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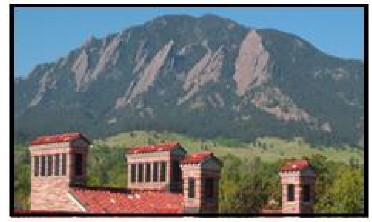

**WORKING PAPER**

# Effects of Early Childhood Health and Family Planning Interventions on Adolescent Cognitive Functioning: Evidence from Matlab in Bangladesh

Tania Barham

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# Effects of Early Childhood Health and Family Planning Interventions on Adolescent Cognitive Functioning: Evidence from Matlab in Bangladesh*

Tania Barham
Department of Economics and Institute of Behavioral Science
University of Colorado Boulder

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**Abstract:** Early childhood health and nutrition interventions have been shown to improve the health status of young children in developing countries. It is believed that improvements received early in life may lead to improved cognitive development, health, educational achievements, and labor market opportunities. Yet there is little evidence that the benefits of early childhood health interventions continue into adolescence and adulthood, especially in environments where there are many competing health risks. This paper exploits a quasi-random placement of the Matlab Maternal and Child Health and Family Planning Program in Bangladesh to determine whether typical family planning and early childhood health interventions have lasting effects on cognitive functioning in late childhood and adolescence. Intent-to-treat effects show no program effect on children aged 15-19 in 1996, who, while their parents may have benefited from family planning, were beyond the appropriate age for the child health interventions. Somewhat younger children (8-14 years old in 1996), whose mothers benefitted from family planning and who themselves were eligible to receive the child health interventions (mainly immunizations) , demonstrate a 0.38 standard deviation increase in cognitive functioning. The program most benefited boys and second or higher birth order children.

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## 1. Introduction

Improving health and nutrition of young children is important not only for the immediate benefit to their well-being, but because it is believed to lead to improved cognitive development, health, educational achievements, and labor market opportunities (Barker 1995, Currie and Thomas 1999, Thomas and Frankenberg 2002, Finch and Crimmins 2004, Painter et al. 2005, Case and Paxson 2006, Maluccio 2006, Strauss and Thomas 2008). Economic theories of human capital development and reduction of intergenerational transmission of poverty rely on this postulated link and are part of the rationale for important programs in the United States, such as Head Start, as well as in developing countries, such as Conditional Cash Transfers. Yet there is limited evidence of the longer-run effects of early childhood interventions (Maluccio et al. 2006), and it is unclear whether the benefits from such interventions continue (Pollitt et al. 1993) or fade out (Garces et al. 2002). The issue of fade-out is particularly pertinent in developing countries, where there are many competing health risks and a greater frequency of shocks coupled with limited ability to smooth consumption.

There is a growing body of literature examining the causal effect of childhood health status (often measured by height) on longer-term human capital, usually educational outcomes, in developing countries.[1] The majority of these studies use exogenous shocks (famines, weather, or changes in prices) or sibling/twin fixed-effect models to identify the effect. They largely confirm results from earlier cross-sectional studies of the importance of early childhood health (height) for schooling outcomes. There are few studies that examine what can be done to counter the negative effects of shocks or poor health and nutrition. In addition, there is a lack of research on cognitive functioning as opposed to educational outcomes. Educational outcomes, such as level of education attained or test scores, are certainly correlated with cognitive ability. However, these educational outcomes are also a function of many other factors, including income, cost of enrollment, school quality and access, policies on automatic promotion, community security, religious or cultural beliefs, labor market opportunities, and the opportunity cost of time (Connolly and Kvalsvig 1993). As a result, educational outcomes are unlikely to reflect cognitive functioning accurately and may fail to show effects for certain subpopulations such as girls.[2] Furthermore, improved cognitive functioning may very well improve adult well-being even for those who do not obtain more education, especially in countries where high levels of education are not needed for many jobs.

Thus far, research on cognitive functioning as an example of human capital formation is limited and has tended to focus on short-term impacts of nutrition interventions on young children. Past surveys lacked cognitive data, and there are few well-designed interventions or program that took place 10 or more years ago that allow rigorous analysis of the longer-term effects of early childhood interventions. While the short-term effects of health and nutrition interventions are on the whole beneficial, the longer-term effects even from randomized interventions are mixed. [3]

In this paper, I provide evidence of the effect of a quasi-randomly placed early child health and population program in Bangladesh, the Matlab Maternal and Child Health and Family

[1] See Strauss and Thomas (2008), Glewwe and Miguel (2008), and Behrman (1996) for a review of this literature.
[2] Girls may drop out of school early because of early marriage or their higher productivity in the home, rather than lower cognitive ability.
[3] See Grantham-McGregor (1997,1999a, 1999b) and Walker et al. (2007) for a review of the literature on nutrition and infectious diseases, and Pollitt et al. (1993), Mortell et al. (1995), and Stein (2005) for results from the INCAP studies in Guatemala such as Maluccio et al., 2006

Planning (MCH-FP) Program, on cognitive functioning of older children and adolescents. The Matlab MCH-FP program began in 1977 with family planning and maternal health interventions. Measles vaccinations for children under five were introduced in 1982. Other child health interventions such as vaccination against DPT[4], polio, and tuberculosis, and vitamin A supplementation followed in 1986.

The ideal way to determine the long-term effects of family planning and early child health program would be a randomized treatment and control experiment with two treatment groups: one treatment that received early childhood vaccinations and another that received family planning interventions. The treatment and control areas would need to be separated by sufficient geographical distance to minimize spillover effects from the vaccinations. However, such an experiment does not exist and may be difficult to do for the diseases examined in this paper, because today's rates of vaccination are fairly high worldwide. Yet evaluation of population and early childhood health interventions including vaccinations is extremely important, since they are perhaps the most essential and widespread health intervention in the developing world to date, and are commonly provided in combination.

The MCH-FP program, however, was not randomly placed; rather, the treatment (or MCH-FP) and comparison areas are contiguous geographic blocks. Placing the program in contiguous geographic areas, rather than randomizing at the individual or even the village level, is likely to have been important for minimizing the potentially large positive spillover effects from childhood vaccinations to the comparison area, which would have made program effects more difficult to discern. Previous research shows that the two areas had similar fertility and mortality rates before the program. I demonstrate that household characteristics were also similar before the program.

Using data collected in 1996, I separately estimate intent-to-treat effects for children who were born when the family planning program was available but before the child health interventions were introduced (those aged 15-19 in 1996), and for children who were born when both components of the program were available (those aged 8-14). Models using propensity score weighting and regression adjustment as well as mother fixed-effects are used to test the robustness of the results. The findings show that the program led to an approximate 0.38 standard deviation increase in cognitive functioning, as measured by the Mini Mental State Exam, for the 8-14 year olds but had no effect on the 15-19 year olds. These effects are greatest for boys and second or higher birth order children; the treatment-on-the-treated effects are over twice as large. While it is difficult to separate out the effects of the family planning and early child health interventions, I perform various analyses that together suggest that the effect on cognitive functioning may result primarily from the early child health interventions.

The rest of the paper proceeds as follows. Section 2 describes the MCH-FP program and the mechanisms through which this program may affect cognitive functioning; section 3 describes the various data sources and variables used in the analysis. I present the identification strategy in section 4 and results and conclusions in sections 5 and 6 respectively.

## 2. Background

### *2.1 The Matlab MCH-FP program*

In the early 1960s, ICDDR,B (formerly known as the International Center for Diarrhoeal Disease Research, Bangladesh) began the Matlab Health and Demographic Surveillance System

[4] The DPT vaccine protects against diphtheria, pertussis (whooping cough), and tetanus.

(HDSS) in Matlab, a rural region of Bangladesh. This study site covered approximately 200,000 people and provided at least monthly data on important demographic events such as births, deaths, and migration (Faveau, 1994). In 1977, ICDDR,B started the MCH-FP program in approximately half of the HDSS site (referred to as the MCH-FP area or treatment area), leaving the other half as a comparison area. The MCH-FP and comparison areas were not randomly assigned but were contiguous geographic areas (Figure 1). The goal of the program was to reduce fertility and improve children's health through home delivery of family planning and maternal and child health interventions. Most of the interventions were not available in the comparison area until after 1988, providing an experimental period between 1977 and 1988 to evaluate the success of these interventions. The Matlab Health and Socioeconomic Survey (MHSS) was implemented in1996, making available data to examine effects of the program eight years after the experimental period ended.

The family planning and health interventions were phased in over time starting in late 1977 and were delivered to the beneficiary's home by local female community health workers (CHW). Between 1977 and 1981 CHWs provided women with a variety of modern contraceptives and regularly followed up to encourage continued use. In addition, CHWs provided some simple health interventions such as administering tetanus toxoid shots to pregnant women, providing iron and folic acid tables, and giving advice on nutrition and breastfeeding (Fauveau, 1994).

Starting in 1982, ICDDR,B phased in a number of child health intervention in all or some of the MCH-FP area. Some interventions were preventive (such as vaccinations) and available to all children in the targeted area, while others (such as nutrition rehabilitation and treatment for respiratory infection or dysenteric diarrhea) were aimed only at the sick. In particular, in 1982, in half the MCH-FP area (referred to as the MCH-FP1 area), the measles vaccination was available to all children between the ages of 9 and 59 months, and eligibility for tetanus toxoid immunization was expanded to all women of reproductive age (not just pregnant women).[5] In 1985, these interventions were introduced in the other half of the MCH-FP area (referred to as the MCH-FP2 area). Later interventions were introduced in the entire MCH-FP area including, in 1986, vaccination against DPT, polio, and tuberculosis, vitamin A supplementation, and nutrition rehabilitation for children aged 5 and under and, in 1988, control for acute respiratory infections and dysenteric diarrhea (Fauveau, 1994).

While government health services, including access to contraception, were available at clinics in both the MCH-FP and the comparison areas during the experimental period (1977-1988), the comparison area did not receive the intensive care or access provided through home visits. In addition, the government clinics did not provide early childhood vaccinations and were unlikely to provide the other child health interventions (Tasim, 2008). While data are not available on vaccination rates in the comparison area before 1989, they are believed to have been near zero (Koenig et al., 1991). Nationally, measles vaccination was less than 2 percent in 1986 (Kahn & Yoder 1998), and despite concerted efforts in 1989 to cover the whole country by 1990, it remained below 40% for children under age five in the comparison area (Fauveau 1994).

Figure 2 demonstrates, for two of the main program interventions (contraception and the measles vaccine), that implementation followed the planned timeline and uptake was rapid. Trends in the contraceptive prevalence rate (CPR) for married women 15-49 rapidly increased to

[5] Limited antenatal care was also provided. The focus of the antenatal visits was to screen pregnant women to identify women with high mortality risk. The actual interventions were limited to providing nutrition and hygiene advice during pregnancy, some of which was already being provided by the program, bu no referral medical care was provided (Faveau, 1994). Safe delivery kits were provided to pregnant women starting in 1983.

