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Evidence from a maternity support programme in India

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Income Expectations and Child health:
Evidence from a maternity support program in India

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December 2016

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1. Introduction

Can expectations of additional income in child-bearing years reduce child malnutrition, even in poor regions where credit markets function imperfectly, limiting the ability of households to borrow against future income? We address this question in the context of a pilot maternity benefits program in India intended to provide an income transfer to all pregnant women in a critical 9 month period covering the last trimester of pregnancy till 6 months after the child's birth. The program, the Indira Gandhi Matritya Sahayog Yojana (IGMSY), is unique in that it provides maternity benefits to women in rural areas, the majority of who are engaged only in household work in their own homes.

Because the majority of targeted women are not employed in the formal economy and hence lack incomes, the program differs from traditional maternity benefit programs in developed countries. It is, instead, best viewed as an income support program, targeted at pregnant women, intended to provide income to women at the start of their childbearing years. The period encompassing pregnancy and the first 6 months of a child's life is widely viewed as a "critical" period for health, with long-lasting effects on adult health, education and incomes (Cunha and Heckman, 2007). In poor rural economies, where the potential to borrow against future income is likely limited, improving maternal and infant child health would require any targeted income support to be delivered during this critical period.

Field visits revealed widespread knowledge of the program, including eligibility rules and the amount of the income support, perhaps because the program is run through local institutions that serve the habitation, a sub-group of the village economy, staffed by females who are also resident of the habitation. In all program villages there were at least some women who had received support payments, and this fact was known by other residents of the same habitation. There was also no concern of the program being discontinued. In September 2013, the Government of India enacted the National Food Security Act that converted this maternity support program, as well as other programs under the umbrella of India's Integrated Child Development Services into a legal entitlement, requiring the Government to universalize the program and ensure its continuity.

However, delayed receipt of payments was the norm: over 50% of eligible women were yet to receive any support and, payments, when received, rarely came on time. Of eligible women, only 5% reported receiving the income support before the onset of their second pregnancy. 39% of payments intended for the first child arrived between the onset of the second pregnancy and 12 months after the birth of the second child, and hence closer to the critical period for the *second* child. Despite this, we find that eligibility for the program benefited both the first and second eligible child, with coefficients for the first born child being larger in magnitude.

Building on a literature that explores the economic determinants of birth intervals (Heckman and Walker 1990, Rosenzweig 1986, Wolpin 1984), we explain this result as a consequence of the effect of income expectations on the birth interval between the first and second child, and the positive effect of a longer birth interval on child health. Using data on health behaviors such as recorded immunization and ante-natal checkups, and outcomes from other programs run through the same local health institutions, we are able to rule out any direct effect of program conditionalities on health or on the effectiveness of other programs. Similarly, we also provide evidence against the hypothesis that the differential effect on children of different birth orders is a consequence of a (current) income gradient that increases with age.

We identify the effect of the program using a triple difference methodology and data from a household survey we implemented in the North Indian state of Bihar in early 2016. The program was introduced in 2011 in only 52 of India's districts, selected so as to be representative of the distribution of maternal and child health outcomes across the country. Our study is based on the two pilot (treatment) districts selected from the North Indian state of Bihar. While details of our methodological approach are provided later in the paper, we selected two control districts by matching treatment districts to others in the state on the basis of the covariates used to select pilot districts. In both treatment and control districts, we surveyed cohorts of women with young children born just before and after the program eligibility date (mid 2011). This enables a difference-in-difference methodology, standard to most studies of maternity and other programs that exploit a cut-off date, either for eligibility or for a change in benefits, to identify an intent-to-treat estimate of the effect of eligibility. We provide stronger identification by exploiting the fact that program benefits were only available for the first two live births. This enables a triple difference estimator that not only exploits the difference in health outcomes across cohorts and districts, but also utilizes the fact that, within eligible cohorts, benefits were only available to certain children identified by birth order.

Given that the IGMSY is best viewed as an income support program for pregnant women, our research relates to the empirical literature that examines the effects of income, particularly income in early childbearing years, on child outcomes. Much of this literature, however, examines the effect of current income on current outcomes. For example, Case, Lubotsky and Paxson (2002) document the positive effect of current income on the health of children using US data.[1] Supportive evidence of a positive effect of income during early childhood years on child health also comes from Fernald, Gertler and Neufeld (2008) who study the impact of the income component of Mexico's *Oportunidades* program on outcomes, including height-for-age and weight-for-age scores, for children born after the program was initiated and hence after program funds were received. However, unlike the transitory improvement in incomes provided by the IGMSY, *Oportunidades* grants eligible households income support over a relatively long period of time, covering the child's schooling years.

There is, of course, a relatively large empirical literature that examines the effect of maternity benefits in developed economies, primarily the duration of paid and unpaid maternity leave, on a wide range of outcomes. This literature includes research on the effect of child support programs on fertility in Israel (Cohen, Dehejia and Romanov, 2007) and on child support provided to families in early nineteenth century England under the system of Poor Laws (Boyer, 1989). Cohen, Dehejia and Romanov report a significant effect of income transfers on fertility, as does Boyer. As in the literature on the effect of current income on child outcomes, these studies generally identify the effects of benefits received during pregnancy and the child's early years on subsequent outcomes such as completed education.

As in our research, most of these studies exploit the timing of a policy change to implement a difference-in-difference methodology (Carneiro, Loken and Salvanues, 2015; Dustmann and Schonberg, 2011; Wurtz-Rasmussen 2010; Baker and Milligan, 2008 and 2010; Rossin, 2011). In doing so, a primary concern addressed by researchers is the possibility that women may manipulate the timing of child birth so as to ensure their eligibility for the program (Gans and Leigh 2009). Far less attention has been paid, in this research, to the possibility that maternity support programs may also cause women to adjust successive birth intervals, or the time between births. This may be

[1] They also report that the correlation between income and child health increases with the child's age. Including correlations with income, a primary determinant of income, significantly reduces the size of the income gradient. And, they find that the effect of education on health does not vary with a child's age.

because the effect of birth spacing on outcomes such as child health is less of a concern in developed economies, given low fertility and relatively large birth intervals.

It is, however, of critical importance in developing economies such as India. In these countries, policy documents frequently cite short birth intervals as amongst the most important explanations for high infant mortality and poor child health, and for the growth faltering that occurs amongst very young children, particularly in the first six months of life (Government of India, Ministry of Women and Child Development 2011; International Institute for Population Studies 2008).

Dynamic models of fertility that allow for the endogeneity of the timing and spacing of births have a long history.[2] This literature includes empirical analyses by Heckman and Willis (1975), Heckman, Hotz and Walker (1985), Heckman and Walker (1990), Hotz and Miller (1988), Wolpin (1984), Rosenzweig (1986) and Rosenzweig and Wolpin (1988), and establishes that birth intervals reflect female wages as well as current income. As emphasized by Heckman and Willis (1975) and others, decisions regarding the timing of births will also reflect credit constraints and life cycle income profiles, and can help households mitigate the effect of credit constraints: credit constrained households can shift births to periods of higher income, increasing birth intervals in the process. Additionally, a further implication of this work is that birth intervals can link the health outcomes of children to expectations of income in subsequent periods, even when households are credit constrained. There is little empirical evidence on whether decisions regarding birth intervals do allow households to shift consumption expenditures over time and hence mitigate the effect of credit constraints.

A number of studies have examined the effect of birth intervals on health and other outcomes. Such studies must confront the difficult endogeneity issues that arise because birth intervals, being an input into health production, are jointly selected by the parents along with other health inputs. Rosenzweig and Wolpin (1988) and Bhalotra and van Soest (2007) identify the effect of the preceding birth interval, the time between the birth of child j and the older child, j-1, on outcomes for child j, by utilizing the sequencing of child births and the fact that the preceding birth intervals is orthogonal to child j's unobserved health characteristics. Rosenzweig and Wolpin (1988), for example, use characteristics of the older child, as well as parental characteristics, as instruments to determine the effect of this birth interval on child weight. Identifying the effect of the subsequent birth interval, between child j and child j+1, on outcomes for child j is more difficult, since this choice will reflect observed and unobserved attributes of child j. However, some evidence of the importance of this interval comes from research by Buckles and Munnich (2012), who use the incidence of a miscarriage between births as an instrument for the effect of birth intervals on educational achievement. They find that increased spacing increases the education of the older sibling, without significant impacts on younger sibling.

This paper proceeds as follows. We first describe the IGMSY program. Section 3 then discusses the survey data, describes sample statistics and review program implementation based on the survey data. Section 4 describes the theoretical framework underlying the empirical analysis of this paper, while section 5 details the empirical methodology. Results are in section 6 and the last section concludes.

[2]Early reviews of the literature include Hotz, Klerman and Willis (1997)

2. The Program

The Indira Gandhi Matritya Sahayog Yojana (IGMSY) was introduced by the Government of India in 2010-11, to provide income support to mothers in the period extending from the last trimester of pregnancy through the first 6 months of the child's life, for the first two live births. Though maternity benefits, under the Maternity Benefit Act of 1961, were previously available to those employed in the formal economy,[3] the program represents an attempt by the Government to provide additional income to all mothers, including those who are primarily employed in home production or in the informal economy. The scheme was initially piloted in 52 districts spread throughout the country, and provided a cash grant of Rs. 4000, in three installments, if certain health enhancing practices were undertaken.

This grant was in addition to the Rs. 1400 cash incentive provided to women for institutional deliveries under a central Government program introduced in 2005, the Janani Sureksha Yojana (JSY). The health conditionalities imposed by the IGMSY, however, were the same as those required by the JSY: registration of pregnancy; at least one ANC (with a tetanus shot and provision of iron pills); registration of the child at birth; immunization; attendance at growth monitoring and counselling services; and exclusive breastfeeding for 6 months. As a consequence, since a majority of women are availing of JSY, the IGMSY did not, in effect, impose any new conditions.

Verification of conditions was to be done at habitation level institutions, known as Anganwadi Centers (AWCs), that provide services under India's flagship program for maternal and child health, the Integrated Child and Development Services (ICDS). These services include providing take home rations to pregnant women and children under the age of 3, as well as counselling to women on matters related to pregnancy and child care. On registration of pregnancies at these centers,[4] women were issued a Maternal and Child Protection (MCP) card in which ANCs, immunizations and other health outcomes during pregnancy such as weight-gain, consumption of ITT tablets and blood pressure, were to be recorded. Adherence to health conditionalities was then intended to be confirmed through the MCP card, or by self-certification in the case of attendance at counselling sessions and duration of breast feeding.

In order to ensure that the program does not increase fertility, the income transfer is available only for a woman's first two live births. The program had a starting date of end December 2010, applying to all women who were pregnant with either their first or second child as revealed through a baseline survey conducted in this month. In September 2013, the Government of India enacted the National Food Security Act that converted the provision of all ICDS services, including the IGMSY, into a legal entitlement. With the passing of this Act, the Government is now required to increase the scale of maternity benefits provided under IGMSY to Rs. 6000 and to extend it to all districts of the country. This has not yet been done; the Government is planning to do so under a phased process.[5]

The first set of 52 target districts were chosen by ranking all districts in the country on the basis of 6 indictors using data available from the third round of the national District Level Household

[3] It applies to those who work in mines, factories, plantations, and shops and establishments that employ 10 or more persons.

[4] Pregnant women could also register at the health sub-centers run by the Ministry of Health.

