is health facility	.227	.125***	.336	.176***	[.055]	.290	.112***	.398	.176***	[.099]
		(.031)		(.034)			(.035)		(.036)	
C. Perinatal Practices										
Best to start breastfeeding immediately	.428	.265***	.648	.188***	[.018]	.375	.136***	.506	.123***	[.774]
		(.028)		(.026)			(.030)		(.037)	
Baby should not receive other liquids on first day	.665	.220***	.672	.257***	[.246]	.625	.190***	.669	.176***	[.748]
		(.029)		(.031)			(.034)		(.034)	
Colostrum is good for the baby	.713	.187***	.783	.151***	[.107]	.583	.141***	.626	.112***	[.514]
		(.023)		(.020)			(.032)		(.041)	
D. Postnatal Practices										
Do not give baby water when hot outside	.347	.392***	.404	.426***	[.233]	.231	.250***	.298	.314***	[.141]
		(.035)		(.034)			(.029)		(.037)	
Breastfeed exclusively for six months	.358	.291***	.338	.268***	[.429]	.135	.121***	.112	.070***	[.016]
		(.037)		(.0394)			(.019)		(.017)	

Notes: Significance levels: *(10%), **(5%), ***(1%). The table shows parental knowledge of specific child-related practices. The sample is based on women who were pregnant at baseline and their husbands. Panel A shows impacts on a Knowledge index constructed from a group of indicators using the method in Anderson (2008). The indicators used are about parental beliefs over the following practices: it is good practice to bring the baby to a health check-up even if healthy, the best place to give birth is a health facility, it is best to start breastfeeding immediately after birth, the baby should not receive other liquids on the first day, colostrum is good for the baby, the baby should not be given water when the temperature outside is hot, the baby should be breastfed exclusively for the first six months of life. The Knowledge index is standardized to have mean zero and variance one in the Control group. Panels B to D show impacts on all components used to construct the Knowledge Index. Columns 1 and 3 report the mean for mothers in control villages at the two-year midline and four-year endline. Columns 2 and 4 report ITT estimates for mothers at midline and endline respectively. We report p-values testing for equality between Columns 2 and 4. Columns 5 and 7 report the mean for husbands in control villages at midline and endline respectively. Columns 6 and 8 report ITT estimates for the husband at midline and endline respectively. The final column reports p-values testing for equality between Coumns 6 and 8. Each ITT is estimated using OLS, controlling for LGA, randomization tranche and wave fixed effects, pooling across midline and endline survey waves and interacting treatment status with wave fixed effects. Standard errors are clustered at the village level throughout.

Table 5: Resources

Sample: Households with pregnant women at baseline (N=3688)

ITT estimates, standard errors in parentheses clustered by village, standard deviation in braces

	Mother					Father				
	(1) Control Mean ML	(2) Midline	(3) Control Mean EL	(4) Endline	(2) = (4)	(4) Control Mean ML	(5) Midline	(6) Control Mean EL	(7) Endline	(5) = (7)
Panel A: Labor										
Any work in past year	.766	.060***	.807	.107***	[.050]	.996	.003	.996	.003	[.827]
		(.019)		(.016)			(.002)		(.002)	
Has business/self-employed	.655	.063***	.699	.128***	[.013]	.643	-.028	.592	.034	[.009]
		(.024)		(.023)			(.026)		(.021)	
Farming own land	.051	0.006	.012	.014*	[.534]	.964	-.007	.978	.001	[.387]
		(.013)		(.008)			(.010)		(.007)	
Panel B: Investment										
Owning any livestock	.783	.059***	.782	.115***	[.014]					
		(.020)		(.022)						
Panel C: Earnings										
Total monthly earnings from employed and self-employed activities	25	4.34**	45.5	20.8***	[.001]	190	17.7	131	4.03	[.519]
	{39.6}	(1.95)	{85.4}	(4.93)		{338}	(18.5)	{270}	(14.1)	
Panel D: Household Outcomes										
Change in monthly net resources	348	37.8**	351	43.2***	[.803]					
	{265}	(17.9)	{275}	(17.0)						

Notes: Significance levels: *(10%), **(5%), ***(1%). The sample is based on households in which a woman was pregnant at baseline. Columns 1 and 3 show the mean (and standard deviation for continuous outcomes in braces) outcome in control households at the two-year midline and four-year endline respectively. Column 2 and 3 report ITT estimates at midline and endline respectively. Each ITT is estimated using OLS, controlling for LGA, randomization tranche and wave fixed effects, pooling across midline and endline survey waves and interacting treatment status with wave fixed effects. Standard errors are clustered at the village level throughout. In Panel A, 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 monthly earnings for the wife from employed and self-employed activities. There are slight methodological differences in how earnings are 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. In Panel net resources are calculated as income + transfers - saving + borrowing. As saving and borrowing are measured as stocks, we convert these into monthly flows assuming they accumulate at a constant rate between survey waves. In Panels A to C we report p-values of tests of equality of ITT estimates at midline and endline, for each spouse. In Panel D we report p-values of tests of equality of ITT estimates at midline and endline. All monetary amounts are converted from Nigerian Naira to PPP US dollars at the 2014 rate.

Table 6: Outcomes for Older Siblings

Sample: Households with pregnant women at baseline (N=3688)

ITT estimates, standard errors in parentheses clustered by village, standard deviation in braces

	Target Child		Older Sibling			
	(1) Control Mean, Midline	(2) Midline	(3) Control Mean, Midline	(4) Midline	(5) Equality of ITT Estimates Across Siblings (2) = (4)	(6) Proportionate Impacts Across Siblings
Panel A: Physical Development						
Height-for-Age (HAZ)	-2.46	.217***	-2.16	-.046	[.006]	-.239***
	{1.33}	(.069)	{1.08}	(.077)		(.417)
Stunted (HAZ < -2)	.662	-.057**	.579	-.001	[.183]	.013
		(.024)		(.037)		(.735)
Panel B: Health and Nutrition						
Had illness or injury in past 30 days	.696	-.082***	.645	-.040	[.116]	.528
		(.023)		(.027)		(.320)
Had diarrhoea in past two weeks	.378	-.065***	.203	-.046**	[.401]	1.31
		(.022)		(.019)		(.557)
Dietary diversity	3.22	.349***	3.78	.227***	[.062]	.553***
	{1.49}	(.074)	{1.1}	(.068)		(.137)
Panel C: Behavior Towards Child						
If had diarrhea in past two weeks:						
Anyone sought advice/treatment	.783	.058*	.804	.070*	[.766]	1.18
		(.031)		(.038)		(.750)
Given ORS for diarrhea	.408	.084**	.457	.092*	[.881]	.981
		(.040)		(.054)		(.606)
Given deworming medication in past six months	.164	.089***	.212	.109***	[.347]	.948
		(.020)		(.025)		(.199)