30 percent in the MCH-FP area during the first year contraceptives were provided by the program. A gradual increase followed and the CPR reached almost 50 percent by 1988. In the comparison area the CPR was much lower, with rates still below 20 percent by 1988. The measles take-up data for children 12-59 months are presented separately for MCH-FP1, in which measles vaccine became available in 1982, and for MCH-FP2, in which measles became available in 1985. In both areas, the measles vaccination rate reached more than 60 percent during the first year vaccination became available.

*2.2 MCH-FP program eligibility by age and possible effects on cognitive functioning*

Table 1 summarizes program eligibility by year of birth and age in the 1996 MHSS survey. Among those born before the 1977 program initiation, women of childbearing age (14-40) were eligible for the family planning and some maternal health interventions. By 1996, these women were aged 25-59. It is doubtful that the cognitive functioning of the 25+ age group would have been affected by the program: they were not eligible for the child health interventions, and, since cognitive development is largely completed before childbearing age, their cognition is not likely to have been affected by the maternal health and family planning interventions.

Many 20-24 year olds in 1996 were of childbearing age during the experimental period (between ages 12 and 16 in 1988) but were unlikely to use family planning because they were either not yet married or not postponing a first birth. The cognitive development of this group may have been affected indirectly by competition from younger siblings born during the MCH-FP program if parents changed their child investment patterns as a response to the family planning or child health interventions.

Children born after 1977, when the MCH-FP program was available, are split into two groups that experienced “non-intensive” and “intensive” interventions. The mothers of children in both these groups were eligible for family planning and maternal health interventions. Children in the “non-intensive” group were aged 15-19 in the 1996 MHSS survey. Of this group, only children in the MCH-FP1 area were eligible for the measles vaccine, though past the recommended age of one. None of the children in this group were eligible for the other vaccines or health interventions. Cognitive development of children in this group could, however, have been affected by the effects of the family planning program on resources and by sibling competition from their younger siblings who were eligible for more intensive child health interventions.[6]

The children in the “intensive” child health group are aged 8-14. Those in the MCH-FP1 area were eligible for the measles vaccine by the recommended age of one, while those aged 12-14 in the MCH-FP2 area were eligible for the measles vaccine but past the recommended age. All MCH-FP children in this age group were eligible for the other vaccinations and child health interventions; however, only those aged 11 or younger were eligible to receive vaccinations at or close to the recommended age. The mechanisms by which the cognitive development of these children may be affected are described in the next section.

Finally, children under the age of 8 in 1996 were born after the experimental period, when some of the same child health interventions were available in health clinics in the comparison area. There may still be a program effect for this group, since in the MCH-FP area vaccinations were delivered to the home rather than through a clinic.

[6] Some in this group may have also benefitted from late measles vaccination.

*2.3. Mechanisms through which the MCH-FP program may affect cognitive development*

Reduction in the incidence of measles and pertussis due to the vaccinations can have a direct effect on cognitive functioning because encephalitis, a complication of both these diseases, results in long-term brain damage (Greenberg et al. 2005, Reingold and Phares 2006). Vaccine-preventable diseases can also indirectly affect children's cognitive development because the morbidity caused by these diseases may lead to undernutrition and decreased physical activity and play. These effects are likely to be much larger in developing than developed countries, partly because lower levels of nutrition before infection may weaken the immune system. Infections impair the child's nutritional status through reduced appetite and food intake, malabsorption of nutrients, increased demands from the body due to fever and immune response, and in some cultures food deprivation resulting from parental beliefs about caring for the sick (Reddy 1987, Grantham-McGregor 1999a, b). Measles, in particular, is known to severely impair the child's nutritional status through secondary complications such as pneumonia and diarrhea, and prolonged illness (Reddy 1987). While children's growth may catch up once the illness has passed, in high-disease environments children may experience a number of episodes of illness or diarrhea in combination or in close succession and reducing the time for catch-up growth. Indeed, measles can leave a child weakened and at increased risk of illness for a year, and pertussis for months (Greenberg et al. 2005). Nonrandomized and randomized studies show that in developing countries undernutrition, especially before the age of 3, can have long-lasting effects on cognitive functioning (Grantham-McGregor et al. 1999a and b, Mortell et al. 2005, Walker et al. 2007). In addition, infections and undernutrition cause general malaise and apathy, resulting in lower levels of play, and apathetic children generally receive less stimulation from adults. Lack of stimulation and learning opportunities have also been shown to have long-term effects on cognitive development in developing countries (Walker et al. 2007).

It seems unlikely that children's cognitive functioning will increase as a result of their mothers' receiving the tetanus toxoid vaccination. This is because, as mentioned earlier, tetanus in the neonatal period tends to lead to death rather than increased morbidity.

Each element of the MCH-FP program may also have an indirect effect on cognitive development through changes in parental investment in children (in the form of quality time or resources spent on education or health care), perhaps because of sibling competition. Healthier children may receive greater parental investment because of the increase in their potential future returns. An increase in investment in a child who received the interventions may come from an increase in total household resources as a result of the program (i.e., time and resources gained from having fewer children or not having to care for sick children), or from a reduction in investment in the siblings who did not receive the interventions. In addition, the family planning program could drive a quality-quantity tradeoff (Becker 1960, Becker and Lewis 1973, Becker and Thomas 1976), with low-fertility parents bringing greater resources to bear on their children. Lastly, both family planning and maternal health interventions could improve maternal survival, so children might get more nurturing from mothers as well as a more favorable allocation of resources.

## 3. Data

*3.1 Data sources*

This paper draws on the rich data available for the Matlab area. The 1996 Matlab Health and Socioeconomic Survey provides data on cognitive functioning and other demographic and socioeconomic variables. These data are publicly available from the Rand and websites (http://rand.org/labor/FLS/MHSS/. Preprogram data on MCH-FP and comparison area households are available from the 1974 census taken by ICDDR,B. Finally, the ICDDR,B Record Keeping System (RKS) contains data on receipt of program interventions in the MCH-FP area. It includes information on the date and type of each childhood vaccine received, date tetanus toxoid was received, and types of family planning methods used. A great advantage of these datasets is that they can be linked together at the individual and household level.

The 1996 Matlab Health and Socioeconomic Survey (MHSS) is a comprehensive household survey covering a wide array of topics typical of large household surveys in developing countries. Unlike most household surveys taken before 2000, it includes a measure of cognitive functioning, the Mini Mental State Exam (MMSE). The MHSS was carried out on a random sample of approximately one-third (2,687) of the baris (residential compounds, which include a number of households who live together) in the MCH-FP and comparison areas. Within each bari, a primary household was selected at random.[7]

The MMSE should have been collected for all household members age six and older in the MHSS. By mistake, it was collected from children aged 6 to14 in only the last quarter of households surveyed. In order to obtain a random sample of all children, a 10 percent random sample of the unsurveyed baris was selected and the MMSE was administered to the appropriate respondents in those baris. I restrict the sample in this paper to those who have non-missing information on the MMSE and were younger than age 59. People older than 59 are excluded to avoid issues of dementia and because 91 percent have no mother information available, so that few observations are available for analyses that include mother fixed-effects. After these exclusions, the sample consists of 5,801 6-59 year olds, of whom approximately 45 percent (2,636) are from the MCH-FP area.

The MHSS was designed to be linked to ICDDR,B periodic censuses of the Matlab area. A census taken in 1974 provides information for the period before the interventions. It includes information on household location, composition, assets, employment, and education, and offers the opportunity to test for preprogram similarity between the MCH-FP and comparison areas.

*3.2 Dependent variable: The Mini Mental State Exam (MMSE)*

The MMSE examines five areas of cognitive functioning: orientation, attention-concentration, registration, recall, and language. It was originally developed to assess the cognitive status of geriatric patients, but adaptations of the MMSE are effective at evaluating the cognitive development of children as young as 3 years (Jain and Passi 2005, Ouvrier et al. 1993). The MMSE used in the MHSS was adapted so that it would not depend on literacy and would be culturally and age appropriate. Kabir and Herliz (2000) designed a very similar MMSE version, the Bangla Adaptation of the Mini-mental State Examination (BAMSE), which was also adapted for an illiterate population and cultural relevance to Bangladesh. They implemented both the BAMSE and the original MMSE in a literate population in Bangladesh and found that there was a high correlation between the two, indicating that the changes made to adapt the instrument for an illiterate population do not change the ranking of scores.

The MMSE in the MHSS asks 33 questions and gives one point for each correct response, for a maximum score of 33. As an example, the recall section asks the respondent to remember three

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[7] The survey also collected information on a second household in each bari that was selected purposively. In order for the sample to be representative of the study area, data on the second households are not used in the analysis. The results are, however, similar when the second households are included.

words (orange, house, cat) and then asks him/her to repeat those words a few minutes later. One point is given for each word remembered. In order to enhance comparison to other studies, the test score for each observation is normalized into a z-score by subtracting the comparison area mean and dividing by the comparison area standard deviation.[8]

There are a total of 5801 observations in the sample. All "don't knows" are recoded as zero, since the respondent did not correctly answer the question, but "missing" responses have not been adjusted. It is unclear how to handle the missing observations. If the respondent refused to answer a question, it is likely because the respondent did not know the answer, in which case the code should be zero. However, a response could be missing because an enumerator accidentally skipped the question. Each of the five cognitive areas of the MMSE had fewer than 120 missing observations with the exception of recall, which had over 650. This much larger number likely reflects not the respondents' answers but rather a mistake in data entry or coding, or a lack of clarity in the survey instrument. In addition, the majority of the missing observations for recall have no code at all in the public data (not even a "missing" code). Since it is unclear how to recode missing information in the recall section, the recall question is excluded, so in these analyses 30 is the maximum MMSE score.

The MMSE score is known to vary with age (Holzer et al. 1984). This issue is particularly salient for these analyses because of the wide age range being examined (6 to 59). The mean MMSE for each age group of interest, provided in Figure 3, demonstrates substantial variation by age. Age fixed-effects are included in the regression analysis in order to control for the association between age and the MMSE score.

