ECONOMIC GROWTH CENTER

YALE UNIVERSITY

P.O. Box 208629
New Haven, CT 06520-8269
http://www.econ.yale.edu/~egcenter/

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Endowments and Investments within the Household: Evidence from Iodine Supplementation in Tanzania

Achyuta Adhvaryu
Yale University

Anant Nyshadham
Yale University

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Endowments and Investments within the Household: Evidence from Iodine Supplementation in Tanzania

Achyuta Adhvaryu and Anant Nyshadham

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Abstract

Standard theories of resource allocation within the household posit that parents' investments in their children reflect a combination of children's endowments and parents' preferences for child quality. We study how changes in children's cognitive endowments affect the distribution of parental investments amongst siblings, using data from a large-scale iodine supplementation program in Tanzania. We find that parents strongly reinforce the higher cognitive endowments of children who received in utero iodine supplementation, by investing more in vaccinations and early life nutrition. The effect of siblings' endowments on own investments depends on the extent to which quality across children is substitutable in parents' utility functions. Neonatal investments, made before cognitive endowments become apparent to parents, are unaffected. Fertility is unaffected as well, suggesting that inframarginal quality improvements can spur investment responses even when the quantity-quality tradeoff is not readily observable.

Key Words: endowments, intra-household, child health, Tanzania

JEL Codes: I14, I15, I18, O12

1 Introduction

A growing number of recent studies provide evidence that children's "endowments" in early life–for example, in health or cognitive ability–can have large effects on later-life health and economic outcomes, some of which persist even into adulthood.[1] It is natural to ask, then, how household behaviors, particularly as relate to resource allocation amongst children, respond to shifts in these endowments. Investments in children, as outcomes of the household's allocation problem, are in general jointly determined by parents' preferences for each child; the production function for child quality; and the intra-household distribution of initial endowments. Depending on the parameters of this allocation problem, parents may choose to reinforce or compensate for initial endowments. The sign and magnitude of these behavioral responses indicate whether–and by how much–parental investments magnify or dampen the biological effects of early-life interventions. Moreover, these responses contain information about the extent to which interventions targeting one child can have an impact on other children in the household via shifts in the intra-household distribution of resources.

In this paper, we study the way in which health investments in children respond to changes in their cognitive endowments, using variation induced by an iodine supplementation program in Tanzania. The program, which came as a stopgap measure by the Tanzanian government to curb the high rate of iodine deficiency, distributed iodized oil capsules in at-risk districts, primarily targeting mothers of childbearing age and young children. We focus on the effects of *in utero* exposure to iodine, because fetal iodine is crucial to the development of neural networks in the first trimester of pregnancy. Access to fetal iodine thus partially determines cognitive ability at birth. Using a procedure developed by Field et al. (2009, hereafter FRT), who find that the same intervention had large effects on schooling attainment, we assign a probability of being treated to each child in our sample.[2]

We find that children treated with iodized oil capsules while *in utero* are more likely to receive necessary vaccines and are breastfed for longer. Further, our data allow us to exploit exogenous variation in the *child-specific* price of quality induced by the iodine intervention, enabling identification of the effects of both own *and* siblings' endowments on investments. In

[1] For example, Behrman and Rosenzweig (2004) find a positive relationship between birth weight and adult height and schooling attainment in the US. Black et al. (2007) verify these results using data from Norway, finding positive effects on IQ and earnings. Almond (2006) shows that *in utero* exposure to the 1918 influenza pandemic in the United States negatively affected completed educational attainment, income, socioeconomic status and adult health. Similarly, Almond et al. (2009) find negative effects of prenatal exposure to radioactive fallout from Chernobyl on academic performance, but no effect on health outcomes. Field et al. (2009) find positive effects of *in utero* iodine supplementation on schooling attainment in Tanzania. Cutler et al. (2010) show that cohorts who benefited in early life from a malaria eradication campaign in India had higher economic status as adults. Baird et al. (2011) find that school-based de-worming can lead to gains in educational attainment, health and productivity in adulthood. Bharadwaj et al. (2011) find that birthweight differences in twins are correlated with test score differentials which persist until at least the 8th grade.

[2] In the appendix, we replicate our main results using alternate definitions of treatment.

line with the predictions of a simple model of intra-household resource allocations, we find that the effects of siblings' endowments on own investments depends on the extent to which quality across children is substitutable in parents' utility function. Finally, we find that while health investments in infancy respond to endowment shifts, neonatal and later-childhood investments do not. That neonatal behaviors do not change provides evidence that parental investments respond directly to observed changes in the child's cognitive ability, rather than being driven by other aspects of the intervention program. That later childhood investments do not respond to endowments is consistent with children acquiring agency as they grow older.

Previous studies of the link between endowments and household resource allocations have relied on one of four endowment measures: 1) birth weight (Rosenzweig and Zhang 2009); 2) imputed residuals from health production function estimation (e.g. Rosenzweig and Schultz 1983; Rosenzweig and Wolpin 1988; Pitt et al. 1990); 3) sickness in infancy (Conti and Heckman 2011); and 4) early cognitive function as measured by a psychological test (Aizer and Cunha 2011). In general, these papers used fixed effects models to examine how within-siblings or within-twins differences in endowments generate differential parental investments.

We believe our study improves upon this previous work in four ways. First, the iodine supplementation program we study induces variation in a very specific dimension of the child's endowment–cognitive ability–whereas all but the fourth measure above are non-specific proxies which may reflect many dimensions of the child's endowment. Thus, for example, while the studies above which examine birth weight differences within twins isolate variation which is uncorrelated with maternal or household preferences, spacing decisions, household income paths and the like, it is unclear what the mechanism driving the investment responses of parents is. Indeed, even amongst monozygotic twin pairs, larger twins are larger due to unequal consumption in the womb, and the probable mechanisms underlying this differential consumption are many (Behrman and Rosenzweig 2004).

Second, previous studies which rely on siblings fixed effects models for identification may be misspecified, since even within siblings, unobserved prenatal, neonatal and early-infancy investments in each child likely correlate with whichever endowment measure is used and also with the extent of investment during the remainder of childhood. As we argue later in the paper, in our context exposure to the supplementation program was effectively random, and thus cognitive gains from program exposure, which become apparent to the parent only after birth, should be orthogonal to investments before and at the time of birth. Further, we test directly for this orthogonality using data on neonatal behaviors, and find evidence in support of our claim.

Third, we expand the set of resource allocation behaviors studied in previous work to include not only neonatal and early-life investments, but also the allocation of resources in later childhood. Using investments at different stages of childhood enables us to examine when the link between parental investments and endowments is most salient.

Fourth, and perhaps most importantly, since all the previous work in this area has restricted attention to within-sibling or within-twin estimates, studies have been unable to separately identify own endowment vis-à-vis sibling endowment effects. Since our empirical strategy does not necessitate the imposition of similar fixed effect models, we are able to identify these two endowment effects separately.

The remainder of the paper is structured as follows. Section 2 describes the conceptual framework. Section 3 describes the data. Section 4 explains the empirical strategy. Section 5 presents the results, and section 6 concludes.

2 Model

In this section, we develop a simple model which relates child endowments to intra-household allocations. The model generates predictions regarding parents' investment responses to shifts in their children's endowments of quality.[3] The key assumption is that an upward shift in a child's endowment lowers the child-specific price of quality, which in turn affects investments in that child, as well as all other children in the household, through the optimal re-allocation of quality. We show that the way in which children are affected by a shift in their sibling's endowment depends on the elasticity of substitution of child quality in the household utility function. This parameter is also related to the extent to which parents are averse to inequality amongst their children.

2.1 Setup

Consider a household with two children indexed $i = 1, 2$.[4] Each child is born with an exogenously given endowment of quality e_i. The distribution of endowments determine the within-family distribution of prices of investment in additional quality, q_i, which parents may decide to undertake. We denote the price of quality for child i as $p_i \equiv p(e_i)$, where p is a decreasing function of e, capturing the fact that a dollar of investment in quality will yield larger returns for the child with a relatively higher endowment.[5]

Parents value the quality of their children, with preferences represented by a household utility function $u(q_1, q_2)$. We adopt a standard constant elasticity of substitution (CES) utility

[3] We abstract away from cognitive vs. non-cognitive measures of quality here; the model delivers qualitatively similar predictions when there are multiple dimensions of quality.

[4] The model, with some additional restrictions, easily generalizes to an n-child household.

[5] Note that this assumption is equivalent to assuming that endowments and investments are complements in the production function for quality. Our model would yield similar predictions even when endowments are taken as inputs into the production of quality rather than as price shifters. The assumption of complementarity is common in the literature on the effects of health endowments as well as cognitive endowments (see, for example, Aizer and Cunha (2011)).

function, which parameterizes the extent to which children's qualities are complements or substitutes. Specifically, let parents' utility be given by

$$u(q_1, q_2) = \left(\alpha q_1^{\frac{\gamma-1}{\gamma}} + (1-\alpha) q_2^{\frac{\gamma-1}{\gamma}} \right)^{\frac{\gamma}{\gamma-1}}. \tag{1}$$

$\alpha \in (0,1)$ represents the relative utility weight given to the quality of child 1, and $\gamma \in (0, +\infty)$ is the elasticity of substitution. Child quality is complementary when $\gamma < 1$, and substitutable when $\gamma > 1$.