Notes: Significance levels: *(10%), **(5%), ***(1%). The sample is based on households in which a woman was pregnant at baseline. In Panel A, stunted is a dummy indicating children with height-for-age-z-score (HAZ) two standard deviations or more below the WHO defined guidelines [WHO 2009]. In Panel B, the dietary diversity index is obtained from a 24-hour food recall module administered to the child's mother or main carer. Each meal consumed in the day before the interview from waking up to bedtime is recorded, and each ingredient is coded into categories. The index sums the number of food groups the child has received from the following seven food groups: 1. Grains, roots and tubers, 2. Legumes and nuts, 3. Dairy products, 4. Flesh foods, 5. Eggs, 6. Vitamin-A rich fruits and vegetables, 7. Other fruits and vegetables. Columns 1 and 3 report the mean (and standard deviation for continuous outcomes in braces) for control households at midline for the target child and their older sibling respectively. Columns 2 and 4 report ITT estimates at the two-year midline for the target child and their older sibling respectively. Each ITT is estimated using OLS, controlling for LGA and randomization tranche fixed effects. We report the p-values testing for equality between Columns 2 and 4 obtained using seemingly unrelated regression. The final column reports the ratio of older sibling ITTs as a proportion of the control group mean, divided by target child ITTs as a proportion of the control group mean. For these, we compute standard errors using the delta method. We test against the null that the ratio is equal to one. Standard errors clustered at the village level.

Table 7: Fertility, Mortality and Birth Spacing

Sample: Households with pregnant women at baseline (N=3688)

ITT estimates, standard errors in parentheses clustered by village, standard deviation in braces

	(1) Control Mean, Endline	(2) Endline
Panel A: Fertility, Mortality and Household Size		
Any child born between baseline and endline	.836	-.022
		(.041)
Number of children born between baseline and endline	1.58	-.068
	{.805}	(.080)
Any child born between baseline and endline that died	.175	-.004
		(.017)
Household size	9.09	-.044
	{4.1}	(.209)
Panel B: Birth Spacing		
Birth spacing between target child and their younger sibling (months)	31.6	-.773**
	{5.83}	(.349)
Birth spacing between target child and their younger sibling <= 24 months	.056	.034**
		(.014)

Notes: Significance levels: *(10%), **(5%), ***(1%). The sample is based on households in which a woman was pregnant at baseline. Column 1 shows the mean (and standard deviation for continuous outcomes in braces) value in control households at the four-year endline. Column 2 reports ITT estimates at endline, estimated using OLS, controlling for LGA and randomization tranche fixed effects. Standard errors are clustered at the village level throughout.

Table 8: Outcomes of the Younger Sibling

Sample: Households with pregnant women at baseline (N=3688)

ITT estimates, standard errors in parentheses clustered by village, standard deviation in braces

	Target Child		Younger Sibling			
	(1) Control Mean, Midline	(2) Midline	(3) Control Mean, Endline	(4) Endline	(5) Equality of ITT Estimates Across Siblings (2) = (4)	(6) Proportionate Impacts Across Siblings
Panel A: Physical Development						
Height-for-Age (HAZ)	-2.46	.217***	-1.78	.041	[.038]	.259
	{1.33}	(.069)	{1.61}	(.079)		(.478)
Stunted (HAZ < -2)	.662	-.057**	.458	.016	[.005]	-.391**
		(.024)		(.022)		(.635)
Panel B: Health and Nutrition						
Had illness or injury in past 30 days	.696	-.082***	.633	-.084***	[.947]	1.13
		(.023)		(.026)		(.447)
Had diarrhoea in past two weeks	.378	-.065***	.324	-.062**	[.930]	1.12
		(.022)		(.026)		(.537)
Dietary diversity	3.22	.349***	2.66	.494***	[.116]	1.71*
	{1.49}	(.074)	{1.67}	(.077)		(.390)
Panel C: Behavior Towards Child						
If had diarrhea in past two weeks:						
Anyone sought advice/treatment	.783	.058*	.811	.010	[.288]	.170
		(.031)		(.036)		(.598)
Given ORS for diarrhea	.408	.084**	.439	.141***	[.319]	1.56
		(.040)		(.046)		(.842)
Given deworming medication in past six months	.164	.089***	.210	.109***	[.424]	.961
		(.020)		(.021)		(.252)
Number of vaccinations	1.36	.402***	1.37	.347***	[.666]	.856
	{1.50}	(.107)	{1.81}	(.104)		(.295)

Notes: Significance levels: *(10%), **(5%), ***(1%). The sample is based on households in which a woman was pregnant at baseline. In Panel A, stunted is a dummy indicating children with height-for-age-z-score (HAZ) two standard deviations or more below the WHO defined guidelines [WHO 2009]. In Panel B, the dietary diversity index is obtained from a 24-hour food recall module administered to the child's mother or main carer. Each meal consumed in the day before the interview from waking up to bedtime is recorded, and each ingredient is coded into categories. The index sums the number of food groups the child has received from the following seven food groups: 1. Grains, roots and tubers, 2. Legumes and nuts, 3. Dairy products, 4. Flesh foods, 5. Eggs, 6. Vitamin-A rich fruits and vegetables, 7. Other fruits and vegetables. In Panel B, the number of vaccinations is the sum of the following vaccinations: BCG, Polio, DPT, measles, hepatitis B, yellow fever. Columns 1 and 3 report the mean (and standard deviation for continuous outcomes) for the target child in control households at the two-year midline, and for their younger sibling in control households at the four-year endline respectively. Columns 2 and 4 reports ITT estimates for the target child at midline, and their younger sibling at endline respectively. Each ITT is estimated using OLS, controlling for LGA and randomization tranche fixed effects. The ITTs for younger sibling control age dummies for the following categories: 0-6m, 6-12m, 12-18m, 18-24m. In Column 5 we report p-values on tests of equality of ITT estimates between the target child and their younger sibling using seemingly unrelated regressions. The final column reports the ratio of younger sibling ITTs as a proportion of the control group mean, divided by target child ITTs as a proportion of the control group mean. For these, we compute standard errors using the delta method. We test against the null that the ratio is equal to one. Standard errors clustered at the village level.

