Maternal cash transfers for gender equity and child development: Experimental evidence from India

Jeffrey Weaver*
University of Southern California

Sandip Sukhtankar†
University of Virginia

Paul Niehaus‡
UC San Diego

Karthik Muralidharan§
UC San Diego

August 15, 2025¶

Abstract

Cash transfer programs to women in India now reach over 130 million beneficiaries at an annual cost of 0.6% of GDP, yet evidence on their effects remains limited. We study the impact of unconditional transfers to new mothers in India using a large-scale randomized evaluation. Treated households saw a 9.6–15.5% increase in calorie intake for mothers and children, along with gains in dietary diversity and nutrient consumption. Gender disparities in food consumption narrowed. We find significant gains in children's functional development. While anthropometric indicators did not improve on average, they may have in areas with better sanitation.

JEL codes: D13, I12, I15, I38, O15, Q53
Keywords: cash transfers, child development, malnutrition, India

*University of Southern California and BREAD, jbweaver@usc.edu

†University of Virginia, JPAL, NBER and BREAD, sandip.sukhtankar@virginia.edu

‡UC San Diego, JPAL, NBER and BREAD, pniehaus@ucsd.edu

§UC San Diego, JPAL, NBER and BREAD, kamurali@ucsd.edu

¶We thank the Government of Jharkhand State Nutrition Mission for their support of this project, and are particularly grateful to Mukhmeet Bhatia, Dr. DK Saxena, and Ajay Verma. We thank Radu Ban, Abhijit Banerjee, Prashant Bharadwaj, Hoyt Bleakley, Joe Cummins, Ajinkya Keskar, Abhijeet Singh, Dean Spears, Tom Vogl, and numerous seminar and conference participants for helpful comments and suggestions. Hannah Blackburn, Thomas Brailey, Soala Ekine, Burak Eskici, Stefan Faridani, Simoni Jain, Pankhuri Jha, Wilson King, Prerna Kundu, Xinyi Liu, Frances Lu, Avantika Prabhakar, Sabareesh Ramachandran, Akanksha Saletore, Mayank Sharma, and Prashansa Srivastava provided excellent research assistance and field support. This study was registered with the AEA Trial Registry (RCT ID: 0002899). It was approved by the Institutional Review Boards of the University of California, San Diego and the Institute for Financial Management and Research, India. We thank the Bill and Melinda Gates Foundation and the Cash Transfers for Child Health Initiative at the Abdul Latif Jameel Poverty Action Lab for funding this study. All views expressed are those of the authors and not any of the funders or institutions with which they are affiliated.

1 Introduction

Reducing child malnutrition is a critical global development goal. It is important both intrinsically for the well-being of affected children and instrumentally for boosting long-term human capital and incomes in low- and middle-income countries (LMICs). Reflecting this, the UN Sustainable Development Goals aim to end child malnutrition by 2030. Any strategy to achieve this must address India, which is home to 24.6% of all stunted children globally (World Health Organization, 2023). As of 2019, 36% of Indian children under five were stunted (Ministry of Health and Family Welfare, 2022a), with malnutrition rates persistently higher than in many *poorer* sub-Saharan African countries (Ramalingaswami et al., 1996; Klasen, 2008; World Health Organization, 2023). The World Bank has called child malnutrition India's "silent emergency," and recent projections suggest stunting is likely to remain above 22% even in 2047.[1]

Debates over policy have unfolded alongside debates over why progress has been so limited. Since 1984, real per-capita incomes in India have quintupled, yet stunting has declined only modestly. Potential explanations span the causal chain from income to child development, including: (a) rising average incomes may not have proportionately benefited the poor, among whom malnutrition is concentrated; (b) the income elasticity of calorie consumption may be low (Deaton and Drèze, 2009); (c) consumed food may be low in nutritional value, either due to preferences for taste over nutrition (Banerjee and Duflo, 2012) or poor access to nutritious foods (Khera, 2014); (d) intra-household inequality in food allocation may disadvantage women and higher birth-order children (Jayachandran and Pande, 2017); and (e) poor sanitation may limit the health benefits of improved diets (Coffey and Spears, 2017). Uncertainty about the relative importance of these factors complicates policy design. While well-targeted cash transfers may address point (a), questions about the roles of (b)-(e) have fueled an inconclusive debate on the efficacy of such interventions (Narayanan and Saha, 2020). A key limitation in this literature is the lack of studies with both exogenous variation in income and detailed measurement of each intermediate step in the causal chain from income to child outcomes.

Child malnutrition in India is also likely to be intertwined with low female control over household resources, as hypothesis (d) suggests. India ranked 131st out of 148 countries on the World Economic Forum's Global Gender Gap Index 2025 (World Economic Forum, 2025). Gender inequity within households is an intrinsically important concern, underscored by recent estimates showing large numbers of undernourished people—predominantly women and children—in non-poor households (Calvi, 2020; Brown et al., 2021). It may also inhibit child development through channels such as reduced spending on child healthcare and nutrition, and poor maternal health, which is strongly linked to low child birth weight—itself a key predictor of stunting.

Against this backdrop, India has seen a remarkable surge in cash transfers programs targeted to women in recent years, from near zero in 2018 to 0.6% of GDP by 2024 (The Economist, 2025). In principle, such transfers, especially when targeted to mothers, could yield triple dividends:

[1]See World Bank (2013) for the quote, and Muralidharan (2024) for projections based on the historical time-series income elasticity of stunting reduction in India.

directly improving women's welfare, enhancing maternal health and nutrition (and thus the health of breastfed infants, as well as that of future children), and improving current child outcomes by shifting household resources toward children. Yet it remains an open question whether these transfers actually lead to more equitable intra-household resource allocations or are used inequitably like other sources of income often are.[2]

With these questions as motivation, this paper reports results from a large-scale randomized evaluation of a maternal cash transfer program in the Indian state of Jharkhand. The setting is 480 public early childhood care centers (anganwadi centers, or AWCs), representative of 90% of the state. AWCs provide health, nutrition, and development services to mothers and children under India's Integrated Child Development Services (ICDS) program. For this study, AWCs first registered pregnant women in their catchment areas, yielding a study sample of ∼2,400 women. Half the AWCs were then randomly selected, and starting from their child's birth, registered mothers at these AWCs received monthly payments of Rs. 500, equal to ∼10% of average household consumption. Transfers continued for two years for a total value of Rs. 12,000 (USD $170) and at an annual value similar to that of India's flagship maternal transfer program (Pradhan Mantri Matru Vandana Yojana). Mothers also received messages encouraging them to use the funds for nutritious food for themselves and their children, a typical feature of this type of program. Messaging was implemented in a light-touch, low-cost way to facilitate scalability.

Our data collection captured each step of the causal chain from increased income to child development outcomes. We collected unusually detailed consumption data, recording the exact quantity of each ingredient in all foods consumed in the day prior to the survey. We combined this with measurements of the quantity of food consumed by targeted household members, allowing us to construct precise measures of nutrient intake for these individuals and the household as a whole. Child development outcomes were measured at approximately ages one and three to capture effects both during and after the transfer period.[3]

We report five main sets of results. First, transfers significantly increased household food expenditure and nutritional intake for both mothers and children. Total household food expenditure increased by over 11%, and the intended beneficiaries (mothers and target children) consumed at least a proportionate share of this increase. Their caloric intake rose by 9% in the first year, and 14% in the second. Food quality also improved, as measured by dietary diversity and consumption of key nutrients such as protein and iron.

Second, the treatment improved intra-household equity. Since maternal calorie consumption was lower than the household average, proportionate increases in *absolute* calorie consumption for mothers implied a greater *relative* increase for them. This resulted in more equal food consumption.

[2]The tension is reflected in Duflo's (2012) observation that "welfare agencies that restrict credit or transfers to women on the grounds that the money will be put to use germane to development implicitly recognize that women are not entirely powerless. If women were powerless, then the money would be immediately appropriated by their spouses, and we would see no impact of distributing the money to women rather than to men. Conversely, if households were harmonious entities where everyone had the same preferences and desires, then the nominal ownership of money would not matter within the household."

[3]Data collection after two years of the program was affected by the COVID pandemic. See Section 3.2.

Further, the treatment effect on maternal calorie intake in year 2 was nearly double the effect in year 1. This is consistent with growing empowerment over time, perhaps because women became more accustomed to receiving and collecting money (Field et al., 2021).

Third, despite these improvements in diet, there were no significant gains in standard child anthropometric measures for targeted children (height- and weight-for age z-scores, or HAZ and WAZ). Our estimates are precise enough to rule out gains larger than 0.10 standard deviations (SD). The point estimates fall well within the confidence intervals from two recent meta-analyses of the effects of cash transfers in LMICs, which report small and not always significant effects of cash transfers on child anthropometrics (Crosta et al., 2024; Manley et al., 2022). However, our estimates are smaller than some positive recent estimates from other LMIC settings, such as Carneiro et al. (2021) in Nigeria and Field and Maffioli (2025) in Myanmar.

Fourth, consistent with Coffey and Spears (2017), we find suggestive evidence that sanitation conditions may mediate the translation of improved nutrient intake into child anthropometric outcomes. Comparing children in areas with better and worse sanitation (measured primarily by neighborhood rates of open defecation), we see no difference in treatment effects on nutritional intake, but find larger child anthropometric gains in treated areas with better sanitation environments, with this difference being significant for WAZ. Interestingly, mean WAZ increased by 0.11SD for non-targeted *older* children in treated households, without significant heterogeneity by sanitation. The former result is consistent with increased caloric and nutrient intake by *all* household members, and the latter suggests that the impact of poor sanitation in weakening the translation of increased nutrient intake into child growth may be more pronounced for younger children.

Fifth, and in contrast with the effects on anthropometrics, we find significant average improvements in children's functional development. Using the standard Ages and Stages Questionnaire (ASQ-3) to measure age-appropriate developmental milestones (Bricker et al., 1999), we find treatment effects of 0.12SD at age 3 ($p < 0.01$), with gains in parental reports of cognitive, gross motor, and fine motor skills. These are the first experimental estimates of the impacts of income transfers on child functional development in India, and the effect sizes are similar to estimates for cash transfer interventions in the literature from Latin America.[4]

Our results contribute to several literatures spanning cash transfers, nutrition, gender, and child development, and also have direct policy implications (discussed further in Section 5).

Our first contribution is to present, to our knowledge, the first large-scale experimental evidence on the impacts of unconditional cash transfers in India. Until recently, such transfers were rare in India (Sukhtankar, forthcoming), perhaps reflecting paternalistic concerns that the poor may spend such transfers "sub-optimally" (e.g., Khera (2014)). However, following substantial investments in infrastructure for electronic payments, India has seen a sharp increase in cash transfers to women in recent years. Such programs now reach over 130 million beneficiaries, at a cost of 2

[4]In Latin America, Fernald et al. (2008), Paxson and Schady (2010), Fernald and Hidrobo (2011), and Macours et al. (2012) find effects ranging between 0.1 and 0.23SD on various child development outcomes from larger transfers (ranging from $170 to $800, compared to $150 here) in Mexico, Ecuador, and Nicaragua. In Nepal, Levere et al. (2024) find no significant effects on the same ASQ measure, albeit with smaller cash transfer amounts.

trillion rupees (USD 23 billion) (The Economist, 2025). Yet, there is very limited evidence on the impacts of such transfers in India (Niehaus and Suri, forthcoming),[5] and it is unclear whether results from other contexts with different patterns of gender equity and intra-household resource allocation will translate to India.[6] Our results show that cash transfers to women significantly improved several intermediate nutrition outcomes that policymakers care about—including calorie consumption, dietary diversity, and nutrient quality—and are especially timely given the rapid expansion of such programs.

Second, our results help clarify the puzzle noted by Deaton and Drèze (2009) that real food expenditure in India remained essentially flat between the 1987-88 and 2004-05 rounds of the National Sample Survey (NSS), and mean caloric consumption *fell* in this period, despite considerable economic growth. These findings may have contributed to the concern that income transfers may not move the needle on nutrition. We provide the first experimental evidence that, *all else equal*, poor Indian households have a meaningfully positive income elasticity of food expenditure, and caloric and nutrient consumption, at least out of income assigned to women. These results increase the posterior likelihood that the pattern noted by Deaton and Drèze (2009) is driven by omitted factors that may have reduced caloric needs over time (such as agricultural mechanization, reduced physical intensity of labor, and lower disease burden).

Third, our results enable a comparison of the effects of cash transfers on food consumption to the effects of food transfers themselves. We calculate that recipients' marginal propensity to consume calories from a rupee of cash transfers in our study is similar to the best available estimates for a rupee's worth of food transfers from India's Public Distribution System, from recent results in Gadenne et al. (2024). This suggests that—whether one takes the paternalistic view that households should consume more food, or the non-paternalistic one that they should choose for themselves—the distinction between cash and in-kind transfers may not be first-order with regard to food consumption in this setting.

Fourth, our findings reinforce the argument that poor sanitation may be a key explanatory factor for India's uniquely poor child health outcomes. Prior work has shown that *levels* of sanitation affect child health by increasing susceptibility to intestinal disease and diarrhea, which sap the body of nutrients and energy needed for growth (e.g., Esrey (1996); Checkley et al. (2004); Fink et al. (2011); Rah et al. (2015); Spears (2020); Cameron et al. (2022)). We add suggestive evidence of an *interaction* in the child health production function, where a poor sanitation environment may limit the translation of improved nutrition into child growth.[7] Combined with our finding

[5]Davala et al. (2015) find that cash transfers in India were spent well, but their study was limited to 20 clusters (8 treatment and 12 control villages in the state of Madhya Pradesh) and did not report statistical inferences.

[6]For example, the relationship between income and child malnutrition appears to be meaningfully different in India than the rest of the world (Ramalingaswami et al., 1996; Jayachandran and Pande, 2017), and studies tend to find lower relative resource shares for women in South Asia than other contexts (Jayachandran and Voena, 2025). As another example, the positive developmental impacts we find contrast with recent U.S. evidence: a major study by Noble et al. (2025) found no effects from large, multi-year transfers, underscoring how similar interventions can generate varying impacts across contexts.

[7]The closest related study is Geruso and Spears (2018), which finds that sanitation moderates the effect of breastfeeding on child mortality. However, neither breastfeeding nor sanitation are experimentally varied, and mortality is

that food and cash transfers yield similar effects on calorie consumption, these results suggest that policy should focus more on identifying constraints that limit the translation of nutrient intake to physical development – especially those like sanitation, which involve significant externalities that may lead to gaps between privately and socially optimal behavior.

Fifth, our results also speak to broader questions of gender and intra-household decision-making. A large literature has documented that women in India (and other LMICs) are disadvantaged in education, health, income, and leisure—likely reflecting lower control over household resources and decision making (Duflo, 2012; Zimmermann, 2012; Calvi, 2020; Jayachandran and Voena, 2025). Boosting women's agency is thus both a national and global development goal. We find that cash transfers labeled for nutrition and delivered into women's accounts can disproportionately benefit them, and improve gender equity. Methodologically, our detailed data on individual food consumption offer a better measure of intra-household inequity than clothing-based spending shares typically used in the literature (Deaton, 1997; Dunbar et al., 2013); other work from South Asia suggests that the household expenditure share on food is nearly ten times that on clothing (Brown et al., 2021), making it a more meaningful measure of equity. Our results show that marginal income transfers controlled by women may reduce intra-household consumption inequality, which may be an important benefit from India's growing use of cash transfers to women.

Finally, the fact that we find gains in child functional development, but not anthropometrics, highlights the importance of considering a broader set of child development outcomes. Despite the lack of gains in average anthropometrics, our finding gains on cognitive and motor skills suggest that maternal cash transfers did improve child development. These gains may be especially important in light of the possibility that, over time, "brains" may become more important than "brawn" for children's futures (Pitt et al., 2012).

2 Context & intervention

2.1 The ICDS program

The prevalence of stunted and underweight children in India is among the highest in the world, and "has its origins almost entirely during the first two to three years of life" (World Bank, 2009). The Indian government created the Integrated Child Development Services program in 1975 to address the health, nutrition and developmental needs of children and mothers. ICDS services are provided through 1.35 million anganwadi centers, where each AWC serves a catchment area of 500 to 1000 people and is typically staffed by one anganwadi worker (AWW) and one helper. The AWW oversees the wide range of services provided at the AWC, including supplementary nutrition programs, community health education, and immunization.

To improve nutrition for pregnant women, lactating mothers, and children aged 6 months to 3 years, the status-quo approach in many states has been for AWCs to provide ready-to-eat packets of flour fortified with vitamins and protein ("take-home rations"). These are substantial, providing

a different margin of child health.

over 15,000 calories and 480 grams of protein each month, often in areas where such nutritionally-fortified food is otherwise not easily accessible. However, doubts have been raised about the effectiveness of this program, based on accounts of households giving the food to their animals rather than eating it themselves (Malik, 2016) and high rates of leakage (Fraker et al., 2013). Reflecting these doubts, India's central government has introduced cash transfers as an additional instrument through its Pradhan Mantri Matru Vandana Yojana (PMMVY) scheme, which was launched in 2017 and provides a transfer of Rs. 5,000 to first-time pregnant and lactating mothers.

How much cash transfers will impact child development is an open question, echoing broader uncertainties about the translation of income growth into child development in India more generally (Jayachandran and Pande, 2017; Spears et al., 2022). As discussed above, the answer depends (among other things) on the extent to which households spend additional income on food, the nutritional content of that food, and which household members consume it. Further, the extent to which both income growth and income-transfer programs translate into child development may also depend on contextual and environmental factors mediating the nutrition-development relationship, such as sanitation.

2.2 The cash transfer intervention

Our study is set in the north Indian state of Jharkhand, which was recently ranked as the second poorest state in India on a multi-dimensional poverty index (NITI Aayog, 2021). Child development outcomes are poor: 39.6% of children are stunted, and 39.4% are underweight (Ministry of Health and Family Welfare, 2022b). Among women aged 15-49 years, 65.7% are anaemic, and 26.2% are classified as underweight on the Body Mass Index scale (Ibid).

Seeking to reduce these stubbornly high figures, the Government of Jharkhand (GoJH) decided to test the impact of cash transfers to new mothers. For a period of 3 months, AWWs in the 480 sampled AWCs informed pregnant women in their 1st and 2nd trimesters in their catchment areas that they were eligible to register to receive cash transfers. There were no further ex ante eligibility requirements and no ex post actions upon which transfers were conditional. To register, women were asked to fill out a one-page form and supply their bank account information and a photocopy of their personal identification card. Assistance in registering for bank accounts was provided to women who did not currently have one, including through organized registration "camps", although most of the sample already had an account due to the government's Jan Dhan campaign to promote universal banking access.