### *3.3 Intent-to-treat indicator*

An individual is eligible for the MCH-FP interventions if the household resides in a MCH-FP village and the individual meets the age requirements outlined in Section 2.2. A variable indicating eligibility based on 1996 MHSS village location might be endogenous, since households could have moved to the MCH-FP area to benefit from the MCH-FP program. To avoid this potential endogeneity, I use 1974 location information to create the intent-to-treat variable*, Eligible*. This variable takes on the value 1 if the individual (or household if the individual could not be matched to the 1974 census data) resided in a MCH-FP area in 1974, and zero otherwise.[9]

### *3.4 Summary statistics*

Figure 3 displays the mean MMSE score by age group for the MCH-FP and comparison areas. The same data, but including the standard deviations and the t-statistic on the difference in means, is shown in Table 2, Panel A. The figure highlights that the mean MMSE score is higher in the MCH-FP area than in the comparison area for children who were eligible for the child health interventions (i.e., those under age 15). While the difference in means between the MCH-FP and comparison areas is significant for the 8-14 year olds, it is not for the 6-7 year olds (perhaps because of the small sample size). The mean MMSE score is slightly lower for the 15-19 and 20-24 year old groups in the MCH-FP area than in the comparison area, though the differences are not statistically different. For the 25 and older (25+) age group, the mean MMSE score is the same in the MCH-FP and comparison areas.

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[8] This is a common practice in the literature on the economics of education.

[9] 1974 individual or household information was not available for 400 observations. For these observations 1982 location information was used. The results do not change if these 400 observations are excluded.

Panel A of Table 2 also shows that in 1996 the average age in the sample is approximately 32 in both areas, but that the MCH-FP area has a higher percent of females, people from the Hindu religion, and educated people.[10] Mothers in the MCH-FP and comparison areas have similarly low levels of education, approximately one year. Mother's education is not available for the period before 1977, but many mothers would have finished their education before the program began. It is possible that the slightly higher level of education in the MCH-FP area reflects access to the family planning program and the ability, especially of younger mothers, to delay their first birth. While mother's literacy is included as a control in some of the regression analyses, because of this potential endogeneity it is not included in the preferred models.

## 4. Identification Strategy and Empirical Specifications

I seek to determine the intent-to-treat (ITT) or overall program effects of the MCH-FP program on cognitive functioning. I take advantage of the variation in the program implementation across location (MCH-FP versus comparison areas) and by age group within the MCH-FP area. In particular, the analysis divides the 6-59 year old sample into the five age groups presented in Table 1. I first use 1974 census data to show that the MCH-FP and comparison areas are similar with respect to many observable characteristics and may indeed mimic a randomized intervention. I then present a number of different models to estimate the ITT effect and finally present a treatment-on-the-treated (TOT) model.

It is important to note that since the family planning and child health interventions were not randomly introduced in a factorial design, it is difficult to determine their effects separately. The total effect is itself of great interest, since these programs are commonly combined in developing countries. Nevertheless, I provide some evidence on the possible separate effects of the child health interventions. First, I separately estimate the effect of the program for children whose mothers were eligible for the family planning program but who received little if any child health intervention (15-19 year olds), and for children whose mothers were eligible for the family planning program and who were also eligible for the child health interventions (8-14 year olds). However, this is insufficient since the family planning program can be expected to have a greater effect on later-born children (many younger than 15) since parental resource constraints may be tighter. Therefore, I include a control for the family planning program in the ITT and TOT models. Lastly, I use the phasing-in of the measles vaccine in the MCH-FP area for the 12 to 14 year olds to estimate the effect of being eligible for the measles vaccine.

### *4.1 Quasi-random program design*

A comparison group was built into the design of the MCH-FP program; however, randomization was not used to determine which households or villages belonged to the MCH-FP and comparison areas. Instead, the MCH-FP and comparison area are contiguous geographic areas that were thought to be very similar (Figure 2). It is possible that this choice was deliberately made in order to mitigate potential spill-over effects to the comparison area from the positive externalities created by the vaccines. Research shows that the MCH-FP and comparison areas are indeed similar with respect to a number of pre-intervention variables including rates of mortality and fertility (Koenig et al., 1990; Menken & Phillips 1990; Joshi & Schultz, 2007). This shows that the MCH-FP program was probably not placed first in areas that had poor child health or high fertility–potential targeting criteria for such programs.

[10] The higher levels of education are likely driven by the effect of the program (Joshi and Shultz, 2008).

I would like to be able to show that the level of cognitive development was similar between MCH-FP and comparison areas before the interventions. Since there are no pre-intervention data on cognitive functioning, I instead examine differences in the MMSE score for those aged 25 and older, whose cognitive functioning is unlikely to have been affected by the MCH-FP program or by any potential sibling competition resulting from the program. As discussed above, the mean MMSE scores for this group are exactly the same, at approximately 24 out of 30.

I further test whether the areas are similar by using a wider array of household and household head characteristics from the 1974 census. Table 2, Panels B and C display the means and standard deviations of each characteristic for the MCH-FP and comparison areas, the difference in the means between the two areas, the t-statistic for the difference in means (treatment minus control mean), and the normalized difference in means (the difference in means divided by the standard deviation of the difference). The differences in means are statistically insignificant at the five percent level for all variables except drinking water sources and household head's religion. I also examine whether the normalized differences in means are less than 0.25 standard deviations apart (Imbens and Wooldrige, 2008), and find the same results. These findings, together with previous results on fertility and mortality, strongly suggest that the two areas are very similar at least in observable characteristics.

Before the program, the MCH-FP area had a larger Hindu population than the comparison area and a 14 percent greater proportion of households used tubewell water for drinking. Shallow tubewells were introduced by donors and the government starting in the 1970s and served 95 percent of the Bangladeshi population by the mid-1990s. Tubewell water is often thought to be cleaner than other sources of water. If this is the case, the MCH-FP population could have been healthier before the program, biasing the program effect upwards. However, in Bangladesh there is widespread groundwater arsenic contamination in the tubewells (Smith et al. 2000), a serious health concern (Chowdhury et al. 2000; MM Rahman et al. 2001; Alam et al. 2002) that has been found to lead to reduced IQ of school-aged Bangladeshi children (Wasserman et al. 2006). So greater access to tubewell water in the MCH-FP area might, alternatively, have biased the estimate of program impacts downwards. I use propensity score weighting with regression adjustment and interaction of the MCH-FP effect with the source of drinking water to help determine whether such a bias exists.

*4.2 Intent-to-treat single and double-difference models.*

The first model to be presented is a single-difference (SD) model estimated using ordinary least squares (OLS). This model assumes that the MCH-FP and the comparison group would have had the same mean MMSE in the absence of the MCH-FP program. This is not a testable assumption, but it seems likely given that the mean MMSEs for those who likely did not benefit from the program, the 25+ age group, are similar. The estimation equation is

$$\begin{aligned}(1)\quad C_{imv} &= \beta_1(E_{v74} * AG_{imv}^{6-7}) + \beta_2(E_{v74} * AG_{imv}^{8-14}) + \beta_3(E_{v74} * AG_{imv}^{15-19}) + \beta_4(E_{v74} * AG_{imv}^{20\text{-}24}) \\ &\quad + \beta_5(E_{v74} * AG_{imv}^{25+}) + \alpha_a + X'Z + v_{imv},\end{aligned}$$

where $C$ is the standardized MMSE score (or MMSE z-score) for person $i$ of mother $m$ in village $v$. $E_{v74}$ (referred to as *Eligible* in the tables) is a binary variable that takes on the value 1 if person $i$ or *i's* household was from a MCH-FP village (or eligible for the program) before the interventions, and 0 if from a comparison village. $AG^{Y}$ is a binary variable used to indicate whether person $i$ is or is not in age group Y. Thus, the $\beta$'s are the single-difference ITT effects for the various age groups of interest. $\alpha_a$ are age fixed-effects to control for differences in the MMSE score due to age as well as other events that may be correlated with age and common to the study population. $X$ is a vector of individual (gender and religion) and baseline household and household head characteristics, presented in Table 2, and controls for preprogram differences in characteristics.

The error term $\nu_{imv} = \varepsilon_i + \varepsilon_m + \varepsilon_v + \varepsilon_I$ is a composite of four terms: $\varepsilon_i$, which represents individual effects such as genetics; $\varepsilon_m$, which captures time-invariant mother characteristics and household environment (for example, whether the child had stimulating parents who might have enhanced the child's cognition); $\varepsilon_v$, which captures village effects; and $\varepsilon_{I,}$ for white noise. This model assumes that various individual, mother or household, and village unobservables are not correlated with $E_{v74}$ because of the quasi-random placement of the program. Standard errors are clustered at the 1996 village level to account for the likely intracluster correlation in the error term.[11]

Even when interventions are successfully randomized, there is a chance that the particular outcome of interest will vary slightly before the intervention between the treatment and comparison areas. In this case a double-difference estimator may be more appropriate because it takes into account these preprogram differences. In addition, this estimator will control for non-time-varying unobservables that may differ between the treatment and comparison areas, such as level of arsenic in the wells, and could be correlated with the eligibility variable, $E_{v74}$. Given the long time span between the pre- and postintervention time periods and the fact that cognitive functioning data is available only for the postintervention period, it is not possible to examine the before-after difference in cognitive functioning for any one individual. Instead, the age 25 and older group (25+) represents the pre-intervention difference between MCH-FP and comparison areas. The double-difference model is estimated using the following linear regression:

$$(2)\quad \begin{aligned} C_{imv} &= \beta_1 E_{v74} + \beta_2 (E_{v74} * AG_{imv}^{6-7}) + \beta_3 (E_{v74} * AG_{imv}^{8-14}) + \beta_4 (E_{v74} * AG_{imv}^{15-19}) + \beta_5 (E_{v74} * AG_{imv}^{20\text{-}24}) \\ &\quad + \alpha_a + X'Z_v + \nu_{imv}, \end{aligned}$$

where the variables are defined as in Equation (1) but the interpretation differs because the 25+ age group is not interacted with the eligibility variable. As a result, $\beta_1$ *re*presents the difference in means between the MCH-FP and comparison area for the 25+ age group, and the other *β's* the double-difference estimators for the various age groups of interest.

Finally, mother's education and a binary variable indicating whether person *i*'s mother was ever eligible for the family planning portion of the MCH-FP program, are also included in some of the models to partially control for the family planning program and mother's characteristics. In addition, village of residence fixed-effects, $\lambda$, which controls for non-time-varying village characteristics, are is also included in some models as a robustness check.[12]

### *4.3 Maternal fixed-effect model*

The single and double-difference models will be biased if unobservable mother or household characteristics are not similar between the MCH-FP and comparison areas. Mother fixed-effects (MFE) are added to the double-difference model to partial out the $\varepsilon_m$ component of the error term in equation (2). Because of the mother fixed-effect, the coefficient $\beta_1$ cannot be identified in equation 2; however, the age group left out is still the 25+ group, and the basic interpretation of

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[11] Results clustering the standard errors at the treatment group-birth year level had slightly more significant.

[12] Village fixed-effects are included as a robustness check only because they are identified from few observations in models where the sample size is lower.

the coefficients does not change. The benefit of the MFE model is that it controls for non-time-varying unobservable household or mother characteristics that may be correlated with the outcome variable as well as with determinants of attrition. In addition, it will control for nonrandom program placement. A disadvantage is that the sample size is substantially smaller. This is a result of the difficulty in determining the mother identification code if the mother is dead at the time of the survey, and not being able to identify siblings in the dataset if they live in different baris.