Table 9: Early Life Practices Towards the Younger Sibling

Sample: Households with pregnant women at baseline (N=3688)

ITT estimates, standard errors in parentheses clustered by village

	Target Child		Younger Sibling		
	(1) Control Mean, Midline	(2) Midline	(3) Control Mean, Endline	(4) Endline	(5) Equality of ITT Estimates Across Siblings (2) = (4)
A. Prenatal Practices					
Had antenatal care	.610	.102***	.696	.171***	[.013]
		(.036)		(.034)	
Born at health facility	.128	.053***	.151	.112***	[.018]
		(.020)		(.024)	
B. Perinatal Practices					
Fed colostrum	.381	.291***	.576	.278***	[.685]
		(.030)		(.029)	
Put to breast immediately	.443	.262***	.672	.203***	[.048]
		(.030)		(.028)	
C. Postnatal Practices					
Excl. breastfed for six months	.382	.045*	.685	.063**	[.584]
		(.024)		(.025)	

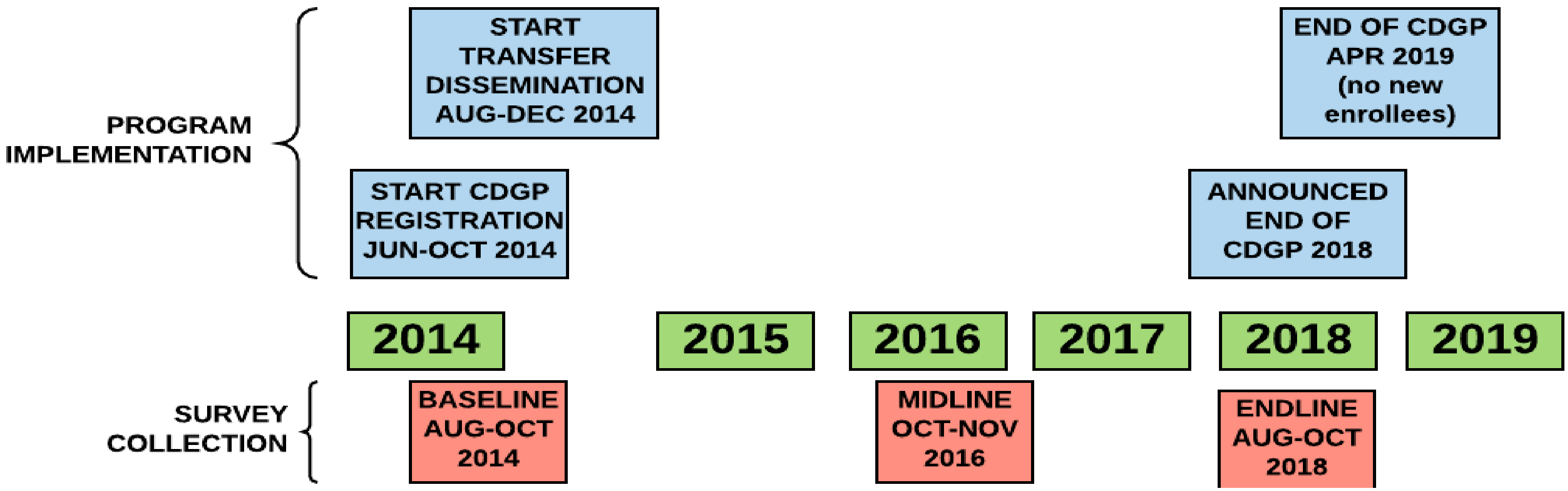
Notes: Significance levels: *(10%), **(5%), ***(1%). The sample is based on households in which a woman was pregnant at baseline. In Panels A, B and C, all practices are measured at the two-year midline for the target child and an endline for their younger sibling. Columns 1 and 3 report the mean for the target child in control households at the two-year midline, and for their younger sibling in control households at the four-year endline respectively. Columns 2 and 4 reports ITT estimates for the target child at midline, and their younger sibling at endline respectively. Each ITT is estimated using OLS, controlling for LGA and randomization tranche fixed effects. In Column 5 we report p-values on tests of equality of ITT estimates between the target child and their younger sibling using seemingly unrelated regressions.

Table 10: Internal Rate of Return

	Target Child	Target Child, Older and Younger Siblings	All Siblings Aged 5 and Under
	(1)	(2)	(3)
Social discount rate = 5%, Resource gains sustained for 5 years, earnings gains from age 16-60			
A. Cost parameters			
NPV Cash transfer			
Administrative costs of cash transfers	54	54	54
Administrative costs of information	54	54	54
B. Estimated total benefits			
NPV change in total resources year 1 until year 5			
NPV change earnings for children as a result of increased deworming treatment	122	361	599
Gain/cost ratio	**1.14**	**3.37**	**5.59**
Internal rate of return (IRR)	**.400%**	**4.44%**	**6.85%**

Notes: We assume the administrative costs of cash transfers and the administrative costs of information are each 10% of the value of cash transfers. All costs are presented in NPV terms with a 5% social discount rate. To calculate the impact on earnings from changes in deworming medication we use the estimates from Hamory et al. [2021]. We combine these estimates with our estimated ITT for the target child, separately for boys and girls, for their older sibling, again separately for boys and girls, and for their younger sibling, again separately for boys and girls. For other siblings in the household who are aged 0-5 at baseline, we use age-specific effects on deworming estimating by pooling information about the target child, older and younger siblings. The average family has two children aged 0-5 at baseline, who are aged two and three respectively.