[5] The increase of the payment to Rs. 6000 was to be available in the initial 53 districts during FY 2014-15. Phasing of the program to an additional 200 "high burden" districts was to be undertaken in 2015-16, while extension to all remaining districts was to be undertaken in 2016-17, "depending on the availability of financial resource."

and Facility Survey (DLHS, round 3, 1007-08).[6] Using this ranking, all districts were divided into three groups of high, medium and low performing districts. 11 districts were randomly selected from high and low performing districts, 26 from medium performing districts and 4 from union territories, subject to the requirement that 2 districts being selected from each major state. Funds from the Ministry of Women and Child Development would first be transferred to state-level ICDS cells, and then to district cells. District cells would oversee the program through a tiered system comprising ICDS project offices (that generally operated at the level of the block), and the village-level anganwadi centers.

Anganwadi workers (AWWs) were responsible for registering pregnant and lactating women and certifying their compliance with the scheme. They were required to maintain an IGMSY register, listing eligible beneficiaries, and to pass on this list to the block-level ICDS project office, who would then pass it on to the district level office. On receipt of the list of beneficiaries from the project office, the district office would then transfer the cash amount directly to the beneficiary's bank account. For the purpose, each woman was required to open a savings account in a bank or a post office in her name. Members of the local village government (the Gram Panchayat) were to aid women in opening up bank accounts. Since a number of women did not have bank accounts in their names, initial payments under the scheme were delayed on this account, as well as due to delayed receipt of lists of eligible women from the AWC at the block level, and from the block level to the district level. Details regarding the implementation of the program, based on our survey data, are reported in the section below.

3. Survey Design, Sample Statistics and Program Implementation

3.1 Survey Design

Our study is limited to the north Indian state of Bihar. In this state, the two districts included in the IGMSY in its pilot phase were Vaishali, in the relatively more developed western part of the state, and Saharsa, in the backward eastern region. In terms of the index used by the Government to select pilot districts, Vaishali had a score of 192, while Saharsa's score was 139.3. We constructed the same index for other districts in the state, and selected the two that were closest to the pilot districts, after eliminating districts that were also piloting other maternal and child health programs. This meant that we did not select the closest matched district to Saharsa, the district of Madhepura, because it was amongst districts selected for a pilot project intended to improve Anganwadi centers. Instead, we selected the district of Supaul that also neighbors Saharsa. Supaul was part of Saharsa district until March 1991, when it became an independent district. Supaul's score on the selection index was 135.2, 4 points lower than Saharsa's. There is almost no difference in the scores between the other pilot district of Vaishali (192) and its selected control (192.6).

In each district, we randomly selected 4 blocks, 4 Gram Panchayats (local governments) per block, and an average of 2 villages per Gram Panchayat.[7] Within each village, we selected an average

[6] These were: percent literate female population; mothers registered in the first trimester when they were pregnant with last birth; mothers who had at least 3 ante-natal care visits du ring the last pregnancy; institutional births; children full immunized and children breastfed within one hour of birth.

[7] We selected only one village when the Gram Panchayat included just one village, and more than 2 when it comprised a large number of relatively small villages.

of 2.5 Anganwadi centers (AWCs), resulting in a total of approximately 320 AWCs for the study. In each AWCs, we randomly selected 15 households, 5 each from 3 target groups: households with a pregnant woman or with at least one child under 6 months, women with either a first or second child over the age of 6 months but born after 2011, and households in which the second child had been born before 2011. The division of households between these three was not exact; in some AWCs it was difficult to identify and interview women from any particular target group.

We fielded two survey instruments, a household survey and an AWC survey. The AWC survey collected basic data on the number of households per AWC, as well as information on the state of the center, and whether all required registers, including the IGMSY register, were being maintained. Through our household survey, we collected basic demographic information, details about pregnancy and health for each child under the age of 6, maternal background, and height and weight information. A distinguishing feature of rural Bihar is the relatively large number of joint households. In such households, we collected demographic information on all members of the household. This implies that we have data on several mothers within a household. For the sake of uniformity, we also collected data on the extended family, even when our "index" or targeted household had separated from the parent household to form a nuclear household. In such cases, we identified households by "kitchens" and collected data on all kitchens of the extended household. Our demographic data therefore relate to the extended household, as does our information on the height and weight of all children under the age of 6. Our final survey included 1,073 pregnant women and 917 children under the age of 6 months; 3509 children between the ages of 6 months and 3 years; and 3,800 children older than 3 years but less than 6 years of age.

3.2 Sample Statistics

Table 1 provides summary statistics for the full sample, as well as separately for the two treatment (pilot) and two control districts. The table reveals the low proportion of households owning land, as well as the small amount of land owned (for those who do own land) in all regions of the country. For the sample as a whole, only 31% report owning land. Mirroring this, the primary income earning activity for the fathers in our sample is employment in the casual unskilled labor market, in which 40% of sample fathers were employed. The overwhelming majority of the mothers in our sample are primarily engaged in domestic work within their homes (98%). Comparing outcomes for treatment versus control districts, there is little variation across these two groups for some outcomes, such as the proportion owning land, the proportion of mother's with formal schooling and mother's age, but more so in others. This includes fathers' education years, and the proportion of fathers reporting their primary source of employment to be the unskilled casual wage market.

The table also reports weight and height for age, relative to WHO (2006) standards (WAZ and HAZ scores). These data show high levels of malnourishment (weight for age) and stunting (height for age). The data reveal that the average child in our sample (of children below the age of 6) suffers from malnutrition and stunting (WAZ and HAZ scores between -2 and -3, respectively), with the percentage reporting WAZ and HAZ scores of less than -2 being 50% and 67%, respectively. These high rates are supported by data from India's National Family Health Survey (NFHS). The latest round of the NFHS provide data for 2015-16 reveal that the proportion of children who suffer from stunting (HAZ<-2) is 49%, while 45% suffer from moderate malnourishment.[8] The greater incidence of stunting in our sample suggests greater measurement error in the collection of height data, also repeated in the higher degree of variance in this measure. The regressions of this paper are thus

[8] Data from the previous round, NFHS 3 (2005-06) reveal rates of 56.5% and 57% for moderate stunting and malnourishment, respectively.

based on a sample of children with improbably high HAZ scores (less than -8) removed. This is approximately 5% of the total sample of 6000 children. Only 3 children of this sample had a WAZ score of less than -8. For regressions on WAZ, we report results from a trimmed sample with removes the lowest 1% of scores (WAZ <=-6) removed.

3.3 Program Implementation

Implementation of the program was weak, particularly in its first year. Official quarterly statements relating to the program reveal that no funds were utilized in 2010-11, while only Rs 60.5 million (approximately US $900,000 in 2016) was utilized during 2011-12, almost all of which was utilized in the first quarter of 2012 (Rs. 57.5 million). During 2011-12, no coordinators specific to the program, at either the state or district level, had been appointed, even though 6 such positions were sanctioned. Additionally, no steering and monitoring committee meetings for IGMSY were held at either the project (block), district or state level. Implementation of the program picked up in 2012-13, when the state reported utilization of Rs. 305 million under the program, even though state-level oversight through meetings remained weak.[9]

Reflecting this, we found considerable laxity in official records relating to the program and in program implementation (table 2). The program requires all AWCs to maintain an IGMSY register at the Anganwadi center, since it is on the basis of this register that the Anganwadi worker makes claims for funding for pregnant women. Table 2 reveals that though the register existed in a majority of AWCs, it was regularly maintained and updated only in half of the treatment districts. Despite this, AWCs in this district reported that funds were still received for eligible women, through the submission of claims on paper by the AWW to the block office, even without recourse to a register.[10]

Weak implementation is also reflected in delayed receipt and non-receipt by eligible women of income support payments. The majority of eligible women reported that they were yet to receive entitled support, with only 31% reporting receipt of payments. This partly reflects a low percentage of payments in the first year of the program (27%), when implementation guidelines were not well known (these were made available to state governments only by the end of April 2011). In the first full year of the program, 2012, 42% of women reported receiving income under the program.

The lack of payments does not necessarily imply that no payments would be forthcoming, but instead reflects the significant delays in receiving payments. Of women who reported receiving funds, only 17% reported doing so within the stipulated time frame (6 months after birth). Including the households who had not yet received support, payments were received on time for only 5% of first-eligible children. In fact, for these first-born eligible children, only 5% reported receiving funds before the initiation of the second-eligible pregnancy. As previously stated, the primary explanations for delayed payments were delays in the opening of bank accounts, or in the submission of claims at different reporting levels (AWCs, blocks, district).

Of those who did received funds, most reported receiving an amount close to what they were entitled to under the 2011 guidelines (an average of Rs. 3816, as opposed to an entitlement of Rs. 4000). However, this was generally received in one installment, rather than 3, primarily because of delays in providing the transfer. The number of women who reported receiving the larger funds suggested by the 2013 guidelines was negligible, reflecting the delayed implementation of these

[9] These data are from Quarterly and Annual Statements of Expenditure under IGMSY released by the State ICDS office.

[10] In villages in which an official IGMSY register was not being regularly maintained, the Anganwadi workers would show us slips of paper in which the necessary claims had been made, with the papers bearing the stamps of the Anganwadi worker and high level officials.

guidelines that also called for the universalization of the program.[11]

Even with delayed receipt of payments, the program carries the potential to improve maternal and child health, since the payments were to be conditional on the woman undertaking health-enhancing behaviors during pregnancy and in the first 6 months of a child's life. Table 3 provides information that allows us to assess whether the conditionalities of the program were imposed. As previously described, these included Ante-natal care (ANC) visits and immunization of children, conditions that were intended to be recorded and confirmed through the MCP card.

Almost all women (95%) reported registering their pregnancy either with the AWC or the health sub-center, and similarly almost all women (97%) reported having an MCP card. However, only 38% of women, all of who were surveyed in their homes, could show us their MCP cards. The failure to produce MCP cards meant a similar discrepancy between the self-reported and verifiable incidence of health care behaviors. For example, while almost all women reported having had at least one ANC and full immunization of their children, these were entered in the MCP card only in a minority of cases.

Since the MCP card was intended to be the basis for confirming the conditionalities of the program, it was clear that women were receiving benefits without these conditions being verified through the MCP card. In fact, the percentage of women who were able to show us their MCP cards was slightly lower in treatment districts (36%) relative to control (39%).

The receipt of funds despite a failure to adhere to stipulated conditions is not unique to this program. It also exists in other programs such as the JSY. As previously noted, the JSY imposes the same conditions as the IGMSY, with conditionalities to be verified through the use of a JSY card (of which the MCP card is a component) intended to be issued to the mother at the time of registration of the pregnancy. An evaluation of the program conducted by UNFPA in 2009 revealed that 93% of beneficiaries in Bihar were unable to produce their JSY cards (UNFPA 2009). The report also reveals that a high percentage of beneficiaries in the state, who received payments under the scheme, did not satisfy program conditionalities. For example, only 51% of beneficiaries reported receiving 3 ANCs and only 32% reported taking the necessary IFA tablets.[12] One reason for lack of adherence to conditionalities, both in this program and the IGMSY, is that conditionalities are enforced at local levels (AWC and block), and not at the higher levels of government (state and district) that are responsible for disbursing funds. Payments from the central government to the State, for example, are conditional on utilization certificates, no on whether conditionalities are being met.