Eligible women were informed that some AWCs would be randomly selected, and that if their AWC was selected they would receive monthly cash transfers of Rs. 500 (approximately US$7) for one year.[8] The resulting amount (Rs. 6000 annually) was selected to be similar in total to

[8] At the start of the project, the government was uncertain whether funding would be available for a second year, and so only informed potential beneficiaries about one year of transfers at the time of registration. After one year, the government confirmed funding for another year, and the treatment group was informed that their transfers would continue for a second year. However, given the large cash amounts involved, the set of registrants would have likely been the same if the second year had been initially publicized.

related government cash transfer programs (PMMVY and the Janani Suraksha Yojana, or JSY).[9] The randomization was done *after* registration to ensure that the universe of eligible women who had registered for the program was comparable across treatment and control groups.

Panel A of Figure A.1 plots the distribution of wealth for women who registered for this program as compared to all of rural Jharkhand.[10] Although the sample of registrants is slightly wealthier on average (0.15SD), it contains households throughout the full distribution of wealth in the state. This is consistent with high rates of engagement with the ICDS system in rural Jharkhand, where aside from the wealthiest 5% of households, over 75% of women report receiving supplementary nutrition from their anganwadi centre during their most recent pregnancy (Panel B of Figure A.1).

GoJH began issuing transfers in the treatment group around four months after registration closed, and so most beneficiaries began receiving transfers around the time of the child's birth, though some children were slightly older or younger.[11] The program had aimed to deliver transfers during pregnancy to aid gestational development, but this proved infeasible due to bureaucratic delays. Such long payment delays are common in other Indian government programs: for example, pregnant women are supposed to receive the first PMMVY transfer prior to their child's birth, but this rarely happens (Drèze et al., 2021; Sekher et al., 2019).[12] Transfers were made into the registered woman's personal bank account and were typically withdrawn through in-person visits to bank branches (50%) or in-village common service centers (37%), with the rest being from ATMs or banking correspondents.

GoJH also delivered messaging encouraging registered mothers to buy nutritious food for themselves and their child. This occurred at two points. First, at registration, all mothers (regardless of eventual treatment status) received flyers and a verbal message from AWWs. While the content was similar to standard AWC messaging, GoJH deemed it necessary as part of recruitment. Second, mothers assigned to treatment received monthly automated (IVR) calls delivering a recorded message informing them that a transfer had been sent to their accounts, and offering child age-specific suggestions on nutritious foods to purchase. Slightly fewer than half of beneficiaries received calls each month, reflecting poor cellular connectivity in many areas; we show below that the calls had limited impact and do not explain the observed treatment effects. Overall, the transfers should be seen as unconditional but "framed," though the framing and messaging around behavioral change

[9] Those programs differ from the one studied here in being conditional: JSY transfers (Rs. 1000) are conditional on institutional delivery, while PMMVY transfers are conditional on receiving antenatal care (Rs. 3000) and the first cycle of child vaccinations (Rs. 2000). These conditions aim to encourage institutional delivery, improve child health, and compensate for any lost maternal earnings due to pregnancy. Receipt of the transfer studied in this paper did not affect eligibility for either JSY or PMMVY.

[10] For a representative sample of rural Jharkhand, we use data from round 5 of the National Family Health Survey (NFHS), which was collected contemporaneously with our study (2019-2021). We construct an asset index based on 8 assets that were measured in both the NFHS and in our baseline surveys.

[11] Appendix Table A.14 tests for and finds no heterogeneity in effects with respect to child age at first transfer.

[12] One practical take-away from this study is that low capacity states may often struggle to deliver targeted cash transfers within the window between formal registration of pregnancies and birth, presenting challenges for government-led cash transfer interventions targeting the *in utero* period. In other recent studies in which cash transfers were delivered prior to birth, implementation was often done by NGOs (e.g., Carneiro et al. (2021); Field and Maffioli (2025); Ahmed et al. (2025)) or the transfers were not based on the registration of pregnancies (e.g., in Amarante et al. (2016) and Barber and Gertler (2010)).

was considerably lighter than in many other recent studies.[13] The approach implemented here reflects both a desire to build on past studies' findings that information can affect the use of transfers, but also the reality that a less resource-intensive approach is needed for implementation at scale in settings of limited state resources and capacity.

Implementing cash transfers in this context posed many challenges. Transfers were often systematically delayed by government fiscal processes, arriving more than one month late several times.[14] Such delays are common in Indian government transfer programs, so these ITT effects are the appropriate ones for policy evaluation here.

For logistical reasons the government implemented the project in two phases. In five districts, registration began in October 2017 and transfers in March 2018. In the other three districts, these occurred approximately six months later. We pool both phases for analysis since procedures were the same and we do not observe significant differences in estimated treatment effects on primary outcomes. Figure A.2 provides a full timeline of project activities.

3 Experimental design & methods

Our design and methods follow a set of registered pre-analysis plans: one for data from year 1, one for data from year 2, and one that updated the analysis plans to account for how the COVID-19 pandemic shifted data collection.[15] This was part of a larger experiment that included an additional 480 AWCs allocated to two other treatment arms (receiving a year of transfers during either the first or second year of the child's life). This paper focuses on the two-year treatment arm, while analysis of the other two arms is available in a separate report (Weaver et al., 2023).[16]

3.1 Experimental design

The study population was selected to comprise a near-representative sample of pregnant mothers in Jharkhand. We randomly selected 8 of the state's 24 districts, excluding 2 that already had a maternal cash transfer program. Within each district, AWCs are grouped into "sectors" of ∼30 geographically proximate AWCs. We first randomly sampled 10 sectors in each district, and then sampled six AWCs per sector, for a total of 60 AWCs per district, and 480 AWCs in total; 1.9% of

[13] For example, in Field and Maffioli (2025) and Levere et al. (2024), the messaging was delivered in-person on at least a monthly basis.

[14] The April, May, June and July 2019 transfers were delayed until June 12, June 25th, August 1st and August 29th, respectively. November and December 2019 transfers were delayed until February 11 and February 20 of 2020. March and April 2020 transfers were delayed until April 21st and May 28th of 2020 as a result of COVID-19. In addition, approximately one-tenth of the sample experienced idiosyncratic delays beyond those described above at least once due to problems with their bank account or documentation. These issues likely would not have been resolved without the intervention of JPAL staff, highlighting the demands that even a relatively simple transfer can place on government systems with limited state capacity.

[15] See https://www.socialscienceregistry.org/trials/2899

[16] During the periods of cash transfer receipt, the effect of the treatment on nutritional intake and other intermediate outcomes was similar in all three treatment arms. Our pre-analysis plan report (Weaver et al., 2023) provides results from the full set of pre-committed analysis for completeness. The current paper focuses on drawing insights from the best-powered treatment, which was the one that provided transfers for two years as opposed to one.

sampled AWCs were replaced with another AWC via resampling (primarily due to inactivity of the assigned worker), but all sampled AWCs participated. The sample is representative of Jharkhand (population of ∼40 million): when these sectors are compared to the rest of the state on 26 variables from the 2011 census, there is only one statistically significant difference at the 5% level.

We randomly assigned half of the AWCs to receive two years of cash transfers and half to not receive any transfers, corresponding to 240 treatment and 240 control AWCs with approximately 1200 women in each group. The randomization was stratified by sector (with three AWCs per sector in the treatment and control groups), and further stratified within sectors based on the number of registrations in the AWC. Figure A.3 shows the geographical distribution of treatment and control AWCs. Conducting the randomized experiment in a near-representative study sample, with the treatment implemented through existing institutional mechanisms, strengthens the external validity of results (Muralidharan and Niehaus, 2017).

Compliance with the randomization was high, with no women in control AWCs and nearly all women in treatment AWCs receiving transfers. Administrative records state that over 99% of transfers were made successfully, and we see this reflected in survey data, where treatment households were around 70% more likely than control households to report having withdrawn any money from their bank account in the month prior to the survey (Table A.1). Treatment respondents were also more likely to state that they withdrew money from their account for the specific purposes of daily expenses (typically food) and medical expenses, consistent with the goals of the policy.

Table A.2 tests for balance on a pre-registered set of characteristics, grouped into three broad categories: household-level characteristics, number of women who registered at the AWC, and village characteristics as measured by the 2011 census of India. There was no baseline survey since the target children had not yet been born, so we test household characteristics that are invariant over the study period. We focus on characteristics that may be relevant to the implementation of the program, such as poverty, sanitation environment, and difficulty of travel to pick up cash transfers from the bank. Across the 12 tests, none of the differences between treatment and control are statistically significant at the 5% level and the joint p-value is equal to 0.80. Table A.3 also finds no differential attrition across treatment and control groups in any round of the survey.

3.2 Data collection

We gathered three rounds of survey data over the three years after the start of transfers (Figure A.2 provides a timeline), with the data collection instrument changing across rounds to reflect the relevant stage of child development. We surveyed the mother who registered for the transfers, and collected anthropometric measurements of the mother, child, and other children in the household.

In the first year (Y1), we conducted a survey after 11 months of transfers. The household component of these surveys measured spending on food; nutritional intake for the mother, child, and household; child morbidity and mortality; and maternal health knowledge, stress and depression, and empowerment. To measure nutritional intake, enumerators asked respondents about each meal eaten the previous day. For every dish prepared at home, the enumerator recorded the ingredients used and the exact weight or volume of each ingredient (see Appendix C for details).[17] By combining these intensive measurements with data on the nutritional content of each ingredient from the Indian Food Consumption Database (Longvah et al., 2017), our consumption-based approach provides more precise measures of calories, protein, and micronutrients (e.g., iron) consumed than approaches based on measuring purchases of food items, which require accurate unit cost information and cannot account for at-home production of food (e.g., Subramanian and Deaton (1996); Gadenne et al. (2024)). Enumerators also measured the quantity of the final dish consumed by the mother and child, meaning that we can measure nutritional intake for those two household members as well as per capita consumption of the rest of the household (by dividing the remaining consumption by the remaining household members).

In the second year (Y2), we conducted an endline survey after 23 months of transfers to measure the same outcomes as in Y1 as well as the cognitive/motor development of the child and weight-for-age of one randomly selected sibling under age 10 (if any). The outbreak of the COVID-19 pandemic in March 2020 caused the suspension of field operations after 613 surveys had been completed across four districts (21% of the sample). We then collected data over the phone for households that had not been reached in-person. Instead of the more intensive method based on measuring ingredients, the phone survey instrument asked whether the household had consumed particular food items in the previous day; based on this, we can measure maternal and child dietary diversity (whether they had consumed foods from different categories) in the phone data, but not calories or micro-nutrients consumed. We also were not able to measure anthropometrics, maternal stress, or depression over the phone.[18]

From September to November 2021, we conducted a final round of in-person data collection in all eight study districts during a period of low COVID-19 prevalence in Jharkhand (less than 0.1 daily cases per 100,000). Since this was approximately 1.5 years after the last transfer in five of the districts and a year after the last transfer in three districts, these data measure the longer run effect of receiving the cash transfers and we refer to these as year 3 (Y3) data. This survey measured height and weight for the target child, mother, and up to three randomly selected siblings under 10 years of age,[19] the neighborhood sanitary environment (e.g., prevalence of open defecation), and child cognitive and motor skills.

We use these data to investigate each link in the causal chain between income increases and child development. We examine impacts on pre-specified measures of household spending, quantity and quality of food consumption, and the allocation of nutritional intake within the household during the period when households were still receiving transfers (using the Y1 field survey and Y2 phone and field surveys). We also examine measures of child morbidity, maternal outcomes, beliefs and health-seeking behavior, and interaction with government services.

[17] For food consumed away from home, we use estimates on the average nutritional content of those items.

[18] Table A.4 examines how likelihood of contact over the phone varies with respondent characteristics (as measured in the Y1 survey). 62% of households were contacted for at least one Y2 phone survey.

[19] This random sample accounted for 95.9% of all siblings in this age range.

Finally, we examine whether changes along these links in the causal chain manifest in the physical and cognitive development of the child. For anthropometric measures of physical development, we measured height using stadiometers and weight using SECA-876 scales, the standard scale in the nutrition literature. We focus on child weight-for-age (WAZ) and height-for-age (HAZ), standardized into z-scores using growth charts from the World Health Organization. These anthropometric measures are important measures of child development and have been shown to predict later economic productivity (Currie and Vogl, 2013).

We measure child cognitive and motor skills development in year 3 using questions from the Ages and Stages Questionnaire that are developmentally appropriate for this age group (ASQ) (Doyle, 2020). This questionnaire asks mothers a series of questions about their child's ability to perform tasks across five categories—communication, gross motor, fine motor, problem solving, and personal-social (e.g., "can your child count to 10?", "can your child unbutton one or more button on their own?"). Each question is assigned a maximum of ten points—with ten points for a response of "yes," five for "sometimes", and zero for "no/never"—and these are summed for our measure of cognitive and functional child development.

Table A.5 summarizes the outcomes of interest, and when they were collected. Our main analysis of these outcomes focuses on data from Y1 and Y3, as the much smaller sample in the Y2 field data significantly reduces precision. There is a high correlation between anthropometric outcomes in Y2 and Y3 (e.g., a correlation of 0.845 for weight-for-age among children measured in both rounds), so Y3 is likely a good approximation of what happened in Y2, as well as being of interest in its own right. We also report results with respect to three pre-registered dimensions of heterogeneity—sanitation environment (Coffey and Spears, 2017), which we specified we would report if it meaningfully affected the interpretation of our results, as well as child birth order and gender (Behrman, 1988; Jayachandran and Kuziemko, 2011; Barcellos et al., 2014), which we specified we would report regardless of the results.[20]

3.3 Analytical methods

We estimate the effects of receiving transfers for two years using the following specification:

$$y^t_{has} = \beta_0 + \beta treatment_a + \phi_s + \epsilon_{has} \tag{1}$$

where h indexes households, a indexes AWCs, and s indexes sectors. In this specification y^t is an outcome measured in year t, *treatment* is a dummy variable indicating whether the AWC was assigned to the treatment arm, and ϕ_s represents sector-level fixed effects.[21] Given high rates of compliance with the experimental assignment, we focus on ITT estimates. Standard errors are

[20] See tables A.12, A.13, A.14, A.15, A.16, and A.17.

[21] While additional control variables are not necessary given random assignment, the PAP proposed including controls selected using the post-double-selection approach of Belloni et al. (2014). In practice, enough observations are missing for these characteristics that gains would be more than offset by losses in sample size and representativeness. We therefore prefer results estimated without controls, but results are similar either way (Table A.18).

clustered at the AWC level, the level at which treatment was assigned.

4 Results

4.1 Effects on food consumption and nutrition

We first examine impacts on food expenditure. Given high dispersion and a small number of zero values (e.g. households not going to the market during the recall period), we apply the inverse hyperbolic sine (IHS) transformation to spending amounts. Column 1 of Table 1 shows substantial and significant increases in both Y1 (15 IHS points) and Y2 (21 IHS points). We find no increase in reported "sin good" spending in either year (column 5), although we have fewer observations in Y2 since we did not measure this in the phone survey. For non-food expenditure (column 8), which we observe only in Y2,[22] the estimated increase (19 IHS points) is similar to that for food (21 IHS points), and we cannot reject equality between the two ($p = 0.83$). The data are consistent, in other words, with homothetic preferences within the range of expenditures induced by the experiment (at this broad level of disaggregation).[23]

This result is important in light of widely-discussed expenditure trends documented for example by Deaton and Drèze (2009), who show that real per capita food expenditure in India was essentially flat from the 1987-88 round to the 2004-5 round of the NSS, even as overall expenditure increased substantially. As they point out, this puzzling fact must surely be part of the explanation for the (equally puzzling) fact that caloric consumption fell over the same period. However, without well-identified estimates of the causal effect of income on food expenditure in India, it has been difficult to separate out preference-based explanations from other time-varying factors, such as a reduction in caloric requirements—which in turn could reflect factors like a reduction in physical labor intensive jobs or a reduction in disease burden. Our results provide the first experimental evidence that we are aware of that, all else equal, poor Indian households have a meaningfully positive elasticity of spending on food with respect to income—albeit with the important caveats that the income was accompanied by messaging encouraging this and transfers were made to the mother's bank account, which may increase the likelihood of usage for maternal and child nutrition.

At the same time, not *all* of the incremental spending was on food, which is unsurprising given the unconditional nature of the transfer. This may be acceptable or even desirable according to standard notions of welfare, but a policy-maker narrowly focused on nutrition might see it as a "tax" on the intervention. We can gauge the marginal propensity to spend on food from impacts on the *levels* of spending (rather than IHS-transformed spending), although this is estimated less

[22]Non-food spending was not captured in Y1, due to a survey form error that caused this section to be skipped and that was undetected until the survey was completed.

[23]Chen and Roth (2024) note challenges in interpreting IHS-transformed outcomes. Following their recommendation, we also report results along both extensive and intensive margins in the remaining table columns (Table 1, columns 2, 3, 6, 7, 9 and 10). Effects are concentrated on the intensive margin for food spending, consistent with more than 97% of households reporting non-zero food spending in each round. Results are similar using a log transformation. Results are unchanged for sin good spending, and we lack power to decompose effects for non-food spending, which exhibits more dispersion and noise than food spending.

precisely due to the significant dispersion in that measure. The estimates are nearly identical across years (increase of Rs. 203.11 in Y1 and Rs. 207.96 in Y2; column 4 of Table 1), and when we run a specification pooling both years of data, the estimated effect of the treatment on food spending is statistically significant at the 5% level (Rs. 205.20, $p = 0.038$). This implies that around 41% of the transfer was spent on food, which is similar to the share of food expenditures in the control group and consistent with homothetic preferences.

Table 2 shows that increased food expenditure translated into substantial nutrition gains, both overall and for targeted household members (mothers and infants).[24] Daily caloric intake from solid foods increased by 40 calories (18%) for targeted infants and 141 calories (8.5%) for mothers in Y1, and by 121 calories (14%) and 271 calories (16%) in Y2 (Table 2, columns 1 and 2). Non-targeted members saw similar increases: 131 calories (8%) in Y1 and 96 (6%) in Y2 (Table 2, column 3), with similar estimates when calories are translated into adult equivalent units (column 4).[25,26]

Dietary quality also improved for targeted household members. For both mothers and children, we use a standard dietary diversity score from 0-7, capturing the number of distinct food groups consumed (Ruel, 2003; Arimond and Ruel, 2004). These scores increased for both mothers and targeted children in both years (Table 2, columns 5 and 7). We also find significant gains in an index of maternal nutrient intake in both years (column 8), constructed as an average of the fraction of recommended daily consumption met for key nutrients such as protein, visible fat, and iron.[27] The increases are sizable: in Y2, child dietary diversity rose by 0.21SD, maternal dietary diversity by 0.21SD, and maternal nutrient intake by 0.33SD.