*4.4 Double-difference model with propensity score weighting and regression adjustment*

Finally, if the treatment effect is not homogenous our estimates may suffer from two additional sources of bias. The first arises because there may be no comparison observations with comparable characteristics to the treated observations, or the two groups may not have a common support with respect to baseline characteristics.[13] The second bias may arise if the distribution of the vector of covariates differs between the MCH-FP and comparison areas within the common support.[14]

To address the first potential bias I estimate a propensity score (Rosenbaum and Rubin 1983) and, following Crump et al. (2008), keep only those observations for which the propensity score is greater than .1 or less than .9. To estimate the propensity score I use a logistic model and pre-intervention census data presented in Table 2, which included many variables that may be correlated with cognitive development such as measures of household wealth, occupation (as a proxy for income), and sources of drinking water (to proxy for cleanliness of drinking water), as well as the person's age, sex, and religion. Higher powers of these variables and interaction terms were included until the propensity score and the variables that compose it were balanced between MCH-FP and comparison areas in 10 blocks created using the propensity score.[15] Figure 4 presents the distributions of propensity score for the MCH-FP and comparison areas separately. It shows that the propensity scores for the MCH-FP and comparison area almost completely overlap, but that the scores are distributed slightly differently between the two areas, so the second bias may be the most relevant.

A number of different methods are used in the literature to control for the second bias.[16] In order to present results in a format similar to that of the previous models I use a combination of propensity score weighting and regression adjustment. Robins and Rotnitzky (1995) demonstrate that when these two techniques are combined, the estimator is consistent if either the parametric model for the propensity score or the regression functions are misspecified. The weights are the inverse probability of treatment or nontreatment, where the probability of treatment for those in the treatment group is the estimated propensity score and for the comparison group is one minus the estimated propensity score. The regression adjustment involves including the set of pre-intervention variables, but adjusting them so they are deviations from the mean for each observation. Lastly, I bootstrap the standard errors (Imbens and Wooldridge (2008).

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[13] If the distributions do not overlap, the assumption of unconfoundedness may not hold.

[14] Heckman et al. (1998) suggest that, in practice, the first of these two sources of bias is likely to be the most severe.

[15] There were no significant differences in the propensity score between the treatment and comparison areas at the 5 percent significance level in any of the 10 blocks. The balancing of the covariates is also very good, with approximately 3 percent of the covariates in Table 2, Panels B and C being significantly different at the 5 percent level in the 10 blocks.

[16] See Imbens and Wooldridge 2008 for a review of these methods.

It is important to remember that these models cannot ensure that the unobservable characteristics are similar. As placement in the program was quasi-random, and there are few pre-intervention differences in the observable characteristics, this analysis is used as a robustness check.

*4.5 Treatment-on-the-treated analysis*

It is important to determine the impact on those who participated in the MCH-FP program, rather than those who were eligible. Since program take-up is likely to suffer from self-selection bias, I use an instrumental variables approach in which program take-up (or receipt) is instrumented by program eligibility. The main preventative interventions provided by the program to children were vaccinations. The indicator of receipt of child health interventions is whether the child received all the program vaccinations.[17] The program also provided family planning interventions that could affect family size. To control for the family planning program, I also include in the analyses the number of siblings born alive. Both receipt of vaccinations and number of siblings are endogenous variables. Two-stage least square (2SLS) is used to estimate the model using two instrumental variables. "MCH-FP eligible village" interacted with a binary variable indicating the age group that was eligible for child health interventions during the experimental period ($E_{v74}$*age 8-14) and with a binary variable indicating whether the child's mother was ever eligible for the MCH-FP program in any of her potentially fertile years (age 11 to 49).[18]

## 5. Results

*5.1 Intent-to-treat program effects on the MMSE Z-score*

Columns 1 and 2 of Table 3 present results for the single-difference model.[19] Results in column 1 show a statistically significant 0.35 standard deviation (SD) increase in the MMSE score for the 8-14 year old age group due to the MCH-FP program. The effect size is similar to those in studies of the benefit of good nutrition, in particular iron, on cognitive-language abilities (Walker et al. 2007). The point estimates are small, negative, and insignificant for the 15-19 and 20-24 age groups. Importantly, the point estimate is almost zero (0.02) and insignificant for the 25+ age group, the group that represents pre-intervention differences in cognitive functioning between the MCH-FP and comparison areas. The results remain unchanged with the inclusion of preprogram characteristics in column 2, providing some confidence that the differences at baseline in some of the household characteristics (such as access to tubewell water) are not biasing the results.

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[17] Only children born in 1982 or later (or age 14 or less in the survey) were eligible to receive all the vaccinations. The majority of these children should have received their vaccinations at the recommended age. I do not examine the effect for children who received any vaccinations because children born between 1977 and 1981 were eligible for only the measles vaccination, and that after the recommended age of receipt. Given that these children, if they received the vaccine, received it late, they are not a good measure of the impact of the interventions on cognitive functioning.

[18] Sex composition of siblings is sometimes used to instrument for family size. Sex composition can have lifetime wealth effects, for example due to dowry, or income effects and thus may not necessarily predict how population policies would affect behavior (Shultz, 2008). Results are similar if such instruments are used, but those results are not presented here.

[19] Standard errors are clustered at the 1996 village level. Clustering at the treatment group-birth year level was also examined and provides a greater level of significance, so the more conservative of the two clustering methods is reported.

Results for the double-difference models are provided in columns 3 through 6. Column 3 mimics the regression in column 2 except that a double-difference instead of a single-difference model is estimated. The point estimates remain almost unchanged for all the age groups and show a statistically significant 0.38 SD increase in the MMSE score for the 8-14 year old age group. These are the preferred estimates, but a number of robustness analyses are included.

Two mother-level controls are added in column 4, one for a person's mother being eligible for the family planning program at any point in her fertile years and another to indicate whether the mother is literate (i.e., has more than 3 years of education). The inclusion of these controls again leaves the results unchanged.[20] The sample size is slightly reduced when the mother-level controls are included, since mother information is not always available for people whose mother has died or who does not live with them.[21] Village fixed-effects for 1996 residence are included in column 5, and the results remain unchanged, showing that time-invariant unobservable village effects do not appear to be biasing the results.[22]

While the variable for level of education is endogenous, since the MCH-FP program affected education, education level fixed-effects are included in column 6 to examine whether increased education is a mechanism through which cognitive functioning increased. The point estimates are stable, providing some evidence that the MCH-FP program effect on cognitive functioning for the 8-14 years olds is not a result of increased levels of education. An examination of the point estimates on the education level fixed-effects (estimates not reported) shows that the program effect of 0.38 is equivalent to the effect size for completing the first 3 years of primary school.

Introducing mother fixed-effects (MFE) reduces the sample substantially, to 950 observations.[23] In order to separate out the effect of the change in sample size from the addition of the MFE, I present the double-difference estimates using the MFE sample but without the MFE in column 7 and those with the MFE in column 8. The effect of the change in sample size is determined by comparing columns 3 and 7. For the 8-14 year olds, the point estimate increases from 0.38 for the full sample to 0.55 for the smaller sample. Adding the MFE leads to a 0.1 SD increase in the point estimate for the 8-14 year olds to 0.65, but is only marginally significant at the 10 percent level. The program effect remains statistically insignificant for all the other age groups. This result suggests that not including maternal fixed-effects may bias the results for the 8-14 year olds downwards. One reason the effect for the 8-14 year olds is not more significant is that there are relatively few observations (82) in the 25+ group in the MCH-FP area, so the eligibility-age interaction term lacks power.

Estimates using propensity score weighting with regression adjustment and only observations on the common support based on the propensity score are presented in Table 3, column 9. There is a slight increase in the effect for the 8-14 year olds, to 0.44 SD, but this higher effect is not statistically different from estimates from the single or double-difference models. Again, none of the effects are statistically significant for any of the other age groups. It is not surprising that the

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[20] The results are also unchanged if these mother-level variables are added separately instead of together.

[21] Results are the same if a control for the percent of fertile years a child's mother was eligible for the MCH-FP program is included as the family planning control. This control variable is not presented because it is highly correlated with mother's age at birth when there are age fixed-effects in the model. Mother's age at birth is likely to be endogenous because of the family planning program.

[22] Results are the same if 1974 village of residence (or household residence) is included instead of 1996.

[23] This sample excludes all observations for which the mother's ID is missing and for which there was no identifiable sibling in the data. The later restriction is used to be clear on the number of observations that are being used to identify the impact.

coefficients remain essentially unchanged, because the MCH-FP and comparison groups are very similar in pre-intervention characteristics.

*5.2 Results exploiting the phasing- in of child health interventions in the MCH-FP area*

The measles vaccine was phased in over time. Vaccination was available to children in MCH-FP1 starting in 1982 and in MCH-FP2 in 1985; thus this phasing-in affected children aged 12-14 in1996. It is possible to use the phasing-in of the measles vaccine in the MCH-FP area to provide some evidence on the separate effect of this intervention.

Before I exploit the phasing-in over time, I first examine whether the results differ between the 8 -11 and the 12 -14 year olds in the MCH-FP group as a whole, since some of the children in the older age group received the measles vaccination after the appropriate age. Double-difference models mimicking those in column 3 of Table 3 show that the program effect is lower and not significant for the whole MCH-FP 12-14 year group compared to the 8-11 year olds (Table 4, Panel A, column 1). Predictably, the program effect is significant for 12 -14 year olds in the MCH-FP1 area, and lower and not significant for 12-14 year olds in the MCH-FP2 area.

A double-difference model OLS compares the effects by age group between the MCH-FP1 and MCH-FP2 areas (Panel B, Table 4). The difference in the MMSE score is small and not significantly different for the 25+ group (*MCH-FP area*), showing that there were no pre-intervention differences in MMSE between these two areas. There is also no difference for the 15-19 year old group, born when these areas received the same interventions. However, for the 12-14 year old group there is an advantage of 0.28 SD for the MCH-FP1 area. Since these children were all eligible for the other child health vaccinations at the same time, and their mothers were all eligible for the family planning program, this program effect can be interpreted as the intent-to-treat effect for measles. However, this effect is not statistically significant (perhaps because there are only 96 individuals in this age group in the two areas). Nevertheless, the point estimate suggests that the MCH-FP program effect on the 8-14 year olds is in large part due to the measles vaccine.

There continues to be a larger program effect in the MCH-FP1 area for the children aged 8-11, even though for this age group the two areas received the same interventions. It is unclear why this is the case, but it could be that program implementation was better in the MCH-FP1 area, where there was longer experience at providing the interventions in the home. The point estimate on the 6-7 year olds is very large and needs to be interpreted with caution, as there are only 23 observations in the 6-7 year old group in the MCH-FP1 area.

