Do Parents Selectively Time Birth Relative to Ramadan? Evidence from Matlab, Bangladesh *†

Job Market Paper

Md. Nazmul Ahsan ‡

University of Southern California

December 2015

Abstract

Studies examining health effects of maternal fasting during Ramadan find that *in utero* exposure to Ramadan has negative implications for child health, labor and education outcomes. One key methodological concern in those studies is that the identification is based under the assumption that parents do not selectively time birth relative to Ramadan. In Matlab, a region in Bangladesh, a family planning program was initiated in 1977 in which women in the treatment areas received free contraceptives door to door, but those in the control areas were not part of the program. Using the Matlab Health Socio-economic Survey 1996, I find women, living in the treatment areas, are five to six percentage points less likely to give birth 8 to 9 months after Ramadan after being exposed to the family planning program. Moreover, more educated mothers, living in the treatment areas, are less likely to overlap their pregnancies with Ramadan. This suggests that parents selectively time birth relative to Ramadan, and selection in *in utero* exposure to Ramadan should be examined allowing for time varying changes. I examine child height, which reflects both genotype and phenotype influences *in utero*, and find evidence suggesting that presence of parental selection may lead us to wrong conclusions about the effect of maternal fasting on child height. To explore whether or not the negative association between mother years of education and Ramadan exposure is limited to the treatment areas of Matlab, I use all four waves of the Indonesian Family Life Survey and find similar parental selection in Ramadan exposure.

*I am indebted to John Strauss and Anant Nyshadham for their guidance and advice on this paper. A special thanks to Douglas Almond for discussing this paper at ASSA 2015 in Boston. Suggestions from Jeffrey Nugent, Ummul Ruthbah, Hashem Pesaran, Fei Wang, Shahe Emran, Jared Rubin, Yaoyao Zhu, Joseph Cummins, Khawaja Mamun, Brian Duncan, Jenna Stearns and discussions with Rakesh Banerjee, Riddhi Bhowmick and Ashish Sachdeva were essential to this research. I would like to thank conference and participants at USC Development Economics seminar, USC CESR Brown bag, ASREC, ASSA, PACDEV, WEAI, SEA, IRES Chapman University graduate workshop and WEAI graduate student workshop. All remaining errors are my own. Any comments are welcome.

†Previously circulated as "Do Parents Selectively Time Birth Relative to Ramadan? Impact of Maternal Fasting on Height: Evidence from Matlab, Bangladesh"

‡Department of Economics, Dana and David Dornsife College of Letters, Arts and Sciences, University of Southern California, 3620 S. Vermont Ave. KAP 300, Los Angeles, CA 90089-0253. Email: mdnazmua@usc.edu

1 Introduction

More than 1 billion Muslims alive today were exposed to Ramadan *in utero* (hereafter *in utero* exposure to Ramadan would be referred to as "exposure to Ramadan")[1]. Ramadan is the 9th month in the Islamic calender. During this month, Muslims are required to fast from dawn to dusk in each day of the month except some who can seek exemptions. Although according to Islamic law, pregnant women are exempted from fasting, in practice however, it has been documented that a majority of the Muslim pregnant women fast during Ramadan. According to the fetal origins hypothesis, this may be a serious concern. It is well established that *in utero* experience of nutrition shocks and adverse conditions has serious implications for an individual's health and human capital formation (Almond and Currie, 2011; Currie, 2009)[2]. This has led researchers to explore the consequence of maternal fasting during Ramadan on her child's various health outcomes as well as economic outcomes. The findings so far overwhelmingly corroborate with the earlier evidence based on fetal origins hypothesis (Almond and Mazumder, 2011; Ewijk, 2011; Almond et al., 2014; Majid, 2015; Karimi, 2014; Ewijk et al., 2013). These studies find that exposure to Ramadan leads to poorer health, education outcomes, and labor market outcomes.