These results also inform an answer to a central question in debates on food security, the relative impacts on food consumption of cash transfers versus equivalent-valued in-kind transfers—such as through free or highly-subsidized foodgrains as in India's Public Distribution System (PDS) (Khera, 2014). We find that our estimates of the marginal propensity to consume (MPC) food out of cash transfers closely match recent benchmark estimates out of food transfers presented by Gadenne et al. (2024), who report impacts of past PDS expansions. While the samples are not directly comparable—our study covers Jharkhand in 2019, whereas theirs covers major Indian states in 2003–2012—they are comparable in terms of real incomes (Rs. 79,873 vs Rs. 82,979 in per-capita GDP). Our estimates imply a 131-calorie increase in per capita food consumption from a Rs. 500 monthly cash transfer (a 7.8% increase over the control mean). After adjusting for inflation and transfer size, their estimates imply a 164-calorie gain from Rs. 500 worth of monthly food transfers (a 7.6% increase over control mean). This modest, statistically insignificant difference suggests that

[24] In Y1, nutritional outcomes were measured for all households. In Y2, caloric and micronutrient intake data are only available for the 596 households for whom an in-person survey was completed. For other outcomes, we supplement the in-person data with phone data on prior day food consumption, which allows measurement of dietary diversity and minimum meal frequency.

[25] Caloric needs differ by factors such as age and gender. Adult equivalent calories include an adjustment for age and gender-specific caloric needs, enabling comparisons across households with different demographic compositions.

[26] Difference between Y1 and Y2 estimates are not due to differences in samples: Table A.6 finds the Y1 estimates are similar in the sample observed in Y2 (panel A) and the full sample (panel B).

[27] Table A.7 presents effects on individual nutrients, as some – such as iron and protein – have been shown to matter more for cognitive outcomes (Roberts et al., 2022; Ip et al., 2017).

food and cash transfers to women may be fungible in this setting, with both likely yielding similar increases in food consumption.

These estimates focus on the period when households were actively receiving transfers. However, such interventions may induce lasting changes via shifts in norms or investment behavior that affect outcomes beyond the transfer period. To assess persistence, we measured a limited set of nutrition-related outcomes during the Y3 survey, conducted 12–18 months after the final transfer. We randomly selected one sibling of the target child and recorded their consumption of rice, roti, milk, and fruit. For the target child, we collected the same measures, along with egg and dal consumption and a full dietary diversity index. Table A.8 finds increases in roti and fruit consumption for both child and sibling, indicating sustained effects. Moreover, the increase in dietary diversity of the target child remains comparable to that measured during the transfer period.

4.2 Effects on intra-household equity of food consumption

Beyond increasing caloric and nutrient intake for mothers and targeted infants, the results also suggest that maternal cash transfers improved intra-household equity and maternal welfare. In the control group, mothers consumed 17% fewer calories than the household adult average (1,657 vs. 2,002 calories in Y1). Even this likely understates inequity as we cannot measure what fraction of those calories were expended in breastfeeding. The Indian Ministry of Health and Family Welfare recommends 2500 calories/day for lactating mothers, which is 500–700 calories more than for non-lactating women (Ministry of Health and Family Welfare, 2018). Yet, average maternal caloric intake in the control group remained essentially unchanged between Y1, when 96% were breastfeeding, and Y2, when only 64% were (1,657 vs. 1,646 calories). This suggests that households were not adjusting maternal diets to account for the energy demands of breastfeeding. This may be a contributing factor to high rates of child malnutrition in India, and is an interesting topic for future research.

Thus, while the absolute increase in calories consumed was similar for mothers and other adults in Y1 (141 vs. 131), the *relative* increase for mothers' was higher since their baseline levels of consumption were lower. In Y2, the maternal gains were even greater: 271 calories vs. 63 for other household members (p=0.08).[28] This pattern is consistent with increasing maternal empowerment over time as women became more accustomed to receiving and using transfers (Field et al., 2021). Accessing the funds also required recipients to travel to banks or cash points, since collection typically required biometric authentication and could not be delegated. Thus, the treatment may have also increased women's mobility and autonomy, consistent with our finding of a significant increase in a proxy measure of travel (taking children to the doctor) in Table 3.

Methodologically, our collection and use of detailed individual-level food consumption data also

[28] We focus on the adult equivalence unit estimate for other household members, as this places them on a common scale to the mother in terms of caloric needs. Since the point estimate is nearly 4 times higher, this difference is statistically significant, despite the smaller sample of in-person surveys in Y2, prior to the halting of fieldwork due to COVID-19. Statistical significance is similar when comparing to the average caloric intake (non-adult equivalence units) of other household members in column 3 (p=0.08).

contributes to the literature on measuring intra-household equity. This literature has traditionally relied on clothing expenditure shares to recover intra-household consumption weights because clothing is an individually assignable good, and data on individual clothing spending is more readily available in household surveys (Deaton, 1997; Dunbar et al., 2013). However, in the similar context of Bangladesh, Brown et al. (2021) find that the average share of expenditure on clothing is ∼4%, compared to ∼40% on food. Thus, equality (or lack thereof) in food consumption is a much better measure of intra-household equity.

Overall, transfers caused mothers and young children to consume more nutritious food. This is notable in light of recent concerns about intra-household allocation norms in Indian households (e.g., Jayachandran and Pande (2017)).[29] At the same time, a meaningful share of the transfers was—as one would expect given the transfers were unconditional—spent on non-food items, or on food for other household members. These facts underscore the point that cash transfers may be a relatively blunt policy instrument for addressing specific nutritional deficiencies. This also highlights that a comprehensive estimate of the welfare benefits of cash transfers to women would need to account for the value of non-food spending.

4.3 Effects on other outcomes

Before looking at impacts on child development, we assess impacts on intermediate outcomes other than food consumption that could play a role in the causal chain from the transfers to child development. We examine both non-food spending, and design features of the program that could have changed household behaviors.

First, non-food spending might also contribute to the goal of child development. Since children in this setting typically do not attend preschool before age 3, we focus on healthcare. We find that children in treated households were no more likely to fall ill (Table 3, column 6), but were substantially more likely to be taken to a formal medical provider when needed (Table 3, column 5). Respondents (the mother in 90.5% of cases) also report that they were more likely to withdraw money from their bank account for medical expenses (appendix table A.1). Together, these results suggest that children were likely better cared for in cases of illness, and that mothers may have experienced an increase in agency to do so.

Second, specific features of the transfer design may have led recipients to use them the way they did. In particular, transfers were delivered into accounts controlled by women and accompanied by messaging encouraging purchases of nutritious food. Columns (1) and (2) of Table 3 report effects on outcomes plausibly shaped by these design features: indices of the mother's nutritional knowledge and empowerment. Nutritional knowledge is modestly higher in the treatment group, and maternal empowerment is higher in Y1. While these design features may have mattered, we find quantitatively that these channels do not "explain" much of the effects on nutrition. Multiplying the estimated treatment effects on nutritional knowledge and empowerment by their cross-sectional correlation with the nutrition outcomes in Table 2, the sum of these products is never more than

[29] See Spears et al. (2022) for a re-analysis of the data from this paper reaching different conclusions.

6.9% of the estimated treatment effect on nutrition.[30]

To assess the role of messaging specifically, we test whether treatment effects on nutrition were larger for mothers who listed a mobile number when registering for the program, and hence received IVR messages regarding nutrition. As shown in Table A.9, treatment effects on nutrition outcomes are no stronger for these mothers, suggesting that the messaging had little independent impact. This is consistent with the meta-analysis in Crosta et al. (2024, table 8), which finds no significant effects of transfer framing on any child-related outcomes (including HAZ and WAZ). We do not see significant effects on a pre-specified index of maternal depression, though point estimates are in the direction of reductions (Table 3, Column 3).

A separate question is whether the treatment might have affected downstream outcomes like anthropometrics through channels other than increased spending. An important alternative channel is uptake of other AWC services. While policymakers initially worried that cash transfers might reduce demand for these services, they could also crowd them in; for instance, mothers might visit AWCs to check on transfer status, even though no extra visits were required for either eligibility or collection. In practice, we find marginal crowd-in. In Y1, the total number of services received from the AWC increased by 0.26, or 5.1% of the control mean (Table 3, Column 4). In Y2, the estimate is smaller and insignificant, but this is more difficult to interpret due to travel restrictions related to COVID and lack of power due to the much smaller sample size in Y2 (the standard errors are so large that we cannot reject equality of Y2 and Y1 effects). Table A.10 reports the specific services in which increases occurred in Y1 (in addition to nutrition information, discussed above), which include deworming (6 percentage points(pp)), obtaining iron or calcium tablets (4pp), and signing up for other government programs (6pp) such as the PMMVY scheme (4pp).[31] From the point of view of policy-makers, who generally wanted to encourage engagement with the ICDS system, these are positive results, and may reflect greater trust and confidence in the ICDS system as a result of receiving transfers.

As above, we estimate the share of nutrition gains that can plausibly be explained by these channels by multiplying treatment effects on AWC service uptake by their cross-sectional correlations with nutrition outcomes. This plausible upper-bound yields a modest figure: we find that treatment effects on uptake of these services "explain" at most around 7% of the treatment effect on any nutritional outcome.

4.4 Effects on anthropometrics

Given the increases in nutritional intake, one might hope to see improvements in children's anthropometrics. However, estimated treatment effects are small and typically not significant. Columns

[30] If, as seems plausible, unobservables are positively correlated with both nutrition and these measures, then this exercise yields an upper bound on the share of the impact on nutrition caused by these channels.

[31] The PMMVY scheme is a cash transfer program for women who are pregnant or recently gave birth. Although these estimates imply that treated households receive additional income from PMMVY, this only amounts to an additional Rs. 268 relative to control households (Table A.10 - Column 11). This is only a 2.2% increase over the value of the Rs. 12,000 transfers we study, and so not large enough to affect the interpretation of our results.

1 and 3 of Table 4 show insignificant effects on HAZ and WAZ in Y1 and Y3.[32] Results are broadly similar if we focus on thresholds for being stunted or underweight (Columns 2 and 4); there is a reduction in the probability of being underweight in Y1, but the result does not persist to Y3.[33]

How do these results fit into the landscape of related work? Our estimates fall squarely within the confidence intervals from meta-analyses of cash transfer studies globally (Crosta et al., 2024; Manley et al., 2022). The point estimates from the meta-analyses tend to be small, especially for outcomes of children under two,[34] and the confidence intervals are wide, with limited consistency across anthropometric measures (HAZ, WAZ, or stunting). Given the wide range of studies included in these analyses, with many relying on non-experimental methods, and wide variation in the cash transfer amounts,[35], we find it instructive to focus on studies more closely comparable to ours.

Three recent studies report significant mean effects of maternal transfers on child anthropometrics: Ahmed et al. (2025) in Bangladesh, Carneiro et al. (2021) in Nigeria, and Field and Maffioli (2025) in Myanmar.[36] To benchmark our year 1, 2, and 3 HAZ and WAZ estimates against theirs, we normalize estimated effects by the total amount of money transferred (in PPP adjusted 2010 US dollars) up to the time the outcome was measured.[37] With just one exception (the Y1 HAZ effects in Carneiro et al. (2021)), all point estimates lie within the 95% confidence interval of our Y3 estimate; similarly our estimate lies within the 95% confidence intervals for all other estimates (Figure A.4). Thus, statistically, our results are in line with that literature, as well as recent meta-analyses (Manley et al., 2022; Crosta et al., 2024). Yet the confidence intervals are wide, implying that the data are also consistent with the conjecture that there are economic or biological factors that could make these settings meaningfully different.

To explore this further, we examine one key difference across these study contexts – sanitation. Medical research shows that a poor sanitation environment can result in diminished physical development through factors such as malabsorption of nutrients due to intestinal disease, loss of nutrients due to diarrhea, and energy expended in fighting disease (e.g. Checkley et al. (2008); Petri et al. (2008); Lin et al. (2013)). Spears (2020) shows that sanitation environment explains a large fraction of international differences in child stunting, and that the higher prevalence of open defecation in India can account for much or all of the excess stunting in India relative to

[32]Table A.11 shows similar results in Y2, but the smaller sample size limits power.

[33]We find no heterogeneity in impacts on anthropometric outcomes along the pre-registered dimensions of child gender, age at first transfer, or birth order (Tables A.12, A.13, and A.14), consistent with a lack of heterogeneity in treatment effects for nutritional outcomes (Tables A.15, A.16, and A.17).

[34]It is possible that sustained intervention is necessary to see these impacts; compelling evidence comes from Cahyadi et al. (2020), who find that a CCT in Indonesia had no detectable impacts on stunting two years after the start of the intervention, but large and significant effects of 23% six years afterwards.

[35]Impacts per dollar across the set of studies included in the Manley et al. (2022) meta-analysis are not statistically significant for *any* anthropometric outcome, as per analysis shared with us by James Manley.

[36]We focus on studies that target children in the first few years of life as in the current paper, rather than those that target slightly older children (e.g., Fernald et al. (2008); Paxson and Schady (2010); Macours et al. (2012)) or outcomes at birth (e.g., Amarante et al. (2016)).

[37]Applying a normalization is important since the transfer amounts differ across studies. However, this may miss important non-nonlinearities — for example, the larger effects in Carneiro et al. (2021) may reflect the relatively larger transfer size, which induces increases in maternal labor supply that generate higher income for the household even after the transfers end.

Africa. Hammer and Spears (2016) show experimentally that a sanitation intervention in rural Maharashtra significantly improved child HAZ scores, consistent with other evidence from India and other contexts (e.g. Bleakley (2007); Cameron et al. (2019, 2022)). Moreover, the Manley et al. (2022) meta-analysis suggests that WASH (water, sanitation, and hygiene) communication allied with cash transfers has stronger impacts on anthropometric outcomes.

Consistent with this literature, we find a strong link between poor sanitation and child development outcomes in our sample. As a summary measure of sanitation environment, we construct a principal components index from variables such as whether the household's neighbors use a toilet.[38] Although these are measured post-treatment due to the absence of a baseline survey, the sanitation index is not correlated with treatment status (column 5 of table A.3), suggesting that households did not use the transfers on improved sanitation. Thus, it is still econometrically valid to analyze heterogeneity with respect to it.[39] The neighborhood is likely the right level for thinking about effects of sanitation (Geruso and Spears, 2018), and the index is closely linked to neighborhood-level open defecation—the R^2 of a regression of the index on neighbor *usage* of toilets is 0.70. For ease of interpretation, we construct a percentile-based index of the sanitation distribution, where 0 indicates the best sanitation environment (lowest open defecation rates) and 1 is the worst. In the control group, moving from the best to worst index value is associated with a 0.33σ reduction in WAZ and a 0.45σ reduction in HAZ, even after conditioning on household food expenditures.

Sanitation conditions in our study areas are poor. Figure 1 shows the distribution of open defecation across villages in Jharkhand as per the National Family Health Survey - Round 5 (2019-2020). In the median community, 35% of households practice open defecation. A poor sanitation environment may not only *directly* harm child development, but also dampen the benefits of improved nutrition on child development; for instance, frequent incidence of diarrheal disease could limit the retention of the additional nutritional intake enabled by the transfers.

Table 5 tests whether treatment effects vary with the sanitation environment. We estimate treatment effects across the sanitation distribution (10th to 90th percentiles). For WAZ, effects are significant at the 10th and 25th percentile of the"poor sanitation" index (panel A), and the linear interaction of the sanitation index with treatment is statistically significant at the 5% level (panel B). Patterns on HAZ are directionally similar, but not significant. The effect sizes on WAZ are meaningful—a 0.12SD and 0.1SD increase in WAZ for communities at the 10th and 25th percentiles of the sanitation index, which are comparable to results from other experimental cash transfer studies.

This heterogeneity could explain the lack of significant *average* effects of the cash transfers on

[38] The full set of variables are the presence of either (i) observable feces, (ii) wastewater or (iii) an open sewage ditch around the house; and the fraction of the household's neighbors who (iv) own or (v) use a toilet. We prefer the principal components approach to other types of indices (e.g. equal weighting) since the PCA weights account for both correlations between the variables and differences in how much they matter for the sanitation environment. See appendix C for more details on the index.

[39] Alternative data sets for measuring village-level sanitation are either too out of date to be useful (e.g., the 2011 census, which occurred prior to the large expansion in toilet ownership due to the Swachh Bharat campaign), only have data for a small fraction of villages in our sample (e.g., National Family Health Survey), or only have data on sanitation in schools rather than the full community (e.g., U-DISE).

anthropometrics despite comparable nutrition gains to other recent experimental studies that *do* find significant anthropometric gains. Figure 1 shows that rates of open defecation were considerably lower in those contexts (e.g., Field and Maffioli (2025) in Myanmar, Carneiro et al. (2021) in Nigeria, Fernald et al. (2008) in Mexico): this ranges from 13.8% in Myanmar to 21.0% in Mexico, equal to about the 27th percentile in our sample.[40] When we restrict our sample to areas with similar rates of open defecation, the point estimates are quite similar, consistent with sanitation environment being a mediator of the nutrition-anthropometric relationship.[41]

We also examine anthropometric impacts on older siblings (under age 10) of targeted infants, a secondary outcome in our pre-analysis plan). Sibling WAZ increased by 0.11σ in Y3 ($p < 0.05$), and by a similar 0.13σ in Y2 (Table 6). We similarly find a 10 percentage point decline in the likelihood of being moderately underweight in Y2 (column 4, $p < 0.05$), though the Y2 WAZ estimate is statistically insignificant, likely due to the much smaller sample size (standard errors are twice as large in Y3 — 0.10 vs. 0.05).[42]. Estimated effects on HAZ in year 3 are positive (at 0.07σ), but not significant, though they are also not significantly different from the WAZ effects; height was not measured in Y2. These findings are consistent with the increased caloric intake for *all* household members (Table 2, column 3), and provide valuable evidence to the child nutrition literature that "catch up" growth of children is possible.[43] They also highlight that cash transfer programs can have broad benefits beyond those directly targeted by policymakers. For instance, while we do not measure education outcomes for older siblings, it is plausible that some of the transfers may have also been spent on their education.

Interestingly, we do not find significant heterogeneity with respect to sanitation for sibling anthropometric outcomes (Table A.19). This is consistent with evidence on age-based differences in vulnerability to poor sanitation: the loss of nutrients from diarrhea, which is strongly positively correlated with the prevalence of open defecation (Lin et al., 2013), has been shown to be more costly for the physical growth of younger children (Nasrin et al., 2023). Similarly, Adukia (2017) finds that health benefits from school latrine construction are concentrated among younger children, who are more susceptible to disease.

There are two main nuances involved in interpreting the heterogeneity with respect to sanitation. First, treatment effects on nutritional intake itself could vary by sanitation quality. For instance, areas with better sanitation may also be areas where women have more influence in how money is used, or have easier access to payments. To test this, we examine whether treatment effects on

[40] We calculate open defecation rates from the DHS round for the country closest to the time of the study, using sub-national data to match the regions studied.