*5.3 Results by sub-component of MMSE*

Table 5 presents the results by the main subcomponents of the MMSE (orientation, attention-concentration, registration, and language) for the 8-14 year olds. The effects of the program by subcomponent are significant at the 5 percent level for attention-concentration (0.28) and registration (0.37), and at the 10 percent level for orientation (0.28). There are no significant impacts for language, likely because the questions in this section were too easy for this age group. The average score on the language section for 8-14 year olds is six out of seven and the standard deviation is one.

*5.4 Heterogeneity of the intent-to-treat effects*

5.4.2 Drinking water and religion effects

I examine the heterogeneity of the intent-to-treat effects with respect to the child's religion and the use of tubewell water for drinking in the pre-intervention period to determine whether the poor balance in these variables between MCH-FP and comparison areas could be biasing the results. Interaction effects of the double-difference estimator for children age 8-14 and 15-19 with a binary variable indicating whether the household used tubewell water for drinking in 1974 (*tubewell drinking water*) or whether a child is *Hindu* are presented in Table 6 Panel A. The interaction effects for each variable are estimated in separate regressions, and results are reported in columns 1 and 2. These interaction effects are also included in one regression (column 4) to account for any possible correlations between the interaction variables. Though the point estimates are not reported, the models include all interactions of these binary variables with the age group dummies and the eligibility variable (for example, *Eligible*Hindu* and *Age20-24*Hindu*). The results highlight that, if anything, cognitive functioning is lower in households that had access to tubewell water before the program. Since a higher percent of families used tubewell water for drinking in the MCH-FP area before the program, if anything this imbalance is likely to bias downward the estimates of the program effect. The downward bias is consistent with the presence of arsenic in some tubewell water in Bangladesh. There is no difference in the effect of the program between Muslims or Hindus.

5.4.3 Birth order and gender effects

Patterns of mortality risk, which in part reflect differential investments (resources and time) in children, vary by family composition in Bangladesh. In particular, first-born children, girls with no sisters, and boys with at most one brother tend to have lower mortality risk (Muhuri and Menken 1997). There are approximately 5 percent more first-born children in the MCH-FP area than in the comparison area for those aged 8-14, most likely because of the family planning program. If the patterns seen in mortality risk also hold for cognitive functioning and are not changed by the introduction of the program, it is possible that the positive program impact on cognitive development is in part a result of there being a higher percent of first-born children in the MCH-FP area. To explore this possibility and the effects by gender and birth order, the ITT effects for the age 8-14 and 15-19 year olds are made to interact with binary variables indicating whether the child is male (*Male*) and whether the child's birth order is second or higher[24] (*Birth Order 2+*), and with the interaction between these two binary variables (all other interactions of these variables with age group and eligibility dummies are included in the regressions). Birth order information is missing for most of the sample older than 22, making double-difference models that rely on a good estimate of the program effect for the 25+ age group unreliable. Therefore a single-difference estimator is used for analyses involving birth order, and the sample is restricted to those who are 24 and younger and are not missing birth order data. Column 1 in Panel B of Table 6 shows that the single-difference estimates are similar using the smaller sample, though the effect for the 15-19 year olds is more negative and significant than with the full sample.

As is true for mortality risk, females have not benefited cognitively from the program as much as males. Column 3 in Panel A of Table 6 shows that the program effect for girls aged 8-14 is a statistically insignificant 0.19 SD increase in the MMSE score, but the effect for boys is 0.38 SD higher (though the difference is only marginally significant at the 10 percent level). Column

[24] Unfortunately it is not possible to examine the specific effects of second, third, or higher order births separately because there are not enough observations to obtain a reliable estimate of the program effect (e.g., there are only 25 girls in the MCH-FP are who are second birth order 8-14 year olds).

2 in Panel B replicates the gender analyses using the smaller birth order sample, and the results are similar.

Birth order interactions are presented in column 3 of Panel B. Again, the estimates for the 15-19 year old group follow the pattern for mortality risk. The MMSE score is a statistically significant 0.29 SD lower for second or higher birth order children than for firstborn children. These results may reflect that the program interventions did not change the investment pattern for this age group. In contrast, among the 8-14 year olds the program did not have an effect on firstborn children but led to a 0.49 SD increase in cognitive functioning for second and higher birth order children, indicating that the program benefited those who otherwise would have been likely to receive fewer investments.[25]

Finally, the interactions between birth order and gender are examined in column 4. The interactions between female and birth order are not statistically significant, indicating that second or higher order born boys and girls in the age 8-14 year old group benefitted similarly, and more than firstborn children.

*5.5 Treatment on the treated effects*

The TOT results are presented in Table 7. To determine whether the MCH-FP child health interventions were successful, a binary variable indicating whether a child received all the program vaccinations (*All Vaccinations*) is instrumented by a variable indicating whether the child was eligible to receive the vaccinations (*Eligible*Age 8-14* ). The first stage regression (column 1) shows that the instrument (*Eligible*Age 8-14*) is highly correlated with a child's receiving all the MCH-FP program vaccinations and that the analysis does not suffer from weak instruments (the f-statistic on the excluded instrument is 114). At 0.92 SD (column 2), the effect of the program on the treated is more than double the intent-to-treat effects, and demonstrates that the provision of vaccinations has a large effect on cognitive functioning.

In order to control for the possible separate effect of the family planning interventions on cognitive functioning, a control for the number of siblings (born alive) is included. Because the number of siblings is endogenous, it is instrumented with a variable indicating whether the child's mother was ever eligible for the family planning program in any of her fertile years. The sample size is reduced since the number of siblings is missing for the majority of observations on those over age 22. The first stage regressions (columns 4 and 5) show that the instruments are strongly correlated with the endogenous variables and are not weak (the f-statistics are 16 and 79). The inclusion of the number of siblings does not substantially change the results, though the point estimate of the impact of vaccinations increases to 1.03 SD. The number of siblings does not have an effect on a child's cognitive functioning.

*5.6 Spillover effects*

Spillover effects occur when the program indirectly affects nonparticipants and will bias the ITT effects. Spillover effects could affect the untreated through the positive externalities of some of the interventions, such as vaccinations, or through informational spillovers. In the comparison area, spillovers are likely to occur in those areas that border or are closer to the MCH-FP villages, since knowledge about the programs is likely to be spread by word-of-mouth in the local area.

We explore the possibility of spillovers to comparison area villages that border a MCH-FP village using the following linear regression:

[25] The effect for the firstborn children aged 8-14 is identified from 58 observations.

$$C_{imv} = \beta_1 NE_{v74} + \beta_2 (NE_{v74} * AG_{imv}^{6-7}) + \beta_3 (NE_{v74} * AG_{imv}^{8-14}) + \beta_4 (NE_{v74} * AG_{imv}^{15-19}) + \beta_5 (NE_{v74} * AG_{imv}^{20\text{-}24}) + \beta_6 \mathrm{B}_{v74} + \beta_7 (NE_{v74} * AG_{imv}^{6-7} * B_{v74}) + \beta_8 (NE_{v74} * AG_{imv}^{8-14} * B_{v74}) + \beta_9 (NE_{v74} * AG_{imv}^{15-19} * B_{v74}) + \alpha_a + X'Z + v_{imv}. \tag{3}$$

$NE_v$ is defined as 1-$E_v$, where $E_v$, is defined as above. $B_v$ takes on the value 1 if person *i* or person *i*'s household lived in a comparison village that borders a MCH-FP village in 1974 and 0 otherwise.[26] All other variables are defined as above. Equation 3 examines the spillover effect by splitting the comparison area into two groups, those who lived in a village that borders a MCH-FP village in 1974 and those who did not. $\beta_2 - \beta_5$ are the double-difference estimators for each age group and show how much lower the outcome variable is in the comparison area that does not border the MCH-FP village than in the MCH-FP area. $\beta_7 - \beta_9$ give the difference in effect for the various age groups between those who lived (in 1974) in a village that borders and one that does not border a MCH-FP village.

The point estimates in Table 8, column 1, indicate a positive spillover effect in control areas that border MCH-FP areas for the 8-14 age group, but this effect is not statistically significant. Villages are of varying sizes in this comparison area, and it may be that on average there are no significant spillover effects because informational spillovers may not extend throughout the whole village, especially in a larger village. I use GIS data to determine the Euclidean distance between the centroid of a comparison village and the border of an MCH-FP village, and create a binary variable to indicate the comparison villages that are in the lowest quartile of distance to an MCH-FP border (*Border MCH-FP village – closest quartile*). Using this specification, the spillover effects are close to zero and statistically insignificant for the 15-19 year olds, but the MMSE score for the 8-14 year olds is 0.33 SD higher and marginally significant (at the 10 percent level) for those who live in comparison villages that are close to an MCH-FP village. Indicating that there may have been a substantial spill-over effect in the comparison area and the intent-to-treat effects are an underestimate.

### *5.7 Spatially correlated errors*

Since the MCH-FP and comparison areas are in contiguous geographic areas, it is possible that errors are spatially correlated in either the MCH-FP or the comparison area. This could arise, for example, if there was a health shock such as a disease outbreak in a given year in one area but not the other. These outbreaks are likely to affect human capital more for younger children than for older ones, so a double-difference model will not control for such unobservables. Clustering at the village level is not sufficient to correct for the resulting lack of independence. To check the possible between-village clustering in the MCH-FP and comparison areas for the age 8-14 group I use the following test. First, I predict the errors from the base model in Table 3, column 3 (the preferred specification), and average the errors for the 8-14 year olds at the village level. To test whether these village level error terms are correlated I use Moral's I test using the Euclidean distance between village centroids as weights. I examine whether village level error terms are correlated for the whole sample and for the age group of most interest (8-14 year olds) in the MCH-FP and comparison areas separately. In all cases, I fail to reject the null hypothesis of zero spatial correlation between villages.

[26] For those people whom we cannot match to their 1974 location, we use the 1996 village information. The results are the same if we exclude these observations. Some observations are also lost in this analysis because the GIS maps used to determine which villages border each other are missing a few villages.

### *5.8 Mortality and migration attrition*

Two prominent causes of attrition in this context are mortality and migration. Even if the MCH-FP were truly randomized, the program itself is likely to cause mortality and migration to differ between MCH-FP and comparison areas over time, potentially biasing the results. For example, if frailer individuals (or those with lower health endowments) are more likely to survive in the MCH-FP area, then there will likely be a higher probability of observing someone with a lower level of cognitive functioning in the MCH-FP than in the comparison area in the follow-up period, biasing the results downwards. Since migration from rural areas in Matlab tends to occur among families with few resources (Kuhn 2003, 2006b), the MCH-FP program will likely encourage worse off families who might have migrated to stay in the MCH-FP area, as the program subsidizes the cost of raising children. This type of endogenous migration will leave a higher proportion of children who likely have lower human capital (since they come from worse off families) in eligible than in ineligible areas, also potentially biasing the results downwards. This paper partially addresses attrition by including mother fixed-effects to control for non-time-varying household variables that may be correlated with mortality or migration. The results suggest that the findings reported here are underestimates; however, this is not a sufficient test.