Under the JSY, payments to beneficiaries were to be given to them at the hospital or health center, immediately following the birth of the child. To ensure this, every local worker responsible for the program was given a monthly cash amount intended to ensure timely payments. Yet, mirroring the delayed payments in the IGMSY, slightly more than 1/3[rd] of eligible beneficiaries had not received the financial payment at the time of the evaluation report, and 65% reported that the payments were not provided at the time of delivery, as intended, but only after a delay. This delay in payments, even in programs that have a very strict delivery schedule (i.e., at time of birth), explains why there is a general acceptance of such delays, and an expectation that funds will be received.

3.4 Household knowledge

The last few panels of table 3 provide information on sources of knowledge regarding weight gain during pregnancy and nutrition, and on decision makers within the household. These questions

[11] The program is not yet being universally implemented in all districts.
[12] Since JSY cards were not available, presumably adherence to conditionalities were self-reported.

were asked only of women who are currently pregnant. These data reveal that approximately 60% of women report having received some information on weight gain and appropriate nutrition during pregnancy. This means, however, that a relatively high percentage (40%) have never been counselled on these topics. Of those reporting discussions on these topics, an overwhelming majority (93 to 94%) state that they received information from formal sources – Anganwadi workers, Auxiliary Nurse Midwives from government health sub-centers, or the ASHA village level functionaries associated with the JSY. The data on decision makers suggests that mothers-in-law are more likely to be making decisions regarding food expenditures and what is eaten than mothers. While the difference in the importance of mothers-in-law relative to mothers is relatively small as regards food expenditures (48% reporting mothers-in-law and 44% reporting mothers as the primary decision maker), it is much larger for decisions on *what* is cooked (51% mothers-in-law, 38% mothers). We also investigated the primary decision maker for women's hours of work outside the house. While 35% of women reported that they made such decisions, an equal percentage reported that such decisions were made by their husbands. Mothers-in-law were far less important in this aspect (18%).

The relatively high level of contact that mothers have with the AWC reflects the fact that these are local health institutions, serving homogenous groups of households of similar socio-economic backgrounds that reside in sub-divisions of the village known as habitations. Anganwadis are staffed by women who also reside in the habitation, and hence are well-known by all residents. The fact that the IGMSY operates through AWCs plays a large role in explaining the familiarity of women with the program. At this local level, knowledge of government programs is widely shared. Our visits to AWCs and habitations generally resulted in a large meeting with many women from the habitation, in which they showed us their IGMSY savings pass books (with details regarding funds received). Such public discussions are common in villages, and explain the high degree of familiarity of women with government programs that operate through local institutions. Of our survey villages, all villages reported at least one eligible person having received funds. Similarly, we found that, of the eligible women in any given (extended) household, 50% of them had received some support under the program.

We found a high level of awareness of the program including eligibility criterion that restricted benefits to the first two children. Though few stated an exact cut-off date for eligibility, most women were aware that the program applied only to younger mothers, who were pregnant in 2011. The familiarity of a cut-off date and a restriction to just two children may reflect the fact that the JSY similarly utilizes a cut-off date and also restricts eligibility to a mother's first two live births.[13]

4 Theoretical Framework

The theoretical underpinnings of this paper is the literature on fertility dynamics that models parents' decisions regarding the timing of each birth based on the current stock of children, a start-of-period information set, and a period-by-period budget constraint (Heckman and Willis 1976; Wolpin 1984). It draws particularly heavily on the framework developed by Rosenzweig (1986) that considers the effect of fertility dynamics on child health. Given this, I simply sketch the framework, focusing on highlighting predictions for the effects of income and income expectations on child health and fertility dynamics.

[13] The restriction to the first two live births was relaxed in Bihar and other low-performing states, provided that the mother underwent sterilization immediately following the birth of the child.

Let children be indexed by the super-script i, and time periods by sub-scripts. For notational simplicity, we suppress indexing of households. The health of a child is affected by the intervals between his or her birth and preceding as well as subsequent births, as well as by period-specific purchased health inputs, Z. Let B^i be the birth interval between child i and child i-1. Suppressing the effect of all other inputs, including other birth intervals, the health of child i in period t is given by:

(1) $H_{it} = h(B^i, Z_t^i)$

Household utility is a function of the health of all children, the number of children (N), and an aggregate consumption good, X, so that period t utility is $u(H_t, N_t, X_t)$. Time periods are defined as a unit of time that enables the birth of a single child, so that a decision not to have a child within a period enables longer preceding birth intervals. Let n_t be an indicator variable that takes the value 1 if a child is born in period t.[14] The number of children evolves according to the following equation:

(2) $N_{t+1} = N_t + n_t$

Each child incurs a cost c in each period. Additionally, delaying child birth to the next period involves a cost ζ. Let Y_t be household income in period t. Decisions are made subject to a period specific budget constraint that equates expenditure in that period to income and hence reflects credit constraints and the inability to transfer income from one period to the other. With this, households maximize expected discounted utility,

(3) $Max\ E_t \sum_{t=1}^{T} \beta^k u(X_t, H_t, N_t)$

subject to the period-by-period budget constraint:

(4) $p_t^z Z_t + p_t^x X_t + p_t^n N_t + cn_t + \varsigma(1 - n_t) = Y_t$

To focus on timing issues, we abstract from fertility choices and assume that all parents only have two children, child i and an older child, i-1. Their planning horizon comprises two periods of equal length, each divided into two sub-periods s, s=1, 2. One child is born in each period, with child i born in period i. Parents, however, choose whether to have the child in sub-period 1 or 2. Births occur at the start of the sub-period.

Credit constraints apply within each sub-period. The decision making period is consequently a sub-period and variables are correspondingly indexed by sub-period. Thus n_1^i takes the value 1 if child i is born at the start of sub-period 1 (of period i), 0 otherwise. The choice of which sub-period to have the second child in determines the interval between births. Let $A_{i,1}^{i-1}$ be child (i-1)'s age at the start of sub period 1 of period i.[15] Standardizing the length of each sub-period to 1,

[14] Infant mortality is ignored.

[15] Relating age to the sub-period in which child i is born, $A_{i,1}^{i-1} = 3 - s^{i-1}$, where s^{i-1} = 1 if child i-1 is born in sub-period 1 of period (i-1) and 2 otherwise.

(5) $$B^i(n_1^i) = \begin{cases} A_{i,1}^{i-1} & if\ n_1^i = 1 \\ A_{i,1}^{i-1} + 1 & if\ n_1^i = 0 \end{cases}$$

Consider household decisions at the start of period i. Expected utility is:

(6) $$u\big(H^{i-1}\big(B^i\big(n_1^i\big), Z_1^{i-1}\big), N_1^i, X_1\big) + E_1\, \beta\, u\big(H^{i-1}\big(B^i\big(n_1^i\big), Z_2^{i-1}\big), N_2^i, X_2\big)$$

Let $V_1(n_1^i)$ be the maximized value of expected sub period 1 utility subject to the sub period budget constraint and the start-of-period information set, I_1. The household will choose to have child i in the first sub-period, $n_1^i = 1$, if:

(7) $$\mathcal{J}_i = E_1(V_1 \mid n_1^i = 1;\ I_1) - E_1(V_1 \mid n_1^i = 0;\ I_1)$$

Heckman and Walker (1990), amongst others, also working within a similar theoretical framework, consider the effect of income and income expectations on birth intervals. In the model outlined above, increased spacing improves utility through its effect on health, but also implies a direct utility cost. Similarly, putting off a birth reduces the direct expenditure on children in the current period, but also incurs a "waiting" cost. Assuming that the net effect of delaying a child birth on current period costs and utility is positive, then increases in current income will increase spacing.

This framework demonstrates how expectations of income in future periods can affect current health through birth intervals, even when households are credit constrained and lack the ability to move income from one period to another. Parents who expect greater income in sub-period 2 will delay having child i, since this allows them to defer child costs to periods when income is higher. This increases the interval between child (i-1) and child i, B^i, improving the health of child (i-1) (and child i).

To use this framework to understand the effects of IGMSY on child health, let T_i be an indicator variable that takes the value 1 if child i is eligible for IGMSY. We assume that eligibility affects Z_i, consumption of health-related goods in the period of birth of child i, and affects expectations of income in subsequent periods, thus assuming delayed receipt of income support. Thus, the health of child i in period i+1 is:

(8) $$H_{j,i+1}^i = h(B_j^{i+1}\big(Y_{ji}, E(Y_{j,i+1}|T_j)\big),\ \ Z_{j,i}^i\big(T_j, Y_{ji}\big),\ \ Z_{j,i+1}^i(Y_{j,i+1}))$$

From this, eligibility for the program will affect the current health of the eligible child, in periods subsequent to the child's birth, either through its effect on income expected in future periods, or through its direct effect on current health through health conditionalities imposed around the time of the child's birth.

5 Empirical Methodology

Our identification of the effects of eligibility for IGMSY benefits on child health utilizes a difference estimator based on eligibility rules that specified eligible regions, cohorts and children within cohorts. These rules were previously described in section 3. While rules regarding program districts and eligible children were clear, that section also detailed the ambiguity regarding the cut-off date for program eligibility. It was clear that no specific date was utilized. Instead, there was a broad recognition that women who were pregnant during 2011 were eligible for the program. In line with this broad guideline, and given delayed implementation of the program, we similarly assume that all children born in 2012 or later years were eligible for program benefits. In actual practice, as discussed subsequently, we utilize a more general cut-off criteria based on the mother's marriage year, so as to reduce concerns regarding the endogeneity of cut-off dates based on the child's birth year.

5.1 Estimating equations and identification conditions

A standard difference-in-difference methodology, commonly used in studies that exploit either the introduction of a policy or a change in policy to identify its effects, utilizes a regression of the following form:

$$(9)\quad (H_{it} = \alpha_o + \alpha_1 tdistr_{it} + \alpha_2 byr\_12_{it} + \alpha_3(tdistr_{it} * byr\_12_{it}) + \beta_4 Z_{it} + u_{it}$$

In this regression *tdistr* is an indicator variable that takes the value 1 for the two pilot districts included in the program, 0 otherwise. Similarly, *byr_12* is an indicator variable for eligible cohorts, defined as those who were born on or after January 2012. The variable Z_{it} includes both child-specific characteristics such as the child's age, as well as household variables. As is well known, the identifying assumption in the difference-in-difference estimator is that the growth rate in health outcomes across consecutive cohorts is identical in treatment districts relative to control. If this assumption holds, then the coefficient α_3 in the above equation identifies the "intent-to-treat" estimate, a reduced form estimate of the benefits of program eligibility that could operate through multiple pathways.

The regressions we report utilize stronger identification, since they also exploit the fact that the program is limited to the mother's first two children. This enables the use of a triple difference estimator that contrasts the growth rate amongst the first two children and other children across cohorts, in treatment versus control districts. The differencing takes care of the concern that a contrast between the first two children and later children implies comparing children who differ in terms of the household resources available at the time of their birth (a family size effect), since we compare the difference across children of different birth orders both across cohorts and also across districts. The identifying assumption requires the much weaker condition that, in the absence of the program, any difference in growth rates for health outcomes across the first two children, relative to those of higher birth order, across cohorts of treatment districts would be identical to the difference in growth rates across children in these two birth order groups across cohorts of control districts.