The household's utility maximization problem, given endowments (prices) and income M, can be written as

$$\max_{q_1, q_2} \left(\alpha q_1^{\frac{\gamma-1}{\gamma}} + (1-\alpha) q_2^{\frac{\gamma-1}{\gamma}} \right)^{\frac{\gamma}{\gamma-1}} \qquad \text{s.t.} \tag{2}$$

$$p_1 q_1 + p_2 q_2 \leq M \tag{3}$$

$$p_i = p(e_i), \quad i = 1, 2 \tag{4}$$

Combining the two first order conditions from the Lagrangian of the above maximization problem, we obtain the following relationship between the demands for quality q_1 and q_2:

$$q_1 = \left(\frac{\alpha p_2}{(1-\alpha)\, p_1} \right)^{\gamma} q_2. \tag{5}$$

Denote $\Gamma \equiv \left(\frac{\alpha p_2}{(1-\alpha) p_1} \right)^{\gamma}$. Using the budget constraint and the above equality, we obtain the demands for quality as functions of prices, income and the model's primitives:

$$q_1 = \frac{M\Gamma}{\Gamma p_1 + p_2} \tag{6}$$

$$q_2 = \frac{M}{\Gamma p_1 + p_2}. \tag{7}$$

2.2 Investment responses to endowment changes

We now examine the effects of a shift in the endowment of child 1 on investments in quality for both children. These comparative statics generate predictions on the optimal intra-household re-allocation of parental investments in response to shifts in child-specific endowments. Because we have modeled endowments as factoring only into prices, equivalently we can examine the effects of a shift in p_1. Differentiating the demand functions 6 and 7 with respect to p_1 and rearranging terms, we obtain

$$\frac{\partial q_1}{\partial p_1} = \frac{M}{(\Gamma p_1 + p_2)^2}\left(p_2\frac{\partial \Gamma}{\partial p_1} - \Gamma^2\right) \tag{8}$$

$$\frac{\partial q_2}{\partial p_1} = -M\frac{\Gamma + p_1\frac{\partial \Gamma}{\partial p_1}}{(\Gamma p_1 + p_2)^2}. \tag{9}$$

The signs of the own- and cross-price elasticities thus depend in part on $\frac{\partial \Gamma}{\partial p_1} = -\Gamma\left(\frac{\gamma}{p_1}\right) < 0$. From here, it is apparent that $\frac{\partial q_1}{\partial p_1} < 0$. The sign of $\frac{\partial q_2}{\partial p_1}$ depends on $\Gamma + p_1\frac{\partial \Gamma}{\partial p_1}$. From above, $\frac{\partial q_2}{\partial p_1} < 0$ if and only if $\Gamma + p_1\frac{\partial \Gamma}{\partial p_1} > 0$. With some algebra, this reduces to the condition that $\frac{\partial q_2}{\partial p_1} < 0$ if and only if $0 < \gamma < 1$. Thus, the main predictions of the model, which we later test in the data, are the following:

1. A rise in a child's own endowment generates increases in parental investments in that child.
2. A rise in a sibling's endowment increases own investments if child quality is complementary in the household's utility function, and decreases own investments if quality is substitutable.

3 Data

In this section, we provide details on the data; variables used in analysis and their construction; and the matching procedure used to identify residence in treatment districts.

3.1 Tanzania Demographic and Health Surveys (DHS)

We use the 1999, 2004 and 2007 rounds of the Tanzania DHS. As described below, many of the early-life investment variables, such as receipt of vaccinations and duration of breast-feeding for example, are only recorded for children under five years old. Thus, our main sample is the sample of live children under five in 1999. There are 483 such children in 1999.

Data on bed net usage was not collected in the 1999 wave of the DHS and is therefore only available in the 2004 and 2007 waves. In these waves, children under the age of 5 exhibit little variation in treatment probabilities, as they were mostly born after the supplementation program ended and its immediate effects on protection against IDD had dissipated. Accordingly, for analysis of bed net related investments, we use the pooled sample of all children aged 5 and above in 2004 and 2007 waves. There are 4656 such children.

Finally, we make use of a third sample of children aged 5 and above in only the 2007 wave for analysis of later-life possessions. Information on later-life possessions is not available for the

1999 and 2004 waves. There are 2028 children aged 5 and above in our extract of the 2007 DHS.

Note that we are restricting attention to only those children living in districts that were visited at least once during the supplementation program. This is because program districts were not chosen randomly, but rather selected on the basis of visible goiter rates. In this sense, comparison between districts visited at least once during the program and those excluded from program participation is rendered invalid.

The DHS collects information on demographic characteristics of all household members. We use the following demographic variables: child's birth year and birth month, child's gender (female dummy), child's gender-specific birth order, mother's age, mother's educational attainment (categories of education completed; see next section for details), total number of children in the household, number of older and younger siblings, the sum of ages of all children in the household, the maximum and minimum age of children in the household, total household size (including adults), and an urban dummy. Means and standard deviations of these variables are reported in Table Ia by sample.

The first two columns of Table Ia present means and standard deviations of child, mother, and household level covariates included in the analysis. These statistics are reported separately for the under 5 1999 DHS sample, 5 and over 2004 and 2007 pooled sample, and the 5 and over 2007 sample. Columns 3-6 show the same summary statistics for two subsamples of each sample: children with treatment probability of .75 and above and children with treatment probability below .75. The treatment probability is largely determined by month and year of birth and so we might expect to find slight variations in the mean of age and mother's age across these two subsamples. This, of course, motivates the inclusion of these variables as covariates. We will, in fact, conduct all of our analysis using fixed effects for the child's age in years to avoid any issues deriving from this systematic relationship between timing of birth and treatment probability.

Nevertheless, we see that the differences in mean age of the child and mother across the two subsamples are small, measuring less than a standard deviation in across all samples and in most cases less than a year. Otherwise, the means of the remaining covariates show only minimal differences across the two subsamples, with no difference having any statistical significance at conventional levels.

Apart from demographic information, the DHS contains detailed information on early-life health investments for all children under five in surveyed households. We use the following investment variables: dummies for receipt of the first, second and third doses of polio and DPT (diphtheria, pertussis and tetanus combination) vaccines; dummy for receipt of the measles, BCG and polio 0 vaccines; dummy for formal-sector delivery and assisted delivery; dummy for possession of a health card to track vaccinations; the duration of breastfeeding in months; and a simple additive index for receipt of various fluids (water, milk, juice etc.). Means and standard deviations of these variables are reported in Table Ib.

Finally, we examine some later-childhood investments which are recorded in the DHS. Specifically, we look at bed net ownership and usage variables (household owns any net; household owns a currently treated net; household owns a net that was treated at some point), and at basic ownership of resources (does the child own a blanket, shoes, and more than one pair of clothes). Means and standard deviations of these variables are reported in Table Ic.

3.2 IOC Supplementation Program Data

The DHS also includes, upon request, geocode data for the sampling clusters. Coordinates are skewed using a random skewing algorithm which skews the coordinates by a distance drawn from a uniformly distributed probability area with radius of 15km around the actual coordinate pair. To identify which households live in intervention districts, we plotted the geocode data in each wave onto a raster map of Tanzania with district borders. We used this mapping to identify which clusters fell inside treatment districts (with a very small random probability of misclassification due to geocode skewing by DHS). We obtained data on the names of intervention districts and program years from FRT. We have reproduced their table of intervention districts, years and coverage rates here as Table A.1 in the Appendix. We use these data (as described in the next section) to construct, as FRT do, a measure of program participation.[6]

4 Empirical Strategy

In this section, we define program participation and discuss our empirical specification.

4.1 Definition of Program Participation

Our goal in this section is to define a treatment variable which encapsulates the extent to which an individual was treated with IOC supplementation while *in utero*. We generally follow the procedure outlined in FRT, and note where our procedure differs from theirs. To construct the treatment indicator, we combine information on the following:

1. the month and year of birth of the child;
2. the district of the mother's residence at time of survey;
3. years in which the program was rolled out in each district;
4. and the biological properties of iodine within the body.

[6] We are very grateful to Erica Field and Omar Robles for their help in this process.

Suppose we knew exactly when each mother received IOC supplementation. If this were true, we could calculate how long after receiving supplementation the woman was pregnant, and thus we could determine (after making some assumptions on the rate of decay of iodine in the body) the exact amount of exposure for the fetus.

However, we do not know the date of supplementation (nor, in fact, whether a given mother received the IOC at all). Thus, as in FRT, we must instead calculate the probability with which an individual was treated with IOC while *in utero*. FRT begins by assuming, using administrative records from the program roll-out, that each roll-out took three months, and that the timing of this three-month period was uniformly distributed over the roll-out year.

FRT then couples this probability with information on the birth month and birth year of the child and the biological properties of iodine within the body to arrive at a final probability of treatment for each individual. IOC supplementation allows for normal development of neural networks of fetuses in the first trimester of pregnancy, but not thereafter. Thus, the intervention can only be effective if iodine from the IOC is present in the body during the first trimester.

To approximate *how much* iodine is present at various times, FRT use information pertaining to the decay of iodine in the body. 85 percent of the iodine is extracted immediately through urination, and the rest is assumed to follow a hyperbolic decay pattern. Additionally, a lower cutoff level is assumed, after which there is too little iodine left in the body for adequate protection against fetal iodine deficiency disorder. These values, as well as the half-life formula deriving from the assumed hyperbolic iodine depletion, are detailed in FRT's Web Appendix.

The procedure described above generates a probability of treatment for each month following a roll-out for four years after a roll-out year (after which the probability is uniformly 0). These probabilities are reported in Appendix Table 2 (replicated from FRT). Coupling data on the birth month and birth year of each child with data on program roll-out years in each intervention district, we can assign each individual in our data a probability of treatment.[7] Since program roll-outs happened up to five times in a given district, individuals may have multiple instances of exposure to IOC. In these cases, we use the maximum of the multiple assigned probabilities for that individual.

To estimate the effects of siblings' treatment probabilities on the individual, we add up the treatment probabilities of the two immediately older siblings and the two immediately younger siblings in the child's household (which may include half-siblings from a different mother) to generate a total sibling probability of treatment. If the child has, in fact, less than two older and/or two younger siblings, zeros are imputed for the treatment probabilities of these non-existent siblings. In this way, we may compare children with a differing number of siblings on

[7] Unlike FRT, who use the Tanzania Household Budget Survey for the majority of analyses, we know the birth year *and* birth month of each child, so we need not generate a year-level measure of treatment probability using the weighting procedure (using seasonality of births) outlined in FRT. We instead use the raw treatment probabilities in the matrix shown in Appendix Table 2.

the basis of total treatment within the household. Treatment probabilities of siblings more than two spaces ahead or behind the child in the birth order are ignored, with the assumption that, for example, decisions of whether to vaccinate two children of vastly different ages are not made contemporaneously.

For later analysis, we separate this sibling treatment into same sex and opposite sex sibling treatments. To do so, we perform the same procedure as just discussed including the two older and two younger siblings only if they are of the same or opposite sex as the child in question, alternately. That is, we still exclude siblings more than two spaces ahead or behind the child in birth order, but further exclude siblings who are of the unwanted sex in each variable, such that the sum of opposite and same sex sibling treatments will give us the composite sibling treatment defined above.