Although *in utero* experience of maternal fasting during Ramadan has serious consequences for an individual's human capital formation and well being, not much is known in regards to what policy might be effective in reducing number of pregnancies overlapping with Ramadan. Moreover, relatively little is known regarding fertility selection relative to Ramadan. Previous studies have examined selection in Ramadan exposure by comparing different parental socio-economic measures between children who are exposed to Ramadan and children who are not exposed to Ramadan (Almond and Mazumder, 2011; Ewijk, 2011; Almond et al., 2014; Majid, 2015; Karimi, 2014; Ewijk et al., 2013). Overall, these studies do not find any differences in observable parental characteristics between children who are exposed and not exposed to Ramadan. Here, it is important to note that Ramadan is a recurring event, and each year Ramadan starts 11 days ahead from the day it started in the last year. The use of sufficiently large number of cohorts allows the disentanglement of the effect of Ramadan exposure from seasonality[3]. One potential problem arising from the use of large numbers of cohorts is that within a geographic unit over time there can be time varying changes specific to that geographic unit that will not be captured by the geographic unit fixed effects [4]. Some of these changes such as availability of contraceptive devices may allow parents to time birth relative to Ramadan. More importantly, parents from different SES backgrounds may make use of these changes differently in relation to birth relative to Ramadan. As a result, there could be parental selection in Ramadan exposure because of these changes, and ignoring such changes may result in falsely concluding that there is no selection concern.

Using the Matlab Health and Socio-Economic Survey (1996), I study whether provision of an intensive family planning program can help parents to time birth relative to Ramadan. In Matlab, a region in Bangladesh, the International Center for Diarrhoeal Disease Research, Bangladesh (ICDDR, B) initiated a family planning program in some villages (treatment areas) but not in others (control areas). Under the family planning program, women of reproductive ages, living in the treatment areas, were given free contraceptives door to door in every two weeks (Joshi and Schultz, 2013). Another appealing feature of the MHSS is that reliable birth

[1] According PEW research center number of Muslim population in 2010 was 1.6 billion. The period of human gestation if about 9 months long. This implies children born in about 75% Islamic calender year days will be exposed to Ramadan at some point of pregnancy.

[2] Almond and Currie (2011) provides a good review on existing literature on fetal origins hypothesis.

[3] It takes Ramadan about 33 years to complete full circuit of western calendar.

[4] These changes can be change in prices and community infrastructures.

data can be obtained from the pregnancy histories of women. A demographic Surveillance System (DSS) has been operating in Matlab from 1966, and it collects information on vital events like birth, marriage, death, and migration. Reliable birth data is available from 1974 onward for both the treatment and control villages, which gives us 22 birth year cohorts to study birth timing relative to Ramadan[5].

Using a difference-in-differences strategy, this paper finds that Muslim mothers are about five to six percentage points less likely to give birth eight to nine months after Ramadan after being exposed to the family planning program. They are also about eight percentage points more likely to give birth four to seven months after Ramadan. These two results imply that Muslim mothers increase conceptions five to two months ahead of Ramadan but avoid conceptions when Ramadan is either 1 month away or at the time of Ramadan. Using the same strategy on births by Hindu mothers, this paper does not find any change in birth in months relative to Ramadan on Hindu births. This strengthens the findings that the intensive family planning program in Matlab helped mothers selectively time birth relative to Ramadan. In addition to these results, using information on the timing of the family planning program, this paper finds that in the program areas more educated Muslim mothers are less likely to overlap their pregnancies with Ramadan after the initiation of the family planning program. Such pattern for more educated Muslim mothers in the control areas and more educated Hindu mothers in the treatment areas is not found. This means that children who are exposed and not exposed to Ramadan in the treatment areas differ in one important observed characteristic—mother education. To document the consequence of selection, heights of the children—who are under age ten years during the survey year 1996—are analyzed[6]. Interestingly, a significant difference in child height between Muslim children who are exposed and not exposed to Ramadan in the control areas is not found. In contrast, in the treatment areas a statistically significant difference in child height between Muslim children who are exposed and not exposed to Ramadan is found. In the treatment areas, the Muslim children who are exposed to Ramadan are shorter than the children who are not exposed. This paper concludes that the difference in child height between children exposed and not exposed to Ramadan in the treatment areas could be due to parental selection in Ramadan exposure.

Moreover, using all four waves of the Indonesian Family Life Survey (IFLS), this study also documents that more educated Muslim mothers are less likely to overlap their pregnancies with Ramadan. On other hand, for non-Muslim mothers the likelihood of exposure to Ramadan does not vary by mother years of education. This reinforces the findings in Matlab, Bangladesh and implies that the selection behavior is not only limited for a sub-sample of the MHSS, which received the intensive family planning program. One key distinction between the IFLS data and the MHSS data is that with the IFLS this paper does not evaluate a program effect on the selection in Ramadan exposure but with the MHSS, it evaluates the program effect of the family planning program on the selection in Ramadan exposure.

This paper makes some key contributions to the literature. First, it shows parents time birth relative to Ramadan using two rich data sets from two countries. To the best of my knowledge, this is the first paper that shows timing of birth relative to Ramadan. Earlier literature (Almond and Mazumder, 2011; Ewijk, 2011; Almond et al., 2014; Majid, 2015; Karimi, 2014; Ewijk et al., 2013) do not find any evidence that parents time birth relative to Ramadan.