The equation I estimate is:

$$(10)\quad H_{ijt} = \beta_0 + \beta_1(tdistr_j * byr\_\_12_{it} * order\_\_12_i) + \beta_2(tdistr_j * byr\_\_11_{it}) +$$

$$\beta_3(byr\_\_11_{it} * order\_\_12_i) + \beta_4\,(tdistr_j * order\_\_12_i) \ + \ \beta_5 tdistr_j +$$

$$\beta_6 byr\_11_{it} + \beta_7\, order\_12_i + \beta_8 X_{ijt} + \beta_9 Z_{ij} + u_{it}$$

The ITT estimate of the effect of the program on health indicators is given by the coefficient β_1 in the above regression.

The primary health outcome variables we focus on are height-for-age and weight-for-age Z scores (HAZ, WAZ). These scores are available for all children under the age of 6 and thus for children born after the eligibility cut-off dates, as well as those born prior to this. It should be noted, however, that our analysis is based on one cross-section of data. That is, we only have data on current health outcomes, and do not have data on the height and weight of the older cohort of children when they were between the ages of 0-3. Thus, our identifying assumptions are based not on the height and weight of different cohorts of children observed when they were of the same age, but on the current height and weight of children of different cohorts, but relative to a reference population of the same age and gender. That is, we assume that, in the absence of the program, the difference in height and weight relative to age-specific standards of children distinguished by cohorts, but also by birth-order within cohorts, would be the same across treatment and control districts.

We report regression results using only minimal controls (the distinct elements of the triple interaction that defines eligibility as well as all double interaction terms), but also results using an augmented set of regressors: child's gender, indicator variable for children from scheduled castes and tribes (SC/ST), mother's formal schooling and the father's years of formal education, mother's height, weight and BMI, amount of land owned by the extended household, an indicator for whether the father is the oldest son, he population covered by the AWC, the proportion of this population that is from scheduled castes and tribes, distance of the AWC from the block office, and indicators for whether the child was born during the monsoon months or the (slack) monsoon agricultural season (Kharif season).

As in empirical analyses of other programs that utilize a cut-off date to identify the effects of program eligibility a standard concern is that mothers may choose the child's birth date to ensure eligibility for the child in question. That is less of a concern in the context of this study because, as stated above, eligibility was not based on a specific cut-off date. However, the theoretical framework of the previous section suggests that even if mothers do not manipulate birth days in order to ensure eligibility, the selection of the interval between births, and hence birth dates, may well reflect eligibility for the program: parents may choose the interval between births based on income expectations. Since the interval between births represents the dates of birth of the children in question, this suggests that dates of birth are endogenous, and chosen on the basis of household characteristics, including unobserved characteristics that also determine child health. For example, as discussed in the previous section, credit constrained households might choose longer birth intervals (through selecting later birth dates). If child health is lower amongst credit constrained households, regressions in which eligibility is defined on the basis of the child's birth year will yield downward biased estimates of program eligibility on child health.

We thus also provide estimates that define eligibility in terms of the mother's marriage year. Since women who were pregnant in 2011 were eligible for the program, we define eligible children as those whose mothers were married in 2010 or later. Defining eligibility by age of marriage assumes that age of marriage is uncorrelated with unobserved determinants of health. This assumption is likely valid, given strong cultural norms that dictate age at marriage. We test the validity of our definition of eligibility through auxiliary regressions that also include the triple interaction of *tdistr* and *order_12* with an indicator for households in which the mother was married in an earlier year (2008 in the reported regressions, though we also tested a cut-off year of 2009 with similar results). If the interaction with 2010 represents a “true” effect of eligibility, then coefficient values should not be impacted through the interaction of additional non-linear terms.

To support identification, we restrict the sample to similar cohorts that are narrowly defined in terms of the mother’s marriage year, including only children of mothers who were married in 2005 or later. Through a set of auxiliary regressions, we test the sensitivity of results to alternative regression samples, based on households characterized by an earlier marriage (2004 or later), and those with a later marriage year (2006 or later).

The restriction of the sample to consecutive narrowly defined cohorts, the similarity of control and treatment districts, and the sensitivity analysis that we undertake, taken together, suggest that the identifying assumptions of this study are likely to be met. It is difficult to verify this using data on growth rates across previous cohorts of children. Our survey data only provides height and weight outcomes for children under the age of 6, and hence no data is available on older cohorts. Other available data sets that provide information on child health also do not readily lend themselves to this analysis. For example, it is not possible to compare trends using the National Family Health Survey data from rounds 3 (2005-06) and 2 (1998-99), since district codes were not included in the round 3 data. Similarly, comparisons between rounds 2 and round 1 (1992-93) are not helpful, since the districts of Saharsa and Supaul were actually one district in that period. Additionally, sample sizes for a district are relatively small in the NFHS surveys.[16]

Some supporting evidence is possible from the Annual Health Survey CAB (clinical, anthropometric and bio-chemical) component that was conducted in high focus states, including Bihar, in 2014. The survey collected height and weight data for children in a bigger age range, making it possible to compare the heights and weights of children born between 2005 and 2014 across treatment and control districts.[17] However, the data do not provide information on the child’s birth order. Thus, even if growth rates across districts differ, the identifying assumption (similarly in growth rates across cohorts and regions of children of different birth orders) may still hold.

To examine trends across treatment and control districts using the AHS data for Bihar, we first regress these outcomes on the six variables used to select pilot districts to eliminate the effects of the slight difference in this score across treatment and control districts[18]. We then plot (smoothed) residuals of height and weight by the child’s age in months in figures 1 and 2. Figure 1, that graphs weight by age, suggests similar trends in a child’s weight by age at the time of the AHS survey across treatment and control districts until the age of 40 months, the cut-off age for eligibility for the program. For eligible children, these data suggest that weight is greater in treatment districts relative to the control sample. This evidence, based on an independently conducted national survey, suggests a treatment effect in terms of weight.

As in our survey, data on height are noisier. They suggest greater height-for-age in treatment districts relative to control, even for children between the ages of 40 and 72 months, who were ineligible for the program, with a crossing of the graphs for the two samples at around 72 months.

[16] Other surveys, such as the Human Development Survey (HDS) do not sample all districts. The India HDS 2004-05 does not include any observations for Vaishali. Additionally, small sample sizes in each district render district-level comparisons suspect. For example, the total sample size of children less than 5 in the districts of Saharsa and Supaul is only 106.

[17] Data for the years prior to 2005 display a high degree of variance, particularly in weight data, raising concerns regarding measurement error and data quality.

[18] Since these indicators are at the district level, conditioning on their values amounts to a vertical shift of the graph.

For eligible children, the difference in height for age across treatment and control districts is similar to those for children between the age of 40 and 72 months, perhaps marginally smaller.

These data suggest that even a difference-in-difference estimator may not produce unbiased ITT estimates. Thus, the restriction of the program to the first two children is a critical component of identification. We repeat that the graphs do not control for differences in birth order, since data on birth order are not available in the Annual Health Survey-CAB. Though it is difficult to find external data sources that support this assumption, we note that there were no other programs that were introduced in 2011 that applied *only* in these two districts and were also restricted just to the first two live births. As previously noted, the only other major maternal and child health program is the JSY, introduced in 2005 in all districts of the state. All other entitlements provided under the ICDS, such as take home rations, are available in all districts and to all children, regardless of cohort or birth order.

5.2 Identifying pathways

As previously noted, the regressions above recover reduced form intent-to-treat estimates of the effect of eligibility on health outcomes, confounding several ways in which eligibility could affect child health. The program intended benefits to accrue as a consequence of an increase in current income, available at the time of pregnancy and the child's early months, on the health of pregnant women (though both additional nutrition and reduced workloads) and children 6 months and younger. Given that current health reflects maternal health during pregnancy as well as health in earlier periods of a child's life, any effect through income in these critical periods should be reflected in improved health at the time of the survey. However, the fact that only 6% of eligible women received the intended transfer for the first-eligible child before the initiation of her second pregnancy suggests that the program did not, on average, increase household income in this critical period.

Despite this, the program could nevertheless enhance child health through other pathways. First, the health conditionalities such as ANCs and immunization required by the program could directly improve child health, even if income support was *never* received, since they were a pre-condition for receiving income support. Second, implementation of the program in pilot districts might have caused the government to re-distribute resources towards AWCs in pilot districts, in order to ensure smooth functioning of the program. This suggests that for programs such as the JSY, that also targets only the first two children, program effectiveness might have increased for children eligible for IGMSY benefits. This may also be true of other programs implemented through AWCs that are universal in their coverage of all children such as the supplementary nutrition program that provides take home rations for pregnant women and children through AWCs. Even though such rations are supposed to be provided to all children, AWC staff may differentially provide them to households with whom they have greater contact due to IGMSY.[19] In both these cases, a significant effect of IGMSY eligibility may result, even if the program does not, by itself, improve health, due to a spillover effect on the functioning of other programs. Such spillover effects might be far less if the IGMSY is scaled to all districts, since this remove any redistribution towards pilot districts.

Finally, as theorized in the previous section, expectations of additional income at a later date might influence fertility dynamics specifically the interval between the births of successive children. Eligible, credit-constrained mothers might put off having a second child until receipt of the income

[19] Even though services delivered under ICDS were made universal under a Supreme Court decision, lack of funds in Bihar has precluded this from being the case. Thus, all pregnant women and children under the age of 3 in the state do not receive take home rations, as of the survey date.

support for the first child, resulting in a longer interval between the first and second births, even should the payment be delayed or never received. This could only affect the interval between the birth of the first and second child, not the prior interval to the first birth, since funds were conditional on the birth of the child; parents could delay having a second child, until funds were received for the first child, but could not similarly manipulate the timing of the first child.

Examining differences in ITT estimates across the first and second eligible child provides some evidence on these potential pathways. There is no reason to expect any direct effect on health (due to the imposition of health conditionalities or through the effectiveness of other programs) to differ across first and second-born eligible children, unless compliance improves with program duration. If this is the case, then we would expect a large effect on the second child, relative to the first.

The effect of received income support, the “current income” effect, could differ across the first and second eligible child, depending on when the income support was received and on whether and how income effects on health vary by age. As previously noted, income support payments for the first child were generally received around the birth of the second child. This is documented in figure 3 that graphs the difference in months between the receipt of the first received installment of income support and the birth of the second child, for households in which the first child was eligible for the program. If the availability of income around the time of a child’s birth is more important (for current health) than household income when the child is 2 years of age or older, then ITT estimates of the program would be greater for the second eligible child than for the first, with the opposite being true if the health-income gradient increases with age.

Expectations of additional household income would likely increase the interval between the first and second birth for credit constrained households. If a longer subsequent birth interval improves the health of the older child, then this expectation of income would have a large effect on the health of the first eligible child. Expectations of child support for the second child, however, are likely to be conditioned on the receipt of the first payment: Non-receipt or delayed payments may limit the extent to which households condition decisions regarding the interval between the second and third birth on expected payments for the second child.

To provide evidence on pathways, we first report results from regressions that distinguish between the first and second eligible child. This means that the variable *order_12* in equation 10 is now separated into two variables, that identify the first and second born children, and that, correspondingly, all interaction terms with *order_12* are replaced by two terms allowing for differences between the first and second born child.

We then report results from auxiliary regressions that examine the effect of age on the health-income gradient, based on two primary determinants of household income, the father’s years of education and father’s age. We regress child WAZ and HAZ scores on interactions of father’s education and age with the child’s age to test whether current income has a greater effect on children distinguished by age. If ITT estimates of the effect of the program on WAZ and HAZ reflect the actual receipt of income support payments, then we would expect any estimated differential effects by birth order to be mirrored in these regressions. That is, if we find that the program primarily benefited second born children, then we would expect that determinants of (current) income would have a larger effect on younger children.