4.2 Empirical Specification

We now turn to estimation of the effects of IOC supplementation on early-life (and some later-childhood) health investments, using the above constructed indicator of treatment. Denote the investment (e.g. vaccinations, length of breastfeeding, etc.) as I; own treatment probability as T^o; siblings' total treatment probability as T^s; i as child (which is the level of observation); j as household; k as district; and X as child- and household-level controls. We estimate models of the following specification:

$$I_{ijk} = \alpha + \beta T_i^o + \gamma T_j^s + \mathbf{X}_{ij}'\delta + \mu_k + \sum_{a=0}^{A} \zeta_a \mathbf{1}(age = a) + \varepsilon_{ijk} \tag{10}$$

We focus attention on two important sets of controls: district fixed effects (μ_k) and (integer) age fixed effects (ζ_a). The district fixed effects capture time-invariant elements of districts which may be correlated with demand- and supply-side factors governing adoption of health investments, as well as with treatment intensity. For example, districts with low access to vaccinations may also have been targeted more intensively for IOC supplementation due to a higher level of observed IDD (via visible goiter rates, as described in Section IIC, on program targeting, in FRT).

The age fixed effects restrict our treatment comparisons to children of the same integer age who have different treatment probabilities (either because they were born in different districts, or in separate months). Since variation in T, the treatment probability, is entirely determined by an interaction of district of birth and age in months, we must be careful to empirically distinguish between age-related trends in health investments and the true endowment effects we seek to estimate. Integer age fixed effects are thus essential, as they flexibly absorb variation in health investments related to age.

In the subsequent analysis, we also estimate the following altered specification, in which sibling treatment is split into same and opposite sex sibling treatments:

$$I_{ijk} = \alpha + \beta T_i^o + \gamma_s T_j^{ss} + \gamma_o T_j^{os} + \mathbf{X}'_{ij}\delta + \mu_k + \sum_{a=0}^{A} \zeta_a \mathbf{1}(age = a) + \varepsilon_{ijk}. \tag{11}$$

This analysis will allow us to test the predictions of the model presented in section 2 above that the cross-sibling effect depends on the elasticity of substitution between the qualities of the different children. If we believe that sex is one characteristic which affects the degree of substitutability of child qualities in the household utility function, we might expect to find positive effects of opposite sex sibling treatments and attenuated or even negative effects of same sex sibling treatments. That is, if having a high quality son is substitutable for another high quality son, but less substitutable for a high quality daughter, we would expect high treatment probabilities amongst the sons to have positive effects on investments in the daughter.

4.2.1 Controls

In addition to integer age and district fixed effects, we control for the demographic composition of siblings in the individual's household, which we must hold constant as well to make valid inferences regarding the effects of siblings' endowments on own health investments. Included are the number of girl siblings, fixed effects for the number of younger and of older siblings, the sum of age across siblings, and the minimum and maximum ages of siblings.

Finally, **X** includes various child-level and household-level controls. Included are a female dummy, gender-specific birth order fixed effects, total household size (including members other than siblings), an urban dummy, age of the mother and fixed effects for categories of the mother's educational attainment (none, incomplete primary, primary, incomplete secondary, secondary, and post-secondary).

4.2.2 Sample Restrictions

Our base sample is all children in the data born between 1986 and 2002 (inclusive) in intervention districts (i.e. districts which were targeted for IOC supplementation at least once). We focus on these years of birth because this is the maximum range within which children were potentially exposed to the program with positive probability. Outside of this birth year range (and obviously outside of intervention districts), the treatment probability is uniformly zero.

The majority of our analysis is run on the sample of children under five years old (i.e. with a maximum age of 59 months) in 1999 for whom data on vaccinations, breast-feeding etc. are available. We do not include children under five from the later waves, since their treatment exposure has insufficient variation. For later-childhood investments (bed net usage and ownership of various articles of clothing), we use the 2004 and 2007 samples of children. As relates to bed net ownership and usage, we use the sample of children aged 5 and above from both the 2004 and 2007 waves (still under the restriction outlined in the last paragraph). As relates to clothing, shoes and blankets, these variables are only defined for the 2007 sample of children between 5 and 22 (inclusive).

4.3 Potential Threats to Validity

Our goal in estimating the specification above is to capture the causal effects of shifts in own cognitive endowments and the cognitive endowments of siblings on own health investments. The most salient threat to validity of the estimation strategy in this context is the endogenous determination of fertility. If some households (or mothers) time their fertility so as to optimize the gains from IOC supplementation, then the realized treatment probability would be larger for these households. If these same households, who may hold a high preference for their children's health, make health investments more frequently, then the coefficient on treatment probability would be an upward-biased estimate of the true endowment effect.

To investigate whether households' fertility behaviors are affected by treatment, we reshaped the mother-level DHS data (appended across the 1999, 2004 and 2007 rounds) into a mother-by-year-level data set which expands the fertility histories of each mother into a panel of 5968 women spanning 47 years (the earliest birth reported in the data was in 1961). We restrict our analysis to the sample of years between 1986 and 2002 inclusive (the same birth year restriction used in the data). We are left with approximately 100,000 individual-year observations. Denote an indicator for a child birth for mother i in district j in year t as B_{ijt}. Denote T^k_{jt} as a dummy which equals one k years after a program year (t) in district j, for $k \in \{0, 1, 2, 3, 4\}$. We run the following specification relating births to program years in intervention districts:

$$B_{ijt} = \alpha + \beta T^k_{jt} + \sum_{i=1}^{I} \mu_i \mathbf{1}(\text{Mother } = i). \tag{12}$$

The above specification restricts attention to within-mother-level variation by employing mother-level fixed effects. We can thus determine whether program roll-out has effects on fertility, and if so, with how much lag. The results of the estimation of this specification for $k \in \{0, 1, 2, 3, 4\}$ are reported in Table A.3 in the Appendix. The first five columns report results for the program roll-out indicators as described above, while the last four columns in the table report results for specifications using cumulative indicators which span from the program year to the kth year after. Across all of these specifications, we find extremely small estimates tightly bound around zero, indicating that program roll-out does not have any discernable effects on mothers' fertility patterns.

Finally, we check whether the treatment probability affects the quantity of children. We do this by regressing the number of children born to a mother after a particular child on the treatment probability of that child. Again, we employ the baseline restriction by including only those children born between 1986 and 2002 surveyed in intervention districts. This leaves a sample of 6603 children. We run various fixed effect models–specifically, district-level, household-level, and finally mother-level. The results are reported in Table A.4. The estimated coefficients, which are small and again tightly bound around 0, confirm that endogenous fertility decisions do not appear to be significant confounders in our context.

5 Results

5.1 Early-life Healthcare Investments

5.1.1 Vaccinations

Table II presents results from the regression of binaries for whether the child received various vaccinations on own probability of treatment and the sum of the treatment probabilities of the two older and two younger adjacent siblings in the birth order. As mentioned above, if the child has less than two older and/or younger siblings, zeros are imputed for the treatments of non-existent siblings. Siblings more than two spaces away in the birth order in either direction are ignored, under the assumption that the investment decisions for these distant siblings are not made concurrently with that for the child in question.

All specifications include controls for age (as fixed effects) and gender of the child as well as age and education of the mother. Controls also include a dummy for whether the household is located in an urban area, the household size and fixed effects for number of older and younger siblings of the child as well as the sum, max and min of the ages of all children in the household. Finally, we include the number of females in the household and fixed effects for the child's place in a gender-specific birth order. The sample is restricted to households with at least 1 child under the age of 5 from the 1999 wave of the DHS.

Columns 1-3 show positive effects on all three doses of the polio vaccine. An increase in the probability of own treatment from 0 to 1 leads to an increase of 9 to nearly 13 percentage points in the probability of receiving polio doses. These results are evidence of reinforcement behavior in terms of parental investment decisions. That is, parents are more likely to vaccinate children with higher cognitive endowments as a result of iodine supplementation.

Similarly, the effects of own treatment on all three doses of DPT shown in columns 4-6 of Table II are positive and significant with magnitudes of between 12.5 and 14 percentage points. These effects on DPT vaccinations, like those on polio doses, are all significant at the 5 or 1 percent level. In the last column of Table II, we find a slightly smaller positive effect on receiving the measles vaccine as well, however the estimate is insignificant at conventional levels.

Notably, there appears to be no effects of sibling treatment on vaccinations on average. In the second row of Table II, we find fairly precisely estimated zeros across columns 1-7. This is an interesting result. As mentioned above, previous studies have attempted to estimate the effects of child endowments on parental investments using household fixed effects specifications. To the extent that a child's endowment effects not only parental investments in that child but also in his or her siblings, these estimates will represent the combination of these effects. In our analysis, we are able to disentangle these effects on the high endowment child from effects on siblings.

The estimates of the effects of sibling treatment presented in Table II suggest that, perhaps, these effects are negligible or even zero. However, as discussed above in Section 2, we might expect significant heterogeneity in these effects depending on the degree of substitutability between child qualities in parental utility functions. That is, we might expect high endowments amongst siblings of "distant" types (e.g. opposite sex) of a child to have positive effects on parental investments in the child, given that the qualities of the two types are sufficiently complementary in the parental utility function. On the other hand, we would expect high endowments amongst same type siblings to have a much smaller or even negative effect on investments in the child, as the income and substitution effects in this instance would go in opposite directions. We will explore gender-specific heterogeneity in effects of sibling treatment on investments below.

5.1.2 Nutrition

In Table III, we report results on the duration of breastfeeding and receipt of various fluids. Column 1 shows results from the estimation of the same specification reported in the columns of Table II, but with months of breastfeeding as the outcome. We see no significant effects of either own or sibling treatment on the number of months the child was breastfed. The point estimates are quite small relative to the mean.

However, we find in column 2 of Table III positive and significant effects of own treatment on the probability of being breastfed for at least 6 months. WHO guidelines suggest that at least 6 months of breastfeeding greatly improves the health of the child and reduces the probability of sickness. An increase in own treatment probability from 0 to 1 induces a 10.6 percentage point increase in the probability of meeting these guidelines. The estimate is significant at the 5 percent level.

Column 3 of Table III shows the regression of own treatment and sibling treatment on an index for whether the child was given a variety of fluids. The index includes water, milk, juice, etc. and represents a variety of nutrients the child needs. We find positive and significant effects of own treatment on the receipt of fluids. A treated child receives an entire category of fluids more than an untreated child, and the estimate is significant at the 5 percent level.

Again, we find no significant effects of sibling treatment. The point estimates in the second row of Table III are quite small relative to the means of the dependant variables. We interpret these coefficients as precisely estimated zeros.

5.1.3 Neonatal

We turn our attention next to neonatal investments. We suspect that parental investments are responding to observed higher cognitive endowments in the treated children. However, it is possible that parents are, at least in part, responding to an expectation of higher endowments treated children. That is, it is possible that parents believe even before the child shows signs of higher cognitive ability, indeed perhaps even before the child is born, that he will be of higher quality due to information received during or after the visit of the supplementation program to their district.