## 6. Conclusion

This paper examines the long-term effect of a maternal and early childhood health and family planning program on the cognitive functioning of eligible children once they reached ages between 8 and 19. The analysis takes advantage of the quasi-random placement of the Matlab MCH-FP program across locations and demonstrates that the treatment and comparison areas were similar with respect to most pre-intervention characteristics. The program rolled out the family planning and maternal health intervention during the first five years and then introduced important early childhood health interventions such as vaccination. The analyses examine separately the impact on children who were eligible for all the child health interventions (children aged 8-14 in 1996) and those who received none or just the measles vaccination past the recommended age (those aged 15-19 in 1996). The mothers of children in both these groups were eligible for the family planning and maternal health interventions.

The intent-to-treat double-difference results show that the program led to an approximately 0.38 SD increase in the Mini Mental State Exam score for the 8-14 year olds. Results were unchanged with the inclusion of education level fixed-effects, indicating the improvements in cognitive functioning are not due to an increase in education levels. This effect could be underestimated, since there is evidence of positive spillover effects in control villages neighboring MCH-FP villages. An effect size of 0.38 SD is similar to effect sizes in studies of the benefit of nutrition programs, and is equivalent to the effects size in the same sample for completing 3 years of primary school. A robustness check using propensity score weighting with regression adjustment found similar findings, while another that included mother fixed-effects suggests that the exclusion of mother fixed-effects biases the results downwards.

There were no significant positive effects on the 15-19 year olds in any of the models. The reason for the lack of significant results is unclear. It could be that the family planning and maternal health interventions do not substantially improve cognitive development. Alternatively, this age group could have experienced a positive effect from the family planning and maternal health interventions, but that effect could have been offset by the availability of contraceptives in government clinics in the comparison area, or by the negative impact of sibling competition as parents moved resources to their younger siblings who received the child health interventions. Finally, these programs may have an effect, but only on later-born children who benefit most from the smaller family size. Unfortunately, these data do not yield large enough samples to examine the effect on the later-born children.

It is difficult to rigorously separate out the effects of the family planning and maternal health interventions from the child health effects. I am able to exploit the phasing-in of the measles vaccine in the MCH-FP area to show that the intent-to-treat effect for the measles intervention alone was approximately 0.3 standard deviations. This result coupled with the lack of a program effect on the 15-19 year olds, who benefited mainly from the family planning and maternal health interventions, and the unchanging program effect when controls for the family planning program are included, suggests that much of the program effect could come from the child health interventions.

The treatment-on-the-treated effects were more than twice as large, at approximately 1 SD, with and without controls for the endogenous increase in family size due to the program's family planning interventions. The large increase in the effect on the treated confirms the importance of child health interventions (mainly vaccinations) not only for improved child health but for future opportunities through better cognitive development.

Finally, the heterogeneity of the findings by gender and birth order demonstrates that the program had a larger impact on second or higher order born children. Muhuri and Menken (1997) find that for this population firstborn children and boys tend to have lower mortality risk, suggesting that the program benefitted those who most need it.

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**Figures**

Figure 1: Map of Matlab study area

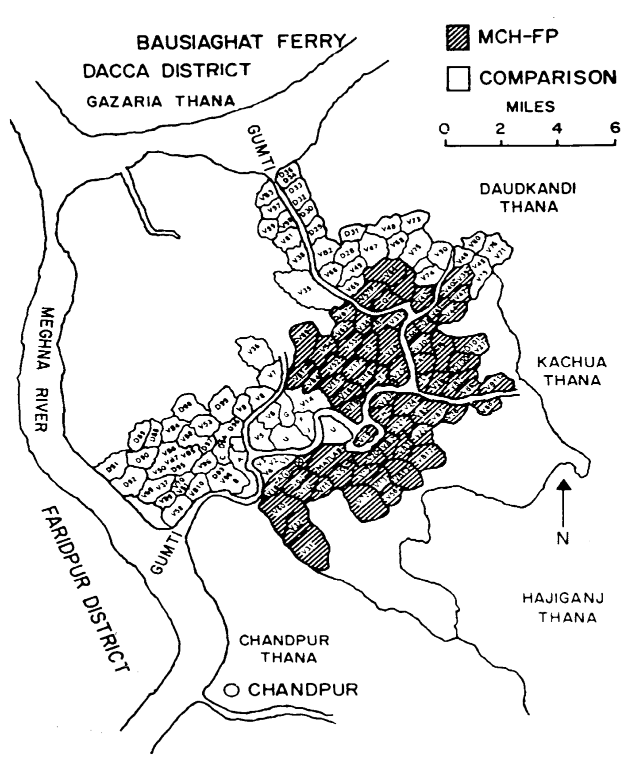


Figure 2: Trends in contraceptive prevalence (CPR) and measles vaccination (MVR) rates by area

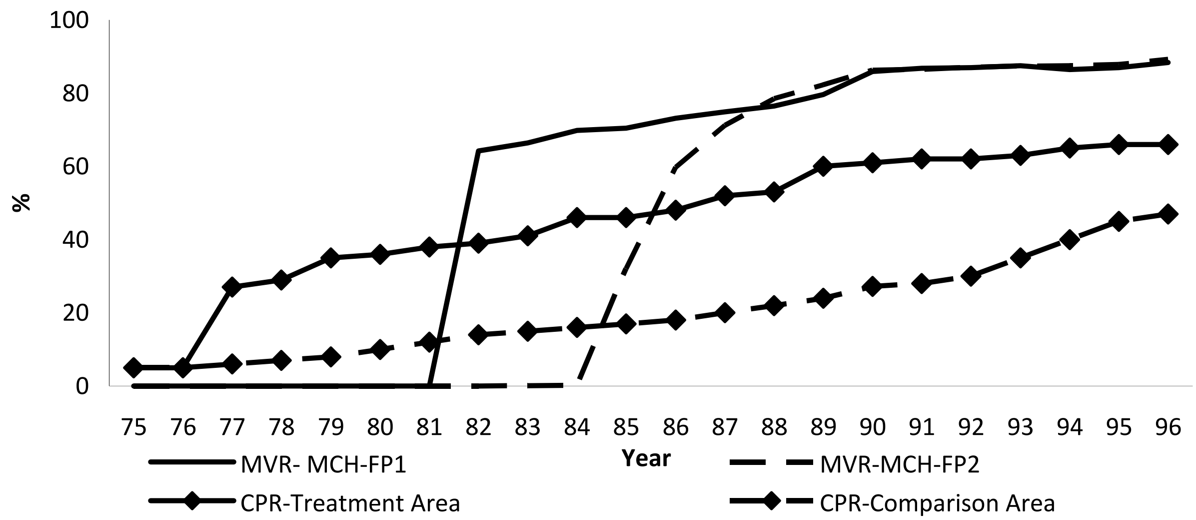


Figure 3:  Mean MMSE score by age group and eligibility status

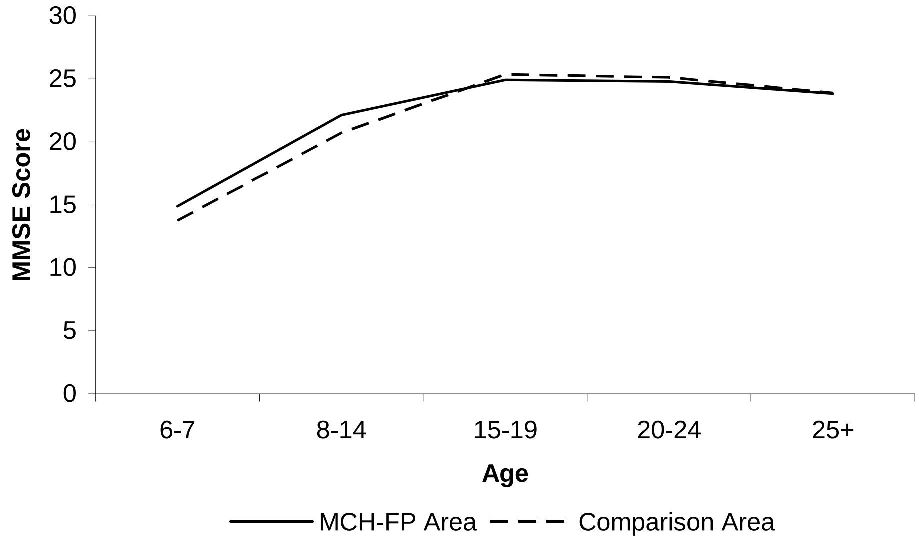
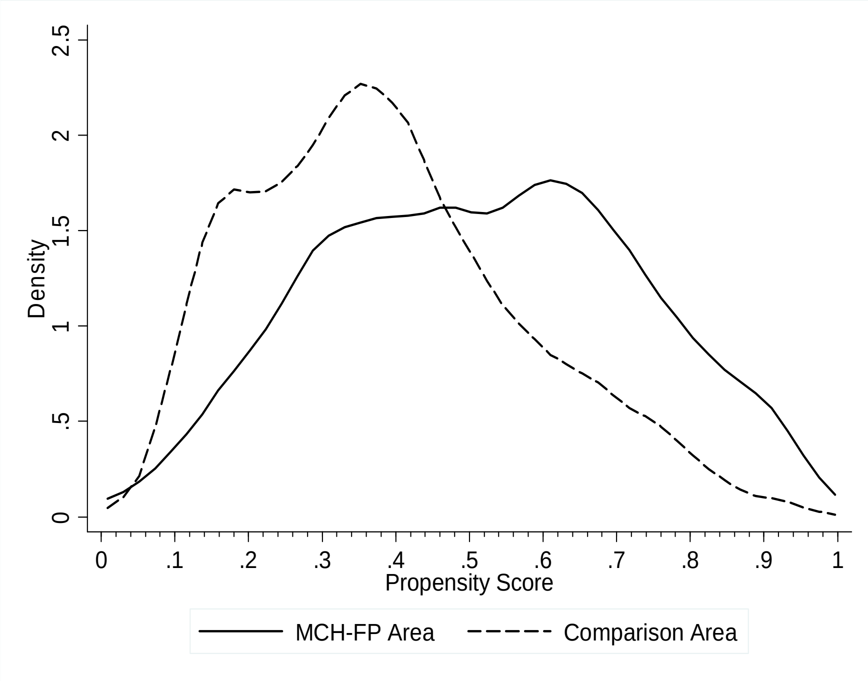


Figure 4: Kernel density graphs of the propensity score



## Tables

Table 1: MCH-FP Program Eligibility by Age

| **Eligibility** | **Year Born** | **Age in 1996 MHSS** |
|---|---|---|
| **Born before MCH-FP experiment* (born 1976 or earlier)** | | |
| Not eligible for MCH-FP interventions at any point in time (age 40+ in 1977) | Pre 1938 | 59 |
| Women of reproductive age during the experiment: potentially use FP and MH | 1938-1971 | 25-59 |
| Adolescent during the experiment; unlikely to be FP or MH users | 1972-1976 | 20-24 |
| **Born during MCH-FP experiment: Mother MH-FP eligible, interventions delivered at home** | | |
| Nonintensive CH interventions: ORT, late or no measles vaccination | 1977-1981 | 15-19 |
| Intensive CH interventions: ORT, on-time or late measles/DPT/polio/BCG, vitamin A | 1982-1988 | 8-14 |
| **Born after MCH-FP experiment: Vaccinations available at health clinics in comparison area** | 1989-2010 | 0-7 |

*MCH-FP experiment refers to the years 1977 to 1988, when most of the program interventions were available only in the MCH-FP area and not in the comparison area.