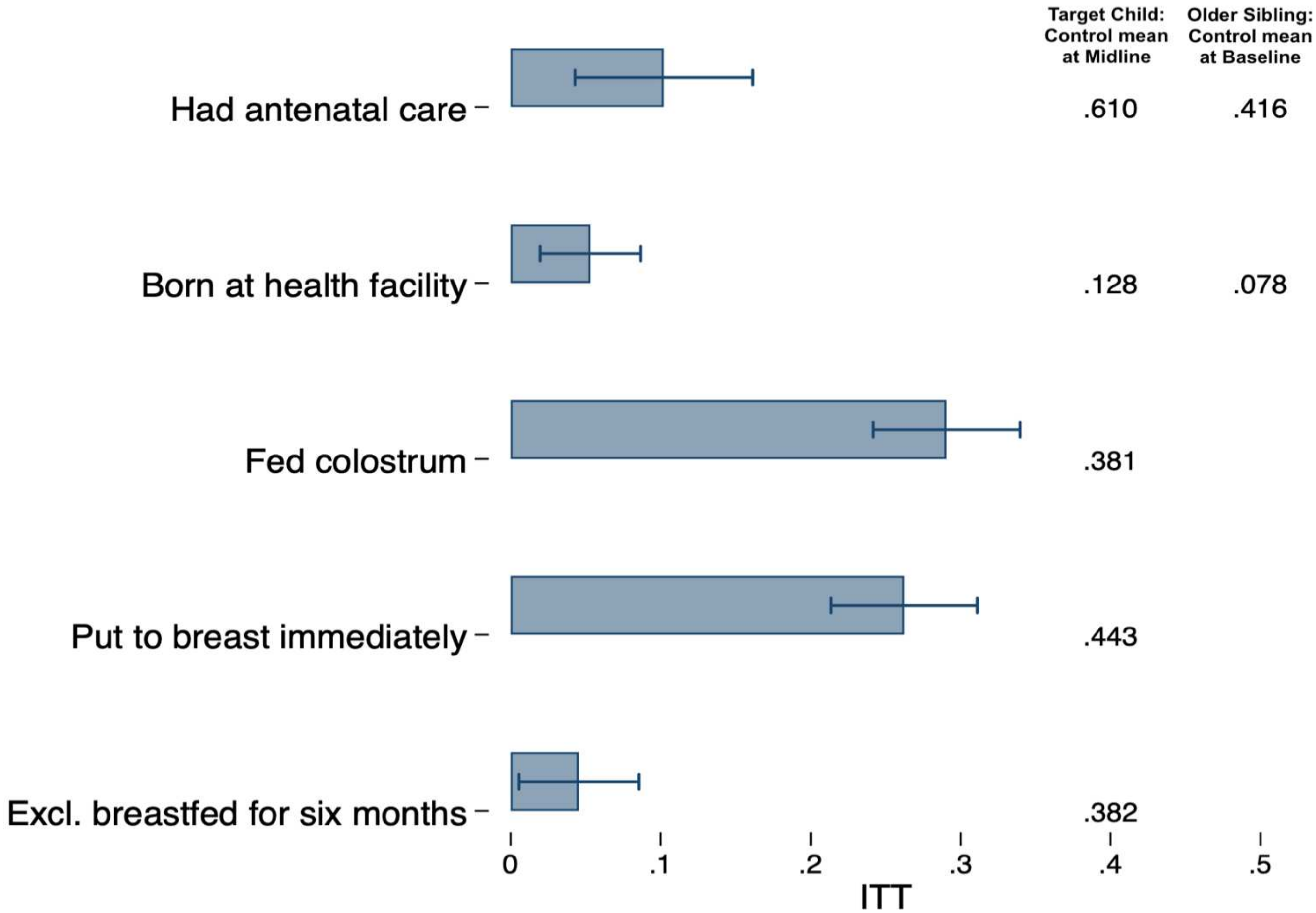
Figure 1: Timeline



Notes: This depicts a timeline of the evaluation process for the CDGP. The upper part of the figure shows program implementation: when the registration began, when cash transfers began, when the program end was announced, and when it stopped enrolling new participants. The lower part of the figure shows survey collection timings: when baseline, midline and endline surveys were collected.

Figure 2: Practices Towards the Target Child

Sample: Households with pregnant women at baseline (N=3688)



Notes: The sample is based on households in which a woman was pregnant at baseline. All practices are measured at the two-year midline for the target child. Each ITT is estimated using OLS, controlling for LGA and randomization tranche fixed effects. Standard errors clustered at the village level. The figure shows each ITT estimate alongside a 90% confidence interval. The right-hand side of the figure reports the control mean for target children at midline, and when available, the control mean for older siblings at baseline.

Table A1: Information Components of the Intervention

A. 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	**Infact 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

	Pregnant Woman at Baseline			Husband	Older Sibling
	(1) Baseline to Four-Year Endline	(2) Baseline to Four-Year Endline	(3) Baseline to Four-Year Endline	(4) Baseline to Four-Year Endline	(5) Baseline to Two-Year Midline
Treatment	.013	.014	.004	.010	-.080
	(.008)	(.008)	(.052)	(.053)	(.076)
Village insecure at midline	.034**	.037**	.059	.052	.922***
	(.013)	(.013)	(.031)	(.029)	(.016)
Village insecure at endline	.902***	.900***	.907***	.892***	
	(.011)	(.011)	(.024)	(.024)	
Treatment * Village insecure at endline			-.027	-.018	-.024
			(.033)	(.030)	(.018)
Treatment * Village insecure at midline			-.011	-.008	
			(.025)	(.025)	
Randomization Strata	Yes	Yes	Yes	Yes	Yes
HH baseline controls	No	Yes	Yes	Yes	Yes
Attrition rate	.227	.227	.227	.241	.198
Joint p-value on interactions			.373	.729	.357
Observations	3688	3687	3687	3687	2596

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. Attrition takes the value of one if the subject surveyed at baseline was not surveyed at endline (except for attrition of the older sibling in Column 5, that is measured at midline). The sample in Columns 1 to 3 are women pregnant at baseline. In Column 4, the sample is husbands of women who were pregnant at baseline. In Column 5, the sample is the older sibling in households where the woman was pregnant at baseline. All Columns include treatment status, village insecurity status, at midline and endline, LGA and randomization tranche fixed effects. Column 2 adds controls for baseline characteristics of the household and mother: household size, poverty score (0-100), equivalised daily per capita expenditures in USD PPP, dummy for whether had not enough food in past 12 months, the number of children aged 0 to 2, the number of children aged 3 to 5, a dummy for being in polygamous relationship, and the mothers age in years. In Column 3 onwards the interactions are between treatment and the household baseline variables and between treatment and the village insecurity dummies. 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: Within Household R-squared, Control Group

Sample: Control households with pregnant women at baseline (N= 1186)

	(1) Target Child and Older Sibling	(2) Target Child and younger Sibling	(3) All Siblings
Panel A: Physical Development			
Height-for-Age (HAZ)	.318	.239	.298
Panel B: Health and Nutrition			
Had illness or injury in past 30 days	.364	.324	.430
Had diarrhoea in past two weeks	.417	.350	.443
Dietary diversity	.184	.199	.243
Panel C: Behavior Towards Child			
If had diarrhea in past two weeks:			
Anyone sought advice/treatment	.122	.348	.337
Given ORS for diarrhea	.109	.204	.268
Given deworming medication in past six months	.192	.248	.282
Panel D: Practices Towards Children			
Had antenatal care		.226	
Born at health facility		.399	
Fed colostrum		.394	
Put to breast immediately		.440	
Excl. breastfed for six months		.376	

Notes: The sample is based on control households in which a woman was pregnant at baseline. Each cell reports (1-R-squared) of a regression of a given outcome on dummies for child age and household fixed effects. The age dummies controlled for are the following categories: 0-6m, 6-12m, 12-18m, 18-24m, 24-30m, 30-36m, 36-42m, 42-48m, 48-54m, 54-60m, 60-66m, 66-72m, 72m+. In Column 1 we pool the data for the target child and the older sibling at midline. In Column 2 we pool that data from the target child at midline and their younger sibling at endline. In Column 3 we pool the data from the older sibling at midline, the target child at midline, and the younger sibling at endline. In Panel B, the dietary diversity index is obtained from a 24-hour food recall module administered to the child's mother or main carer. Each meal consumed in the day before the interview from waking up to bedtime is recorded, and each ingredient is coded into categories. The index sums the number of food groups the child has received from the following seven food groups: 1. Grains, roots and tubers, 2. Legumes and nuts, 3. Dairy products, 4. Flesh foods, 5. Eggs, 6. Vitamin-A rich fruits and vegetables, 7. Other fruits and vegetables. Information on peri-natal practices and ASQ scores are unavailable for the older sibling.