So long as fertility decisions are not made on the basis of this altered expectation, the empirical strategy employed here is valid. We verify that the program does not affect fertility decisions in the Appendix. Nevertheless, it is of some interest to explore how early parental investment responses appear, as the implied mechanism will be affected. In particular, the external validity of our estimates is considerably strengthened if we have reason to believe that parental investments in fact respond to observed variation in endowments rather than to the information dispensed by this particular supplementation program.

In Table IV, we present results from regressions of neonatal investments on individual and sibling treatments. Columns 1 and 2 of Table IV, show effects of increased probability of treatment on binaries for a formal-sector and medically assisted delivery, respectively. We find zeros for these effects, with point estimates less than a percentage point in magnitude. Again, we find no effects of sibling treatment as well.

Column 3 reports estimates of the effects of own and sibling treatment on the initial dose of the polio vaccine which is customarily given at birth. Column 4 corresponds to a regression of receipt of the BCG vaccine on own and sibling treatment. The BCG vaccine is also customarily given between 0 and 6 weeks of age. In both columns, we find insignificant effects of own treatment and sibling treatment. In column 5, estimates from a regression of a binary for whether the child has a health card on own and sibling treatment are reported. A health card is used to keep track of which vaccinations the child has received and is usually issued very early in the child's life. Here again the point estimates are small and insignificant.

In summary, we find no effects of improved cognitive endowments on neonatal investments. This could suggest either that very little information about the benefits of iodine supplementation for initial child quality was communicated to parents, or that this information was communicated but did not sufficiently change beliefs about initial quality. In particular, while it is possible that delivery-related investments simply do not manifest parental preferences for investment in a child, the fact that neonatal vaccinations do not respond when vaccinations later in infancy do is stronger evidence that perhaps parental expectations of quality and subsequent investments in children respond to observed variations in endowments.

5.1.4 Later-life Investments

Now that we have some idea of when early-life health investments begin to respond to improved initial endowments, we explore the persistence of this reinforcing investment behavior into later-life. In Table V, we report results of regressions on a sample of children aged 5 and above from the 2004 and 2007 waves of the Tanzania DHS.

Columns 1-3 present estimates from regressions of bed net use on own and sibling treatment. We find no effects on the use of any net, the use of a treated net, and the use of a net which had ever been treated. Point estimates are very small with tight standard error bands around them. The estimates are insignificant at conventional levels, with the only exception being a small but weakly significant positive effect of sibling treatment on the use of an ever-treated net of roughly 1.5 percentage points.

In columns 4-6 of Table V, estimates are presented from regressions of binaries measuring the child's possession of a blanket, shoes, and more than one pair of clothes, respectively. Again, we find that both own treatment and sibling treatment have no significant effects on the child's possession of these assets. The use of bed nets and the possession of basic necessities such as clothes, shoes, and a blanket are both reasonably health-enhancing and represent parental investments into the welfare of a child.

Therefore, it would appear that, at least in terms of bed net use and basic needs, the effects of improved initial endowments on parental investments do not persist into later childhood. This could be due to the child's strengthening bargaining power as he grows older. That is, when the child is young the resource allocation and investment decisions are made entirely by the parent based on the parents' expectations of quality and returns to investment and preferences over the quality distribution across the children in the household. However, as the child grows older, he can vie for his own resources and bargain within the household, diminishing the effect of initial endowments on investments in later childhood.

5.2 Intra-household Investment Allocation and Parental Preferences for Child Quality

In Tables II-V, we saw that estimates of the average effects of treatment probabilities of adjacent siblings in the birth order on parental investments in a child, holding his own treatment probability fixed, are generally insignificant. However, the results from the model presented in Section 2 suggest that cross-sibling effects on parental investments likely vary with respect to the degree of complementarity of child qualities in the household utility function. That is, for example, if we believe that male and female child qualities are complements in the household utility function, while the qualities of same sex siblings are substitutes, the model predicts that opposite sex sibling treatments will have positive effects on investments, while same sex sibling treatments will have less positive or even negative effects on investments–as the income and substitution effects will be of opposite sign.

In Tables VI-IX, we present results from regressions identical to those whose results are reported in Tables II-V, but with sibling treatment split into same and opposite sex treatments. Specifically, same sex sibling treatment is the sum of the treatment probabilities over same sex siblings in the set of the child's two immediately older and two immediately younger siblings. Opposite sex sibling treatment is the corresponding sum over opposite sex siblings.

The third row of Table VI shows significant and positive effects of opposite sex sibling treatment on the receipt of all three polio doses, the third DPT dose, and the measles vaccine. Though the estimates of the effects on the first two DPT doses are insignificant at conventional levels, the point estimates are of similar order of magnitude. As predicted by the model, estimates of the effects of same sex sibling treatment are smaller, insignificant and even negative in some instances. These results suggest that male and female child qualities are in fact complements in the household utility function, while the qualities of same sex siblings are more substitutable.

In Table VII, we do not find generally strong cross-sibling effects on nutritional investments. We see a positive, but only weakly significant effect of opposite sex sibling treatment on meeting the WHO guideline for sufficient duration of breastfeeding in column 2 of Table VII. The muted effects on breastfeeding are not surprising given the time-dependent nature this investment. In particular, the scope for an effect of treatment among younger siblings on breast-feeding is obviously severely limited. Additionally, it would seem that the routine allocation of standard nutritional resources, such as milk and juices, is perhaps by nature less exclusionary than more singular investments such as vaccinations or medical treatments.

While we expect to find no effect of own treatments on neonatal investments if observed variation in cognitive ability is the predominant mechanism through which cognitive endowments affect parental investments, we have no such expectation of cross-sibling effects. In fact, we would expect that opposite sex sibling treatments might affect investments irrespective of own endowment and therefore induce parental responses well before endowments are observable or even realized. We find in Table VIII evidence of positive effects on the receipt of the BCG vaccination and possession of a health card. Both are investments that most often occur well before endowments are observable.

Finally, in Table IX we explore persistence of cross-sibling effects. Estimates are only weakly significant at best, and exhibit no systematic pattern in sign. We interpret these estimates as evidence of a lack of measurable persistence of cross-sibling effects into later-life investments.

6 Conclusion

Using exogenous variation in initial cognitive endowments of children derived from an iodine supplementation program in Tanzania, we provide evidence of reinforcing investment behavior. Treated children are significantly more likely to receive vaccinations and nutritional investments in the form of adequate breastfeeding and a variety of essential fluids. However, we find no evidence that this endowment reinforcing health investment begins directly at birth, nor does it seem to persist into later childhood.

The absence of neonatal investment responses is likely due to the inability to perceive improved cognitive endowments before and immediately after birth. In infancy, children quickly show signs of cognitive ability, from which parents assess beliefs about initial endowments and adjust investment decisions accordingly. The lack of persistence in reinforcing investments is possibly due to the growing child's mitigating bargaining behavior. As the child grows older, he is able to bargain for his own resources, and investments no longer reflect solely the expectations and preferences of the parent.

Though we initially find no cross-sibling effects on average within the household, when we separate sibling treatment into opposite and same sex sibling treatments we find positive and significant estimates of the effect of the treatment of opposite sex siblings on at-birth and early-life vaccinations. However, we find smaller, insignificant and even negative estimates in some instances of the effects of same sex sibling treatments on these health investments. These results suggest, in the context of the predictions of the model presented in Section 2, that the qualities of two opposite sex siblings are complementary in the household utility function, while the qualities of two same sex siblings are more substitutable.

One can conceive of several mechanisms by which greater investment in one child might spill-over or crowd-out investment in other children in the household. There are first order income and substitution effects from the reduced "price" of quality of the treated child. In particular, if endowments and parental investments are complements in the production of child quality, as is often assumed, the parent can achieve a high level of quality in the higher endowment child more "cheaply" than in their lower endowment children. On the other hand, the exogenous shift in the quality of one child induces an income effect as well which should allow greater investment in all children than would otherwise be possible.

Additionally, to the extent that investments are not particularly time sensitive, scale effects could exist in investments. The prevailing example of such an effect is that of time spent with a child, perhaps reading or helping with school work. That is, if a parent is motivated to spend time with one child because it is of higher initial cognitive endowment, the parent might not want or need to exclude other children. This could potentially be true in our case of vaccinations. If the parent is willing to forego time spent in a production activity to take a high endowment child to the healthcare facility to be vaccinated, they might as well take the other, possibly lower endowment, child as well.

One final way in which investments in one child might affect investments in his siblings is through learning. That is, perhaps parents are moved to invest more in children with higher initial endowments due to a higher expected return on investment, but later update beliefs about returns to investments based on this experience. Then, investment in subsequent children (or existing siblings, depending on the time-sensitivity of the investment) will be affected by the endowment of this child through this learning mechanism.

However, when investments are time-sensitive (e.g. must be made before or after the child reaches a particular age) or are by nature exclusionary by age (e.g. a mother must stop breast-feeding one child in order to conceive the next, and will not likely breastfeed the first child again after 9 months), exploring the degree to which the qualities of children become more complementary in the household utility function as the children move further apart in the birth order is rendered difficult. In fact, both scale and learning effects are difficult to identify in these instances. For this reason, we have only investigated complementarity across gender in this study. Further research is needed to understand child quality complementarity across age or birth order for a more relevant set of investment behaviors.

In our context, if either learning or scale were important mechanisms by which sibling treatments affect vaccinations, nutritional investments, and possession of later-life assets in our context, we might expect to see significant effects of *same sex* sibling treatment. That is, to the degree that returns to investments are gender-specific, parents will likely be most able to update expectations about returns using observed returns to siblings of the same sex. Similarly, to the degree that investments themselves are gender-specific (e.g. clothes, shoes, and even, to some degree, bed nets if same sex siblings are more likely to sleep in the same bed than are opposite sex siblings), we would expect to see the largest spill-overs onto siblings of the same sex.

The fact that we find no significant effects of same sex sibling treatment on any of the investment outcomes employed in this paper, perhaps, suggests that these mechanisms are unimportant in this context. However, this is by no means a central conclusion of this paper and the absence of these results must be interpreted with caution. In particular, though a child may not, in general, be too old to receive a vaccination, he might be too young, or perhaps not yet born. In this sense, scale effects in the context of vaccinations cannot occur and income and substitution effects will likely grow weaker with greater spacing between births, particularly given the relatively low cost of the investment. The effects of learning should not decay quite as quickly, but of course only apply to younger siblings, as mentioned above. For these reasons, further research is needed to better understand cross-sibling effects by relative age and/or place in the birth order.