[41] This finding also argues against the role of genetics in the high rates of stunting among Indian children as posited by Panagariya (2013), consistent with work showing that the height of young children born in England to Indian migrants is similar to that of native English children (Alacevich and Tarozzi, 2017).

[42] In a similar context (Nepal), Levere et al. (2024) find suggestive but mostly insignificant WAZ effects for non-targeted siblings, although they are underpowered to detect statistically significant effects due to an ~85% smaller sample of siblings (245 older siblings as compared to 1,945 target children in their study).

[43] The child nutrition literature has focused extensively on the first 1000 days of life (in utero to age 2) as the critical period for nutrition interventions. However, this does not imply that "catch up" growth in later years is not possible. Yet, there is much less experimental evidence on the possibility of such catch-up growth. See Singh et al. (2014) and Ganimian et al. (2024) for further discussion.

food expenditure, maternal or child consumption, or related outcomes vary with sanitation. Table A.20 shows that they do not, suggesting that sanitation is mediating how nutritional improvements translate into anthropometric gains rather than influencing nutritional intake levels.

Second, sanitation could proxy for other correlated factors that influence the translation of income into child growth. This would not matter for the policy question of where to target cash transfers to promote child development, as that is simply a prediction problem. But it would matter for inferences about what complementary interventions are most likely to amplify the effects of cash transfers. To explore this issue further, we replace our sanitation index with its residual after regressing it on a set of seven observable variables selected using LASSO for their ability to predict the index.[44] Results using this residualized regressor (Table A.21) are larger and more statistically significant than the original ones. While it is possible that unobserved correlates of sanitation affect the income-growth translation, the fact that results *strengthen* after removing observables, supports the interpretation that the same nutritional intake yields less child growth in less sanitary environments.

The ideal design for experimentally testing if interactions are significant would be a fully saturated design with a cash-only arm, a sanitation-only arm, and a combined arm. However, this would require a very large sample to be adequately powered to detect interactions (Muralidharan et al., 2023). Calculations in Appendix B show that a design with 80% power to detect interactions would require at least 6,000 clusters (Figure B.1). This is an order of magnitude larger than any similar field experiments with which we are familiar, and over 12 times larger than our current study. Given these constraints, the approach of interacting experimental income variation with cross-sectional variation in sanitation may be the most practicable way of testing for complementarities between income and sanitation in reducing child stunting.

4.5 Additional measures of children's development

Much of the research in India on how nutritional and health interventions affect child development has focused on anthropometrics (e.g., Hammer and Spears, 2016; Cameron et al., 2022). We augment this by analyzing effects on an index of child development milestones. We use the parent-reported Ages and Stages Questionnaire (ASQ), which measures important milestones for gross motor, fine motor and cognitive development. The ASQ has been shown to correlate well with clinical evaluations of child development via direct observations by trained psychologists.[45] We find a positive treatment effect of 0.12σ ($p < 0.01$) on ASQ scores (Table 4, column 5), which is equivalent to 19.5% of the control-group difference between children in the poorest and wealthiest quartiles (as measured by an asset index).

[44] These variables are selected from a larger list of fourteen variables using LASSO. The selected variables are: indicators for whether the household head is Muslim, Christian, below the poverty line, or a member of a scheduled caste or tribe, respondent education level, household size, and child gender.

[45] For children $\sim$3 years (as in our study at the time of ASQ administration), Schonhaut et al. (2013) find a correlation of 0.75 between ASQ and direct observation using the "gold-standard" Bayley Scales of Infant and Toddler Development. Attanasio et al. (2016) find slightly lower, but still moderate to high correlations across ASQ sub-components in a developing country context.

Further, we see gains in every sub-component of the ASQ across cognitive, gross motor and fine motor skills (Table A.23). Targeted children in treated households were reported as more likely to be able to perform tasks such as saying their own name, counting to 5, serving themselves food, holding a pen correctly, folding paper, and drawing a basic figure (Table A.22, Panels A-C) (Schonhaut et al., 2013; Macours et al., 2012).[46] Taken together, these impacts across a broad range of indicators suggest that improved nutritional intake for mothers and children translated into accelerated child development.[47]

These findings are consistent with experimental studies in other contexts that have found positive links between cash transfers in early childhood and cognitive development (Paxson and Schady, 2010; Macours et al., 2012; Gilligan and Roy, 2013; Barham et al., 2013). The increased nutritional intake we saw above is of course one likely underlying mechanism, but other channels could also matter. Parental involvement, for example, is strongly correlated with child development (e.g., Bono et al., 2016) and could respond positively to cash transfers, through both greater parental time to engage with children's learning activities and ability to purchase toys and play materials for children – as shown in the US context by Gennetian et al. (2024).

5 Policy Implications and Conclusion

Our results contribute to several active policy debates in India and globally including: (a) the impacts of cash transfers on food consumption, (b) the relative impacts of cash vs. in-kind transfers on food consumption, (c) the effects of cash transfers to women on gender equity and empowerment, (d) the impact of maternal cash transfers on child development, and (e) priorities for policy actions to accelerate progress on reducing child malnutrition. We discuss each of these below.

The first policy question we speak to is to address doubts about whether cash transfers would improve food consumption and nutrient intake—especially for women and children. Reasons for skepticism include concerns that expansion of household budget sets may not necessarily translate into more spending on food (Deaton and Drèze, 2009), that households cannot access or would not select nutritious food (Banerjee and Duflo, 2012), or that intra-household dynamics of resource allocation may limit the benefits for mothers and young children (Jayachandran and Pande, 2017).

Our results provide the first large-scale experimental evidence from India on this question, and show that cash transfers to women significantly increased food expenditure, calorie consumption,

[46]While experimenter demand effects are a potential concern in parent-reported outcomes, they seem unlikely here for a few reasons. First, we observe treatment effects even on outcomes that could be directly observed by the surveyors, such as the child ability to say their name or serve themselves. We do not see uniformly inflated responses or stronger effects for or harder-to-observe questions: we can reject equivalence of estimates across outcomes in table A.22, and observe effects on objective questions such as ability to count to 5. Second, the ASQ was administered in Y3 *over a year after the transfers had ceased*, so the incentives to differentially misreport child development across treatment and control should be attenuated. Finally, the treatment group is not told that outcomes are measured in a control group and compared to theirs to evaluate the program. Thus experimenter demand effects seem unlikely here, even if they are plausible in settings where the control and treatment groups are more visible to one another.

[47]We do not find a significant effect on the Y2 child development index (Table A.11). However, this index used fewer questions, relied on phone surveys, and had a smaller sample, and the analysis is therefore underpowered. The Y2 and Y3 questions were tailored to age, and so they have only one question in common.

dietary diversity, and nutrient intake—for both households as a whole, and for targeted mothers and children. We also show that the share of transfers spent on food closely mirrors the food share of household spending in the control group, consistent with homothetic preferences and implying proportionate increases in food and non-food spending.

Second, our results help to answer a central question in food security debates: how does the MPC of food out of cash transfers compare with that from equivalent-valued in-kind transfers. We find that we cannot reject equality between the MPC of food out of cash transfers in our study, and recent estimates for in-kind transfers from the PDS by Gadenne et al. (2024). This is unsurprising, since the value of in-kind transfers through the PDS is well below most households' food expenditure, making them infra-marginal for 93% of households Gadenne et al. (2024). Yet, this equivalence has not been empirically shown in India due to lack of experimental evidence on the MPC of food out of income transfers.[48]

Third, the rapid expansion of cash transfer programs to women in India—currently reaching an estimated 134 million women at a cost of 0.6% of GDP—has made them the *second-largest* category of welfare spending in India, after the PDS, and surpassing the National Rural Employment Guarantee Scheme (NREGS). While this expansion has been largely politically driven (The Economist, 2025), there is little evidence on their impacts on consumption, intra-household equity, or downstream outcomes. Our results contribute timely experimental evidence, and show that labeled cash transfers to women not only increased their food and nutrient intake (and that of their household), but also improved intra-household gender equity in food consumption and other measures of female agency. Given the large gender inequity in India, these findings are intrinsically important, and highlight the potential for cash transfers to women to improve gender equity.

Fourth, maternal cash transfers also improved child development. We find a significant 0.12SD increase in an index of functional development for the targeted child, with improvements on all components of the index. These may be even more important for long-term outcomes than physical development as labor markets evolve to reward "brains" more than "brawn" (Pitt et al., 2012). We also observe improved anthropometric outcomes (WAZ-scores) for older siblings of targeted children, consistent with the increase in mean household caloric and nutrient intake. For the targeted child, we find no average gains in anthropometric measures, but do find suggestive evidence of positive WAZ effects in areas in the top quartile of neighborhood sanitation quality. It is also worth noting that these positive developmental impacts contrast sharply with recent results from the United States, where a highly anticipated study by Noble et al. (2025) of large transfers delivered over the course of four years found *no* effects on measures of child development, further highlighting how

[48]Note that the interpretation of any difference in the MPC of food out of cash versus food transfers depends on one's normative views. If one respects household preferences, higher food consumption from food transfers implies distortion and would justify cash transfers. If one believes that households under-consume food—say, due to limited understanding of long-term benefits, or self-control issues—then food transfers may be preferable (see Gadenne and Singhal (forthcoming) for a more detailed discussion). Our finding of comparable MPCs suggests that paternalists need not be too concerned about cash transfers (at least when made to women and framed as done here), and that pure altruists need not be too concerned about food transfers. Note that in-kind programs like Take Home Rations, which target specific nutrients, may be less fungible than PDS grains, which are staple commodities.

similar interventions can have different impacts across contexts, and the importance of context-specific evidence to inform policy decisions.

Finally, our results can inform policies to reduce child malnutrition. Policy efforts to date have focused on improving child feeding in poor households, with debates centered on the *form* of redistribution – especially cash vs. in-kind. Our findings suggest another concern: even when child nutritional intake improves (via cash or kind), impacts on anthropometric outcomes may be limited due to contextual factors such as poor sanitation. The case for policy focus on sanitation is strengthened because poor households are likely to under-invest in it relative to socially optimal levels. While rising incomes enable greater spending on both food and sanitation, the latter is likely to grow slower than socially optimal due to two classic reasons for underinvestment: (a) households may not internalize the positive externalities from reduced open defecation, and (b) high fixed costs of constructing effective toilets.[49] Both externalities and fixed costs due to indivisibility of capital goods are classic explanations in development economics for low investment in high-return technologies (Murphy et al., 1989). Our results emphasize the need to build on recent government initiatives, and sustain efforts to improve sanitation and toilet use to accelerate the translation of India's income gains into gains in child development.

Taken together, our results suggest that maternal cash transfers can meaningfully improve several outcomes that policy makers care about, including food and nutrient consumption by mothers, children, and all household members; use of ICDS services and formal medical care for children; intra-household gender equity; infant functional development, and anthropometrics for older siblings. One way to enhance their impact on infant anthropometrics may be to identify areas with high child malnutrition and coordinate cash transfers with efforts to improve sanitation. Such coordination may be especially important because these programs are typically implemented by different government departments.

From a policy perspective, our results may underestimate the aggregate economic benefits of cash transfer programs to women, because they come from an experiment designed to study impacts on recipients as opposed to general equilibrium effects. However, large-scale cash transfer programs to women—as currently being implemented in many Indian states—are likely to generate such effects through aggregate demand externalities, which may amplify the benefits as found in Kenya by (Egger et al., 2022). Studying these in other contexts is an important area for future research.

[49]The most cost-effective toilet of high-enough quality to sequester fecal matter is a twin-pit toilet, which costs at least Rs. 12,000. This is over two months of household consumption in our setting.

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Table 1: Household Spending

	Food Spending				Sin Spending			Non-Food Spending		
	IHS	Extensive	Intensive	Total	IHS	Extensive	Intensive	IHS	Extensive	Intensive
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Year 1 Outcomes										
Treatment	0.15***	0.005	0.11***	203.11*	−0.08	−0.01	−0.01			
	(0.05)	(0.004)	(0.04)	(111.87)	(0.14)	(0.02)	(0.07)			
Control Mean	8.33	0.99	7.75	3230.67	4.1	0.63	5.85			
Observations	2,360	2,360	2,334	2,337	2,337	1,469				
Panel B: Year 2 Outcomes										
Treatment	0.21***	0.01**	0.09**	207.96	0.11	0.03	−0.10	0.19*	0.02	0.04
	(0.07)	(0.01)	(0.04)	(139.03)	(0.29)	(0.04)	(0.14)	(0.11)	(0.01)	(0.06)
Control Mean	8.41	0.97	7.99	3889.87	4.16	0.63	5.94	9.02	0.96	8.67
Observations	1,869	1,869	1,824	1,869	582	582	369	1,377	1,377	1,336

The unit of analysis is the household. IHS results pertain to an outcome variable transformed via inverse hyperbolic sine. Extensive margin results concern whether households recorded *any* of the associated spending in the previous month. Intensive margin results concern the subset of households for whom expenditure is positive, and pertain to an outcome variable transformed via natural log. Outcomes in Year 2 (panel B) correspond to a pooled sample of field and phone observations. In panel B, columns (1) to (4) combine field surveys and all of the year 2 phone surveys, columns (5) to (7) use only field survey data since sin good spending was not collected over the phone, and columns (8) to (10) pool data from the field survey and second round of phone surveys as this information was not collected in the first round of phone surveys. Column (4) winsorizes total food spending at the 99th percentile and includes household size fixed effects to deal with the significant dispersion in the outcome, much of which is due to differences in family size. Standard errors are in parentheses and clustered at the AWC-level. $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table 2: Quantity & Quality of Food Intake

	Calories				Child		Mother	
	Child	Mother	Household	Household (adult eq)	Dietary diversity score	Minimum meal frequency	Dietary diversity score	Nutrient index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Year 1 Outcomes								
Treatment	39.80***	140.63***	131.34***	131.09***	0.16***	0.03	0.08***	0.09***
	(13.72)	(31.89)	(42.38)	(45.49)	(0.05)	(0.02)	(0.02)	(0.02)
Control Mean	216.55	1656.99	1678.39	2001.87	1.59	0.61	2.78	0.19
Observations	2,360	2,360	2,321	2,321	2,349	2,344	2,349	2,349
Panel B: Year 2 Outcomes								
Treatment	121.22**	271.32***	95.56	63.00	0.24***	0.01	0.22***	0.17***
	(50.89)	(62.02)	(79.44)	(104.69)	(0.06)	(0.02)	(0.05)	(0.04)
Control Mean	885.61	1646.4	1499.86	1929.62	3.4	0.91	3.35	0.22
Observations	596	596	564	564	1,421	960	1,421	596

Unit of analysis is indicated in the first row of the table. Dietary diversity score (0-7) is defined as the number of the following dietary groups that were consumed by the respondent: (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. Nutrient index is computed by estimating the percent of the recommended daily value of calories, protein, visible fat, calcium, iron, thiamine, riboflavin, niacin, pyridoxin, and dietary folate consumed and averaging. Observation counts differ across year 2 outcomes because some outcomes could not be collected via phone survey, which field work transitioned to after the onset of COVID-19. For the year 2 outcomes in panel B, Columns (1), (2), and (6) use field survey data, as it is necessary to measure the quantity of individual ingredients to create these measures. Columns (3), (4), and (5) pool data from the field and multiple rounds of phone surveys (see table A.5 for a visualization of each of the outcomes of interest and the survey round in which they were collected). Column (4) pertains to the subset of the sampled children who are under 23 months — i.e. those for whom minimum meal frequency is considered a clinically meaningful measure. Standard errors are in parentheses and are clustered at the AWC-level. $^{*}p < .10$, $^{**}p < .05$, $^{***}p < .01$.

Table 3: Additional Outcomes

	Mother				Child	
	Nutritional knowledge index	Empowerment index	Depression index	Anganwadi services received	Probability of visiting formal medical provider	Total illnesses in the past three months
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Year 1 Outcomes						
Treatment	0.17**	0.13***	−0.08	0.26***	0.03*	
	(0.07)	(0.05)	(0.07)	(0.09)	(0.02)	
Control Mean	3.42	2.31	2.58	5.08	0.77	
Observations	2,349	2,349	2,347	2,300	2,242	
Panel B: Year 2 Outcomes						
Treatment	0.29***	0.06	−0.24	0.07	0.10***	0.02
	(0.11)	(0.07)	(0.16)	(0.21)	(0.03)	(0.05)
Control Mean	3.64	2.59	3.19	4.33	0.5	1.88
Observations	596	1,049	596	586	1,271	1,271

The unit of analysis is indicated in the first row of the table. The nutritional knowledge (0-6), empowerment (0-5), and depression (0-5) indices correspond to the number of questions answered by the respondent that suggest a high degree of the corresponding characteristic. Anganwadi services received corresponds to the number of AWC services (0-9) that the respondent received. For results on each AWC service, see Table A.10. Observation counts differ across Y2 outcomes because some outcomes could not be collected via phone survey, which field work transitioned to after the onset of COVID-19. Others were only collected during one round of phone surveys since the phone instrument had to be kept short to reduce respondent fatigue. Nutritional knowledge (column 1), anganwadi services (column 2), and the depression index (column 4) were collected in the field survey; the empowerment index (column 3) was collected in the field survey and second round of phone data collection; and child illness information (columns 5 and 6) was collected in the field survey and first round of phone data collection (see table A.5 for a visualization of each of the outcomes of interest and the survey round in which they were collected). Standard errors are in parentheses and clustered at the AWC-level. $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table 4: Anthropometric Outcomes

	HAZ	Stunted	WAZ	Underweight	Child development index
	(1)	(2)	(3)	(4)	(5)
Panel A: Year 1 Outcomes					
Treatment	0.005	−0.002	0.002	-0.04^{**}	
	(0.05)	(0.02)	(0.04)	(0.02)	
Control Mean	-1.47	0.31	-1.67	0.4	
Observations	2,355	2,355	2,355	2,355	
Panel B: Year 3 Outcomes					
Treatment	0.01	0.01	0.02	-0.02	0.12^{***}
	(0.04)	(0.02)	(0.04)	(0.02)	(0.04)
Control Mean	-1.84	0.45	-1.78	0.41	-0.06
Observations	2,115	2,115	2,155	2,155	2,164

The unit of analysis is the child. HAZ and WAZ denote the child's height-for-age and weight-for-age z-scores, respectively. Children with WAZ and HAZ of less than -2 are classified as moderately stunted and underweight respectively. Standard errors are in parentheses and clustered at the AWC-level. $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table 5: Interactions with Neighborhood Sanitation Environment