*Notes:* FP=family planning; MH=maternal health; CH=child health; ORT=Oral Rehydration Therapy; DPT=Diphtheria-Pertussis-Tetanus; BCG=Bacille Calmette-Guerin (vaccine against tuberculosis).

Table 2: Differences in Means between MCH-FP and Comparison Areas:
Respondent with MMSE score recorded in 1996 MHSS

| | MCH-FP Area | | | Comparison Area | | | Difference (Diff) in Means | | Normalized Difference |
|---|---|---|---|---|---|---|---|---|---|
| | **Mean** | **SD** | **Obs** | **Mean** | **SD** | **Ob** | **Diff** | **T-stat** | |
| ***A. Characteristics of respondents in 1996 MHSS*** | | | | | | | | | |
| MMSE age 25+ | 23.93 | 0.21 | 1808 | 23.93 | 0.18 | 2067 | 0.00 | 0.00 | 0.00 |
| MMSE for age 20-24 | 24.94 | 0.34 | 237 | 25.21 | 0.27 | 271 | -0.27 | -0.64 | -0.07 |
| MMSE for age 15-19 | 25.05 | 0.27 | 342 | 25.39 | 0.27 | 445 | -0.34 | -0.91 | -0.09 |
| MMSE for age 8-14 | 22.67 | 0.55 | 193 | 20.79 | 0.53 | 321 | 1.89 | 2.49 | 0.30 |
| MMSE for age 6-7 | 15.52 | 1.15 | 56 | 13.98 | 1.28 | 61 | 1.53 | 0.90 | 0.20 |
| Age | 32.82 | 0.47 | 2636 | 32.00 | 0.32 | 3165 | 0.81 | 1.45 | 0.06 |
| Female (=1) | 0.57 | 0.01 | 2636 | 0.55 | 0.01 | 3165 | 0.02 | 2.52 | 0.05 |
| Hindu (=1) | 0.16 | 0.03 | 2636 | 0.05 | 0.01 | 3165 | 0.11 | 3.77 | 0.37 |
| Years of education | 3.59 | 0.23 | 2621 | 3.02 | 0.11 | 3149 | 0.58 | 2.78 | 0.16 |
| Mother's years of education | 1.15 | 0.14 | 2563 | 0.93 | 0.06 | 3097 | 0.22 | 1.85 | 0.10 |
| Mother ever eligible for family planning (=1) | 0.58 | 0.01 | 2562 | 0.00 | 0.00 | 3081 | 0.58 | 43.12 | 1.32 |
| ***B. Household Characteristics from 1974 Census*** | | | | | | | | | |
| Family size | 7.00 | 0.10 | 2481 | 6.82 | 0.10 | 2929 | 0.18 | 1.25 | 0.06 |
| Owns a lamp (=1) | 0.66 | 0.03 | 2481 | 0.61 | 0.02 | 2929 | 0.05 | 1.51 | 0.10 |
| Owns a watch (=1) | 0.17 | 0.02 | 2481 | 0.16 | 0.01 | 2929 | 0.01 | 0.48 | 0.03 |
| Owns a radio (=1) | 0.09 | 0.01 | 2481 | 0.09 | 0.01 | 2929 | 0.00 | 0.08 | 0.00 |
| Wall made of tin (=1) | 0.09 | 0.01 | 2476 | 0.07 | 0.01 | 2913 | 0.02 | 1.37 | 0.08 |
| Wall made of tinmix (=1) | 0.24 | 0.01 | 2434 | 0.25 | 0.01 | 2886 | -0.01 | -0.58 | -0.03 |
| Roof made of tin (=1) | 0.83 | 0.02 | 2477 | 0.83 | 0.01 | 2914 | -0.01 | -0.32 | -0.02 |
| Latrine in household compound (=1) | 0.83 | 0.02 | 2481 | 0.85 | 0.02 | 2929 | -0.03 | -1.22 | -0.08 |
| Number of rooms per capita | 0.21 | 0.00 | 2481 | 0.21 | 0.00 | 2929 | -0.00 | -0.02 | -0.00 |
| Number of cows | 1.52 | 0.08 | 2481 | 1.35 | 0.07 | 2929 | 0.17 | 1.70 | 0.10 |
| Number of boats | 0.69 | 0.04 | 2481 | 0.67 | 0.03 | 2929 | 0.02 | 0.37 | 0.03 |
| Drinking water from tubewell (=1) | 0.30 | 0.03 | 2481 | 0.16 | 0.02 | 2926 | 0.14 | 3.85 | 0.34 |
| Drinking water from tank (=1) | 0.38 | 0.04 | 2481 | 0.32 | 0.04 | 2926 | 0.06 | 1.14 | 0.12 |
| Drinking water other source (=1) | 0.32 | 0.05 | 2440 | 0.52 | 0.04 | 2903 | -0.20 | -3.29 | -0.41 |
| ***C. Household Head Characteristics 1974 Census*** | | | | | | | | | |
| Age | 47.19 | 0.50 | 2473 | 46.01 | 0.45 | 2927 | 1.18 | 1.78 | 0.08 |
| Hindu (=1) | 0.16 | 0.03 | 2440 | 0.05 | 0.01 | 2906 | 0.11 | 3.51 | 0.37 |
| Years of education | 2.61 | 0.16 | 2481 | 2.40 | 0.12 | 2929 | 0.21 | 1.38 | 0.07 |
| Primary occupation is agriculture (=1) | 0.60 | 0.02 | 2481 | 0.59 | 0.02 | 2929 | 0.01 | 0.38 | 0.02 |
| Primary occupation fishing or boatman (=1) | 0.05 | 0.01 | 2481 | 0.07 | 0.01 | 2929 | -0.01 | -0.81 | -0.05 |
| Spouse's age | 1.17 | 0.09 | 2258 | 1.27 | 0.07 | 2597 | -0.10 | -1.07 | -0.06 |
| Spouse's years of education | 36.60 | 0.45 | 2254 | 35.56 | 0.40 | 2597 | 1.04 | 1.71 | 0.09 |

Notes: SD = standard deviation, Obs = observation; T-stat = T- statistic. The standard deviation of the whole sample is used to create the ratio of the difference in means to standard deviation. Standard errors are clustered at the village level. Household refers to either the respondent's household in 1974 or the 1996 household head's household in 1974. The normalized difference is the difference in means between the MCH-FP area and the comparison areas divided by the square root of thesum of the variance.

Table 3: Intent-to-Treat Program Effects for the MMSE Z-Score by Age Group

| | Full Sample | | | | | | MFE Sample | | CS Sample |
|---|---|---|---|---|---|---|---|---|---|
| | Single Difference OLS | | Double Difference (DD) OLS | | | | DD OLS | MFE | DD PSW |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Eligible (=1) | | | -0.02 | -0.01 | 0.05 | -0.05 | -0.17 | | -0.03 |
| | | | (0.05) | (0.05) | (0.07) | (0.05) | (0.13) | | (0.03) |
| Eligible*(Age 25+) | 0.02 | -0.02 | | | | | | | |
| | (0.05) | (0.05) | | | | | | | |
| Eligible*(Age 20-24) | -0.04 | -0.11 | -0.09 | -0.10 | -0.12+ | -0.09 | 0.02 | -0.38 | -0.02 |
| | (0.09) | (0.08) | (0.07) | (0.07) | (0.07) | (0.06) | (0.24) | (0.30) | (0.09) |
| Eligible*(Age 15-19) | -0.06 | -0.09 | -0.07 | -0.06 | -0.08 | -0.07 | -0.16 | 0.12 | -0.10 |
| | (0.08) | (0.08) | (0.06) | (0.07) | (0.06) | (0.06) | (0.17) | (0.33) | (0.07) |
| Eligible*(Age 8-14) | 0.35* | 0.36* | 0.38** | 0.37* | 0.38** | 0.38** | 0.55* | 0.65+ | 0.44** |
| | (0.16) | (0.15) | (0.14) | (0.14) | (0.14) | (0.14) | (0.22) | (0.37) | (0.12) |
| Eligible*(Age 6-7) | 0.36 | 0.33 | 0.35 | 0.26 | 0.26 | 0.33 | 0.28 | -0.02 | 0.55 |
| | (0.35) | (0.34) | (0.34) | (0.33) | (0.35) | (0.35) | (0.38) | (0.65) | (0.34) |
| Mother ever eligible for FP | | | | -0.02 | | | | | |
| | | | | (0.04) | | | | | |
| Mother literate | | | | 0.36** | | | | | |
| | | | | (0.04) | | | | | |
| Individual characteristics | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Pre-intervention characteristics | N | Y | Y | Y | Y | Y | Y | Y | Y |
| Village fixed-effects | N | N | N | N | Y | Y | N | N | N |
| Education level fixed-effects | N | N | N | N | N | Y | N | N | N |
| Mother fixed-effects | N | N | N | N | N | N | N | Y | N |
| Observations | 5801 | 5801 | 5801 | 5551 | 5801 | 5770 | 1266 | 950 | 4625 |
| Adjusted R-Squared | 0.21 | 0.25 | 0.25 | 0.28 | 0.31 | 0.34 | 0.39 | 0.62 | 0.24 |

Notes: Standard errors are clustered at the village level. "**" ,"*", or "+" indicates that the difference in the coefficient from zero is statistically significant at the 1 percent, 5 percent, or 10 percent level respectively. All regressions include age fixed-effects. Individual characteristics include dummies for being female and for the Islamic religion. Pre-intervention characteristics include all household and household head characteristics from 1974 presented in Table 2. A mother is literate if she has more than 3 years of formal education. OLS=Ordinary least square, MFE = mother fixed-effects, CS = common support based on propensity score, PSW = propensity score weighting with regression adjustment. The pre-intervention characteristics are included as deviations from the sample mean for the PSW estimates.