This study focuses its analysis on the relationship between variations in cognitive endowments and health investments. As mentioned, it is likely that spillovers to other children in the household also exist in other types of investment such as time spent with the child or education-related investments. Though previous studies have addressed these topics to some degree, our results emphasize the importance of separating the effects of endowments on own investments from effects on investments in siblings.

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A Additional Results

B Construction of variables

The following variables were constructed for use in the analysis:

- $treatprob$

FIGURE 1: BIRTH RATE TRENDS

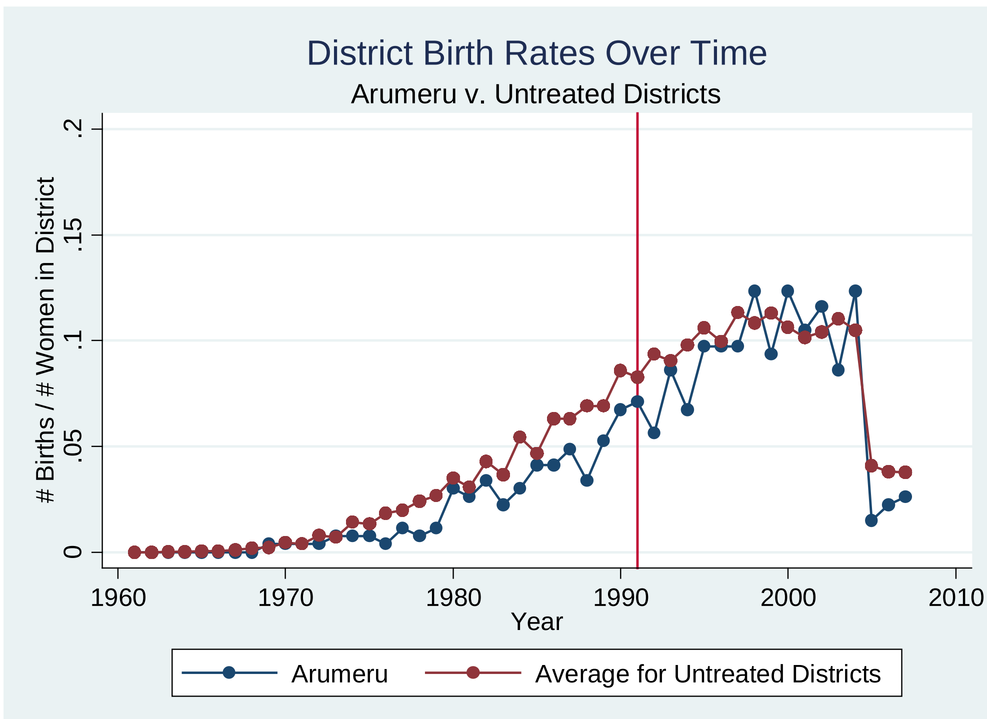

FIGURE 2: BIRTH RATE TRENDS

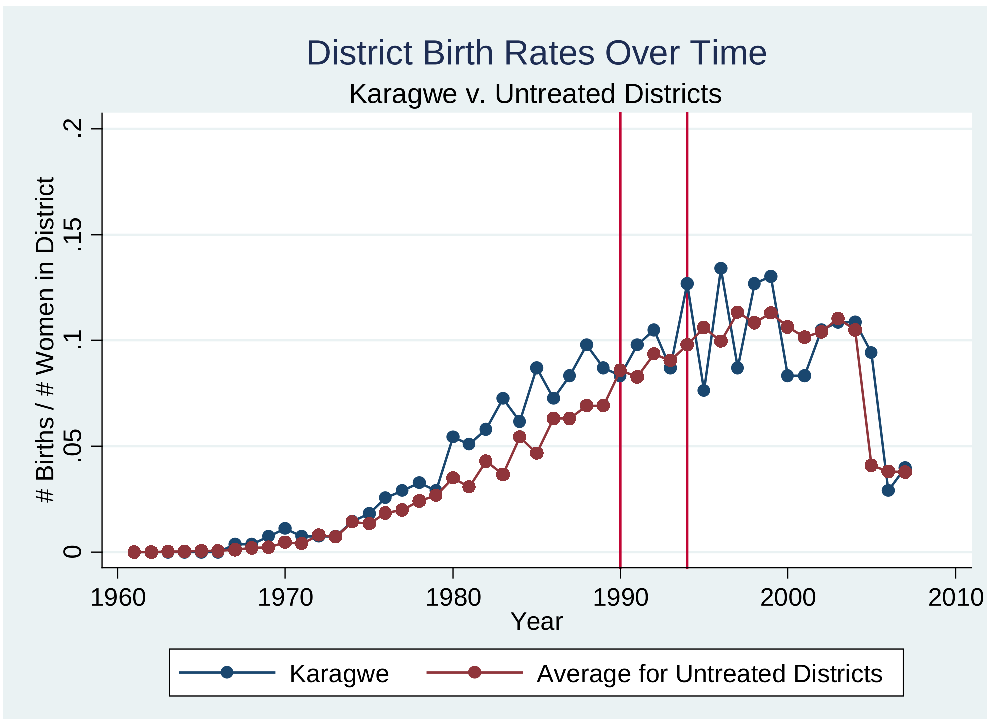

FIGURE 3: BIRTH RATE TRENDS

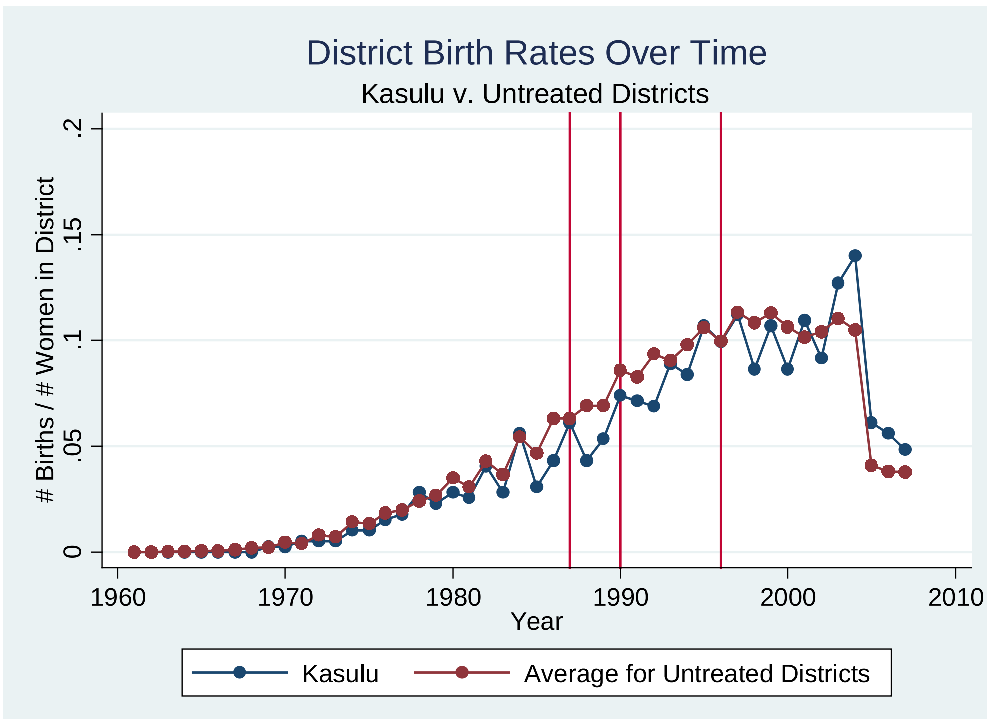

FIGURE 4: BIRTH RATE TRENDS

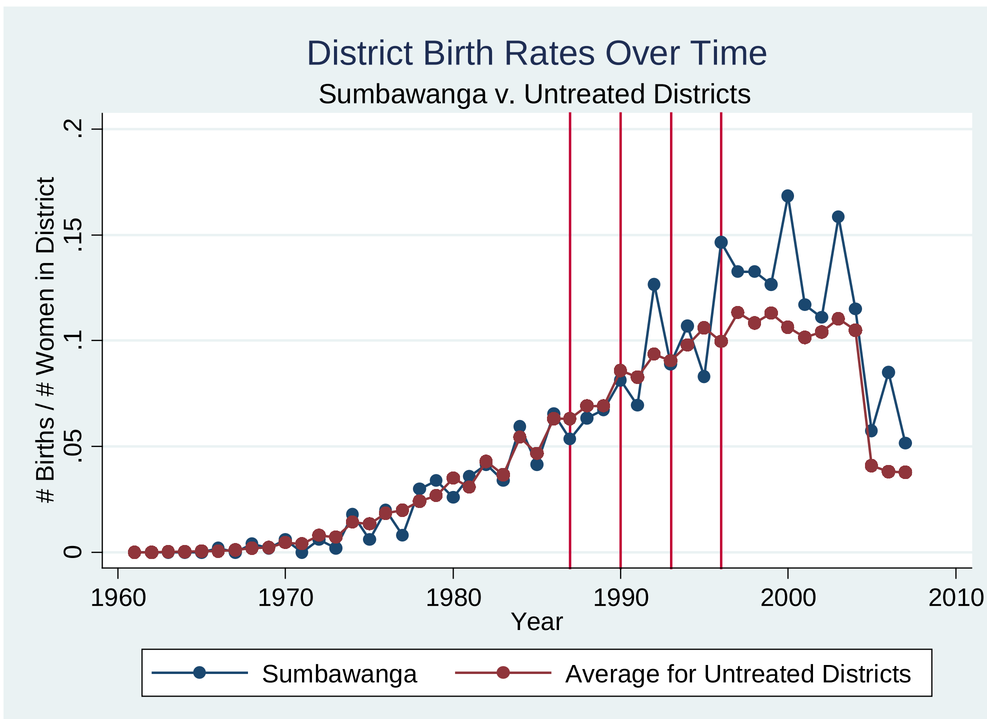

FIGURE 5: BIRTH RATE TRENDS

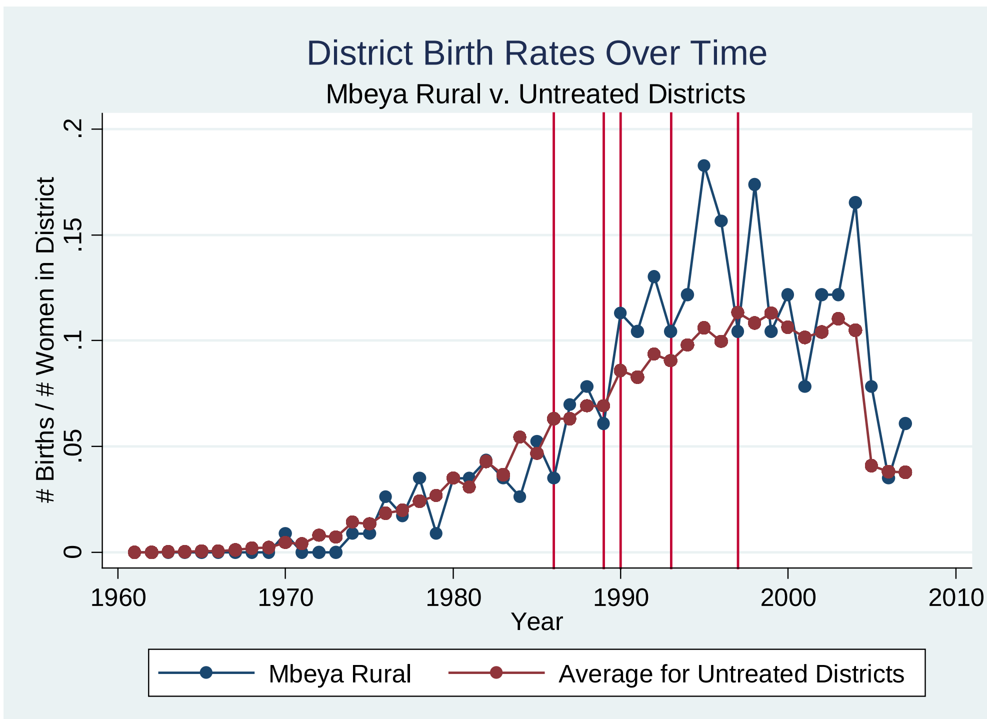

Table Ia: Summary Statistics (Demographic Characteristics)

Summary Statistics of Demographic Characteristics By Sample

Children Aged Under 5 in 1999 DHS

	Whole Sample		Treatment Prob >=.75		Treatment Prob <.75	
Child-Year Observations:	483		162		321	
	Mean	SD	Mean	SD	Mean	SD
Child-level						
Age	1.847	1.404	2.105	1.269	1.717	1.453
Female	0.474	0.500	0.500	0.502	0.461	0.499
Birth Order	3.164	2.109	2.938	1.765	3.277	2.257
Mother-level						
Mother's Age	28.464	6.935	28.747	6.522	28.321	7.140
Mother's Educational Attainment	5.048	3.176	4.988	3.240	5.078	3.148
Household-level						
No. of Children	3.602	2.293	3.414	2.011	3.698	2.420
Household Size	6.720	3.321	6.315	2.905	6.925	3.499
Urban	0.087	0.282	0.099	0.299	0.081	0.273

Children Aged 5 and Above in 2004 & 2007 DHS

	Whole Sample		Treatment Prob >=.75		Treatment Prob <.75	
Child-Year Observations:	4656		1467		3189	
	Mean	SD	Mean	SD	Mean	SD
Child-level						
Age	10.405	4.008	11.147	3.020	10.064	4.347
Female	0.493	0.500	0.515	0.500	0.483	0.500
Birth Order	2.387	1.489	2.296	1.406	2.429	1.523
Mother-level						
Mother's Age	36.389	6.669	37.273	6.222	35.982	6.827
Mother's Educational Attainment	4.722	3.269	4.753	3.193	4.708	3.304
Household-level						
No. of Children	4.747	2.223	4.868	2.403	4.691	2.133
Household Size	7.324	3.122	7.387	3.454	7.294	2.957
Urban	0.101	0.301	0.116	0.320	0.093	0.291

Children Aged 5 and Above in 2007 DHS

	Whole Sample		Treatment Prob >=.75		Treatment Prob <.75	
Child-Year Observations:	2028		514		1514	
	Mean	SD	Mean	SD	Mean	SD
Child-level						
Age	9.912	3.552	12.403	2.419	9.066	3.478
Female	0.504	0.500	0.521	0.500	0.499	0.500
Birth Order	2.389	1.402	2.093	1.188	2.489	1.454
Mother-level						
Mother's Age	35.814	6.580	37.827	5.753	35.130	6.703
Mother's Educational Attainment	4.955	3.210	4.844	3.129	4.993	3.237
Household-level						
No. of Children	4.633	1.964	4.905	1.942	4.540	1.963
Household Size	7.221	2.539	7.424	2.444	7.152	2.568
Urban	0.089	0.284	0.095	0.294	0.087	0.281

Notes:

Table Ib: Summary Statistics (Outcomes)

Summary Statistics of Outcomes By Sample

	Children Aged Under 5 in 1999 DHS	
Child-Year Observations:	483	
	Mean	SD
Program Exposure		
Treatment Probability	0.430	0.425
Treatment Probability >0	0.679	0.467
Vaccinations		
Polio 1 Dose	0.934	0.249
Polio 2 Dose	0.899	0.302
Polio 3 Dose	0.820	0.385
DPT 1 Dose	0.917	0.276
DPT 2 Dose	0.888	0.315
DPT 3 Dose	0.828	0.378
Measles	0.753	0.432
Nutritional Investments		
Mos. Breastfeeding	16.176	8.305
Mos. Breastfeeding >6	0.871	0.336
Given Fluids Index (Water, Milk, Juice, etc.)	2.224	2.754
Neonatal Investments		
Formal Sector Delivery	0.445	0.497