In a final set of regressions, we test for effects of the program on health behaviors and other program outcomes. For testing the effect on behaviors required under the conditions of the IGMSY program, we construct a variable that takes the value 1 for each of the following conditions being met: Any ANC registered in the mother child protection (MCP) card; BCG registered in MCP; DPT registered in MCP; and polio vaccine registered in MCP. We then distinguish between high scores (score of 3 or 4) and low scores, and use this as the outcome variable in equation 10, estimated as a probit regression. We also run this same (probit) equation using the following variables as the dependent variable: indicator for whether the mother received take home rations when pregnant with the child in question, whether the child received take home rations between the ages of 6 months and 3 years, and for an indicator of whether the child was born in a health institution (government or private hospital, and government or private clinic).

5.3 Estimates of the effect of program eligibility on birth intervals

We directly test whether eligibility for the program affected the interval between the first and second birth. We do so on the basis of a conditional hazard function that accounts for women who have not yet had a second child and hence have not closed this interval. Let T be a random variable measuring the duration of a birth interval, and f(t) and F(t) be the associated density and distribution functions. The distribution function defines the “survival function”, S(t) = (1-F(t)) = Prob(T>=t). It also defines the hazard rate, λ(t), the rate at which a spell is completed after duration t, given that it has lasted until at least t:

$$\lambda(t) = \lim_{\Delta t \to 0} \frac{Prof(t \leq T \leq t + \Delta t \,|T >= t)}{\Delta t} = \frac{f(t)}{S(t)}$$

We model the hazard function as:

$$\lambda(t, x, \beta) = \lambda_0(t)\exp(x\beta)$$

Where $\lambda_0(t)$ is the baseline hazard. We assume that this follows a Weibull distribution, so that the hazard function is:

$$\lambda(t) = \alpha t^{\alpha - 1}\exp(x\beta)$$

Define d_i as an indicator variable that takes the value 1 if a mother has had a second birth, 0 otherwise. The Likelihood function, allowing for censored observations, is thus given by:

$$L_i(\alpha, \beta) = [f(t)]^{d_i}[S(t)]^{1-d_i}$$

We estimate this likelihood for the interval between the first and second birth, once again restricting the sample to narrowly defined cohorts by mothers’ year of marriage and defining eligibility based on marriage year. The set of regressors included in this regression are similar to those used for the estimation of HAZ and WAZ reported earlier.

6. Results

6.1 ITT Estimates of Program Eligibility on WAZ, HAZ

Tables 5 and 6 report ITT estimates of the effect of program eligibility on WAZ and HAZ, respectively. The first three regressions in each table identify eligible children based on their birth year (>=2012), while the next three do so on the basis of the mother's marriage year (>=2005). This is the preferred set of regressions, given the hypothesis of this paper that decisions regarding the timing of births may be endogenous. In all regressions, the sample is children whose mothers were married in 2005 or later.

In the first regression, additional regressors are only the indicator variables *tdistr, elig,* and *order_12*, and the full set of double interaction terms based on these three variables. The second regression in each set of results reported includes the additional regressors described in the previous section and also listed in the note to the table. The third regression allows the effect of eligibility to vary across the oldest and second child, with all double interaction terms also similarly varying.

The results suggest an insignificant effect of eligibility on height and weight, when eligibility is defined on the basis of the child's birth year. However, defining eligibility by the mother's year of marriage reveals a positive and statistically significant effect of eligibility on WAZ and HAZ, with the exception of the statistically insignificant coefficient on HAZ in the regression with no additional controls. These results suggest that the program improved both short (WAZ) and long-term (HAZ) measures of health. This is a surprising result, primarily because the income support provided under the program was a one-time transfer, amounting to just several months' income. This is different from welfare programs in other countries, such as *Oportunidades* that provided income support to households over a number of years.

Distinguishing between the oldest and the second born child, the third regression for each set of regressions (with eligibility defined by marriage year) reveals that the magnitude of the effect is larger for the first born child, relative to the second child. A F test of statistical significance of this difference fails to reject the hypothesis that the two coefficients are different. Hence, we do not emphasize this difference, but note that the results suggest larger effects on the oldest child. Again, this appears contrary to expectations: It is reasonable to expect the program to improve functioning over time, in terms of enforcing conditionalities and providing better information and counselling to pregnant women, so that normally we would expect larger effects on second born children.

6.2 Validity of eligibility cut-off and sensitivity to regression sample

Table 7 reports results from regressions that support the eligibility cut-off criterion, and also test sensitivity to different regression samples. Two sets of results are reported: The first three regressions provide results for regressions on WAZ, while the second set of three regressions report results for HAZ. For each set of regressions, the first regression allows the eligibility cut-off to be the indicator variable for whether the mother was married in 2010 or later, but also includes interactions with an indicator for whether the mother was married in 2008 or later. The regressions reveal coefficients on the cut-off based on 2008 that are very small in magnitude and statistically insignificant. The coefficient on the indicators based on the 2010 cut-off remain similar to regressions of the previous table (without the 2008 indicators), though the larger standard errors reduce the level of statistical significance.

The next two regressions gauge the sensitivity of results to the regression sample. The regressions reported in previous tables includes all children whose mothers were married in 2005 or later. We now consider two alternative criteria, first reporting results for the smaller sample of children whose mother was married I 2006 or later, and then for the larger sample with marriage in 2004 or later. The regression results do not vary significantly with these alternative samples. In all cases, the effect of eligibility for the first child is positive and statistically significant. The coefficient is smaller for the second child, and not statistically significant, though, again, the hypothesis of equal coefficients cannot be rejected.

6.3 Pathways: current income, health conditionalities, and other ICDS effects

Our exploration of pathways starts with an examination of the possibility that the regression results reported above reflect a differential effect, by age, of current income on child health. The greater effect on first born children would require that the income support, when received, to have affected the health of the (older) first child, but not the second. At the very least, this requires the effect of current income on child health to increase with age.

We test this by regressing determinants of income, specifically the father's years of education and his current age, on weight- and height-for-age, allowing the effects of these determinants to vary with age. The results are in table 8, with the first four regressions reporting results for WAZ with results for HAZ in the last four regressions. The first regression (1) for WAZ and HAZ report regression results based on father's education, and reveal that the effect of education *falls* with age, instead of increasing. This suggests that the statistically significant and larger effect of program eligibility on the first child's health, relative to the smaller and statistically insignificant coefficient on the health of the second born child, could not be the consequence of a standard income effect. The second regression (for both WAZ and HAZ) explores sensitivity to functional form by including a quadratic in age (and in the interaction with father's education). The results remain unchanged. The third and fourth regressions repeat regressions (1) and (2), but now using father's age as a determinant of income. These regressions reveal no statistically significant heterogeneity by the child's age, again implying that income effects cannot explain the ITT estimates of tables 4 and 5.

Table 9 explores whether the ITT estimates could be a consequence of a direct effect on health, either as a consequence of health conditionalities that were intended to be met in order to receive the income support or because of improved functioning of other ICDS programs. As previously described, we test the effect through health conditionalities by running regressions similar to the height and weight regressions reported in table 4 and 5, but on an index of health behaviors (based on ANCs and immunizations) as the regressand. The other three regressions in the table test potential effects through other ICDS interventions: the provision of "take home rations" (THR) during pregnancy and in the child's first three years, as well as the probability of an institutional delivery. Eligibility for IGMSY is, in all three regressions, based on mother's year of marriage.

All regressions yield a statistically insignificant effect of IGMSY eligibility on health conditionalities and on the outcomes of other ICDS programs, both for first born and for the second born child. These regressions suggest that the significant effect of program eligibility on the child's health cannot reflect a direct effect through health behavior or other interventions that likely enhance health.

6.4 Pathways: The interval between the first and second birth

Table 10 provides results from an OLS regression of the interval between the first and second birth, as well as coefficients from the estimation of a hazard rate for the risk of the second birth, as described in section 5.3. As in the previous table, the regression utilizes the mother's year of marriage to define eligibility. The regression is run only on the oldest child in the household, so that birth order interactions are implied. Thus, eligibility is defined by the interaction term *tdistr x myr_10.* The first regression utilizes only the child's gender as an additional control, while the second regression includes the full set of control variables previously described.

The first OLS regression reveals a significant positive effect of eligibility on the interval between the first and second birth. As pointed out earlier, these estimates are biased, since they do not take into account the fact that many mothers have not yet had a second birth, resulting in open birth intervals. To account for this, the next two regressions estimate the likelihood function specified in section 5.3. These regressions confirm that eligibility has a negative effect on the probability of the second child, implying a longer birth interval between the first and second child. The implications of these regression results for the hazard of a second birth and for survival times (to second birth) are revealed through plots of the hazard rate (figure 4) and survival rates (figure 5). These graphs reveal that survival rates (no second birth) fall with time.

This finding, in combination with earlier results that reveal no effect of eligibility through income or health behaviors, suggests that program eligibility affected child health primarily through its effect on birth intervals. This is an unintended effect of the program: In designing the program, it was thought that income support would either provide the resources necessary to the household to improve nutrition and maternal health (through reductions in hours of work), or would enhance health through its effects on child behavior. Instead, our empirical analysis suggests that the effects accrue primarily through the effects on birth intervals.

On the assumption that program eligibility *only* affects the health of the first born child through its effect on the interval between the first and second births, we can use program eligibility as an instrument for birth interval, and hence evaluate the effect of birth intervals on health. We do this exercise only for the first child. For this child, the relevant birth intervals are the interval to the first birth, and the interval between the first and second birth. It is unlikely that the program affected the time to the first birth through attempts by women to manipulate the date of their pregnancy, given ambiguity in the start date and a generally flexible implementation of the program, as previously described. Thus, for first born children, the use of program eligibility as an instrument for birth interval is justified under the null hypothesis of no additional effects of eligibility through income or health behavior. This condition is unlikely to be met for second born children, whose health reflects the interval between the first and second born child, and the interval between the first and second born child. Since program eligibility is likely to affect both these birth intervals, the reduced form regression of the heath of the second born child on program eligibility reflects an effect through both birth intervals. Given our inability to identify both intervals as well as the large number of open intervals between the second and third child, we only consider birth interval effects for the first child.

The regression results are in table 11 (first stage results are in Appendix table 1). The first four regressions report results for WAZ, while the next four report corresponding results for HAZ. For both WAZ and HAZ, the first regression is an OLS regression that reveals a statistically insignificant effect of birth intervals on health. The second regression uses the indicator for program eligibility as an instrument, and reveals a positive effect on weight-for-age, that is statistically significant at the 10% level. The coefficient on HAZ is also positive, but significant only at a 12% level (Prob>|z|=0.116). The third regression for WAZ and HAZ respectively include an interaction of eligibility with an indicator variable that tales the value 1 if the first child is a male. This regression generates an effect on WAZ that is statistically significant at the 5% level, and a positive coefficient on HAZ, significant at the 10% level. The last set of regressions also include an interaction of eligibility with an indicator variable for whether the father is the oldest son, allowing for potential effects on birth intervals based on the child's order in the extended household. This regression yields estimates similar to those in the previous regression.