Secondly, this paper shows that an intensive family planning program may help Muslim mothers to avoid their pregnancies overlapping with Ramadan. In general, family planning programs are promoted to reduce fertility rate. This study shows an unintended benefit of provision of an

[5]Although DSS started from 1966, the data was computerized starting from 1974.

[6]I only study children under age ten years to reduce the concern of adolescent growth spurt.

intensive family planning program to Muslims is that it allows to them to avoid birth relative to Ramadan to some extent.

Thirdly, this paper is also the first to document the importance of incorporating time varying changes in examining selection in fertility relative to Ramadan. It illustrates that ignoring the time varying changes may result in falsely concluding that there is no selection in Ramadan exposure, whereas incorporating such changes lead to conclusion that there is. The underlying identification assumption in earlier studies is that there is no parental selection in Ramadan exposure. Absence of parental selection in Ramadan exposure implies that higher SES mothers are no less likely to expose their pregnancies to Ramadan than lower SES mothers. This guarantees that Ramadan exposure and non-exposure can be considered as a natural experiment, and the differences in outcomes between children exposed and not exposed to Ramadan can be attributed to maternal fasting during Ramadan[7]. On the other hand, the failure of the assumption would imply that factors other than Ramadan exposure are responsible for the observed difference in outcomes.

Lastly, this study documents how the presence of selection may affect identification of Ramadan exposure on child health outcomes. It documents that the negative association between Ramadan exposure and child height—a measure of long term nutritional status (Strauss and Thomas, 2007)—is more pronounced among the Muslim children residing in the villages which got the intensive family planning program. Coupled with this observation, this paper finds that more educated Muslim mothers residing in the same villages were also less likely to overlap their pregnancies with Ramadan. This result has implications on the findings as well as methodologies applied in the earlier studies. This suggests that mean difference in SES characteristics between children exposed and not exposed to Ramadan may not be sufficient enough to examine selection in exposure to a recurring and predictable event like Ramadan. The earlier studies should also examine parental selection allowing for time varying changes. It is important to note that this study claims neither that the findings based on earlier studies are not valid nor that maternal fasting has no negative implications for an individual's human capital formation. However, what is emphasized here is that parental selection in Ramadan exposure can occur due to changes in community characteristics over time, and ignoring such changes may produce biased estimates. Unfortunately, some of the studies do not have have information on time varying changes, but for some studies there are information on time varying changes both at household as well as community level.

The rest of the paper is organized as follows. Section 2 discusses the existing literature on maternal fasting and its implications for child human capital formation. Section 3 discusses the background of Matlab Family Planning and Child Health programs. Section 4 discusses the data obtained from the MHSS and IFLS. Section 5 discusses the data on the Islamic calender year and the construction of measures of birth months relative to Ramadan. Section 6 discusses the empirical strategies used in this paper to study selective timing of birth relative to Ramadan and also whether presence of selection may bias exposure effect on health outcomes. Section 7 presents the empirical results and explains the extent to which they differ from existing literature. Section 8 provides discussion on the results and implications for existing studies and section 9 provides concluding remarks.

[7]Sleep pattern, working hours, food consumption, etc., also change during the Ramadan. Economics literature recognize such changes during Ramadan but generally attribute the difference in outcomes to exposure to maternal fasting because a majority of Muslim pregnant women fast during Ramadan, and prolonged fasting may have implications for both maternal and fetal health based on insights from bio-medical literature.

2 Fetal Origins Hypothesis, Maternal Fasting and Child Human Capital Formation

There is a growing literature on the "fetal origins hypothesis" which links adverse condition and inadequate nutrition *in utero* to later life health outcomes. Numerous evidences show that critical developments take place during these nine months *in utero* that are important for future health and well being of an individual (Almond and Currie, 2011; Currie, 2009). Early studies were based on association between exposure to shocks and health outcomes. To get causal estimates researchers have exploited shocks which are exogenous to mothers. Some of these studies used historic events, such as famine and disease outbreak, to get causal estimates. Unlike these events, Ramadan is a recurring event, and it causes less severe nutritional compromise. Only recently, nutrition shock due to maternal fasting during Ramadan has been studied by economists. Below I discuss the literature on maternal fasting behavior during Ramadan, fasting effect on maternal health and fetal health and identification issues in existing studies.