Assisted Delivery	0.938	0.242
Has Healthcard	0.441	0.497
Polio 0 Dose	0.940	0.238
BCG	0.959	0.199

Notes:

Table Ic: Summary Statistics (Outcomes)

Summary Statistics of Outcomes By Sample

	Children Aged 5 and Above in 2004 & 2007 DHS		Children Aged 5 and Above in 2007 DHS	
Child-Year Observations:	5564		2028	
	Mean	SD	Mean	SD
Program Exposure				
Treatment Probability	0.328	0.416	0.325	0.410
Treatment Probability >0	0.494	0.500	0.509	0.500
Later-Life Investments				
Any Net	0.150	0.357		
Treated Net	0.077	0.266		
Ever-Treated Net	0.109	0.312		
Blanket			0.360	0.480
Shoes			0.523	0.500
Clothes			0.768	0.422

Notes:

Table II: Healthcare Investments (Vaccinations)

Effects of IOC Treatment on Vaccinations

	Polio			DPT			Measles
	1st Dose	2nd Dose	3rd Dose	1st Dose	2nd Dose	3rd Dose	
Treatment Probability	**0.0911****	**0.126*****	**0.122****	**0.127*****	**0.139*****	**0.125****	**0.0899**
	(0.0402)	**(0.0430)**	**(0.0516)**	**(0.0423)**	**(0.0439)**	**(0.0496)**	**(0.0553)**
Sibling Treatment	**0.0293**	**0.0206**	**0.0410**	**0.0505**	**0.0286**	**0.0326**	**-0.0188**
	(0.0311)	**(0.0342)**	**(0.0393)**	**(0.0319)**	**(0.0340)**	**(0.0394)**	**(0.0376)**
Female	0.0313	0.0547*	0.0286	0.0412	0.0487*	0.0510	0.0674**
	(0.0242)	(0.0280)	(0.0387)	(0.0252)	(0.0292)	(0.0352)	(0.0316)
Age=2	0.122**	0.246***	0.368***	0.161***	0.247***	0.388***	0.723***
	(0.0505)	(0.0572)	(0.0669)	(0.0554)	(0.0590)	(0.0674)	(0.0611)
Age=3	0.174***	0.248***	0.445***	0.211***	0.249***	0.448***	0.874***
	(0.0594)	(0.0656)	(0.0764)	(0.0612)	(0.0668)	(0.0879)	(0.0655)
Age=4	0.157**	0.243***	0.430***	0.210***	0.277***	0.446***	0.937***
	(0.0700)	(0.0787)	(0.0918)	(0.0711)	(0.0800)	(0.0976)	(0.0900)
Age=5	0.171**	0.237***	0.409***	0.200***	0.255***	0.486***	0.966***
	(0.0732)	(0.0822)	(0.0988)	(0.0766)	(0.0845)	(0.115)	(0.0971)
Fixed Effects	District			District			District
Observations	483	483	483	483	483	483	481
Mean of Dependent Variable	0.934	0.899	0.820	0.917	0.888	0.828	0.753

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Stamdard errors are clustered at the district-age level. Sibling Treatment is the sum of treatment probabilities of the two older and two younger siblings. All specifications include controls for age (as fixed effects) and gender of the child as well as age and education of the mother. Controls also include a dummy for whether the household is located in an urban area, the household size and fixed effects for number of older and younger siblings of the child as well as the sum, max and min of the ages of all children in the household. Finally, we include the number of females in the household and fixed effects for the child's place in a gender-specific birth order. The sample is restricted to households with at least 1 child under the age of 5 from the 1999 DHS, unless otherwise noted.

Table III: Healthcare Investments (Nutrition)

Effects of IOC Treatment on Nutritional Investments

	Breastfeeding		Given Fluids
	Months	**1**[> 6 Months]	Index
Treatment Probability	**0.857**	**0.106****	**1.195****
	(0.895)	**(0.0421)**	**(0.485)**
Sibling Treatment	**0.468**	**0.0336**	**0.195**
	(0.725)	**(0.0264)**	**(0.336)**
Female	-0.450	0.0170	-0.108
	(0.720)	(0.0245)	(0.176)
Age=2	11.09***	0.554***	0.511
	(0.862)	(0.0594)	(0.316)
Age=3	14.60***	0.538***	0.435
	(1.388)	(0.0669)	(0.413)
Age=4	16.00***	0.514***	1.757**
	(2.011)	(0.0792)	(0.873)
Age=5	16.16***	0.533***	0.714
	(2.287)	(0.0830)	(0.513)
Fixed Effects	District		District
Observations	478	479	474
Mean of Dependent Variable	16.18	0.871	2.224

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). See Table II for additional comments.