	HAZ	Stunted	WAZ	Underweight	Child development index
	(1)	(2)	(3)	(4)	(5)
Panel A: Implied Effects by Percentile					
10%	0.07	0	0.12*	−0.02	2.55
25%	0.06	0	0.1*	−0.02	2.73*
50%	0.02	0.01	0.03	−0.02	3.39***
75%	−0.02	0.02	−0.03	−0.01	3.97***
90%	−0.05	0.02	−0.09	−0.01	4.47**
Test of Equality: p-value	0.33	0.69	0.06	0.88	0.52
Joint p-value	0.2				
Panel B: Underlying Parameter Estimates					
Treatment	0.08	0.0002	0.13*	−0.02	2.26
	(0.08)	(0.04)	(0.07)	(0.03)	(1.92)
Poor Sanitation Index	−0.45***	0.19***	−0.26**	0.12*	−26.60***
	(0.13)	(0.07)	(0.13)	(0.06)	(3.40)
Treatment × Poor Sanitation Index	−0.18	0.03	−0.30**	0.01	2.91
	(0.18)	(0.09)	(0.15)	(0.08)	(4.37)
Control Mean	−1.84	0.45	−1.78	0.41	85.9
Observations	2115	2115	2155	2155	2164

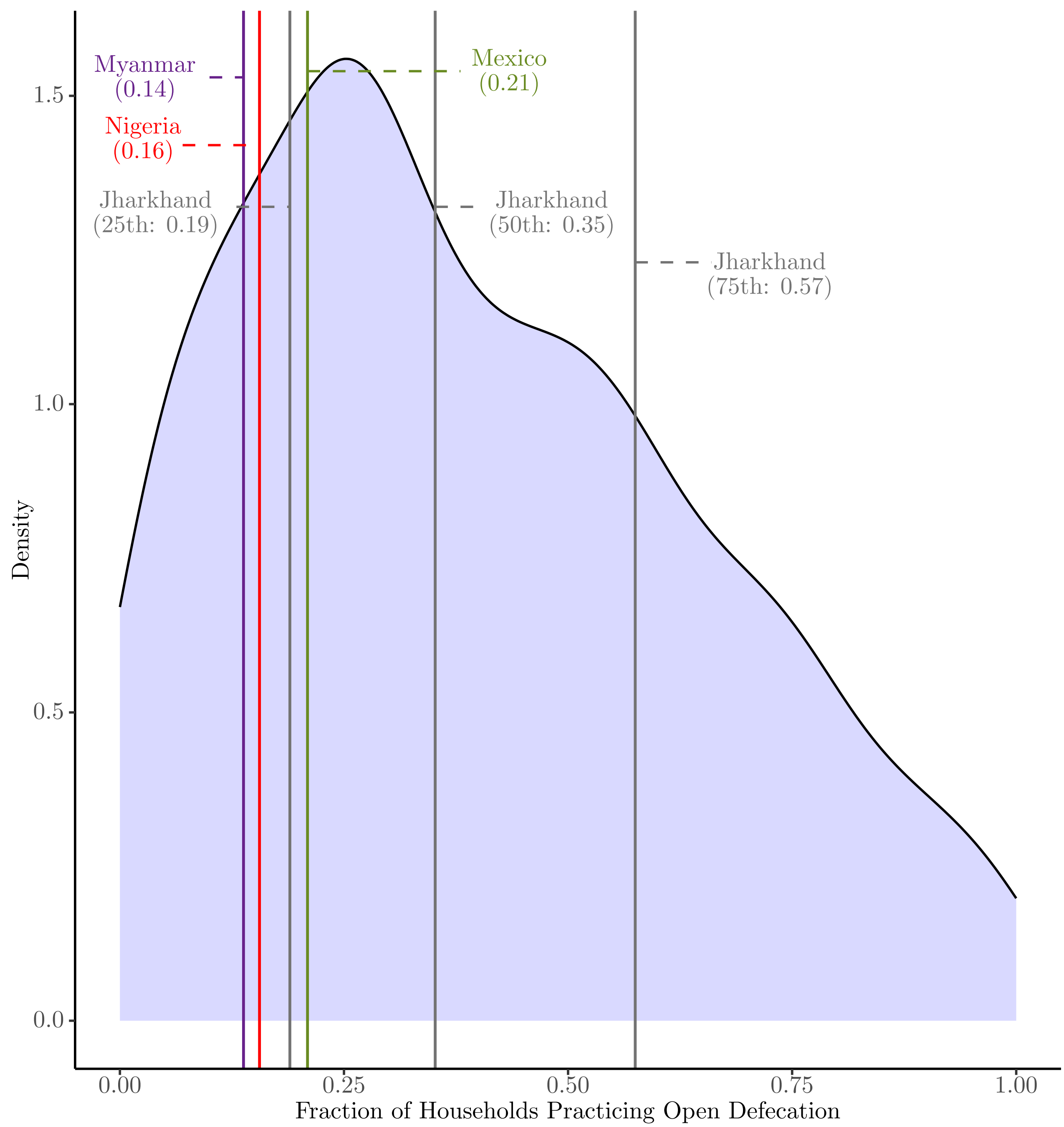
The unit of analysis is the child. Outcomes are from Year 3. HAZ and WAZ denote the child's height-for-age and weight-for-age z-score. Children with HAZ and WAZ scores of less than -2 are classified as stunted and underweight, respectively. As a summary measure of sanitation environment, we constructed a principal components index from 5 variables such as whether the household or their neighbors use a toilet. The poor sanitation index used in this regression is equal to the household's percentile rank on that principal components index, where a value of zero is equal to the best sanitation environment (lowest incidence of open defecation) and one is the worst. Standard errors are in parentheses and clustered at the AWC-level. $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

Table 6: Sibling Anthropometrics

	HAZ	Stunted	WAZ	Underweight
	(1)	(2)	(3)	(4)
Panel A: Year 2 Outcomes				
Treatment			0.13	-0.10^{**}
			(0.10)	(0.04)
Control Mean			-1.45	0.32
Observations			440	440
Panel B: Year 3 Outcomes				
Treatment	0.07	-0.01	0.11^{**}	-0.002
	(0.06)	(0.02)	(0.05)	(0.02)
Control Mean	-0.9	0.17	-1.49	0.29
Observations	2,046	2,046	2,138	2,138

The unit of analysis is the child. HAZ and WAZ denote the sibling's height-for-age and weight-for-age z-scores, respectively. Children with HAZ and WAZ of less than -2 are classified as moderately stunted and underweight respectively. Standard errors are in parentheses and clustered at the AWC-level. $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Figure 1: Distribution of Fraction of Households Practicing Open Defecation in the DHS



This figure presents the distribution of the fraction of households per community in rural Jharkhand practicing open defecation per the Demographic and Health Surveys-VII (2019-2021) program. Vertical gray lines correspond to the quantiles of the DHS community-wise fraction of households practicing open defecation in Jharkhand. The other vertical lines correspond to the mean community-wise fraction of households practicing open defecation in other countries in which studies of conditional cash transfers have taken place: Mexico (Fernald et al., 2008) (use 1987 DHS), Myanmar (Field and Maffioli, 2025) (use DHS 2015-2016), and Nigeria (Carneiro et al., 2021) (use DHS 2018). In each case, DHS data is sub-setted to best approximate the samples used and year of the respective studies. For example, Fernald et al. (2008) uses data from 1998 in Mexico, so we use the most temporally proximate DHS wave (1987), which may overstate the rate of open defecation in 1998 (and certainly in the present).

A Appendix: Additional Exhibits

Table A.1: Bank Account Usage

		Purpose of withdrawal				
	Withdrew in last 30 days	Daily expenses	Medical expenses	Non-food consumption	Loan payments	House
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.14^{***}	0.10^{***}	0.09^{**}	-0.04	-0.07^{**}	-0.04^{***}
	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)	(0.01)
Control Mean	0.2	0.47	0.43	0.21	0.12	0.07
Observations	830	822	822	822	822	822

The unit of analysis is the respondent. Purpose of withdrawal refers to whether their most recent withdrawal was used for the corresponding class of expenditure. "Daily expenses" most commonly refers to food or other small purchases for the household. Non-food consumption refers to non-food items that could be considered consumption rather than investment (e.g., clothes). House refers to spending on their house. The point estimates for withdrawals for loan repayment and home improvements are negative. However, when considering the magnitude of the rise in overall withdrawal activity (column 1), the magnitude of these negative coefficients is most consistent with an unchanged absolute number of withdrawals for those purposes (but a reduced relative frequency). Standard errors are in parentheses and are clustered at the AWC level. $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table A.2: Balance Tests

Outcome	Control (1)	Treatment (2)	p-value (3)
Time Invariant Household Characteristics			
Respondent's Education (Years)	7.05	7.19	0.15
Scheduled Caste or Tribe	0.59	0.59	0.95
Birth Order	2.19	2.13	0.1
AWC-level Characteristics			
Complete Registrations	7.66	7.48	0.61
Village-level Characteristics (2011 Census)			
% Households Living in Poor Condition Houses	0.62	0.64	0.57
% Households with a Toilet	0.08	0.08	0.56
Area of Village (Hectares)	478.33	403.77	0.3
Distance from All-Weather Road (km)	0.58	0.48	0.19
Distance from Nearest Bank (km)	1.90	2.04	0.35
Distance from Regular Market / Mandi (km)	1.88	1.88	0.81
% Scheduled Caste or Tribe (2011)	0.42	0.39	0.18
Village Population	1938.75	1863.12	0.56
Number of AWCs	240	240	
Joint F-test			0.8

This table presents balance tests for key outcomes across the treatment and control group. Columns 1-2 present means for each outcome by treatment group. Column 3 shows p-values for balance tests of each outcome across treatment groups. The final row corresponds to a joint F-test conducted using a seemingly unrelated regression framework. Standard errors are in parentheses and clustered at the AWC-level. $^{*}p < .10,^{**}p < .05,^{***}p < .01$.

Table A.3: Survey Completion

	Year 1	Year 2		Year 3	
	Endline (1)	Endline (2)	Phone (3)	Endline (4)	Insanitation (5)
Treatment	0.00	−0.01	−0.00	0.01	−0.00
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Control Mean	0.78	0.20	0.62	0.72	0.37
Observations	2956	2956	2956	2956	2166

Columns (1) to (4) of this table presents estimates for the relationship between treatment and probability of survey completion across each of this study's survey waves. Column (5) regresses our sanitation index on the treatment indicator to see whether sanitation is related to treatment status. Regressions include sector and AWC fixed effects. ${}^{*}p < .10, {}^{**}p < .05, {}^{***}p < .01$

Table A.4: Likelihood of Year 2 Phone Contact by Year 1 Characteristics

	Reached Over Phone
Respondent Education (Years)	0.004
	(0.003)
Scheduled Caste or Tribe	−0.04
	(0.03)
Birth Order	0.01
	(0.01)
Completed Registration	0.001
	(0.004)
Husband Education (Years)	0.01^{***}
	(0.004)
Rural	−0.05
	(0.08)
Hindu	−0.004
	(0.05)
Christian	0.16
	(0.10)
Aadivaasi	−0.03
	(0.06)
Child Female	0.05^{**}
	(0.02)
Age at First Transfer (Months)	0.01
	(0.01)
Below the Poverty Line	−0.05
	(0.03)
Household Size	0.01
	(0.004)
Bank Distance (Kilometers)	−0.002
	(0.001)
Uses Toilet	0.05^{*}
	(0.03)
Observations	1,550

This table examines how likelihood of completing a phone survey during year 2 is related to respondent characteristics (as measured during earlier surveys). Standard errors are in parentheses and clustered at the AWC-level. This sample only includes households with whom we did not conduct a year 2 field survey. $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

Table A.5: Timing and Measurement of Primary Outcome Variables

	Year 1		Year 2				Year 3
	Phase 1	Phase 2	Phase 1		Phase 2		Phase 1 & 2
	Field	Field	Field	Phone	Phone		Field
	Jan '19	Nov '19	Feb '20	May '20	Sep '20	Nov '20	Nov '21
Anthropometrics & Cognition							
Weight-for-Age (WAZ)	✓	✓	✓				✓
Height-for-Age (HAZ)	✓	✓	✓				✓
Sibling Weight-for-Age (HAZ)			✓				✓
Sibling Height-for-Age (HAZ)							✓
Cognitive & Motor Skills			✓		✓		✓
Expenditures							
Food Spending	✓	✓	✓	✓	✓	✓	
Sin Spending	✓	✓	✓				
Non-Food Spending			✓	✓		✓	
Nutrition							
Caloric Consumption	✓	✓	✓				
Child's Dietary Diversity	✓	✓	✓	✓	✓		✓
Mother's Dietary Diversity	✓	✓	✓	✓	✓		✓
Child's Minimum Meal Frequency	✓	✓	✓	✓	✓		
Mother's Nutrient Index	✓	✓	✓				
Social & Behavioral							
Visitation of Formal Medical Provider	✓	✓	✓	✓		✓	
Illnesses in Past Three Months	✓	✓	✓	✓		✓	
Anganwadi Services	✓	✓	✓		✓		
Mother's Nutritional Knowledge	✓	✓	✓				
Empowerment Index	✓	✓	✓		✓		
Depression Index	✓	✓	✓				

The project was rolled out in two phases, where the treatment began in five districts in March 2018 (labeled "Phase 1") and in another three districts in November 2018 ("Phase 2"). As a result, data collection took place at different times in each phase. For phase 1 districts, there was an in-person survey at the end of year 1 in January of 2019 (column 1) and an in-person survey at the end of year 2 beginning in February of 2020 (column 3). The year 2 survey was halted midway due to COVID-19 and data collection was completed over the phone in May of 2020 (column 4). For phase 2 districts, there was an in-person survey at the end of year 1 in November of 2019 (column 2). Due to COVID-19, the year 2 surveys in phase 2 were conducted over the phone. To reduce respondent fatigue, this was split into two phone surveys: one in September of 2020 (column 5) and one in November of 2020 (column 6). Finally, in both phase 1 and phase 2 districts, a final in-person survey was conducted in November of 2021 (column 7).

Table A.6: Quantity & Quality of Food Intake in Year 1 (Restricted to Year 2 Sample)

	Calories				Child		Mother	
	Child	Mother	Household	Household (adult eq)	Dietary diversity score	Minimum meal frequency	Dietary diversity score	Nutrient index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Sample Observed in Year 2								
Treatment	30.54	183.71***	105.27	66.94	0.10	0.01	0.13**	0.08*
	(32.04)	(62.37)	(96.62)	(99.92)	(0.10)	(0.04)	(0.05)	(0.05)
Control Mean	168.81	1616.66	1770.57	2119.3	1.18	0.57	2.65	0.15
Observations	574	574	560	560	571	568	571	571
Panel B: Full Sample								
Treatment	39.80***	140.63***	131.34***	131.09***	0.16***	0.03	0.08***	0.09***
	(13.72)	(31.89)	(42.38)	(45.49)	(0.05)	(0.02)	(0.02)	(0.02)
Control Mean	216.55	1656.99	1678.39	2001.87	1.59	0.61	2.78	0.19
Observations	2,360	2,360	2,321	2,321	2,349	2,344	2,349	2,349

This table analyses nutrition outcomes in Year 1 to test whether the subset of the sample for whom nutrition outcomes were collected in Year 2 is similar to the full sample. The sample in Panel A is households for which nutrition outcomes were observed in Year 2, whereas Panel B contains the full set of households observed in Year 1. Unit of analysis is indicated in the first row of the table. Dietary diversity score (0-7) is defined as the number of the following dietary groups that were consumed by the respondent: (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. Nutrient index is computed by estimating the percent of the recommended daily value of calories, protein, visible fat, calcium, iron, thiamine, riboflavin, niacin, pyridoxin, and dietary folate consumed and averaging. Standard errors are in parentheses and are clustered at the AWC-level. $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table A.7: Nutrients Index Components (Macro-Nutrients)

	Iron	Protein	Visible Fat	Calcium
	(1)	(2)	(3)	(4)
Panel A: Year 1 Outcomes				
Treatment	0.05***	0.04***	0.02***	0.004
	(0.01)	(0.01)	(0.01)	(0.01)
Control Mean	0.41	0.67	0.17	0.18
Observations	2,360	2,360	2,360	2,360
Panel B: Year 2 Outcomes				
Treatment	0.07***	0.07***	0.04***	0.003
	(0.02)	(0.02)	(0.01)	(0.02)
Control Mean	0.38	0.68	0.17	0.24
Observations	603	603	603	603

This table reports the effect of the treatment on individual macro-nutrients. The unit of analysis is the mother. Outcome variables correspond to the percent of daily value consumed for each macro-nutrient used in the construction of the nutrients index (see Table 2). Observations in Year 2 correspond to only the subset of respondents who were sampled in the field. Standard errors are in parentheses and clustered at the AWC-level. $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table A.8: Quantity & Quality of Food Intake in Year 3

	Roti (quantity)	Rice (quantity)	Milk (any)	Fruit (any)	Egg (quantity)	Dal (quantity)	Dietary diversity
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Main child							
Treatment	0.22***	0.04	−0.01	0.08***	0.01	0.06	0.16***
	(0.06)	(0.06)	(0.02)	(0.02)	(0.01)	(0.05)	(0.04)
Control Mean	1.34	2.44	0.44	0.44	0.1	1.23	3.52
Observations	2,166	2,166	2,166	2,162	2,166	2,166	2,166
Panel B: Sibling							
Treatment	0.33***	0.003	0.01	0.09***			
	(0.09)	(0.11)	(0.02)	(0.02)			
Control Mean	1.88	3.58	0.34	0.4			
Observations	1,443	1,443	1,443	1,438			

This table analyses nutrition outcomes in Year 3. Dietary diversity score (0-7) is defined as the number of the following dietary groups that were consumed by the respondent: (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. Standard errors are in parentheses and are clustered at the AWC-level. $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

Table A.9: Heterogeneity by Reporting of Mobile Phone Number Conditional on Assets

	Nutritional knowledge index	Dietary diversity (child)	Dietary diversity (mother)	Nutrient index
	(1)	(2)	(3)	(4)
Panel A: Year 1 Outcomes				
Treatment	0.19***	0.22***	0.09***	0.11***
	(0.07)	(0.05)	(0.03)	(0.03)
Treatment × Mobile Number | Assets	−0.13	0.20*	−0.05	0.04
	(0.13)	(0.12)	(0.05)	(0.06)
Control Mean	3.42	1.59	2.78	0.19
Observations	2,158	2,158	2,158	2,158
Panel B: Year 2 Outcomes				
Treatment	0.33***	0.27***	0.24***	0.18***
	(0.12)	(0.06)	(0.05)	(0.05)
Treatment × Mobile Number | Assets	−0.21	0.14	0.04	0.01
	(0.26)	(0.12)	(0.10)	(0.11)
Control Mean	3.57	3.4	3.35	0.22
Observations	559	1,327	1,327	553

This table investigates whether the treatment has heterogeneous effects based on whether the mother registers a mobile phone number, meaning that that she is able to receive IVR calls with messaging on nutrition. The unit of analysis is the child or mother, and is indicated in the first row of the table. Mobile number refers to whether the respondent reported a mobile phone number in initial surveys. The interaction variable corresponds to residuals from a regression of mobile phone on an asset index. Regressions estimated are fully saturated, with dummy the interaction variable excluded for brevity. Standard errors are in parentheses and clustered at the AWC-level. In panel B, columns (1) and (4) use only field survey data, while columns (2) and (3) combine field and phone survey data. $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table A.10: Take-Up of AWC Services

	Preschool	Hot cooked meals	Deworming	Growth measurement	Nutrition information	Iron/calcium tablets	Vaccination	Take-home rations	Government schemes	PMMVY	PMMVY amount
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Year 1 Outcomes											
Treatment	−0.01	−0.003	0.06***	0.02	0.09***	0.04*	−0.002	0.001	0.06***	0.04**	268.11***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(72.96)
Control Mean	0.3	0.33	0.42	0.7	0.5	0.59	0.73	0.88	0.62	0.12	358.49
Observations	2,329	2,320	2,325	2,329	2,326	2,327	2,329	2,330	2,323	1,406	1,406
Panel B: Year 2 Outcomes											
Treatment	0.03	0.03	−0.003	0.07**	0.09***	−0.0005	−0.03	0.03	−0.004		
	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)		
Control Mean	0.37	0.44	0.57	0.57	0.31	0.36	0.26	0.85	0.3		
Observations	586	586	1,066	1,066	1,066	1,066	1,066	1,066	1,066	1	1

This table presents estimates for the proportion of respondents that take up various AWC services each year. "Deworming" refers to whether the mother took her child to receive deworming medication. "Government schemes" refers to visiting one's AWC to sign up for other government services or programs. "Measurement" refers to whether the child had their height and weight measured at the AWC. "Hot cooked meals," "iron/calcium tablets", and "nutrition information" refer to whether the mother (and her child) received these items or services, the latter two during pregnancy and lactation. "Preschool" refers to whether the child received pre-school services administered by the AWC. "Take-home rations" refers to whether the mother received these from her AWC. Finally, "vaccination" refers to whether the child visited the AWC to receive immunizations for her child or herself during pregnancy. In panel B, we did not ask about pre-schools or hot cooked meals during the year 2 phone survey since AWCs did not offer them at that time due to the COVID-19 pandemic. As a result, the observation counts are lower for the first two columns. Data on receipt of transfers under the PMMVY scheme was only collected during phase 1 of the year 1 survey at the request of policymakers interested in understanding the current functioning of the scheme. As a result, the number of observations is lower in columns (10) and (11). $^{*}p < .10,^{**}p < .05,^{***}p < .01$.