Table A4: Older Sibling Outcomes by Gender

Sample: Households with pregnant women at baseline (N=3688)

ITT estimates at midline, standard errors in parentheses clustered by village

	Boys		Girls	
	(1) Target Child	**(2) Older Sibling**	**(3) Target Child**	**(4) Older Sibling**
Panel A: Physical Development				
Height-for-Age (HAZ)	.256***	-.097	.166**	-.013
	(.089)	(.120)	(.084)	(.091)
Stunted (HAZ < -2)	-.050*	.042	-.067**	-.033
	(.030)	(.054)	(.033)	(.046)
Panel B: Health				
Had illness or injury in past 30 days	-.093***	-.031	-.067**	-.055
	(.030)	(.035)	(.031)	(.034)
Had diarrhoea in past two weeks	-.082***	-.066**	-.047	-.038
	(.030)	(.027)	(.031)	(.025)
Dietary diversity	.434***	.215***	.258***	.231**
	(.091)	(.077)	(.093)	(.093)
Panel C: Behavior Towards Child				
If had diarrhea in past two weeks:				
Anyone sought advice/treatment	.032	.037	.097**	.110*
	(.037)	(.047)	(.045)	(.061)
Given ORS for diarrhea	.098*	.035	.077	.179**
	(.051)	(.075)	(.059)	(.075)
Given deworming medication in past six months	.110***	.105***	.068**	.121***
	(.024)	(0.031)	(.027)	(.033)

Notes: Significance levels: *(10%), **(5%), ***(1%). The sample is based on households in which a woman was pregnant at baseline. In Panel A, stunted is a dummy indicating children with height-for-age-z-score (HAZ) two standard deviations or more below the WHO defined guidelines [WHO 2009]. In Panel B, the dietary diversity index is obtained from a 24-hour food recall module administered to the child's mother or main carer. Each meal consumed in the day before the interview from waking up to bedtime is recorded, and each ingredient is coded into categories. The index sums the number of food groups the child has received from the following seven food groups: 1. Grains, roots and tubers, 2. Legumes and nuts, 3. Dairy products, 4. Flesh foods, 5. Eggs, 6. Vitamin-A rich fruits and vegetables, 7. Other fruits and vegetables. Columns 1 and 3 report ITT estimates at midline for the target child. Columns 2 and 4 report ITT estimates at midline for their older sibling. Each ITT is estimated using OLS, controlling for LGA and randomization tranche fixed effects, interacting each regressor with a dummy for child gender being male. Standard errors clustered at the village level.

Table A5: Maternal Time Allocation, Target Child and their Older Sibling

Sample: Households with pregnant women at baseline (N=3688)

ITT estimates at midline, standard errors in parentheses clustered by village

	Target Child		Older Sibling		
	(1) Control Mean	(2) Midline	(3) Control Mean	(4) Midline	(5) Equality of ITT Estimates Across Siblings (2) = (4)
Panel A: Time Spent with Mother					
0-2 hours	.023	-.002	.170	-.001	[.961]
		(.006)		(.018)	
2-5 hours	.145	-.020	.480	-.017	[.918]
		(.016)		(.022)	
5 or more hours	.832	.022	.351	.018	[.871]
		(.016)		(.023)	
Panel B: Time Spent Playing with Mother					
0-2 hours	.736	-.064**	.935	-.034**	[.195]
		(.027)		(.015)	
2-5 hours	.215	.011	.059	.027*	[.474]
		(.023)		(.014)	
5 or more hours	.049	.053***	.006	.007	[.002]
		(.015)		(.006)	

Notes: Significance levels: *(10%), **(5%), ***(1%). The sample is based on households in which a woman was pregnant at baseline. Columns 1 and 3 report the mean for control households at midline for the target child and their older sibling respectively. Columns 2 and 4 report ITT estimates at the two-year midline for the target child and their older sibling respectively. Each ITT is estimated using OLS, controlling for LGA and randomization tranche fixed effects. We report the p-values testing for equality between Columns 2 and 4 obtained using seemingly unrelated regression. Standard errors clustered at the village level.

Table A6: Within Village Spillovers for Older Siblings in Never Eligible Households

Sample: Households without a never pregnant women (N=834)

ITT estimates, standard errors in parentheses clustered by village, standard deviation in braces

	Older Sibling	
	(1) Control Mean, Midline	**(2) Midline**
A. Share of All Messages Recalled, Mother	.039	.232***
		(.020)
B: Knowledge of Child Practices, Mother		
Knowledge index	0	.531***
	{1}	(.098)
Panel C: Physical Development		
Height-for-Age (HAZ)	-2.53	.086
	{1.39}	(.153)
Stunted (HAZ < -2)	.680	-.012
		(.054)
Panel D: Health and Nutrition		
Had illness or injury in past 30 days	.679	-.072**
		(.036)
Had diarrhoea in past two weeks	.192	-.015
		(.037)
Dietary diversity	3.71	.223**
	{1.08}	(.099)
Panel E: Behavior Towards Child		
If had diarrhea in past two weeks:		
Anyone sought advice/treatment	.854	.002
		(.069)
Given ORS for diarrhea	.439	.051
		(.103)
Given deworming medication in past six months	.227	.145***
		(.045)

Notes: Significance levels: * (10%), ** (5%), ***(1%). The sample is based on households in which a woman was not pregnant at baseline, nor was she pregnant at midline or endline. Hence the household was never eligible for the receipt of cash transfers. Panel A shows the share of all messages recalled by the woman. Panel B shows impacts on a Knowledge index constructed from a group of indicators using the method in Anderson (2008). The indicators used are about parental beliefs over the following practices: it is good practice to bring the baby to a health check-up even if healthy, the best place to give birth is a health facility, it is best to start breastfeeding immediately after birth, the baby should not receive other liquids on the first day, colostrum is good for the baby, the baby should not be given water when the temperature outside is hot, the baby should be breastfed exclusively for the first six months of life. The Knowledge index is standardized to have mean zero and variance one in the Control group. In Panel C, stunted is a dummy indicating children with height-for-age-z-score (HAZ) two standard deviations or more below the WHO defined guidelines [WHO 2009]. In Panel D, the dietary diversity index is obtained from a 24-hour food recall module administered to the child's mother or main carer. Each meal consumed in the day before the interview from waking up to bedtime is recorded, and each ingredient is coded into categories. The index sums the number of food groups the child has received from the following seven food groups: 1. Grains, roots and tubers, 2. Legumes and nuts, 3. Dairy products, 4. Flesh foods, 5. Eggs, 6. Vitamin-A rich fruits and vegetables, 7. Other fruits and vegetables. A minimum dietary diversity indicates consuming at least four food groups. Column 1 reports the mean (and standard deviation for continuous outcomes in braces) for the older sibling in control households at the two-year midline. Column 2 reports ITT estimates for the older sibling at midline. Each ITT is estimated using OLS, controlling for LGA and randomization tranche fixed effects. Standard errors clustered at the village level.