Table IV: Healthcare Investments (Neonatal)

Effects of IOC Treatment on Neonatal Investments

	Delivery		Vaccinations		
	Formal Sector	Assisted	Polio 0 Dose	BCG	Health Card
Treatment Probability	**-0.000448**	**0.00519**	**-0.0134**	**0.0201**	**0.0164**
	(0.0601)	**(0.0422)**	**(0.0739)**	**(0.0307)**	**(0.0226)**
Sibling Treatment	**-0.0279**	**-0.0388**	**-0.0488**	**0.00226**	**0.0139**
	(0.0456)	**(0.0331)**	**(0.0581)**	**(0.0275)**	**(0.0253)**
Female	-0.0420	-0.0551**	-0.0186	0.0175	-0.00629
	(0.0450)	(0.0272)	(0.0543)	(0.0240)	(0.0186)
Age=2	0.0924	0.0573	0.0330	0.0965**	0.0866**
	(0.0677)	(0.0404)	(0.0774)	(0.0435)	(0.0374)
Age=3	0.107	-0.0201	-0.0209	0.0965*	0.0994**
	(0.0958)	(0.0577)	(0.107)	(0.0538)	(0.0421)
Age=4	0.158	-0.0521	0.0390	0.0927	0.105**
	(0.112)	(0.0791)	(0.139)	(0.0645)	(0.0480)
Age=5	0.288**	-0.00977	0.0327	0.0731	0.0990*
	(0.130)	(0.0842)	(0.158)	(0.0684)	(0.0556)
Fixed Effects	District		District		
Observations	483	483	483	483	483
Mean of Dependent Variable	0.445	0.938	0.441	0.940	0.959

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). See Table II for additional comments.

Table V: Later-Life Investments

Effects of IOC Treatment on Use of Bed Nets and Possessions

	Bed Nets			Possessions		
	Any Net	Treated Net	Ever-Treated Net	Blanket	Shoes	Clothes
	aged 5 and over from 2004 & 2007 DHS			*aged 5 and over from 2007 DHS*		
Treatment Probability	**0.00421**	**-0.00544**	**0.00651**	**0.0101**	**-0.00162**	**0.0112**
	(0.0145)	**(0.0109)**	**(0.0128)**	**(0.0342)**	**(0.0349)**	**(0.0322)**
Sibling Treatment	**0.0134**	**-0.00328**	**0.0148***	**-0.0117**	**0.00233**	**0.00922**
	(0.00960)	**(0.00682)**	**(0.00818)**	**(0.0229)**	**(0.0248)**	**(0.0212)**
Female	0.00676	0.0200**	0.0202**	0.0105	-0.00143	0.0177
	(0.0113)	(0.00839)	(0.00981)	(0.0221)	(0.0242)	(0.0206)
Fixed Effects	District, Age			District, Age		
Observations	4656	4656	4656	2028	2027	2029
Mean of Dependent Variable	0.142	0.0747	0.108	0.360	0.523	0.768

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). See Table II for additional comments. Samples restricted as noted.

Table VI: Healthcare Investments (Vaccinations)

Effects of IOC Treatment on Vaccinations

	Polio			DPT			Measles
	1st Dose	2nd Dose	3rd Dose	1st Dose	2nd Dose	3rd Dose	
Treatment Probability	**0.0997*** **	**0.133*** **	**0.123** **	**0.131*** **	**0.144*** **	**0.129*** **	**0.106* **
	(0.0385)	**(0.0418)**	**(0.0511)**	**(0.0412)**	**(0.0431)**	**(0.0487)**	**(0.0556)**
Same Sex Sibling Treatment	**0.0195**	**0.0343**	**0.0237**	**0.0503**	**0.0366**	**-0.00389**	**-0.0153**
	(0.0386)	**(0.0448)**	**(0.0563)**	**(0.0449)**	**(0.0452)**	**(0.0575)**	**(0.0549)**
Opposite Sex Sibling Treatment	**0.0949*** **	**0.0627* **	**0.0876* **	**0.0596**	**0.0632**	**0.0906** **	**0.0910** **
	(0.0315)	**(0.0378)**	**(0.0469)**	**(0.0383)**	**(0.0393)**	**(0.0451)**	**(0.0433)**
Female	0.0311	0.0518*	0.0243	0.0417	0.0455	0.0475	0.0655**
	(0.0244)	(0.0277)	(0.0388)	(0.0255)	(0.0289)	(0.0352)	(0.0315)
Fixed Effects	District, Age			District, Age			District, Age
Observations	475	475	475	475	475	475	473
Mean of Dependent Variable	0.934	0.899	0.820	0.917	0.888	0.828	0.753

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). See Table II for additional comments.

Table VII: Healthcare Investments (Nutrition)

Effects of IOC Treatment on Nutritional Investments

	Breastfeeding		Given Fluids
	Months	**1**[> 6 Months**]**	Index
Treatment Probability	**0.741**	**0.0999****	**1.194****
	(0.906)	**(0.0422)**	**(0.503)**
Same Sex Sibling Treatment	**-0.573**	**0.00981**	**0.469**
	(1.209)	**(0.0338)**	**(0.318)**
Opposite Sex Sibling Treatment	**0.574**	**0.0542***	**-0.277**
	(0.788)	**(0.0287)**	**(0.313)**
Female	-0.468	0.0155	-0.125
	(0.729)	(0.0245)	(0.178)
Fixed Effects	District, Age		District, Age
Observations	470	471	466
Mean of Dependent Variable	16.18	0.871	2.224

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). See Table II for additional comments.

Table VIII: Healthcare Investments (Neonatal)

Effects of IOC Treatment on Neonatal Investments

	Delivery		Vaccinations		
	Formal Sector	Assisted	Polio 0 Dose	BCG	Health Card
Treatment Probability	**0.00525**	**0.0135**	**-0.0210**	**0.0306**	**0.0259**
	(0.0619)	**(0.0412)**	**(0.0741)**	**(0.0289)**	**(0.0217)**
Same Sex Sibling Treatment	**0.00465**	**-0.0123**	**0.00554**	**0.0222**	**0.0245**
	(0.0635)	**(0.0507)**	**(0.0687)**	**(0.0388)**	**(0.0394)**
Opposite Sex Sibling Treatment	**0.0253**	**-0.0406**	**-0.0297**	**0.0641****	**0.0636****
	(0.0533)	**(0.0429)**	**(0.0624)**	**(0.0323)**	**(0.0301)**
Female	-0.0380	-0.0569**	-0.0209	0.0133	-0.00658
	(0.0454)	(0.0271)	(0.0539)	(0.0237)	(0.0186)
Fixed Effects	District, Age		District, Age		
Observations	475	475	475	475	475
Mean of Dependent Variable	0.445	0.938	0.441	0.940	0.959

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). See Table II for additional comments.

Table IX: Later-Life Investments

Effects of IOC Treatment on Use of Bed Nets and Possessions

	Bed Nets			Possessions		
	Any Net	Treated Net	Ever-Treated Net	Blanket	Shoes	Clothes
	aged 5 and over from 2004 & 2007 DHS			*aged 5 and over from 2007 DHS*		
Treatment Probability	**0.00152**	**-0.00560**	**0.00418**	**0.00635**	**-0.0000173**	**0.0165**
	(0.0145)	**(0.0109)**	**(0.0128)**	**(0.0338)**	**(0.0345)**	**(0.0319)**
Same Sex Sibling Treatment	**-0.00924**	**-0.00928**	**0.00323**	**-0.00694**	**0.0549***	**0.0365**
	(0.0116)	**(0.00818)**	**(0.0101)**	**(0.0296)**	**(0.0306)**	**(0.0266)**
Opposite Sex Sibling Treatment	**0.00434**	**-0.00396**	**0.00454**	**-0.0517***	**-0.00790**	**0.0447***
	(0.0111)	**(0.00804)**	**(0.00960)**	**(0.0284)**	**(0.0307)**	**(0.0269)**
Female	0.00681	0.0200**	0.0202**	0.00841	-0.00138	0.0198
	(0.0113)	(0.00841)	(0.00983)	(0.0221)	(0.0242)	(0.0207)
Fixed Effects	District, Age			District, Age		
Observations	4652	4652	4652	2028	2027	2029
Mean of Dependent Variable	0.142	0.0747	0.108	0.360	0.523	0.768

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). See Table II for additional comments.

Table A.1: Program Years

Region	District	Year 1	Coverage 1	Year 2	Coverage 2	Year 3	Coverage 3	Year 4	Coverage 4	Year 5	Coverage 5
Dodoma	Mpwapwa	1990	0.65	1992	0.58						
Arusha	Monduli	1992	0.71								
Arusha	Arumeru	1991	0.89								
Kilimanjaro	Rombo	1990	0.68								
Morogoro	Ulanga	1988	0.73	1991	0.61	1992	0.34				
Ruvuma	Songea Rural	1987	0.91	1991	0.74	1995	0.85				
Ruvuma	Mbinga	1995	0.92								
Iringa	Mufindi	1986	0.41	1991	0.63	1995	0.54				
Iringa	Makete	1986	0.2	1991	0.62	1993	0.62	1996	0.49		
Iringa	Njombe	1989	0.76	1992	0.68	1995	0.64				
Iringa	Ludewa	1989	0.59	1992	0.62	1995	0.47				
Mbeya	Chunya	1990	0.49								
Mbeya	Mbeya Rural	1986	0.44	1989	0.84	1990	0.9	1993	0.53	1997	0.53
Mbeya	Kyela	1989	0.91	1993	0.57						
Mbeya	Rungwe	1986	0.35	1990	0.73	1993	0.49				
Mbeya	Ileje	1989	0.94	1992	0.71						
Mbeya	Mbozi	1989	0.67	1991	0.63						
Rukwa	Mpanda	1987	0.79	1991	0.6	1993	0.72				
Rukwa	Sumbawanga	1987	0.76	1990	0.89	1993	0.72	1996	0.51		
Rukwa	Nkansi	1987	0.89	1991	0.49						
Kigoma	Kibondo	1989	0.73	1992	0.75	1996					
Kigoma	Kasulu	1987	0.5	1990	0.66	1996	0.49				
Kigoma	Kigoma Rural	1991	0.91								
Kagera	Karagwe	1990	0.96	1994	0.85						
Kagera	Bukoba Rural	1994	0.78								
Kagera	Biharamulo	1990	0.96	1994	0.38						
Kagera	Ngara	1989	0.29	1994	0.51						

Notes: Taken from Field et al. (2009)