Table A.11: Child Development Outcomes (Year 2)

	HAZ	Stunted	WAZ	Underweight
	(1)	(2)	(3)	(4)
Treatment	−0.10	0.03	0.02	−0.02
	(0.09)	(0.03)	(0.08)	(0.04)
Control Mean	−1.92	0.48	−1.91	0.46
Observations	607	607	607	607

This table examines outcomes collected in year 2, when our field survey was interrupted by the COVID-19 pandemic. As a result of this disruption, sample sizes and power are significantly lower than the year 1 or year 3 analysis. The unit of analysis is the child. WAZ and HAZ denote the child's weight-for-age and height-for-age z-scores, respectively. Children with WAZ and HAZ of less than minus two are classified as moderately underweight and stunted, respectively. Standard errors are in parentheses and clustered at the AWC-level. $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

Table A.12: Child Development Outcomes: Heterogeneity by Gender

	HAZ	Stunted	WAZ	Underweight
	(1)	(2)	(3)	(4)
Panel A: Year 1 Outcomes				
Treatment	0.04	0.004	0.05	−0.04
	(0.07)	(0.03)	(0.06)	(0.03)
Treatment × Female	−0.05	−0.02	−0.09	−0.01
	(0.10)	(0.04)	(0.09)	(0.04)
Control Mean	−1.47	0.31	−1.67	0.4
Observations	2,355	2,355	2,355	2,355
Panel B: Year 3 Outcomes				
Treatment	0.11	0.02	0.06	−0.02
	(0.08)	(0.02)	(0.06)	(0.02)
Treatment × Female	−0.09	−0.03	−0.05	−0.004
	(0.11)	(0.03)	(0.09)	(0.03)
Control Mean	−0.88	0.19	−1.2	0.23
Observations	2,124	2,124	2,165	2,165
Panel C: Year 3 Outcomes (Sibling)				
Treatment	0.05	−0.01	0.13	−0.01
	(0.09)	(0.02)	(0.08)	(0.03)
Treatment × Female	0.04	−0.01	−0.05	0.02
	(0.14)	(0.04)	(0.12)	(0.04)
Control Mean	−0.9	0.17	−1.49	0.29
Observations	2,046	2,046	2,138	2,138

This table follows the pre-analysis plan and tests for heterogeneity in treatment effects on child development outcomes related to the gender of the targeted child. The unit of analysis is the child. WAZ and HAZ denote the child's weight-for-age and height-for-age z-scores, respectively. Regressions estimated are fully saturated, with gender dummies excluded for brevity. Children with WAZ and HAZ of less than minus two are classified as moderately underweight and stunted, respectively. Standard errors are in parentheses and clustered at the AWC-level. $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table A.13: Child Development Outcomes: Heterogeneity by Birth Order

	HAZ	Stunted	WAZ	Underweight
	(1)	(2)	(3)	(4)
Panel A: Year 1 Outcomes				
Treatment	−0.04	0.01	−0.05	−0.03
	(0.06)	(0.03)	(0.06)	(0.02)
Treatment × Birth Order	0.04	−0.01	0.04	−0.01
	(0.04)	(0.02)	(0.03)	(0.01)
Control Mean	−1.47	0.31	−1.67	0.4
Observations	2,355	2,355	2,355	2,355
Panel B: Year 3 Outcomes				
Treatment	−0.03	0.01	−0.01	−0.01
	(0.08)	(0.02)	(0.06)	(0.02)
Treatment × Birth Order	0.07	−0.005	0.04	−0.01
	(0.05)	(0.02)	(0.04)	(0.02)
Control Mean	−0.88	0.19	−1.2	0.23
Observations	2,065	2,065	2,105	2,105
Panel C: Year 3 Outcomes (Sibling)				
Treatment	0.03	−0.01	0.04	0.001
	(0.12)	(0.04)	(0.10)	(0.04)
Treatment × Birth Order	0.02	−0.003	0.04	−0.004
	(0.06)	(0.02)	(0.05)	(0.02)
Control Mean	−0.9	0.17	−1.49	0.29
Observations	1,996	1,996	2,086	2,086

This table follows the pre-analysis plan and tests for heterogeneity in treatment effects on child development outcomes related to child birth order. The unit of analysis is the child. WAZ and HAZ denote the child's weight-for-age and height-for-age z-scores, respectively. Regressions estimated are fully saturated, with birth order excluded for brevity. Children with WAZ and HAZ of less than minus two are classified as moderately underweight and stunted, respectively. Standard errors are in parentheses and clustered at the AWC-level. $^{*}p < .10,^{**}p < .05,^{***}p < .01$.

Table A.14: Child Development Outcomes: Heterogeneity by Age at First Transfer (Months)

	HAZ	Stunted	WAZ	Underweight
	(1)	(2)	(3)	(4)
Panel A: Year 1 Outcomes				
Treatment	−0.01	0.01	−0.02	−0.03
	(0.05)	(0.02)	(0.04)	(0.02)
Treatment × Age	0.004	−0.01	0.03	−0.01
	(0.03)	(0.01)	(0.02)	(0.01)
Control Mean	−1.47	0.31	−1.67	0.4
Observations	2,355	2,355	2,355	2,355
Panel B: Year 3 Outcomes				
Treatment	0.06	−0.01	0.02	−0.02
	(0.07)	(0.02)	(0.05)	(0.02)
Treatment × Age	−0.002	0.004	0.004	0.001
	(0.01)	(0.003)	(0.01)	(0.003)
Control Mean	−0.88	0.19	−1.2	0.23
Observations	2,065	2,065	2,105	2,105

This table follows the pre-analysis plan and tests for heterogeneity in treatment effects on child development outcomes related to child age at the time of the first transfer. The unit of analysis is the child. WAZ and HAZ denote the child's weight-for-age and height-for-age z-scores, respectively. Regressions estimated are fully saturated, with age at first transfer excluded for brevity. Children with WAZ and HAZ of less than minus two are classified as moderately underweight and stunted, respectively. Standard errors are in parentheses and clustered at the AWC-level. $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table A.15: Nutritional Outcomes: Heterogeneity by Gender

	Household		Child		Mother	
	Caloric consumption (mother and child)	Caloric consumption (per capita)	Dietary diversity score	Minimum meal frequency	Dietary diversity score	Nutrient index
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Year 1 Outcomes						
Treatment	189.52***	109.53	0.15*	0.01	0.05	0.11***
	(47.75)	(66.59)	(0.08)	(0.03)	(0.03)	(0.03)
Treatment × Female	−17.38	46.57	0.03	0.04	0.06	−0.04
	(73.32)	(96.99)	(0.11)	(0.04)	(0.05)	(0.05)
Control Mean	1873.54	1678.39	1.59	0.61	2.78	0.19
Observations	2,360	2,321	2,349	2,344	2,349	2,349
Panel B: Year 2 Outcomes						
Treatment	343.89**	20.47	0.27***	0.03	0.24***	0.20***
	(137.56)	(106.30)	(0.08)	(0.02)	(0.07)	(0.06)
Treatment × Female	87.66	155.67	−0.06	−0.03	−0.04	−0.06
	(251.32)	(176.99)	(0.12)	(0.04)	(0.10)	(0.10)
Control Mean	2532.01	1499.86	3.4	0.91	3.35	0.22
Observations	596	564	1,421	960	1,421	596

This table follows the pre-analysis plan and tests for heterogeneity in treatment effects on nutritional outcomes related to child gender. The unit of analysis is either the mother or child, and this is indicated in the first row of the table. Regressions estimated are fully saturated, with gender dummies excluded for brevity. Dietary diversity score (0-7) is defined as the number of the following dietary groups that were consumed by the respondent: (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. Nutrient index is computed by estimating the percent of the recommended daily value of calories, protein, visible fat, calcium, iron, thiamine, riboflavin, niacin, pyridoxin, and dietary folate consumed and averaging. For the year 2 outcomes in panel B, Columns (1), (2), and (6) use field survey data, as it is necessary to measure the quantity of individual ingredients to create these measures. Columns (3), (4), and (5) pool data from the field and multiple rounds of phone surveys (see table A.5 for a visualization of each of the outcomes of interest and the survey round in which they were collected). Column (4) pertains to the subset of the sampled children who are under 23 months — i.e. those for whom minimum meal frequency is considered a clinically meaningful measure. Standard errors are in parentheses and clustered at the AWC-level. $^{*}p < .10,^{**}p < .05,^{***}p < .01$.

Table A.16: Nutritional Outcomes: Heterogeneity by Birth Order

	Household		Child		Mother	
	Caloric consumption (mother and child)	Caloric consumption (per capita)	Dietary diversity score	Minimum meal frequency	Dietary diversity score	Nutrient index
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Year 1 Outcomes						
Treatment	236.16***	115.33*	0.19***	0.06**	0.07**	0.16***
	(52.32)	(65.51)	(0.07)	(0.03)	(0.03)	(0.04)
Treatment × Birth Order	−44.69	6.75	−0.02	−0.02	0.01	−0.06**
	(29.35)	(36.20)	(0.04)	(0.02)	(0.02)	(0.02)
Control Mean	1873.54	1678.39	1.59	0.61	2.78	0.19
Observations	2,358	2,319	2,347	2,344	2,347	2,347
Panel B: Year 2 Outcomes						
Treatment	329.06***	109.63	0.17**	0.03	0.18***	0.14**
	(122.58)	(127.25)	(0.08)	(0.03)	(0.07)	(0.06)
Treatment × Birth Order	52.35	−12.33	0.06	−0.01	0.03	0.02
	(66.80)	(64.04)	(0.04)	(0.02)	(0.04)	(0.03)
Control Mean	2532.01	1499.86	3.4	0.91	3.35	0.22
Observations	596	564	1,421	960	1,421	596

This table follows the pre-analysis plan and tests for heterogeneity in treatment effects on nutritional outcomes related to birth order. The unit of analysis is either the mother or child, and this is indicated in the first row of the table. Regressions estimated are fully saturated, with birth order excluded for brevity. Dietary diversity score (0-7) is defined as the number of the following dietary groups that were consumed by the respondent: (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. Nutrient index is computed by estimating the percent of the recommended daily value of calories, protein, visible fat, calcium, iron, thiamine, riboflavin, niacin, pyridoxin, and dietary folate consumed and averaging. For the year 2 outcomes in panel B, Columns (1), (2), and (6) use field survey data, as it is necessary to measure the quantity of individual ingredients to create these measures. Columns (3), (4), and (5) pool data from the field and multiple rounds of phone surveys (see table A.5 for a visualization of each of the outcomes of interest and the survey round in which they were collected). Column (4) pertains to the subset of the sampled children who are under 23 months — i.e. those for whom minimum meal frequency is considered a clinically meaningful measure. Standard errors are in parentheses and clustered at the AWC-level. $^{*}p < .10$, $^{**}p < .05$, $^{***}p < .01$.

Table A.17: Nutritional Outcomes: Heterogeneity by Age at First Transfer (Months)

	Household		Child		Mother	
	Caloric consumption (mother and child)	Caloric consumption (per capita)	Dietary diversity score	Minimum meal frequency	Dietary diversity score	Nutrient index
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Year 1 Outcomes						
Treatment	171.64***	112.06**	0.15***	0.03	0.09***	0.09***
	(37.72)	(46.47)	(0.05)	(0.02)	(0.03)	(0.03)
Treatment × Age	13.44	23.35	0.02	0.0003	−0.01	0.002
	(17.47)	(22.17)	(0.03)	(0.01)	(0.01)	(0.01)
Control Mean	1873.54	1678.39	1.59	0.61	2.78	0.19
Observations	2,358	2,319	2,347	2,344	2,347	2,347
Panel B: Year 2 Outcomes						
Treatment	−588.13	−788.50	0.29***	0.04*	0.30***	−0.21
	(515.29)	(754.24)	(0.08)	(0.02)	(0.08)	(0.25)
Treatment × Age	78.32*	69.44	−0.01	−0.01	−0.01*	0.03
	(41.14)	(59.07)	(0.01)	(0.01)	(0.01)	(0.02)
Control Mean	2532.01	1499.86	3.4	0.91	3.35	0.22
Observations	596	564	1,421	960	1,421	596

This table follows the pre-analysis plan and tests for heterogeneity in treatment effects on nutritional outcomes related to the age of the child at the time of the first transfer. The unit of analysis is either the mother or child, and this is indicated in the first row of the table. Regressions estimated are fully saturated, with age at first transfer excluded for brevity. Dietary diversity score (0-7) is defined as the number of the following dietary groups that were consumed by the respondent: (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. Nutrient index is computed by estimating the percent of the recommended daily value of calories, protein, visible fat, calcium, iron, thiamine, riboflavin, niacin, pyridoxin, and dietary folate consumed and averaging. For the year 2 outcomes in panel B, Columns (1), (2), and (6) use field survey data, as it is necessary to measure the quantity of individual ingredients to create these measures. Columns (3), (4), and (5) pool data from the field and multiple rounds of phone surveys (see table A.5 for a visualization of each of the outcomes of interest and the survey round in which they were collected). Column (4) pertains to the subset of the sampled children who are under 23 months — i.e. those for whom minimum meal frequency is considered a clinically meaningful measure. Standard errors are in parentheses and clustered at the AWC-level. $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

Table A.18: Anthropometric Outcomes with LASSO Controls

	HAZ	Stunted	WAZ	Underweight
	(1)	(2)	(3)	(4)
Panel A: Year 1 Outcomes				
Treatment	0.001	−0.001	−0.01	−0.03*
	(0.04)	(0.02)	(0.04)	(0.02)
Control Mean	−1.47	0.31	−1.67	0.4
Observations	2,269	2,041	2,041	2,269
Panel B: Year 3 Outcomes				
Treatment	0.06	0.002	0.04	−0.02
	(0.05)	(0.02)	(0.04)	(0.02)
Control Mean	−0.88	0.19	−1.2	0.23
Observations	1,937	1,965	1,974	2,012

This table adds controls selected via LASSO to Table 4. This reduces the number of observations relative to Table 4 due to missing values for some of the selected variables. The unit of analysis is the child. WAZ and HAZ denote the child's weight-for-age and height-for-age z-scores, respectively. Regressions estimated are fully saturated, with age at first transfer excluded for brevity. Children with WAZ and HAZ of less than minus two are classified as moderately underweight and stunted, respectively. Standard errors are in parentheses and clustered at the AWC-level. $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table A.19: Sibling Anthropometrics and Interactions with Sanitation Environment

	HAZ (1)	Stunted (2)	WAZ (3)	Underweight (4)
Panel A: Implied Effects by Percentile				
10%	-0.01	0.02	0.16*	0.03
25%	0.01	0.02	0.15**	0.02
50%	0.07	-0.01	0.11**	0
75%	0.12*	-0.03	0.08	-0.02
90%	0.16	-0.05*	0.06	-0.04
Test of Equality: p-value	0.27	0.09	0.46	0.18
Joint p-value	0.19			
Panel B: Underlying Parameter Estimates				
Treatment	-0.03	0.03	0.17	0.04
	(0.11)	(0.03)	(0.11)	(0.04)
Poor Sanitation Index	-0.31	0.10	-0.13	0.04
	(0.21)	(0.06)	(0.17)	(0.06)
Treatment × Poor Sanitation Index	0.26	-0.12	-0.15	-0.11
	(0.27)	(0.07)	(0.22)	(0.08)
Control Mean	-0.9	0.17	-1.49	0.29
Observations	2046	2046	2138	2138

This table tests for treatment effects on sibling anthropometrics. In the year 2 endline survey, we randomly selected one sibling under the age of 10 and measured their weight and height. In the year 3 endline survey, we randomly selected up to three of the focal child's siblings under the age of 10 and measured their height/weight. HAZ and WAZ denote the sibling's height-for-age and weight-for-age z-scores, respectively. Children with HAZ and WAZ of less than -2 are classified as moderately stunted and underweight respectively. As a summary measure of sanitation environment, we constructed a principal components index from 5 variables such as whether the household or their neighbors use a toilet. The poor sanitation index used in this regression is equal to the household's percentile rank on that principal components index, where a value of zero is equal to the best sanitation environment (lowest incidence of open defecation) and one is the worst. Standard errors are in parentheses and clustered at the AWC-level. $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table A.20: Interactions with Sanitation Environment: Nutrition, Expenditure, & Empowerment (Year 1)