To interpret the magnitude of the coefficient estimates we use the mean and standard deviation of closed intervals between the first and second birth.[20] The coefficient estimates from regressions utilizing eligibility and the interaction of eligibility with an indicator for a male child as instruments (the third regression for WAZ and HAZ in table 10) imply that a one-standard deviation increase in the birth interval improves WAZ by 1.3 standard deviations and HAZ by 1.6 standard deviations. This means that increasing the birth interval from the 25th percentile to the median, a difference of 7 months, would improve WAZ by 0.7 standard deviations, and HAZ by 0.9 standard deviations.

7. Conclusion

Poor implementation of programs is common to many developing countries, particularly to regions in these countries characterized by a higher incidence of poverty and female literacy. Implementation is generally viewed to be critical for programs that require the timely delivery of inputs so as to ensure that budget and other constraints at critical periods are addressed. This applies particularly to maternal and child health programs, given the widespread recognition of the importance of income and other health inputs during pregnancy and the first 6 months of a child's life. It was precisely this constraint that India's maternity benefits program, the IGMSY, was intended to address. Hence, the finding that the program generally failed to deliver benefits during this period would suggest that its effect on child health would be minimal.

Instead, our triple difference estimates suggest significant effects on the health of eligible children, particularly for the first eligible child. Through a set of auxiliary regressions, and utilizing data we collected on other maternal and child health programs run by the government, we find that eligibility did not affect the delivery of other health inputs. Similarly, we report results that suggest that the differential effect of the program on first and second born children could not be a consequence of current income effects.

This leaves the possibility that eligibility for the program affected women's decisions regarding the interval between the first and second child, and, through this, child health. Credit constrained households, anticipating additional income, would likely delay the birth of the second child so as to defer the additional consumption expenditures a second birth entails to periods with greater income. This effect, since it is based only on expectations of additional income, would maintain even if the income is never forthcoming; it just requires the program to generate

[20] For closed birth intervals, the mean duration is 26 months with a standard deviation of 12.24. The birth interval at the 25th percentile is 19 months.

expectations of additional income in future periods. Additionally, since the attempt is to delay the second birth so as to have additional income to meet pregnancy and birth related expenses, such an effect would obtain even if the income transfer is a one-time transfer of a few month's income.

We provide regression evidence that eligibility did increase the interval between the births of the first and second child. Using eligibility as an instrument, we then show that a larger birth interval improves both weight and height. This accords with policy statements that have suggested that a longer birth interval is a primary determinant of health. Subject to the validity of using program eligibility as an instrument for birth intervals, our results provide empirical evidence to support this belief.

Our results have important policy implications, in that they suggest that policies that address the interval between births could have a significant impact on child health. More generally, our results emphasize the relationship between dynamic fertility choices and health, and underscore the importance of policies that jointly address fertility choices and health.

These findings are particularly useful as India debates the future of the program. In his 2017 New Year's Eve Address, Prime Minister Modi pledged the continuation of the program and committed the government to maternity support payments of Rs. 6000 per child, for a woman's first two children. However, a recent newspaper article reports on a proposed Cabinet note by the Ministry of Women and Child Development that scales back benefits to cover just the first child, and reduces the Centre's funding to 50% of the costs, due to budgetary constraints.[21] Given the findings of this paper, this reduction is likely to adversely affect child health: If the program induced improvements in child health even in states where the program was poorly implemented, its effect is likely to have been significantly greater in states that ensured timely disbursements and in others, such as Tamil Nadu, where the state government provided far greater funds than mandated by the Central Government.

[21] Reported in the Indian Express, February 18, 2017, available at: http://indianexpress.com/article/india/not-enough-funds-govt-to-limit-maternity-benefit-to-first-child-only-4530927/

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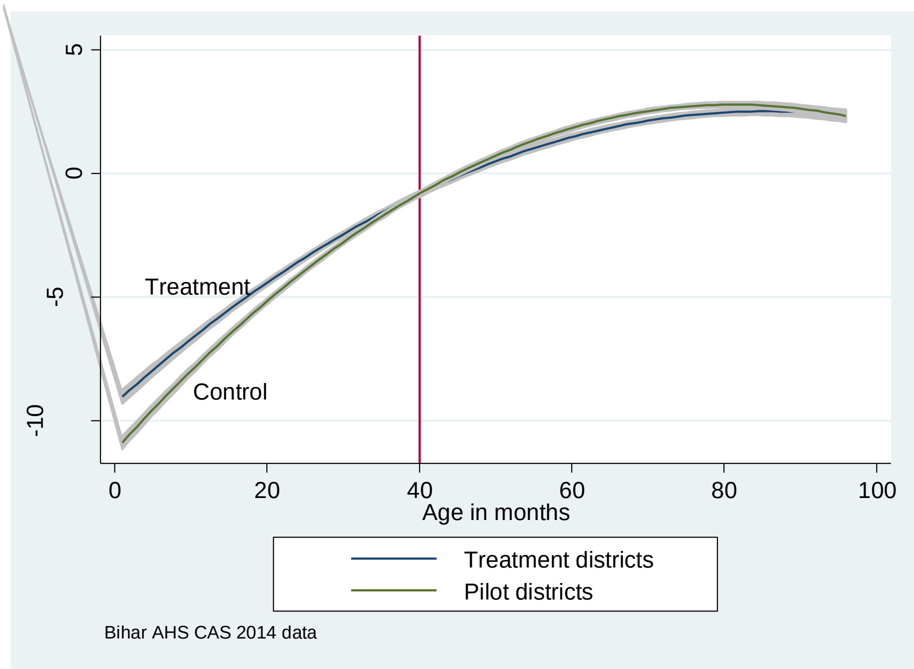

Figure 1: Residual Weight by Age, Bihar AHS_CAB data (residuals from weight on district score on variables used to select pilot districts, regressed on a quadratic in child age)

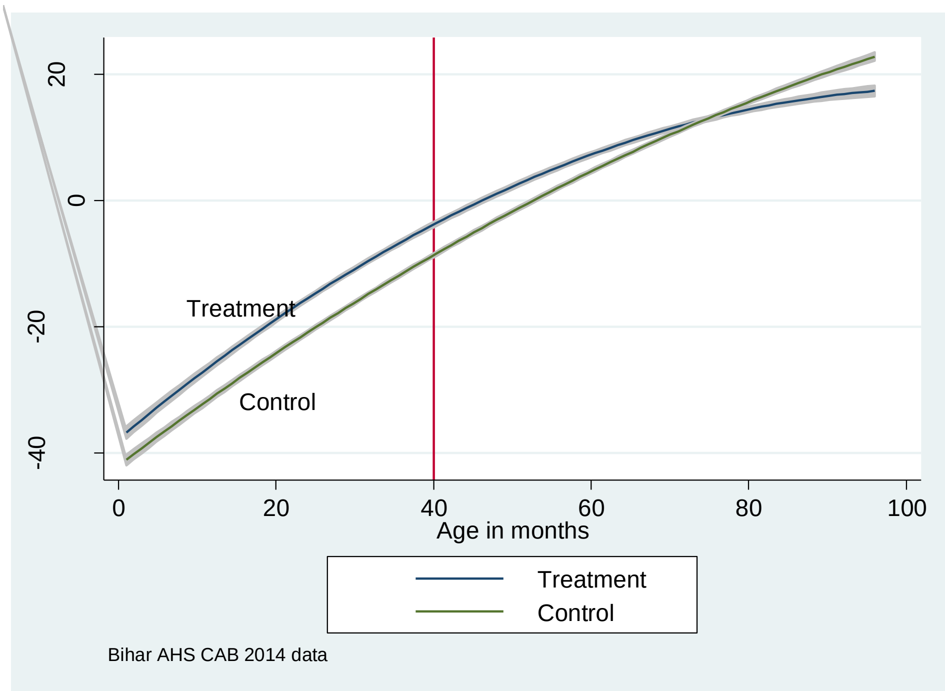

Figure 1: Residual Height by Age, Bihar AHS_CAB data (residuals from weight on district score on variables used to select pilot districts, regressed on a quadratic in child age)

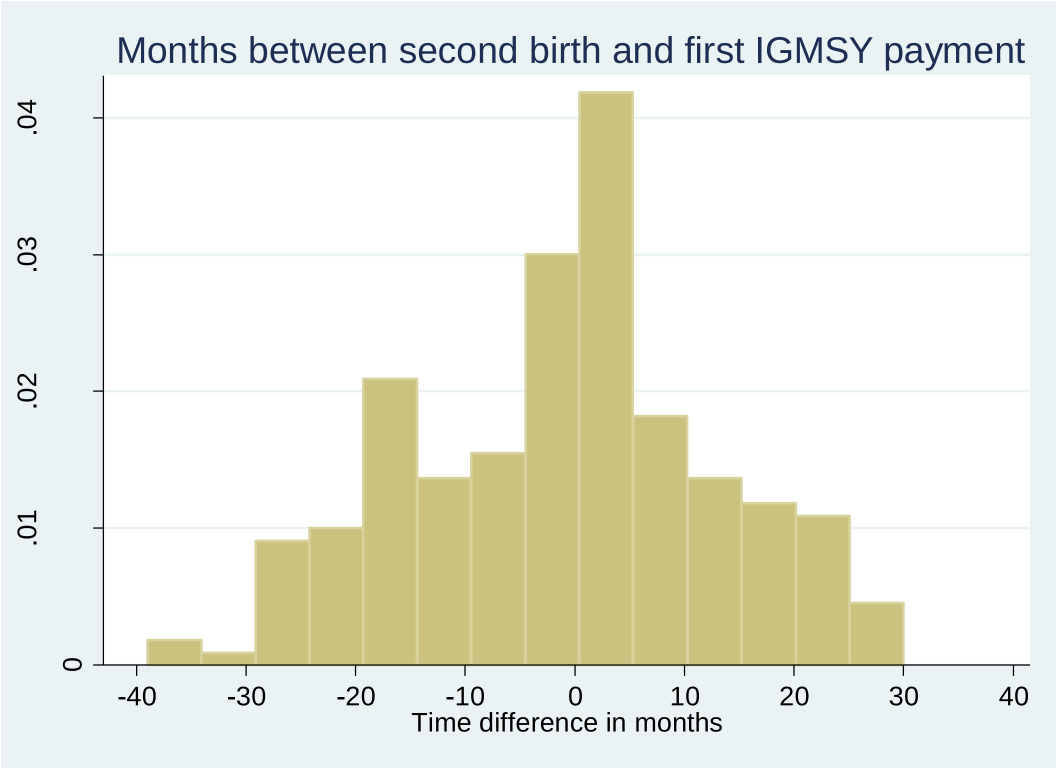

Figure 3: Timing of first IGMSY payment relative to birth of second eligible child (in months)