Table A.2: Treatment Probabilities

	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec
Program Year	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.028	0.083	0.167	0.250	0.333
1st Year After Program	0.417	0.500	0.583	0.667	0.750	0.833	0.917	1.000	1.000	1.000	1.000	1.000
2nd Year After Program	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	0.998	0.991	0.977
3rd Year After Program	0.955	0.927	0.891	0.849	0.802	0.749	0.690	0.627	0.559	0.488	0.419	0.353
4th Year After Program	0.292	0.237	0.189	0.148	0.112	0.082	0.057	0.037	0.022	0.011	0.004	0.001

Notes: Taken from Field et al. (2009)

Table A.3: Robustness Checks (Endogenous Fertility: Timing)

Effects of Treatment on Probability of Birth

	Program and Subsequent Years					Program and Subsequen Years (Cumulative)			
	Program Year	1st Year After Program	2nd Year After Program	3rd Year After Program	4th Year After Program	Program Yr -> 1 Yr After	Program Yr -> 2 Yrs After	Program Yr -> 3 Yrs After	Program Yr -> 4 Yrs After
Birth in This Yr	**-0.000950**	**-0.00142**	**-0.00112**	**0.00257**	**-0.00316**	**-0.00205**	**-0.00350**	**-0.00315**	**-0.00387**
	(0.00300)	**(0.00285)**	**(0.00296)**	**(0.00299)**	**(0.00370)**	**(0.00257)**	**(0.00263)**	**(0.00284)**	**(0.00321)**
Fixed Effects			Mother					Mother	
Observations	101456	101456	101456	101456	101456	101456	101456	101456	101456
Mean of Dependent Variable	0.0930	0.0930	0.0930	0.0930	0.0930	0.0930	0.0930	0.0930	0.0930

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Table A.4: Robustness Checks (Endogenous Fertility: Quantity)

Effects of Treatment on No. of Births After Treatment

	No. of Children Born After Treated Child		
Treatment Probability	**0.0212**	**0.0110**	**0.00757**
	(0.0133)	**(0.00895)**	**(0.00684)**
Fixed Effects	Disctrict	Household	Mother
Observations	6603	6603	6603
Mean of Dependent Variable	1.759	1.759	1.759

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). See Table II for additional comments.

Table A.5: Robustness to Alternate Treatment Definitions (Half)

Effects of IOC Treatment on Vaccinations

	Polio			DPT			Measles
	1st Dose	2nd Dose	3rd Dose	1st Dose	2nd Dose	3rd Dose	
Treatment Probability	**0.0479**	**0.0551**	**0.0800***	**0.0742****	**0.0766****	**0.0717***	**0.0420**
	(0.0340)	**(0.0359)**	**(0.0425)**	**(0.0346)**	**(0.0353)**	**(0.0409)**	**(0.0391)**
Sibling Treatment	**-0.0119**	**-0.0354**	**-0.0146**	**-0.00774**	**-0.0313**	**-0.0230**	**-0.0319**
	(0.0205)	**(0.0220)**	**(0.0247)**	**(0.0199)**	**(0.0216)**	**(0.0247)**	**(0.0219)**
Observations	483	483	483	483	483	483	481

Effects of IOC Treatment on Nutritional Investments and Neonatal Investments

	Breastfeeding		Given Fluids	Delivery		Vaccinations		
	Months	**1**[> 6 Months**]**	Index	Formal Sector	Assisted	Polio 0 Dose	BCG	Health Card
Treatment Probability	**0.145**	**0.0371**	**0.898***	**-0.0126**	**0.0196**	**0.00305**	**-0.00579**	**0.00206**
	(0.800)	**(0.0334)**	**(0.494)**	**(0.0487)**	**(0.0317)**	**(0.0625)**	**(0.0259)**	**(0.0170)**
Sibling Treatment	**0.393**	**-0.0125**	**0.0246**	**-0.00963**	**0.00328**	**-0.0367**	**-0.0345***	**-0.0146**
	(0.421)	**(0.0160)**	**(0.230)**	**(0.0282)**	**(0.0169)**	**(0.0341)**	**(0.0185)**	**(0.0150)**
Observations	478	479	474	483	483	483	483	483

Effects of IOC Treatment on Use of Bed Nets and Possessions

	Bed Nets			Possessions		
	Any Net	Treated Net	Ever-Treated Net	Blanket	Shoes	Clothes
	aged 5 and over from 2004 & 2007 DHS			*aged 5 and over from 2007 DHS*		
Treatment Probability	**0.000732**	**-0.00796**	**-0.00178**	**0.0224**	**0.000137**	**0.0121**
	(0.0119)	**(0.00909)**	**(0.0105)**	**(0.0281)**	**(0.0285)**	**(0.0267)**
Sibling Treatment	**0.000756**	**-0.00987****	**-0.00237**	**-0.00149**	**-0.0132**	**0.0164**
	(0.00650)	**(0.00468)**	**(0.00567)**	**(0.0165)**	**(0.0184)**	**(0.0157)**
Observations	4656	4656	4656	2028	2027	2029

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). See Table II for additional comments. Treatment is a binary for full protection against IDD, with probability of at least .5

Table A.6: Robustness to Alternate Treatment Definitions (3/4)

Effects of IOC Treatment on Vaccinations

	Polio			DPT			Measles
	1st Dose	2nd Dose	3rd Dose	1st Dose	2nd Dose	3rd Dose	
Treatment Probability	**0.0421**	**0.0727****	**0.0823****	**0.0624****	**0.0794****	**0.0903****	**0.0551**
	(0.0286)	**(0.0315)**	**(0.0394)**	**(0.0296)**	**(0.0319)**	**(0.0358)**	**(0.0422)**
Sibling Treatment	**0.00624**	**0.00193**	**0.0150**	**0.0205**	**0.00869**	**0.0207**	**-0.0298**
	(0.0200)	**(0.0238)**	**(0.0301)**	**(0.0225)**	**(0.0253)**	**(0.0289)**	**(0.0257)**
Observations	483	483	483	483	483	483	481

Effects of IOC Treatment on Nutritional Investments and Neonatal Investments

	Breastfeeding		Given Fluids	Delivery		Vaccinations		
	Months	**1**[> 6 Months**]**	Index	Formal Sector	Assisted	Polio 0 Dose	BCG	Health Card
Treatment Probability	**0.767**	**0.0931*****	**1.074****	**-0.0142**	**-0.0141**	**0.0262**	**0.00556**	**0.00777**
	(0.734)	**(0.0294)**	**(0.428)**	**(0.0467)**	**(0.0310)**	**(0.0557)**	**(0.0230)**	**(0.0182)**
Sibling Treatment	**0.0943**	**0.0189**	**0.145**	**-0.0494**	**-0.0456****	**-0.0228**	**0.00274**	**0.0229**
	(0.496)	**(0.0195)**	**(0.190)**	**(0.0315)**	**(0.0222)**	**(0.0380)**	**(0.0192)**	**(0.0158)**
Observations	478	479	474	483	483	483	483	483

Effects of IOC Treatment on Use of Bed Nets and Possessions

	Bed Nets			Possessions		
	Any Net	Treated Net	Ever-Treated Net	Blanket	Shoes	Clothes
	aged 5 and over from 2004 & 2007 DHS			*aged 5 and over from 2007 DHS*		
Treatment Probability	**0.00821**	**-0.00290**	**0.00838**	**0.0106**	**-0.00796**	**-0.00563**
	(0.0120)	**(0.00910)**	**(0.0106)**	**(0.0284)**	**(0.0289)**	**(0.0253)**
Sibling Treatment	**0.00834**	**-0.00656**	**0.00784**	**-0.00493**	**0.00606**	**0.00116**
	(0.00704)	**(0.00499)**	**(0.00622)**	**(0.0172)**	**(0.0185)**	**(0.0165)**
Observations	4656	4656	4656	2028	2027	2029

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). See Table II for additional comments. Treatment is a binary for full protection against IDD, with probability of at least .75

Table A.7: Robustness to Alternate Treatment Definitions (Full)

Effects of IOC Treatment on Vaccinations

	Polio			DPT			Measles
	1st Dose	2nd Dose	3rd Dose	1st Dose	2nd Dose	3rd Dose	
Treatment Probability	**0.0358**	**0.0319**	**0.0404**	**0.0349**	**0.0317**	**0.0427**	**0.0386**
	(0.0256)	**(0.0287)**	**(0.0432)**	**(0.0270)**	**(0.0315)**	**(0.0372)**	**(0.0473)**
Sibling Treatment	**0.0191**	**-0.00213**	**-0.00253**	**0.0214**	**-0.00415**	**0.00905**	**-0.00933**
	(0.0192)	**(0.0241)**	**(0.0320)**	**(0.0224)**	**(0.0252)**	**(0.0270)**	**(0.0275)**
Observations	483	483	483	483	483	483	481

Effects of IOC Treatment on Nutritional Investments and Neonatal Investments

	Breastfeeding		Given Fluids	Delivery		Vaccinations		
	Months	**1**[> 6 Months**]**	Index	Formal Sector	Assisted	Polio 0 Dose	BCG	Health Card
Treatment Probability	**0.0318**	**0.0227**	**0.917**	**0.00891**	**-0.0558**	**0.0662**	**-0.0144**	**-0.0170**
	(0.822)	**(0.0211)**	**(0.574)**	**(0.0557)**	**(0.0344)**	**(0.0610)**	**(0.0277)**	**(0.0199)**
Sibling Treatment	**-0.383**	**-0.000666**	**0.360***	**-0.0480**	**-0.0240**	**-0.0258**	**0.00826**	**0.0274***
	(0.619)	**(0.0203)**	**(0.184)**	**(0.0321)**	**(0.0249)**	**(0.0414)**	**(0.0197)**	**(0.0154)**
Observations	478	479	474	483	483	483	483	483

Effects of IOC Treatment on Use of Bed Nets and Possessions

	Bed Nets			Possessions		
	Any Net	Treated Net	Ever-Treated Net	Blanket	Shoes	Clothes
	aged 5 and over from 2004 & 2007 DHS			*aged 5 and over from 2007 DHS*		
Treatment Probability	**0.0202**	**0.00845**	**0.0185**	**0.0135**	**0.0186**	**0.0336**
	(0.0139)	**(0.0101)**	**(0.0123)**	**(0.0312)**	**(0.0314)**	**(0.0271)**
Sibling Treatment	**0.0178****	**0.00718**	**0.0206*****	**0.0180**	**0.0180**	**0.00603**
	(0.00760)	**(0.00579)**	**(0.00706)**	**(0.0183)**	**(0.0203)**	**(0.0187)**
Observations	4656	4656	4656	2028	2027	2029

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). See Table II for additional comments. Treatment is a binary for full protection against IDD, with probability of 1