	Total food expenditure (1)	Caloric consumption (mother and child) (2)	Caloric consumption (per capita) (3)	Child's dietary diversity score (4)	Nutritional knowledge index (5)	Anganwadi services received (6)	Empowerment index (7)
Panel A: Implied Effects by Percentile							
10%	0.09	197.13***	225.98***	0.26***	0.09	0.39***	0.15*
25%	0.1	196.61***	207.61***	0.25***	0.1	0.36***	0.14*
50%	0.15***	194.62***	137.55***	0.21***	0.14**	0.24***	0.1*
75%	0.19***	192.73***	71.16	0.17**	0.18**	0.13	0.07
90%	0.22**	191.36***	23.21	0.15	0.21*	0.05	0.04
Test of Equality: p-value	0.34	0.95	0.12	0.46	0.49	0.15	0.47
Joint p-value	0.32						
Panel B: Underlying Parameter Estimates							
Treatment	0.08	198.37***	241.87***	0.26**	0.08	0.42**	0.16
	(0.08)	(65.34)	(82.76)	(0.10)	(0.12)	(0.17)	(0.10)
Poor Sanitation Index	-0.43***	178.89	147.51	-0.52***	-0.55***	0.12	0.33*
	(0.16)	(124.16)	(157.93)	(0.17)	(0.20)	(0.32)	(0.18)
Treatment × Poor Sanitation Index	0.20	-8.74	-307.25	-0.17	0.18	-0.52	-0.15
	(0.21)	(159.09)	(201.39)	(0.22)	(0.28)	(0.41)	(0.24)
Control Mean	8.35	1895.43	1678.17	1.59	3.43	5.15	2.33
Observations	2095	2095	2066	2086	2086	2037	2086

This table tests for heterogeneity in treatment effects on intermediate outcomes (e.g., nutrition, expenditure) related to the sanitation environment. The unit of analysis is either the mother, child, or household. Outcomes correspond to a pooled sample of observations from field surveys in P1 and P2 of Y1. As a summary measure of sanitation environment, we constructed a principal components index from 5 variables such as whether the household or their neighbors use a toilet. The poor sanitation index used in this regression is equal to the household's percentile rank on that principal components index, where a value of zero is equal to the best sanitation environment (lowest incidence of open defecation) and one is the worst. Total food expenditure is subjected to an inverse hyperbolic sine transformation. Variety score (0-20) measures variety across both food groups and sources of protein. Dietary diversity score (0-7) is defined as the number of the following dietary groups that were consumed by the respondent: (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. The nutritional knowledge (0-6) and empowerment (0-5) indices correspond to the number of questions answered by the respondent that suggest a high degree of the corresponding characteristic. Anganwadi services received corresponds to the number of the following that the respondent received from their local AWC: e-school, deworming, government schemes, growth measurement, hot cooked meals, iron/calcium tablets, nutritional information, take-home rations, and vaccination. Standard errors are in parentheses and clustered at the AWC-level. $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

Table A.21: Interactions with Sanitation Environment (Residualized)

	HAZ (1)	Stunted (2)	WAZ (3)	Underweight (4)
Panel A: Implied Effects by Percentile				
10%	0.18	-0.11*	0.26**	-0.09
25%	0.15	-0.09*	0.21**	-0.07
50%	0.03	0	0.04	-0.02
75%	-0.08	0.08**	-0.11	0.02
90%	-0.17	0.15**	-0.25**	0.06
Test of Equality: p-value	0.13	0.02	0.02	0.18
Joint p-value	0			
Panel B: Underlying Parameter Estimates				
Treatment	0.29	-0.19**	0.42**	-0.14*
	(0.18)	(0.09)	(0.17)	(0.09)
Poor Sanitation Index (Residuals)	-0.08	-0.06	0.26	-0.07
	(0.23)	(0.13)	(0.23)	(0.12)
Treatment × Poor Sanitation Index (Residuals)	-0.53	0.40**	-0.77**	0.23
	(0.35)	(0.18)	(0.33)	(0.17)
Control Mean	-1.84	0.45	-1.78	0.41
Observations	1956	1956	1993	1993

The unit of analysis is the child. This table takes the sanitation index from table 5, regresses it on variables related to sanitation, and saves the residuals (see section 4 for details). It then uses those residuals in the interaction term. WAZ and HAZ denote the child's weight-for-age and height-for-age z-scores, respectively. Children with WAZ and HAZ of less than minus two are classified as moderately underweight and stunted, respectively. Standard errors are in parentheses and clustered at the AWC-level. $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table A.22: Child Development Index Components

	Child can say their own name	Child can say the names of others	Child can identify their village	Child can tell the day of the week	Child can count to 5	Child can count to 10	Child can count to 20	Child can name primary colors
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Cognitive Outcomes								
Treatment	0.32**	0.08	0.25	−0.02	0.42**	0.30*	0.14	0.14
	(0.15)	(0.14)	(0.20)	(0.07)	(0.19)	(0.17)	(0.09)	(0.19)
Control Mean	7.6	8.54	5.43	0.24	5.58	2.57	0.64	3.62
Observations	2,164	2,164	2,164	2,164	2,164	2,164	2,164	2,163

	Child can walk up stairs	Child can kick a ball	Child can catch a ball	Child can serve themselves food	Child can brush their teeth
	(9)	(10)	(11)	(12)	(13)
Panel B: Gross Motor Skills					
Treatment	0.11	0.17	−0.11	0.60***	0.15
	(0.12)	(0.13)	(0.18)	(0.18)	(0.11)
Control Mean	8.65	8.56	2.77	6.47	9.09
Observations	2,164	2,164	2,162	2,163	2,164

	Child can unbutton their clothes	Child can hold a pen correctly	Child can draw a basic figure	Child can fold paper
	(14)	(15)	(16)	(17)
Panel C: Fine Motor Skills				
Treatment	0.09	0.32*	0.12**	0.37**
	(0.18)	(0.18)	(0.06)	(0.19)
Control Mean	7.46	6.53	0.17	2.02
Observations	2,162	2,163	2,164	2,163

The unit of the analysis is the child, and outcomes are measured in the year 3 survey. Outcomes are indices taking value 0 if the child does not ever, 5 if the child sometimes does, and 10 if the child does the listed action. Standard errors are in parentheses and clustered at the AWC-level. $^{*}p < .10,^{**}p < .05,^{***}p < .01$.

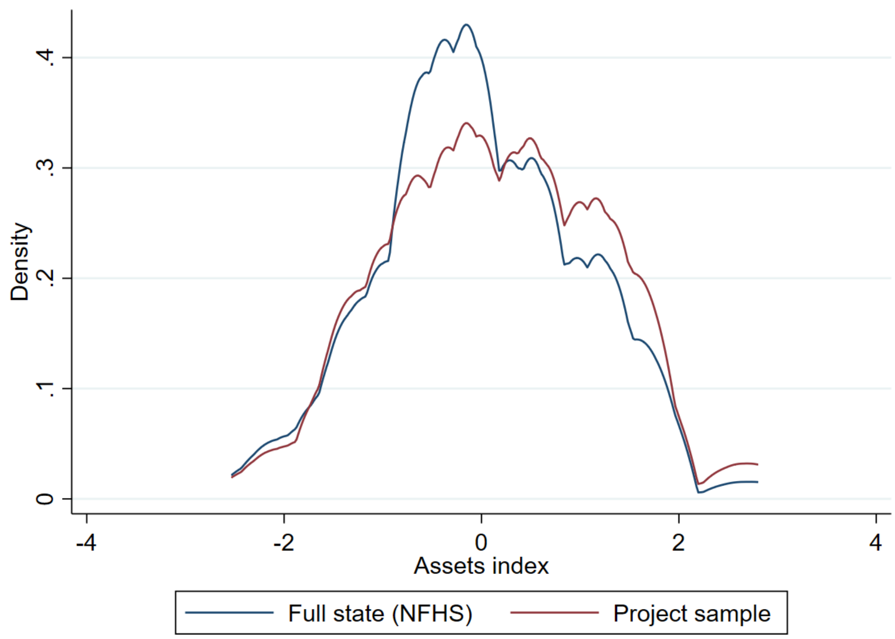
Table A.23: Child Development Sub-Indices

	Child development index	Gross motor skills index	Cognitive index	Fine motor skills index
	(1)	(2)	(3)	(4)
Treatment	0.12***	0.09**	0.09**	0.10**
	(0.04)	(0.04)	(0.04)	(0.04)
Control Mean	−0.06	−0.05	−0.04	−0.05
Observations	2,164	2,162	2,163	2,161

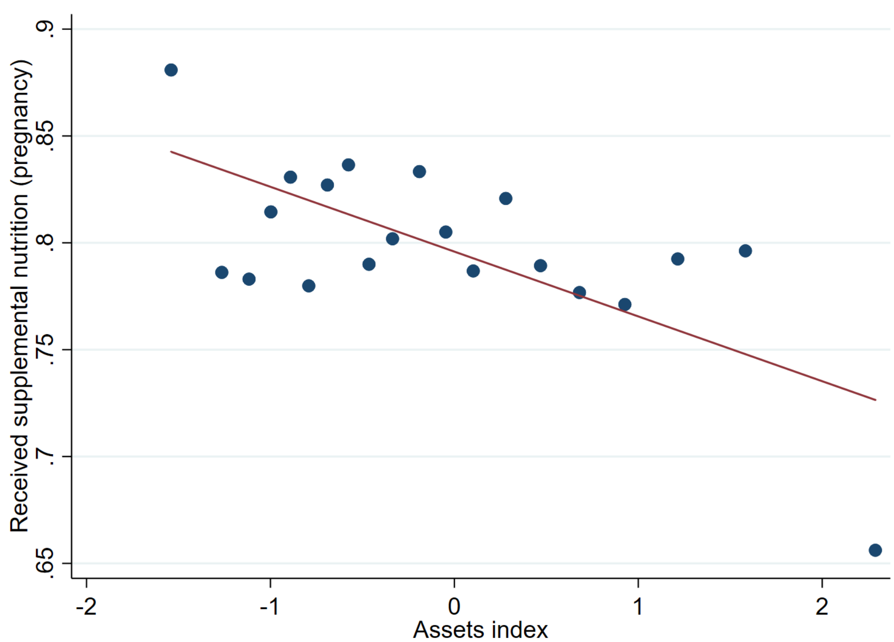
The unit of analysis is the child, and outcomes are measured in the year 3 survey. Indices are demeaned and expressed in standard deviation units. Standard errors are in parentheses and clustered at the AWC-level. $^{*}p < .10,^{**}p < .05,^{***}p < .01$.

Figure A.1: Distribution of wealth within sample

(a) Wealth distribution

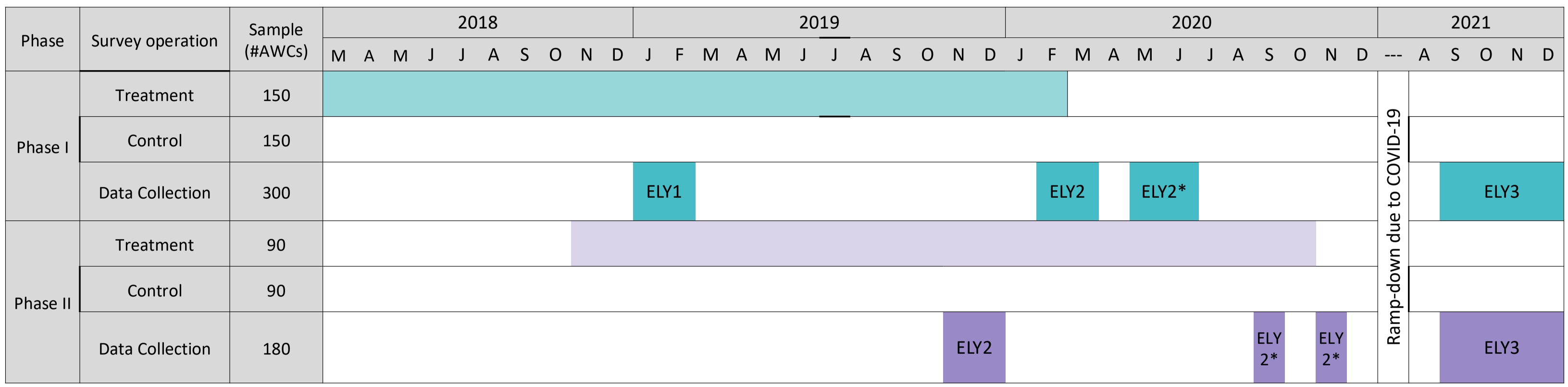


(b) Received nutrition from AWC while pregnant



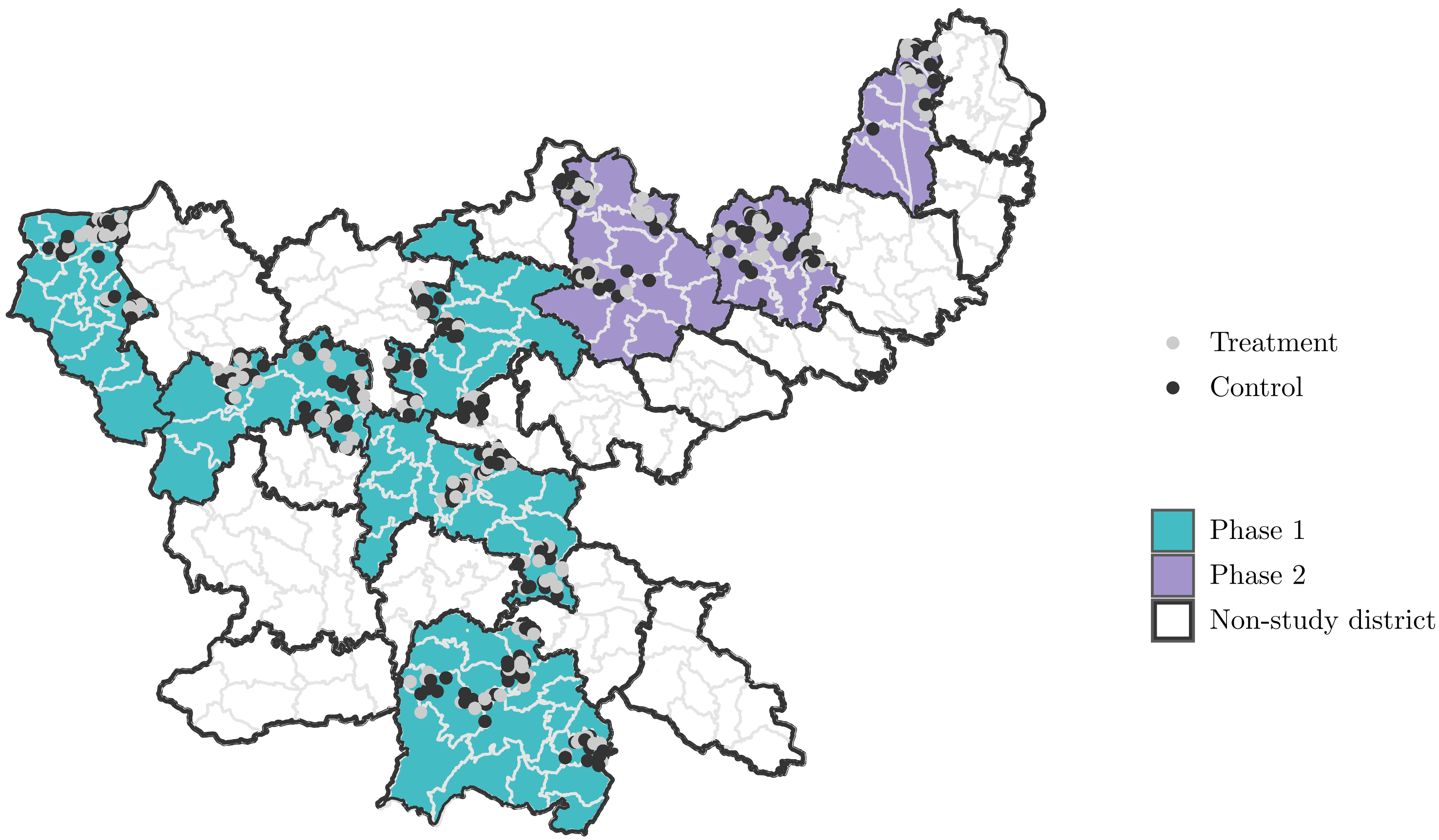
Note: Panel A of this figure plots the distribution of wealth within the project sample and in rural Jharkhand. For the project sample, it takes data from the control group from the survey conducted at the end of Y1. For rural Jharkhand, it uses data from round 5 of the National Family Health Survey, which was collected contemporaneously with our study (2019-2021). It takes ownership of 8 assets that are measured in both NFHS-5 and our surveys (bed, colour TV, 2 wheeler, 4 wheeler, chair, bicycle, mobile phone, pressure cooker) and constructs a common index based on the first principal component. Panel B plots a binscatter of the relationship between this index and receipt of supplemental nutrition at an AWC while breastfeeding, grouping the data into twenty equally sized bins. This uses the same data from round 5 of the National Family Health Survey in Jharkhand.