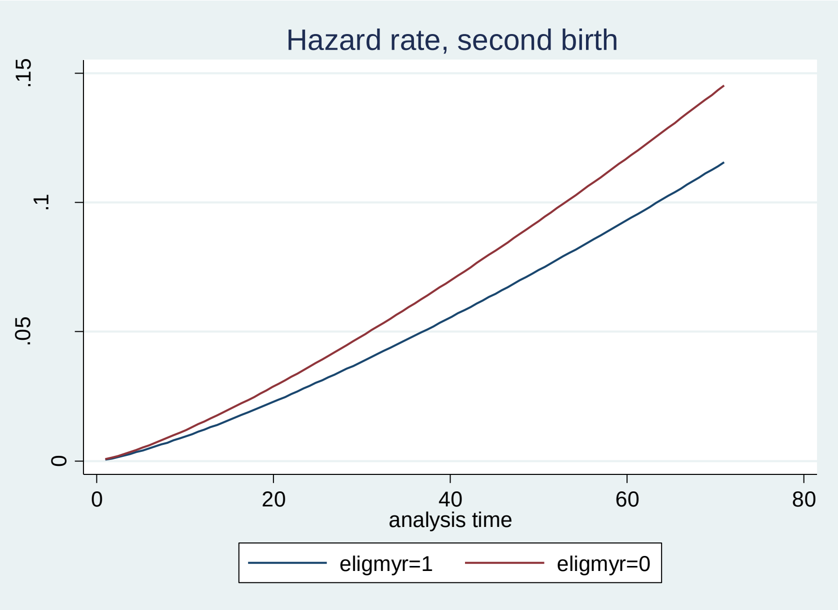

Figure 4: Hazard rate, second birth

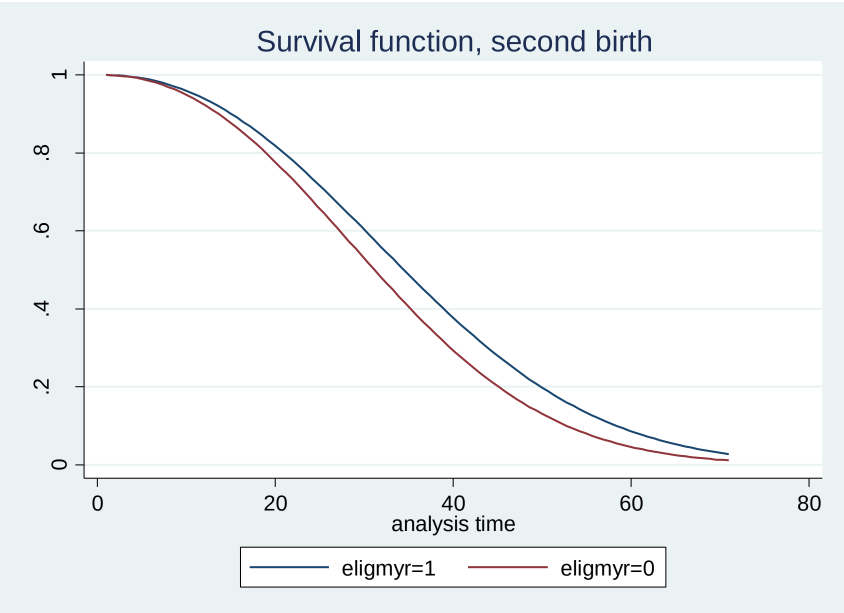

Figure 5: Survival rate, second birth

Table 1: Summary statistics, survey region

Variable	Full sample	Treatment districts	Control districts
Household characteristics			
Proportion owning land	0.31	0.30	0.33
	(0.46)	(0.46)	(0.47)
Amount of land, if positive (ha)	1.58	1.74	1.42
	(1.87)	(2.03)	(1.67)
Scheduled caste or tribe	0.25	0.27	0.22
	(0.43)	(0.44)	(0.41)
Proportion joint household	0.69	0.66	0.71
	(0.46)	(0.47)	(0.45)
Mothers' characteristics			
Proportion with formal schooling	0.44	0.44	0.44
	(0.50)	(0.50)	(0.48)
Mother's age	24.03	24.08	23.98
	(2.76)	(2.80)	(2.72)
Mother's age at marriage	18.60	18.65	18.54
	(1.95)	(1.99)	(1.90)
Prop. Primarily employed in domestic work in own home	0.98	0.97	0.98
	(0.15)	(0.16)	(0.12)
Father's characteristics			
Proportion with formal schooling	0.62	0.59	0.66
	(0.48)	(0.49)	(0.47)
Mean years of schooling	5.47	5.18	5.81
	(5.54)	(5.52)	(5.54)
Prop. Primary occupation own farm	0.04	0.05	0.03
	(0.20)	(0.21)	(0.18)
Prop. Primary occupation unskilled labor market	0.40	0.43	0.37
	(0.49)	(0.50)	(0.48)
Prop. Primary occupation skilled labor market	0.15	0.16	0.15
	(0.36)	(0.37)	(0.36)
Child characteristics			
Age	2.41	2.41	2.42
	(1.76)	(1.75)	(1.76)
WAZ	-2.18	-2.19	-2.18
	(1.73)	(1.78)	(1.69)
HAZ	-2.77	-2.89	-2.65
	(2.15)	(2.19)	(2.10)
Proportion severely malnourished (WAZ<=-3)	0.28	0.30	0.28
	(0.45)	(0.46)	(0.45)
Proportion severely stunted (HAZ <=-3)	0.45	0.48	0.42
	(0.50)	(0.50)	(0.49)
Interval between first and second births	24.30	24.58	23.97
	(14.65)	(14.61)	(14.70)

Note: survey data, regression samples

Table 2: IGMSY Implementation

	Treatment districts
IGMSY register available in AWC	98.77%
(n=163)	
IGMSY register regularly maintained	50.3%
Of eligible children:	
(n=2233)	
Prop for whom any funds received	0.31
	(0.46)
Proportion by birth year	
2011	0.27
	(0.44)
2012	0.42
	(0.49)
2013	0.34
	(0.48)
2014	0.37
	(0.48)
IGMSY payments, eligible children	
Mean amount received, Rs.	3,816.44
	(1303.50)
Prop. Of total received in first installment	0.84
	(0.25)
Of all first-eligible children, proportion receiving payments	0.05
within 6 months of birth	(0.23)
Of all first-eligible children, proportion receiving payments	0.06
before initiation of second (eligible) pregnancy	(0.24)
Of all first-eligible children who received income support,	0.17
proportion receiving within 6 months of birth	(0.37)

Note: AWC data are for the 163 AWCs in the two program districts, while data on eligible children is from the sample of 2233 eligible children.

Table 3: Health practices, survey region, children less than 6 years of age

	Full sample	Treatment districts	Control districts
Pregnancy registered	0.95	0.93	0.96
(n=8218)	(0.22)	(0.25)	(0.20)
MCP card reported	0.97	0.97	0.98
available	(0.16)	(0.17)	(0.16)
MPC card shown	0.38	0.36	0.39
	(0.49)	(0.48)	(0.49)
ANC reported	0.80	0.81	0.80
	(0.40)	(0.40)	(0.40)
ANC registered in MCP card	0.78	0.79	0.78
	(0.41)	(0.41)	(0.41)
BCG reported	0.99	0.99	0.99
	(0.10)	(0.09)	(0.10)
BCG registered	0.42	0.40	0.42
	(0.49)	(0.49)	(0.49)
OPV reported	0.99	0.99	0.98
	(0.12)	(0.09)	(0.13)
OPV registered	0.37	0.36	0.37
	(0.48)	(0.48)	(0.48)
DPT reported	0.96	0.99	0.95
	(0.20)	(0.10)	(0.21)
DPT registered	0.36	0.36	0.36
	(0.48)	(0.48)	(0.48)
Information sources			
Received any information	0.59	0.59	0.59
on weight gain	(0.49)	(0.49)	(0.49)
If yes, prop. Reporting	0.94	0.92	0.96
primary source is govt	(0.23)	(0.26)	(0.19)
Received any information	0.61	0.60	0.63
on nutrition	(0.49)	(0.49)	(0.48)
If yes, prop reporting	0.93	0.91	0.94
primary source is govt	(0.26)	(0.29)	(0.24)
Decision making			
Food exp – self	0.44	0.43	0.45
	(0.50)	(0.50)	(0.50)
Food exp- mother-in-law	0.48	0.51	0.45
	(0.50)	(0.50)	(0.50)
What is cooked – self	0.38	0.37	0.39
	(0.48)	(0.48)	(0.49)
What is cooked – mother-	0.51	0.53	0.48
in law	(0.50)	(0.50)	(0.50)
Work hours – self	0.35	0.35	0.35
	(0.48)	(0.48)	(0.48)
Work hours – mother-in-	0.18	0.21	0.15
law	(0.38)	(0.40)	(0.36)
Work hours - husband	0.35	0.32	0.38
	(0.48)	(0.47)	(0.49)

Note: Survey data, standard errors in parentheses. Responses on information and decision making is by women who were pregnant at the time of the survey

Table 4: Regression sample

	Treatment sample	Control Sample
Number of children	3,272 (53.05%)	2,897 (46.95%)
Proportion born after eligibility cut-off	0.78 (0.41)	0.77 (0.42)
Proportion who are first or second born	0.68 (0.47)	0.62 (0.48)
Proportion eligible	0.53 (0.50)	--

Note: Proportions reported are relative to column totals, with standard deviations in parentheses.

Table 5: Triple difference ITT estimates of eligibility on WAZ

	Eligibility based on birth year (>=2012)			Eligibility based on year of marriage (>=2010)		
	(1)	(2)	(3)	(4)	(5)	(6)
Tdistr*elig*order_12	0.16	0.14	--	1.29[+]	1.23[+]	--
	(0.23)	(0.24)		(0.73)	(0.69)	
Tdistr*elig*order_1	--	--	0.34	--	--	1.37[*]
			(0.25)			(0.69)
Tdistr*elig*order_2	--	--	-0.19	--	--	1.07
			(0.27)			(0.70)
Additional regressors	All single and double interactions	Base + additional regressors	Base + additional regressors	All single and double interactions	Base + additional regressors	Base + additional regressors
Regression F	14.21	27.53	23.32	4.53	21.80	18.87
(Prob > F)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
F Test equality of birth order effects – first and second child	--	--	7.94	--	--	1.77
			(0.00)			(0.18)

Note: Regression standard errors clustered at the level of the household. Sample size in all regressions is 5589. Eligibility for regressions (1) through (3) is defined as those born after June 2011. For regressions (4) through (6) eligible children are those where the mother was married in 2010 or later. Additional regressors include child's gender, indicator for scheduled caste or tribe, mother's height, weight and BMI, indicator for whether mother has formal schooling, father's schooling years, indicator for whether father is oldest son, amount of land owned by the extended household, population of AWC and proportion of population that is from scheduled castes and tribes, distance to block office, and indicators for birth month in the Kharif and monsoon seasons.

[*]Significant at 5% level
[+]Significant at 10% level

Table 6: Triple difference ITT estimates of eligibility on Height-for-Age Z scores (HAZ)

	Eligibility based on birth year (>=2012)			Eligibility based on year of marriage (>=2010)		
	(1)	(2)	(3)	(4)	(5)	(6)
Tdistr*elig*order_12	-0.19	-0.22	--	1.60	1.50*	--
	(0.30)	(0.30)		(0.78)	(0.73)	
Tdistr*elig*order_1	--	--	-0.03	--	--	1.62*
			(0.32)			(0.74)
Tdistr*elig*order_2	--	--	-0.43	--	--	1.24+
			(0.34)			(0.75)
Additional regressors	All single and double interactions	Base + additional regressors	Base + additional regressors	All single and double interactions	Base + additional regressors	Base + additional regressors
Regression F	40.75	60.68	51.42	13.31	53.49	45.79
(Prob > F)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
F Test equality of birth order effects – first and second child			2.87 (0.09)			

Note: Regression standard errors clustered at the level of the household. Sample size in all regressions is 5441. Eligibility for regressions (1) through (3) is defined as those born after June 2011. For regressions (4) through (6) eligible children are those where the mother was married in 2010 or later. Additional regressors include child's gender, indicator for scheduled caste or tribe, mother's height, weight and BMI, indicator for whether mother has formal schooling, father's schooling years, indicator for whether father is oldest son, amount of land owned by the extended household, population of AWC and proportion of population that is from scheduled castes and tribes, distance to block office, and indicators for birth month in the Kharif and monsoon seasons.