Table A7: Heterogeneous Outcomes for Younger Sibling of Target Child

Sample: Households with pregnant women at baseline (N=3688)

ITT estimates, standard errors in parentheses clustered by village

	(1) Mother Owns Livestock at Baseline	(2) Mother Does not Own Livestock at Baseline	p-value (1)=(2)	(3) Has at least one older sister (7-14)	(4) Has only older brothers	p-value (3)=(4)	(5) Boys	(6) Girls	p-value (5)=(6)
Panel A: Birth Spacing									
Birth spacing between target child and their younger sibling (months)	-.687**	-.570	[.805]	-.519	-.468	[.932]	-.506	-.943***	[.325]
	(.318)	(.392)		(.381)	(.441)		(.318)	(.358)	
Birth spacing between target child and their younger sibling <= 24 months	.039**	.013	[.315]	-.023	.064***	[.001]	.025	.033*	[.733]
	(.017)	(.021)		(.020)	(.017)		(.017)	(.019)	
Panel B: Physical Development									
Height-for-Age (HAZ)	.080	-.011	[.480]	-.003	-.054	[.743]	.025	.050	[.846]
	(.100)	(.104)		(.119)	(.118)		(.106)	(.097)	
Stunted (HAZ < -2)	.035	-.016	[.287]	-.006	.029	[.518]	.036	-.005	[.325]
	(.029)	(.037)		(.040)	(.036)		(.031)	(.030)	

Notes: Significance levels: *(10%), **(5%), ***(1%). The sample is based on households in which a woman was pregnant at baseline. In Panel B, stunted is a dummy indicating children with height-for-age-z-score (HAZ) two standard deviations or more below the WHO defined guidelines [WHO 2009]. Columns 1 and 2 show ITTs for the younger sibling at endline for mothers who own livestock at baseline and mothers who do not. Each ITT is estimated using OLS, controlling for LGA, randomization tranche fixed effects, age dummies, all interacted with an indicator for owning livestock at baseline. Age dummies are for the following categories: 0-6m, 6-12m, 12-18m, 18-24m. Columns 3 to 4 show ITTs for the younger sibling at endline for those who have at least one older sister (aged 7-14yrs) and those who only have older brothers. Each ITT is estimated using OLS, controlling for LGA, randomization tranche fixed effects, age dummies, all interacted with an indicator for having at least one older sister. Columns 5 to 6 show ITTs for the younger sibling at endline for boys and girls (where Panel A refers to the gender of the target child and Panel B refers to the gender of the younger sibling). Each ITT is estimated using OLS, controlling for LGA, randomization tranche fixed effects, age dummies, all interacted with an indicator for being male. At baseline, 2149 women own livestock and 1539 do not own livestock. 954 younger siblings have at least one older sister, 865 have only older brothers. There are 929 male younger siblings and 957 female younger siblings. Standard errors clustered at the village level throughout.

Table A8: Practices Towards the Younger Sibling of Target Child

Sample: Households with pregnant women at baseline (N=3688)

ITT estimates, standard errors in parentheses clustered by village

	(1) Mother Owns Livestock at Baseline	(2) Mother Does not Own Livestock at Baseline	p-value (1)=(2)	(3) Has at least one older sister (7-14)	(4) Has only older brothers	p-value (3)=(4)	(5) Boys	(6) Girls	p-value (5)=(6)
A. Prenatal Practices									
Had antenatal care	.158***	.184***	[.498]	.178***	.182***	[.929]	.181***	.164***	[.639]
	(.041)	(.037)		(.046)	(.043)		(.039)	(.038)	
Born at health facility	.072***	.169***	[.009]	.113***	.086**	[.541]	.094***	.131***	[.308]
	(.029)	(.031)		(.034)	(.035)		(.031)	(.029)	
B. Perinatal Practices									
Fed colostrum	.244***	.320***	[.103]	.278***	.247***	[.565]	.250***	.308***	[.130]
	(.036)	(.038)		(.043)	(.042)		(.035)	(.035)	
Put to breast immediately	.180***	.228***	[.245]	.212***	.164***	[.362]	.161***	.244***	[.031]
	(.036)	(.033)		(.039)	(.042)		(.033)	(.034)	
C. Postnatal Practices									
Excl. breastfed for six months	.044	.086**	[.341]	.114***	.052	[.232]	.036	.091***	[.095]
	(.032)	(.034)		(.037)	(.040)		(.028)	(.032)	

Notes: Significance levels: * (10%), ** (5%), ***(1%). The sample is based on households in which a woman was pregnant at baseline. Columns 1 and 2 show ITTs for Younger Sibling at Endline for mothers who own livestock at baseline and mothers who do not. Each ITT is estimated using OLS, controlling for LGA, randomization tranche fixed effects, all interacted with an indicator for owning livestock at baseline. We report p-values testing for differences between column 1 and 2. Columns 3 to 4 show ITTs for Younger Sibling at Endline for those who have at least one older sister (aged 7-14yrs) and those who only have older brothers. Each ITT is estimated using OLS, controlling for LGA, randomization tranche fixed effects, all interacted with an indicator for having at least one older sister. We report p-values testing for differences between column 3 and 4. Columns 5 to 6 show ITTs for Younger Sibling at Endline for boys and girls. Each ITT is estimated using OLS, controlling for LGA, randomization tranche fixed effects, all interacted with an indicator for being male. We report p-values testing for differences between column 5 and 6. In Columns 1 to 2, at baseline, 2149 women own livestock and 1539 do not own livestock. In Columns 3 to 4, 954 younger siblings have at least one older sister, 865 have only older brothers. In Columns 5 to 6, there are 929 male younger siblings and 957 female younger siblings. Standard errors clustered at the village level.

Table A9: Water

Sample: Households with pregnant women at baseline (N=3688)

ITT estimates, standard errors in parentheses clustered by village

	(1) Control Mean ML	(2) Midline	(3) Control Mean Endline	(4) Endline
A. Household Level Outcome				
Has access to safe water source	.150	.000	.194	.040
		(.029)		(.036)