Figure A.2: Project Timeline



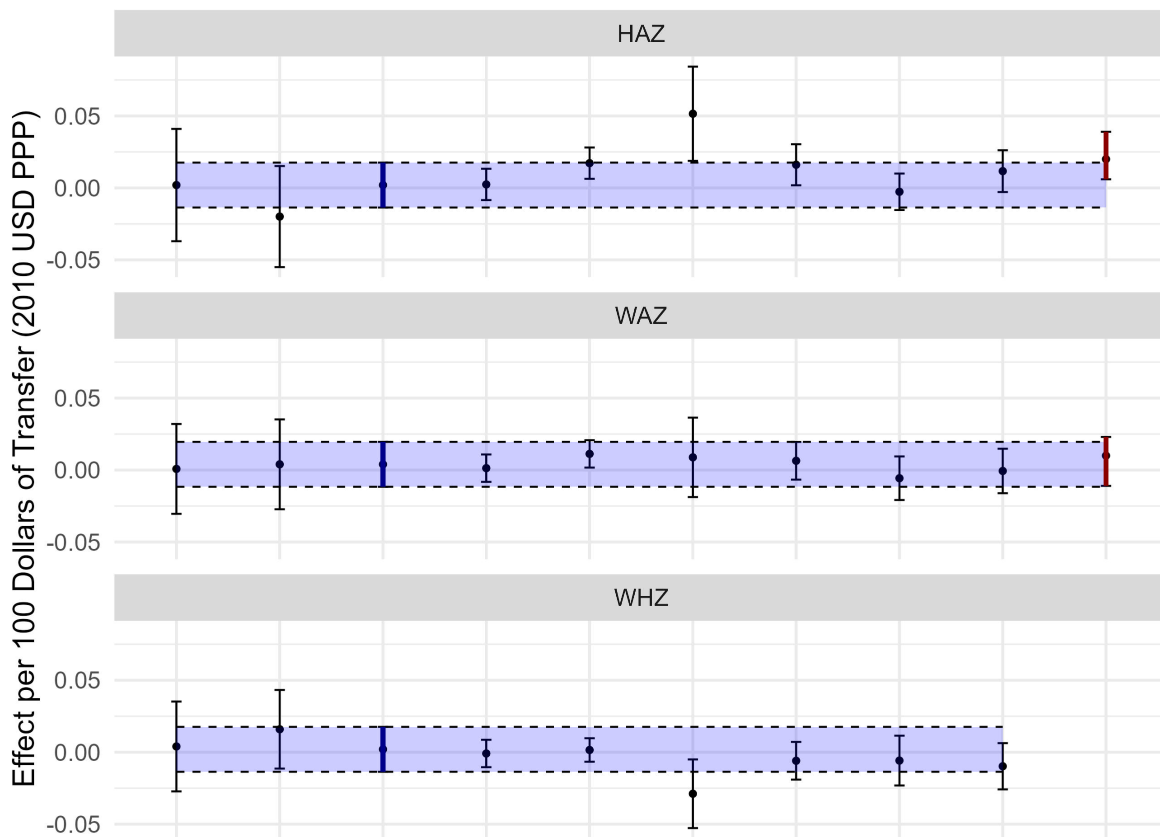
This figure shows how the intervention and data collection were carried out from 2018 to 2021. The project consisted of two separate "phases" – five initial districts (phase 1) and three additional districts (phase 2) in which implementation began approximately six months after the original set of districts. Data collection blocks marked with an asterisk (*) were conducted over the phone rather than in person. Months in which project activity ceased due to the COVID-19 pandemic have been omitted from this figure. In both phases, there was no baseline survey, and an in-person endline survey were conducted after one year of implementation. In phase I, an in-person, year two endline survey was halted midway through March 2020 due to COVID-19, and a phone survey was conducted from May to June of 2020 for the remainder of the sample. In phase II, a year two endline survey was conducted via phone, but split into two separate surveys due to concerns over survey length. In both phases, an in-person survey was conducted in September to December of 2021.

Figure A.3: Study Map



This figure shows the sub-districts of Jharkhand in which this study was conducted. Shaded blocks are those included in our study, with the two colors corresponding to two phases of treatment rollout (see Figure A.2). Dark lines denote district boundaries, and light lines denote Taluks, or sub-districts. Points represent the AWCs in our sample, with the color and shape of each corresponding to the different treatment groups the AWCs were assigned to.

Figure A.4: Effects of Unconditional Cash Transfers on Child Development Outcomes



Study	JHICDS	JHICDS	JHICDS	Ahmed	Ahmed	Carneiro	Carneiro	Field	Field	Crosta
Country	India	India	India	Bangladesh	Bangladesh	Nigeria	Nigeria	Myanmar	Myanmar	Many
Transfer Length	12 Months	24 Months	24 Months	24 Months	24 Months	12 Months	24 Months	24 Months	24 Months	NA
Communication	Framing	Framing	Framing	NA	SBCC	Information	Information	NA	SBCC	NA

This figure displays the estimated effects of unconditional cash transfers on Child Development Outcomes for several studies similar to Jharkhand ICDS. Cash transfer sizes are converted from local currency using World Bank PPP adjustments, calculated cumulatively (i.e. transfer size times total months of transfer), and used to divide point estimates and standard errors for the effects of treatment on HAZ, WAZ, and WHZ. The total length of time that transfers were received and information on any communication received are presented in the key. The studies considered in addition to ICDS are as follows: Ahmed et al. (2025), Carneiro et al. (2021), and Field and Maffioli (2025). The vertical range of the blue rectangles corresponds to the standardized confidence interval for Jharkhand ICDS Y3. WAZ is not reported as an outcome in Ahmed et al. (2025) and is thus excluded.

B Appendix: Simulated Power Calculations

Section 4.4 estimates whether there is an interaction between sanitation and income in the production of child anthropometrics. Although the cash transfer is exogenous, the sanitation variation is cross-sectional, requiring us to interpret the interaction in a nuanced way.

A stronger experimental design to test for interactions would also induce exogenous variation in sanitation, such as the sanitation promotion campaign studied in Patil et al. (2014). Such an experiment would have three treatment arms: one receiving cash transfers, one receiving sanitation promotion, and one receiving both. We would then test for complementarities between income and sanitation intervention with:

$$Y_{ia} = \alpha + \beta_1 * TREAT^1_{ia} + \beta_2 * TREAT^2_{ia} + \beta_3 * TREAT^1_{ia} * TREAT^2_{ia} \tag{2}$$

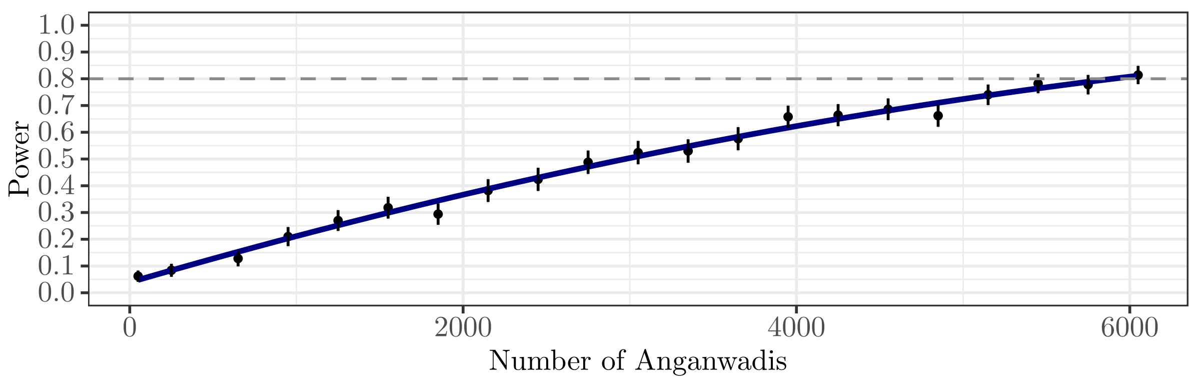
where $TREAT^1_{ia}$ and $TREAT^2_{ia}$ are dummy indicators for the cash and sanitation interventions, respectively. We are interested in evaluating the power of a test of $H_0 : \beta_1 + \beta_2 = \beta_3$ vs. $H_1 : \beta_1 + \beta_2 \neq \beta_3$ under different sample sizes and plausible design choices. Since detecting interactions has significantly greater power requirements than testing for level effects (Muralidharan et al., 2023), we conduct these power calculations to determine whether such an experiment would be feasible.

We simulate this data generating process, varying the number of Anganwadi centers that are evenly divided among four treatment arms (each with five households, our sample average). Household WAZ is generated by taking random draws from a normal distribution with a mean equal to the control mean for WAZ as reported in our household surveys and intra-cluster and inter-cluster variances corresponding to those observed in our sample. Treatment effects are simulated by assigning to each household a baseline level of toilet non-usage drawn from the empirical distribution of that variable as collected in household surveys and subtracting from this rate — in the relevant two treatment arms— the treatment effect as reported in Patil et al. (2014). We then estimate the treatment effect based on the estimates of treatment effects in our sample, add this to the simulated WAZ for the relevant treatment groups, and run the test described above.

We conduct this procedure across a range of possible sample sizes (in terms of AWCs), in each case running the simulation 500 times (see Figure B.1). Results suggest that in order to achieve 80% power, such an experiment would require coverage of approximately 6,000 Anganwadis.

This is an order of magnitude higher than most field experiments recently conducted in similar contexts (Field and Maffioli, 2025; Carneiro et al., 2021; Ganimian et al., 2024). It is also over 12 times larger than the number of anganwadis in our current study. In addition to the much larger budget that such a study would entail, it would also be logistically much more challenging to ensure implementation quality and design fidelity across multiple treatment arms at such large scales. Hence, budgetary and practical constraints would likely preclude a fully experimental test of the hypothesis of significant interactions between income and sanitation in the child health production function. As a result, the evidence we present – of a significant interaction between experimentally varied income transfers and cross-sectional variation in sanitation in the child health production function – probably provides the most practically feasible experimental evidence on the existence of this interaction (along with additional analysis to confirm that this is not being driven by interactions of income with other observable correlates with sanitation).

Figure B.1: Simulated Power Curve for a Cash × Sanitation RCT



C Appendix: Variable Construction

This appendix presents detailed information on the construction of variables in this study, including technical material motivating indices or measures with clinical justification.

C.1 In-person measures of food consumption

In order to accurately gauge household consumption of nutrients, we used an intensive measurement protocol taken from the nutrition literature. We conducted a 24-hour recall history with the household in which they were instructed to list everything that they had eaten during the previous calendar day. The next steps were:

- Collect measurements of ingredients used in every food item. For each food item, respondents were asked to list every raw ingredient used to cook the item. We then measured the amount of that ingredient that they used by measuring the capacity of the utensils used to put in each ingredient.

  For example, suppose the respondent had used a particular ladle to measure out raw yellow daal for their dinner on the previous day. We would take the ladle they used to measure out the daal, fill it with water, and then pour that water into a beaker to measure ladle capacity in milliliters. We would then ask how many ladle servings they had put in (including partial servings) and convert that to an amount used of that ingredient. Using data from Longvah et al. (2017) on the nutritional content of raw ingredients used in Indian cooking, we then converted the raw ingredients to nutrient amounts (e.g. 100 grams of "black gram, daal" contains 23.1 grams of protein, 1.7 grams of fat, etc.).

- Collect measurements of the total amount of cooked food for every meal cooked yesterday. This means measuring the capacity of the vessels (in milliliters) in which food was cooked using the same methodology of pouring water into a beaker. We then record the level until which these vessels were filled after the item was cooked and converted that to a total volume cooked.

- Collect measurements of the share eaten of each cooked item by the mother and child. This meant asking the respondent to show how much they ate out of the cooked items in the unit of serving utensils (e.g. three spoonfuls of cooked yellow daal). We convert that to an amount in milliliters by using the same technique to measure the capacity of the utensils used to serve the food.

We combine these three measures to derive a precise measurement of the amount consumed by individual household members (mother and child). This is equal to the total share of cooked item consumed multiplied by the nutritional content of the ingredients for that item, summed across all the cooked items. Additionally, we collected measurements of any complementary feeding of the child (other than breastmilk) such as biscuits or snacks that were not produced at home. We convert those to nutrients using approximations of the nutritional content of those items. A longer set of instructions is available at this link.

C.2 Nutritional Measures

- **Dietary diversity score**: Computed as the number of the following dietary groups consumed by the respondent or child: (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; based on and calculated in accordance with the WHO's (2008) "Indicators for assessing infant and young child feeding practices"

- **Minimum meal frequency**: Binary indicator taking value 1 if the child meets WHO (2008) guidelines for the minimum number of times that children should consume solid, semi-solid, or soft foods and 0 otherwise; minimum frequency is twice per day for breastfed infants 6-8 months, three times per day for breastfed children 9-23 months, and four times per day for non-breastfed children 6-23 months.

- **Nutrient index**: Taking the recommended daily value (as prescribed by the National Institute of Nutrition (2011) for calories, protein, visible fat, calcium, iron, thiamine, riboflavin, niacin, pyridoxine, and dietary folate, we calculated the fraction of this that respondents consumed and averaged across all items to construct the nutrient index. Foods were translated into nutrients using the Indian Food Consumption Tables as described above (Longvah et al., 2017).

C.3 Sanitation

- **Sanitation index**: As a summary measure of sanitation environment, we construct a principal components index from variables related to the sanitary environment. To do this, we take the first principal component of the five questions listed below. We prefer the principal components approach to other types of indices (e.g. equal weighting) because these variables reflect the underlying sanitation environment to different extents and the PCA weights may at least partially account for this:[50]
  - Observable feces: upon exiting the house after the survey, the surveyor looked to see whether there was human feces in the vicinity of the house and marked either yes/no
  - Wastewater: upon exiting the house after the survey, the surveyor looked to see whether there was any wastewater in the vicinity of the house and marked either yes/no
  - Open sewage ditch: upon exiting the house after the survey, the surveyor looked to see whether there was an open sewage ditch around the house (as opposed to no ditch or closed drains) and marked either yes/no
  - Fraction of the household's neighbors who own a toilet: respondents were asked whether none (0), some (1), or all (2) of their neighbors owned toilets
  - Fraction of the household's neighbors who use a toilet: respondents were asked what fraction of their neighbors use toilets as opposed to defecating in the open: all (1), mostly latrine (0.75), about half-half (0.5), mostly open defecation (0.25) or all open defecation (0).

C.4 Social & Behavioral Measures

- **Nutritional knowledge index**: Discrete variable taking values between 0 to 6, corresponding to the number of the following questions or statements that the mother provides an answer in line with clinical recommendations:

[50] We would ideally construct an index in which the weights reflect the extent to which each factor affects child health, but we lack a well-estimated production function linking sanitation and health to do this. Regardless, the index is closely linked to neighborhood-level open defecation – the R^2 of a regression of the index on neighbor *usage* of toilets is 0.7 and results are quite similar if we examine heterogeneity with respect to open defecation directly rather than the index.

- How much should you eat during pregnancy: more than normal, the same amount as normal, or less than normal?
  - How much should you eat while breastfeeding: more than normal, the same amount as normal, or less than normal?
  - Eating more during pregnancy affects child intelligence.
  - Eating more during pregnancy affects child height.
  - Eating more as a child affects child intelligence.
  - Eating more as a child affects child height.

- **Anganwadi services received**: Discrete variable taking values between 0 to 9 corresponding to the number of the following AWC services that the respondent received in the previous year: (1) deworming, (2) government schemes, (3) growth measurement, (4) hot cooked meals, (5) iron/calcium tablets, (6) nutrition information, (7) pre-school, (8) take-home rations, (9) vaccination

- **Empowerment index**: Discrete variable taking values between 0 to 5 corresponding to the number of the following questions adapted from J-PAL's "Practical Guide to Measuring Women's and Girls' Empowerment in Impact Evaluations" (Glennerster et al., 2018) that indicate respondent empowerment:
  - The last time you went to a relative or acquaintence's house inside the village, did you have to take permission from other members of your household?
  - The last time you went to the market without your village, did you have to take permission from other members of your household?
  - Do you have to ask someone for money if you want to purchase items from the market?
  - Imagine that you were home alone without your spouse or guardian and one of your children was very sick. Could you make the choice on your own to purchase medication to treat your child?
  - Suppose you earned Rs. 300 as part of a government program. Who would decide how to spend it?

- **Depression index**: Discrete variable taking integer values from 0-5, corresponding to the number of the following questions from the Patient Health Questionnaire-9 that are answered in a way indicating the presence of depressive disorders:
  - In the last 2 weeks, how often have you felt nervous or stressed?
  - Often there are multiple tasks that you have to do in a day like cooking, cleaning, taking care of your child, etc. In the last 2 weeks, did you feel that you couldn't manage all these tasks?
  - In the last 2 weeks, how often did you have trouble falling or staying asleep, or sleeping too much?
  - In the last 2 weeks, how often were you feeling tired or having little energy?
  - In the last 2 weeks, how often were you having trouble concentrating on things?

- **Probability of visiting a formal medical provider**: Binary indicator that takes value 1 if the child has visited a government doctor/hospital/clinic/PHC, private doctor/hospital/clinic, or an ANM/sub-centre and 0 otherwise

- **Total illnesses in the past three months**: Discrete index corresponding to the number of the following distinct illnesses or ailments experienced by the child in the previous three months: (1) cold/cough/fever, (2) diarrhea/vomiting/stomach infections, (3) malaria/jaundice/dengue/other vector-borne diseases, (4) measles/chickenpox, (5) pneumonia, (6) physical injuries/fractures, (7) other illnesses not listed here

C.5 Anthropometric Measures

- **HAZ**: Height-for-age z-score; computed using Stata's **zanthro** command (Vidmar et al., 2013) and the WHO Child Growth Charts (2006)
- **WAZ**: Weight-for-age z-score; computed using Stata's **zanthro** command (Vidmar et al., 2013) and the WHO Child Growth Charts (2006)
- **Moderately stunted**: Binary variable that takes value 1 if a child has HAZ $<$ -2 and 0 otherwise
- **Moderately underweight**: Binary variable that takes value 1 if a child has WAZ $<$ -2 and 0 otherwise
- **Severely stunted**: Binary variable that takes value 1 if a child has HAZ $<$ -3 and 0 otherwise
- **Severely underweight**: Binary variable that takes value 1 if a child has WAZ $<$ -3 and 0 otherwise
- **Child development index**: Index taking values 0-90 (Year 2) or 0-170 (Year 3) computed by summing scores associated with one of the following lists of questions asked to respondents, where 10 is granted for "Yes", 5 is granted for "Sometimes", and 0 is granted for "No"
  - **Year 2 Questions**
  - Does your child run, stopping herself and without bumping into things or falling over?
  - Does your child climb on furniture?
  - Can your child remove clothes on her own without your help?
  - When your child wants something does she tell you by pointing to it?
  - When you ask your child to, does she go into another room and find familiar objects or toys? For example you might ask your child to "Bring water" or "Go get your chappal (sandals)"
  - Does your child say words other than "Mama" and "Papa?" For example words may include "Bakri (goat)" or "Gai (cow)" or "Kaan (ear)" or "Naak (nose)"
  - Does your child say short sentences? Such as "Khaana do (Give me food)" or "Mama paani do (Mama, give me water)" or "Yeh kya hai? (What is this?)" or "Mera haath pakdo (Hold my hand)"
  - Does your child scribble?
  - Has your child started eating food on her own?
  - **Year 3 Questions**
  - When you ask "What is your name?" Does your child say her full name?
  - Can your child tell the name of two or more family members or playmates?

- Can your child tell the correct name of the village/tola/block she stays in?
- Can your child tell which day of the week it is today?
- Can your child count to 5?
- Can your child count to 10?
- Can your child count to 20?
- Can your child name the primary colours (red, yellow, blue)?
- Does your child walk either up or down at least two steps of stairs by herself without holding onto the railing or wall?
- Without holding anything for support, does your child kick a ball by swinging her leg forward?
- Does your child catch a large ball with both hands?
- Does your child serve herself, taking food from one container to another using utensils? For example, does your child use a serving spoon to take rice?
- Does your child unbutton one or more buttons?
- Does your child use a pencil, crayon, or pen for writing or drawing and hold it properly like an adult between thumb and finger?
- Can your child draw a basic figure?
- Does your child brush her teeth by putting toothpaste on the toothbrush and brushing all her teeth without help?
- Can your child do paper folding?