*Significant at 5% level

+Significant at 10% level

Table 7: Sensitivity test for different regression samples

	WAZ regression sample			HAZ regression sample		
	Marr yr >=2005	Marr yr>=2006	Marryr>=2004	Marr yr >=2005	Marr yr>=2006	Marryr>=2004
Tdistr*marryr_10*order_1	1.17+	1.37*	1.26+	1.47+	1.47*	1.51*
		(0.70)	(0.69)	(0.78)	(0.75)	(0.74)
Tdistr*marryr_10*order_2	0.98	1.07	1.01	1.16	1.08	1.17
	(0.73)	(0.70)	(0.70)	(0.79)	(0.75)	(0.75)
Tdistr*marryr_08*order_1	0.35	--	--	0.35	--	--
	(0.33)			(0.38)		
Tdistr*marryr_08*order_2	0.10	--	--	0.10	--	--
	(0.30)			(0.36)		
Regression F	15.46	17.11	19.24	37.13	44.71	47.20
(Prob > F)	(0.00)		(0.00)	(0.00)	(0.00)	(0.00)
Sample size	5589	5020	5917	5441	4890	5761
F Test equality of birth order effects – first and second child	0.60 (0.44)	1.83 (0.18)	1.29 (0.26)	1.05 (0.30)	1.91 (0.17)	1.46 (0.00)
F test for equality of coefficients for first child (2010=2008)	0.84 (0.36)	--	--	1.36 (0.24)	--	--
F test for equality of coefficients for second child (2010=2008)	1.01 (0.32)	--	--	1.21 (0.27)	--	--

Note: Regression standard errors clustered at the level of the household. Eligibility for all regressions is based on marriage year Additional regressors include all single and double terms in the triple interaction (tdistr x elig x birth order), child's gender, indicator for scheduled caste or tribe, mother's height, weight and BMI, indicator for whether mother has formal schooling, father's schooling years, indicator for whether father is oldest son, amount of land owned by the extended household, population of AWC and proportion of population that is from scheduled castes and tribes, distance to block office, and indicators for birth month in the Kharif and monsoon seasons.

*Significant at 5% level +Significant at 10% level

Table 8: Regression Estimates of WAZ and HAZ on interactions of child's age with income determinants

	WAZ	WAZ	WAZ	WAZ	HAZ	HAZ	HAZ	HAZ
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Fath ed yrs*age	-0.001*	-0.002*	--	--	-0.001*	-0.0003	--	--
	(0.0002)	(0.001)			(0.0003)	(0.001)		
Fath ed yrs * age sq	--	0.00002+	--	--	--	-2.55 e-6	--	--
		(0.00001)				(0.00001)		
Fath age*age	--	--	0.0003	0.0003	--	--	0.0006	-0.0001
			(0.0003)	(0.001)			(0.0004)	(0.001)
Fath age*age sq	--	--	--	-9.1 e-6	--	--	--	-3.9 e-6
				(0.00002)				(0.00002)
Age (in months)	0.01*	-0.03*	-0.004	-0.05	0.02*	-0.07*	-0.01	-0.07
	(0.002)	(0.01)	(0.01)	(0.03)	(0.002)	(0.01)	(0.01)	(0.04)
Age square	--	0.001*	--	0.001*	--	0.001*	--	0.001*
		(0.0001)		(0.0004)		(0.0001)		(0.0005)
Fath ed yrs	0.04*	0.05*	0.02*	0.02*	0.04*	0.03+	0.02	0.02*
	(0.01)	(0.02)	(0.01)	(0.005)	(0.01)	(0.02)	(0.01)	(0.01)
Fath age	--	--	-0.01	0.01	--	--	-0.01	0.02
			(0.02)	(0.02)			(0.02)	(0.03)
Regression F	30.23	35.99	27.10	32.48	69.21	79.75	61.51	72.72
(Prob > F)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)

Note: OLS regressions, standard errors (in parentheses) clustered at the level of the household. Regression sample size is 5589. Additional regressors include child's gender, indicator for scheduled caste or tribe, mother's height, weight and BMI, indicator for whether mother has formal schooling, indicator for whether father is oldest son, amount of land owned by the extended household, population of AWC and proportion of population that is from scheduled castes and tribes, distance to block office, and indicators for birth month in the Kharif and monsoon seasons.

*Significant at 5% level +Significant at 10% level

Table 9: Probit regressions of health conditionalities, and ICDS health services and programs on eligibility by mother's marriage year (Regression sample: marriage year >=2005)

	Health conditions	THR during pregnancy	THR for child at ages <3	Instit. Delivery
	(1)	(2)	(3)	(4)
Tdistr*myr10*order_1	0.12	0.54	0.12	0.03
	(0.37)	(0.44)	(0.37)	(0.41)
Tdistr*myr10*order_2	0.04	0.39	-0.09	0.03
	(0.37)	(0.44)	(0.36)	(0.40)
Target district	0.06	-0.11	-0.07	0.14
	(0.09)	(0.11)	(0.09)	(0.10)
Marryr_10	0.18	0.11	0.13	0.32
	(0.27)	(0.34)	(0.28)	(0.32)
Birth order 1	-0.48*	-0.04	-0.34*	0.11
	(0.08)	(0.10)	(0.08)	(0.08)
Birth order 2	-0.32*	-0.04	-0.19*	0.17*
	(0.07)	(0.08)	(0.07)	(0.07)
Wald χ^2	194.48	51.76	121.45	219.47
(Prob > χ^2)	(0.00)	(0.00)	(0.00)	(0.00)
F Test equality of birth order effects	0.41	0.84	2.01	0.00
	(0.52)	(0.36)	(0.16)	(0.97)

Note: Regression standard errors clustered at the level of the household. Eligibility for all regressions is based on marriage year >=2010. Additional regressors include double interaction terms from triple interaction (tdistr x elig x birth order), child's gender, indicator for scheduled caste or tribe, mother's height, weight and BMI, indicator for whether mother has formal schooling, father's schooling years, indicator for whether father is oldest son, amount of land owned by the extended household, population of AWC and proportion of population that is from scheduled castes and tribes, distance to block office, and indicators for birth month in the Kharif and monsoon seasons.

*Significant at 5% level +Significant at 10% level

Table 10: Estimates of hazard rate, second birth

(Sample: Oldest child, mothers married on or after 2005)

Variable	OLS – dependent variable: birth interval in months	Hazard rate	Hazard rate
	(1)	(2)	(3)
Eligible (tdistr*myr10)	2.95*	-0.19+	-0.23*
	(1.11)	(0.11)	(0.11)
Tdistr	-1.62+	0.09	0.15+
	(0.92)	(0.08)	(0.08)
Myr10	-9.76*	-0.06	-0.01
	(0.83)	(0.09)	(0.09)
Male	1.16	-0.17*	-0.16*
	(0.54)	(0.06)	(0.06)
Additional regressors	Yes	No	Yes
Wald χ^2 / Regression F	151.44	18.69	97.06
(Prob > χ^2)	(0.00)	(0.00)	(0.00)
Sample size	2535	2535	2535
Number of failures	--	1462	1462
P	--	2.22	2.28
		(0.05)	(0.05)
1/p	--	0.45	0.44
		(0.01)	(0.01)

Note: Standard errors clustered at the level of the household in parentheses. Additional regressors include indicator for scheduled caste or tribe, mother's height, weight and BMI, indicator for whether mother has formal schooling, father's schooling years, indicator for whether father is oldest son, amount of land owned by the extended household, population of AWC and proportion of population that is from scheduled castes and tribes, distance to block office, and indicators for birth month in the Kharif and monsoon seasons.

*Significant at 5% level +Significant at 10% level

Table 11: regressions of birth interval on WAZ and HAZ
(Regression sample: oldest child, mother's marriage year>=2005)

Variable	WAZ				HAZ			
	OLS	IV	IV	IV	OLS	IV	IV	IV
Birth interval	-0.002	0.10+	0.11*	0.12*	-0.005	0.15	0.13+	0.14*
	(0.002)	(0.06)	(0.05)	(0.05)	(0.003)	(0.10)	(0.07)	(0.07)
Tdistr	0.14*	0.08	0.08	0.07	0.01	-0.07	-0.06	-0.06
	(0.07)	(0.10)	(0.10)	(0.10)	(0.09)	(0.13)	(0.12)	(0.12)
Myr10	-0.37*	0.46	0.50	0.62	-0.81	0.45	0.27	0.30
	(0.06)	(0.51)	(0.43)	(0.41)	(0.08)	(0.80)	(0.61)	(0.56)
Male	-0.03	-0.13	-0.13	-0.15	-0.09	-0.26	-0.23+	-0.24+
	(0.06)	(0.10)	(0.10)	(0.10)	(0.08)	(0.16)	(0.14)	(0.14)
Father is oldest son	0.13*	0.08	0.08	0.08	0.03	-0.05	-0.04	-0.04
	(0.07)	(0.10)	(0.10)	(0.10)	(0.09)	(0.14)	(0.13)	(0.13)
Instruments	--	Eligible (tdistr x myr10)	Eligible, eligible x male	Eligible, eligible x male, eligible x father oldest	--	Eligible (tdistr x myr10)	Eligible, eligible x male	Eligible, eligible x male, eligible x father oldest
Regr F / Wald χ^2	38.22	381.30	365.89	325.53	35.86	317.97	367.85	359.58
(Prob > χ^2)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)

Note: Standard errors clustered at the level of the household. Sample size is 2411. Additional regressors include indicator for scheduled caste or tribe, mother's height, weight and BMI, indicator for whether mother has formal schooling, father's schooling years, amount of land owned by the extended household, population of AWC and proportion of population that is from scheduled castes and tribes, distance to block office, and indicators for birth month in the Kharif and monsoon seasons.

*Significant at 5% level +Significant at 10% level

Appendix A: First Stage regressions for birth interval

Regression sample: oldest child, mother's marriage year >=2005
Dependent variable: Interval between first and second births, in months

	(1)	(2)	(3)
Eligible (tdistr x myr10)	2.86*	3.81*	2.69+
	(1.12)	(1.23)	(1.44)
Eligible x male	--	-2.04+	-2.03+
		(1.14)	(1.14)
Eligible x father oldest son	--	--	1.80
			(1.18)
Tdistr	-1.08	-1.10	-1.12
	(0.92)	(0.92)	(0.92)
Dmyr10	-9.73*	-9.72*	-9.73*
	(0.83)	(0.83)	(0.83)
Male	1.01+	1.63*	1.62*
	(0.54)	(0.66)	(0.66)
Father oldest son	0.44	0.46	-0.10
	(0.57)	(0.57)	(0.70)
Regression F	186.97	177.50	168.77
(prob > F)	(0.00)	(0.00)	(0.00)

Note: Standard errors clustered at the level of the household. Sample size is 2411. Additional regressors include indicator for scheduled caste or tribe, mother's height, weight and BMI, indicator for whether mother has formal schooling, father's schooling years, amount of land owned by the extended household, population of AWC and proportion of population that is from scheduled castes and tribes, distance to block office, and indicators for birth month in the Kharif and monsoon seasons.

*Significant at 5% level +Significant at 10% level

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