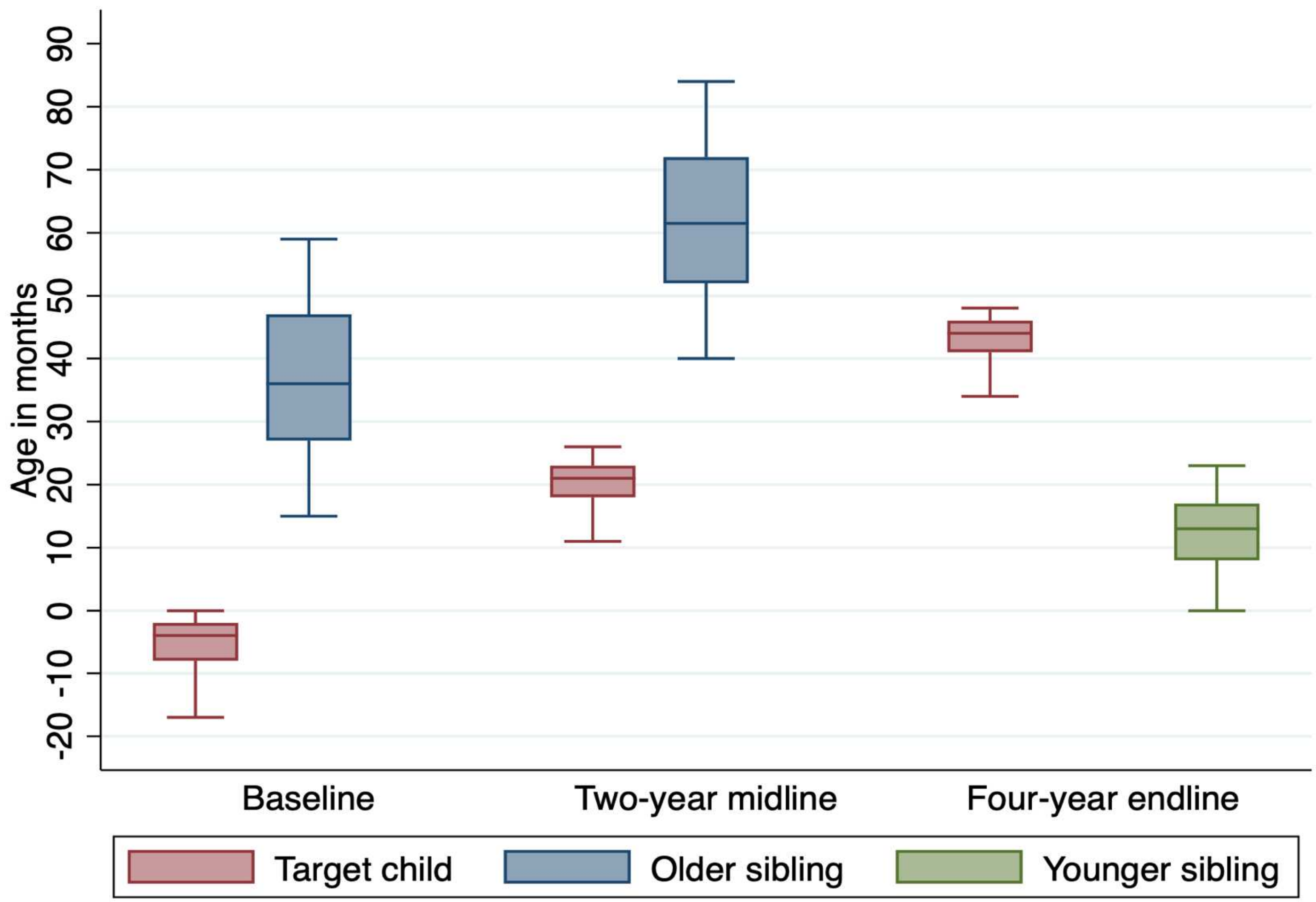
B. Early Life Practices Towards the Younger Sibling, Endline

	Has Access to Safe Water Source	Does Not Have Access to Safe Water Source	Test Equality
Prenatal Practices			
Had antenatal care	.062*	.203***	[.007]
	(.037)	(.039)	
Born at health facility	.192***	.085***	[.048]
	(.048)	(.026)	
Perinatal Practices			
Fed colostrum	.303***	.266***	[.527]
	(.053)	(.032)	
Put to breast immediately	.196***	.199***	[.946]
	(.048)	(.031)	
Postnatal Practices			
Excl. breastfed for six months	.123***	.045	[.157]
	(.048)	(.029)	

Notes: Significance levels: *(10%), **(5%), ***(1%). The sample is based on households in which a woman was pregnant at baseline. In Panel A, the household outcome is having access to safe drinking water. This is defined as them reporting that their main source of drinking water is piped water into structure/piped water to yard or plot/public tap or standpipe. Columns 1 and 3 show the mean outcome in control households at the two-year midline and four-year endline respectively. Column 2 and 3 report ITT estimates at midline and endline respectively.These are estimated using OLS, controlling for LGA, randomization tranche and wave fixed effects, pooling across midline and endline survey waves and interacting treatment status with wave fixed effects. Standard errors are clustered at the village level throughout. In Panel B, we show ITT impacts on practices towards the younger sibling at endline, split between households with and without access to safe drinking water. Each ITT is estimated using OLS, controlling for LGA and randomization tranche fixed effects, fully interacted with having access to a safe water source. From this specification we report the resulting p-values testing for equality between having access to a safe water source and not having access. Standard errors are clustered at the village level throughout. 633 households have access to safe water at endline, 2216 do not have access to safe water at endline.

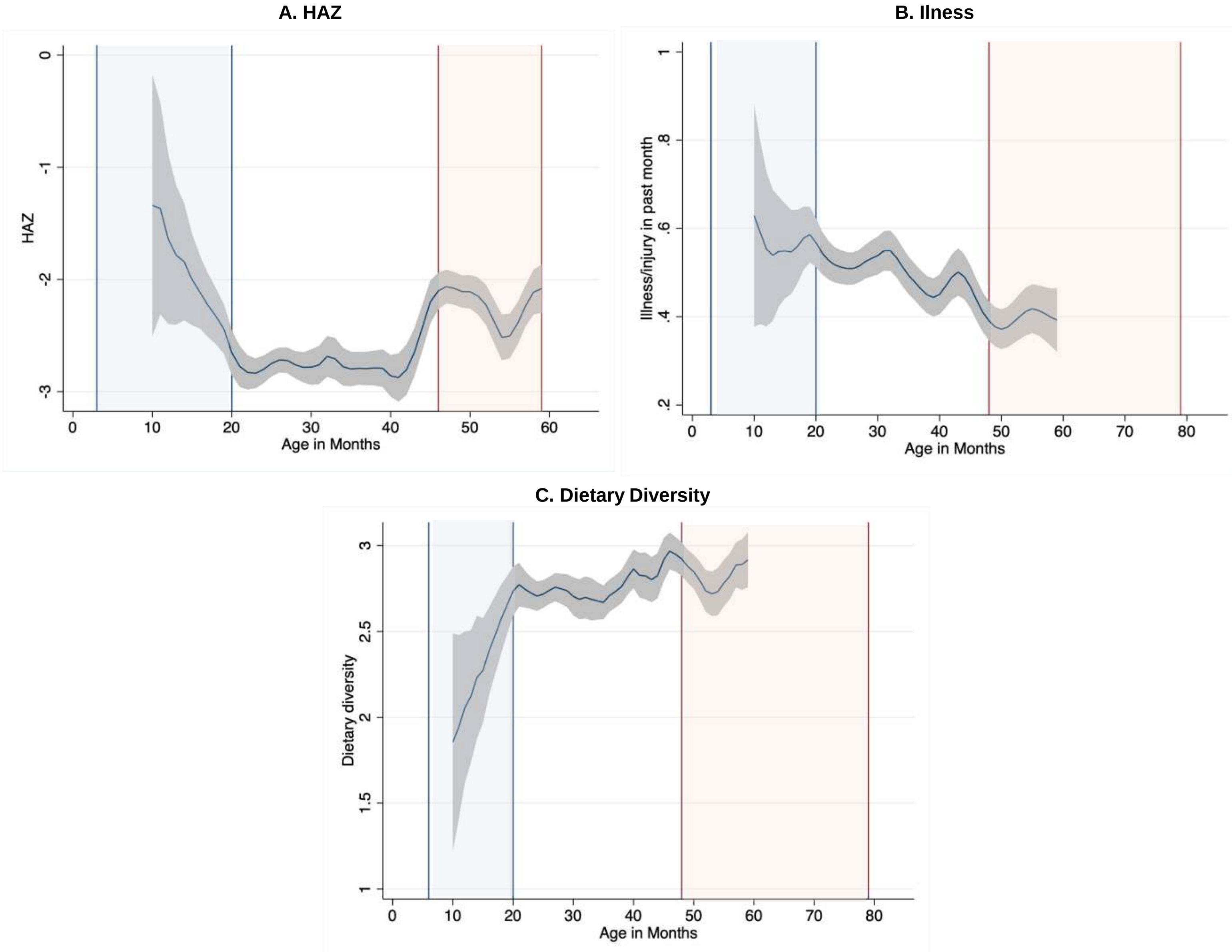
Figure A1: Age of Children, by Survey Wave