Table 4: Intent-to-Treat Effects on MMSE Z-Score Disaggregated in the MCH-FP Area

| **Panel A: Full Sample DD OLS** | | | **Panel B: MCH-FP Area Sample DD OLS** | |
|---|---|---|---|---|
| | (1) | (2) | | (1) |
| Eligible (=1) | -0.02 | -0.02 | MCH-FP1 area (=1) | -0.9 |
| | (0.05) | (0.05) | | (0.07) |
| Eligible*(Age 20-24) | -0.09 | -0.09 | MCH-FP1 area*(Age 20-24) | -.07 |
| | (0.07) | (0.07) | | (0.11) |
| Eligible*(Age 17-19) | 0.00 | 0.00 | MCH-FP1 area*(Age 15-19) | -0.01 |
| | (0.06) | (0.06) | | (0.09) |
| Eligible*(Age 15-17) | -0.15 | -0.15 | MCH-FP1 area*(Age 12-14) | 0.28 |
| | (0.11) | (0.11) | | (0.25) |
| Eligible*(Age 12-14) | 0.30 | | MCH-FP1 area*(Age 8-11) | 0.30 |
| | (0.18) | | | (0.24) |
| Eligible*(Age 12-14)*MCH-FP1 | | 0.42* | MCH-FP1 area*(Age 6-7) | 1.06* |
| | | (0.17) | | (0.41) |
| Eligible*(Age 12-14)*MCH-FP2 | | 0.20 | | |
| | | (0.25) | | |
| Eligible*(Age 8-11) | 0.46** | 0.46** | | |
| | (0.17) | (0.17) | | |
| Eligible*(Age 6-7) | 0.35 | 0.35 | | |
| | (0.34) | (0.34) | | |
| Observations | 5801 | 5551 | | 2636 |
| Adjusted R-Squared | 0.25 | 0.25 | | 0.25 |

Notes: Standard errors are clustered at the village level. "**" ,"*", or "+" indicates that the difference in the coefficient from zero is statistically significant at the 1 percent, 5 percent, or 10 percent level respectively. All regressions include age fixed-effects, individual controls (gender and religion), and household and household head characteristics from 1974 presented in Table 2.

Table 5: Intent-to-Treat Effects by Subcomponent of MMSE

| | Double-difference OLS | | | |
|---|---|---|---|---|
| | **Orientation** | **Attention-Concentration** | **Registration** | **Language** |
| Eligible (=1) | -0.02 | 0.01 | -0.09 | 0.10 |
| | (0.04) | (0.03) | (0.07) | (0.07) |
| Eligible*(Age 20-24) | -0.03 | -0.07 | -0.03 | -0.20* |
| | (0.08) | (0.06) | (0.08) | (0.09) |
| Eligible*(Age 15-19) | -0.04 | -0.06 | -0.07 | -0.03 |
| | (0.07) | (0.06) | (0.07) | (0.06) |
| Eligible*(Age 8-14) | 0.28+ | 0.28* | 0.37* | 0.21 |
| | (0.14) | (0.11) | (0.14) | (0.17) |
| Eligible*(Age 6-7) | 0.28 | 0.21 | 0.34 | 0.20 |
| | (0.28) | (0.34) | (0.27) | (0.27) |
| Observations | 5801 | 5801 | 5801 | 5801 |
| Adjusted R-Squared | 0.27 | 0.28 | 0.1 | 0.07 |

Notes: Standard errors are clustered at the village level. "**" ,"*", or "+" indicates that the difference in the coefficient from zero is statistically significant at the 1 percent, 5 percent, or 10 percent level, respectively. All regressions include age fixed-effects, individual controls (gender and religion), and household and household head characteristics from 1974 presented in Table 2. Each subcomponent of the MMSE is a z-score and therefore represents standard deviations.

Table 6: Heterogeneity of the Intent-to-Treat Effects for MMSE Z-Score.

**Panel A: Double-difference OLS**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Eligible (=1) | -0.02 | -0.02 | -0.01 | (0.07) |
| | (0.05) | (0.05) | (0.06) | -0.06 |
| Eligible*(Age 20-24) | -0.09 | -0.07 | -0.11 | (0.11) |
| | (0.08) | (0.07) | (0.09) | -0.02 |
| Eligible*(Age 15-19) | -0.03 | -0.08 | -0.05 | (0.09) |
| | (0.07) | (0.07) | (0.08) | 0.24 |
| Eligible*(Age 8-14) | 0.39* | 0.40** | 0.19 | (0.19) |
| | (0.16) | (0.14) | (0.18) | 0.13 |
| Eligible*(Age 6-7) | 0.04 | 0.21 | 0.32 | (0.36) |
| | (0.29) | (0.33) | (0.41) | (0.07) |
| Eligible*(Age 15-19)*Tubewell drinking water | -0.20+ | | | -0.21+ |
| | (0.12) | | | (0.12) |
| Eligible*(Age 8-14)*Tubewell drinking water | -0.36 | | | -0.25 |
| | (0.32) | | | (0.31) |
| Eligible*(Age 15-19)*Hindu | | 0.01 | | -0.03 |
| | | (0.18) | | (0.18) |
| Eligible*(Age 8-14)*Hindu | | 0.00 | | -0.03 |
| | | (0.61) | | (0.58) |
| Eligible*(Age 15-19)*Male | | | -0.04 | -0.03 |
| | | | (0.10) | (0.10) |
| Eligible*(Age 8-14)*Male | | | 0.38+ | 0.35+ |
| | | | (0.20) | (0.20) |
| Observations | 5801 | 5801 | 5801 | 5801 |
| Adjusted R-Squared | 0.26 | 0.25 | 0.26 | 0.27 |

**Panel B: Single-difference OLS**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Eligible*(Age 20-24) | -0.01 | 0.06 | -0.15 | -0.21 |
| | (0.11) | (0.18) | (0.25) | (0.39) |
| Eligible*(Age 15-19) | -0.17* | -0.19+ | 0.05 | 0.16 |
| | (0.08) | (0.11) | (0.13) | (0.21) |
| Eligible*(Age 8-14) | 0.37** | 0.15 | -0.12 | -0.21 |
| | (0.14) | (0.18) | (0.26) | (0.41) |
| Eligible*(Age 6-7) | 0.21 | 0.27 | 0.08 | -0.26 |
| | (0.34) | (0.43) | (0.62) | (0.97) |
| Eligible*(Age 15-19)*Male | | 0.03 | | -0.17 |
| | | (0.10) | | (0.24) |
| Eligible*(Age 8-14)*Male | | 0.45* | | 0.30 |
| | | (0.21) | | (0.52) |
| Eligible*(Age 15-19)*Birth Order 2 + | | | -0.29* | -0.44* |
| | | | (0.12) | (0.21) |
| Eligible*(Age 8-14)*Birth Order 2 + | | | 0.61* | 0.47 |
| | | | (0.27) | (0.46) |
| Eligible*(Age 15-19)*Male* Birth Order 2 + | | | | 0.24 |
| | | | | (0.29) |
| Eligible*(Age 8-14)*Male* Birth Order 2 + | | | | 0.15 |
| | | | | (0.60) |
| Observations | 1530 | 1530 | 1530 | 1530 |
| Adjusted R-Squared | 0.38 | 0.38 | 0.38 | 0.39 |

Notes: Standard errors are clustered at the village level. "**" ,"*", or "+" indicates that the difference in the coefficient from zero is statistically significant at the 1 percent, 5 percent, or 10 percent level, respectively. All regressions include age fixed-effects, individual controls (gender and religion), and household and household head characteristics from 1974 presented in Table 2.

Table 7: Treatment on the Treated Results

| | First Stage Equation Received all MCH-FP vaccines | Second Stage Equation MMSE Z-Score | | First Stage Equations Received all MCH-FP vaccines | Number of Siblings | Second Stage Equation MMSE Z-Score |
|---|---|---|---|---|---|---|
| | **OLS** | **2SLS** | **2SLS** | **OLS** | **OLS** | **2SLS** |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Endogenous variables* | | | | | | |
| Received all MCH-FP vaccines (=1) | | 0.92* | 0.95** | | | 1.03** |
| | | (0.39) | (0.34) | | | (0.34) |
| Number of siblings | | | | | | 0.12 |
| | | | | | | (0.11) |
| *Instruments* | | | | | | |
| Eligible*(Age 8-14) (=1) | 0.41** | | | 0.43** | 0.27 | |
| | (0.03) | | | (0.3) | (.27) | |
| Mother Eligible for MCH-FP (=1) | | | | 0.00 | -0.61** | |
| | | | | (0.00) | (0.10) | |
| Sample with no missing number of siblings | N | N | Y | Y | Y | Y |
| F-statistic on excluded instruments | 141 | | | 79 | 16 | |
| Observations | 5681 | 5681 | 1894 | 1894 | 1894 | 1894 |
| Adjusted R-Squared /Partial R-squared | 0.33 | 0.19 | 0.25 | 0.35 | 0.01 | 0.15 |

Notes: Standard errors are clustered at the village level. "**" ,"*", or "+" indicates that the difference in the coefficient from zero is statistically significant at the 1 percent, 5 percent, or 10 percent level, respectively. All regressions include age fixed-effects, individual controls (gender and religion), and household and household head characteristics from 1974 presented in Table 2.

Table 8: Spillover Effects for MMSE Z-score

| | Double-difference OLS | |
|---|---|---|
| | (1) | (2) |
| Not Eligible (=1) | -0.01 | 0.01 |
| | (0.06) | (0.06) |
| Not Eligible*(Age 20-24) | 0.15+ | 0.15+ |
| | (0.08) | (0.08) |
| Not Eligible*(Age 15-19) | 0.06 | 0.07 |
| | (0.07) | (0.07) |
| Not Eligible*(Age 8-14) | -0.45** | -0.47** |
| | (0.15) | (0.15) |
| Not Eligible*(Age 6-7) | -0.44 | -0.32 |
| | (0.40) | (0.41) |
| Not Eligible*(Age15-19)*Border treatment village | -0.02 | |
| | (0.07) | |
| Not Eligible*(Age8-14)*Border treatment village | 0.22 | |
| | (0.18) | |
| Not Eligible*(Age15-19)*Border treatment village - closest quartile | | -0.05 |
| | | (0.07) |
| Not Eligible*(Age8-14)*Border treatment village - closest quartile | | 0.33+ |
| | | (0.17) |
| Observations | 5575 | 5575 |
| Adjusted R-Squared | 0.26 | 0.26 |

Notes: Standard errors are clustered at the village level. "**" ,"*", or "+" indicates that the difference in the coefficient from zero is statistically significant at the 1 percent, 5 percent, or 10 percent level, respectively. All regressions include age fixed-effects, individual controls (gender and religion), and household and household head characteristics from 1974 presented in Table 2.