Sample: Households with pregnant women at baseline (N=3688)



Notes: We plot the distribution of children's age at baseline, the two-year midline and the four-year endline. We show the 10th percentile, interquartile range, median and 90th percentiles of each age distribution. The target child is *in utero* at baseline. This is computed by subtracting elapsed time between baseline and midline interview dates from reported age at midline. Ages are trimmed below the 1st and above the 99th percentile for each child at each wave.

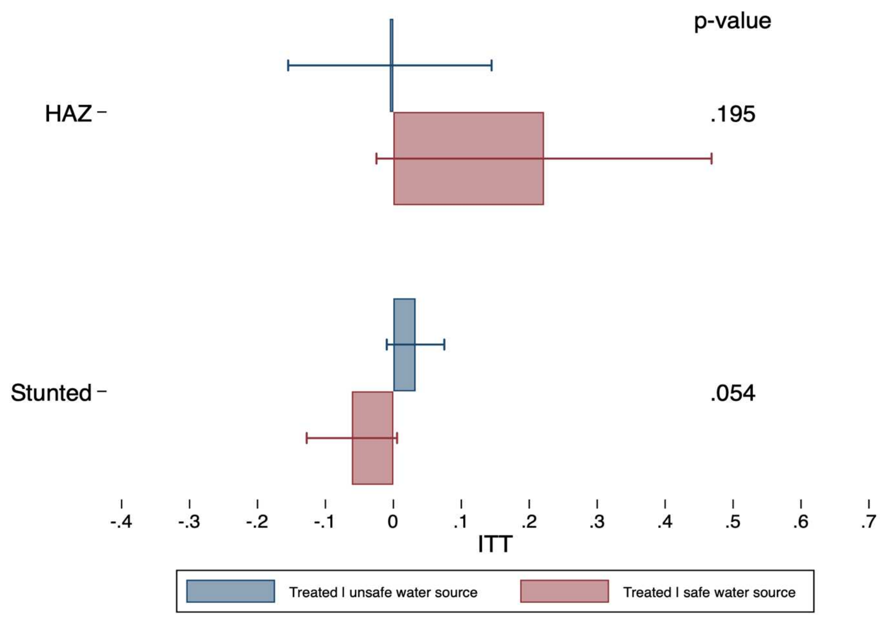
Figure A2: Age Profiles of Outcomes



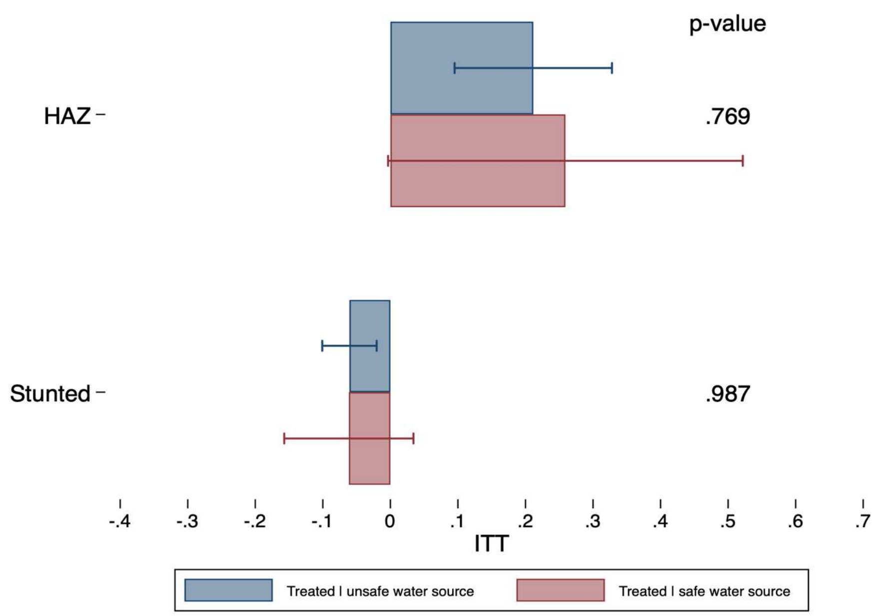
Notes: Each Panel shows the age profiles of different outcomes for the older sibling at baseline in treatment and control groups, based on households in which a woman was pregnant at baseline. In each Panel, the red shaded region covers the 10th to 90th percentile age range for older siblings at midline. The blue shade region covers the 10th to 90th percentile age range for the younger siblings at endline.

Figure A3: Outcomes by Sibling and Access to Safe Water

A. Younger Sibling



B. Target Child



Notes: Panels A and B show ITT estimates for the younger sibling at endline and the target child at midline, respectively. Each set of estimates are split by whether the household has access to safe drinking water. A household is defined to have access to safe water if they report that their main source of drinking water is piped water into structure/piped water to yard or plot/public tap or standpipe. A household is defined to not have access to safe water if they report that their main source of drinking water is a borehole/tubewell; a dug well; a spring; rainwater collection; tanker truck; bottled/sachet; cart with small tank drum; surface water. Each ITT is estimated using OLS, controlling for LGA and randomization tranche fixed effects. In addition, ITTs for Younger Sibling control for age, using age dummies for the following categories: 0-6m, 6-12m, 12-18m, 18-24m. The figure reports 90% confidence intervals as well as p-values that test for equality of the ITTs by safe water source. Standard errors are clustered at the village level throughout.