Maternal fasting behavior during Ramadan — Ramadan is a holy month to Muslims, as Prophet Muhammad received his first revelation in this month. Among the five basic pillars—which are regarded as obligations to every Muslim— fasting during Ramadan is one. During the entire month of Ramadan, fasting entails refraining from eating, drinking, smoking as well as sexual engagement from dawn to dusk. The fasting hours varies considerably by geographic location and the season Ramadan is overlapping with. The further the location is away from the equator, the higher the variation in fasting hours by season. Even though Ramadan is an obligation, it is exempted for children, breastfeeding and pregnant women, elderly, ill, and travelers.

It is puzzling to observe that pregnant women fast during Ramadan, even though they are exempted from fasting. Islamic law requires that if women miss fasting during pregnancy, they should make up for the missed days after delivery. This requirement as well as guilt and cultural expectations may also deter women from seeking the exemption (Reeves, 1992; Mirghani et al., 2004; Robinson and Raisler, 2005). Based on several studies from several countries, Almond and Mazumder (2011) note that about 70 to 90 percent of pregnant women fast during Ramadan.

Fasting and maternal health during pregnancy — medical research is extremely insightful in understanding why it is not recommended for pregnant women to fast or skip meals for prolonged time. Metzger et al. (1982) study effect of fasting on level of circulating fuels and glucoregulatory hormones among pregnant women in their third trimester. They find profound changes in biochemical measures among pregnant women when fasting was prolonged from 12 hours to 18 hours. These changes are known as "accelerated starvation". Further, Meis et al. (1984) find among pregnant women glucose concentration is significantly lower after eight hour of fasting during daytime compared to nighttime. These studies conclude that pregnant women should not fast or skip breakfast during pregnancy.

Maternal fasting and fetal health — based on Jaddoe and Witteman (2006) study, Almond and Mazumder (2011) point out two prominent hypotheses which describe the effect of fasting on fetal health. The first is "fetal under-nutrition". Nutritional deprivation *in utero* leads to adaptations which are beneficial in short term but causes lower birth weight. These permanent changes in physiology and metabolism also make the body more susceptible to heart disease and diabetes. Karimi (2014) provides detail accounts on why fasting may affect the child height. He describes two phases of fetal development pre-ossification and post ossification process. Based on the writings of Gluckman and Hanson (2004) and Cooper et al. (2006), he notes that maternal nutrition affects both stages of fetal growth. Height also reflects phenotype and genotype influences (Martorell and Habicht, 1986).

The second hypothesis is that nutritional restrictions hinder the development of a placental enzyme that is necessary for converting cortisol into inactive cortisone. Thus, it exposes the fetus to excessive amounts of cortisol. Kapoor et al. (2006) and Seckl and Holmes (2007) suggest that glucocorticoids such as cortisol *in utero* leads to a reprogramming of the hypothalamic pituitary adrenal axis (HPA) which in turn may affect diabetes chances, cognitive functioning, and high blood pressure. Dikensoy et al. (2008) reported that at the time of pregnancy fasting during Ramadan is associated with an increased level of cortisol during pregnancy. Rizzo et al. (1991) find association between maternal fasting during pregnancy and cognitive functioning. Studies based on animal linked early pregnancy exposure ketone to neurological impairments (Hunter and Sadler, 1987; Moore et al., 1989; Sheehan et al., 1985).

Epidemiological literature — these findings based on above mentioned studies provide foundation for examining the effect of maternal fasting during Ramadan on child health outcomes. Epidemiological studies find association between maternal fasting with reduced fetal breathing, fetal heart rate *in utero*, and psychomotor development (Mirghani et al., 2004; DiPietro et al., 2007; Cross et al., 1990). However, Malhotra et al. (1989) and Mirghani and Hamud (2006)) do not find any effects on APGAR scores and birth weight.

Almond and Mazumder (2011) note several limitations of the epidemiological studies which examine the exposure effect of Ramadan on individuals' health outcomes. First of all, most of those studies are based on a small number of observations. Secondly, those studies compare the effect on fasters and non-fasters assuming that the decision to fast is exogenous. Thirdly, those studies do not disentangle the fasting effect from the seasonality, as they are based on Ramadan overlapping with only one season.

Economics literature — the application of Intent To Treat (ITT) analysis distinguishes the study of Almond and Mazumder (2011) from the epidemiological studies on Ramadan. They are also the first to study the impact of maternal fasting during Ramadan on child outcomes in the economics literature. Given that human gestation is about 280 days long and the Islamic calender year is about 354 days long, there will be some pregnancies which will not be overlapped with Ramadan but some will be. ITT anaylsis allows them to get rid of the compliance problem related to fasting, and the difference between children exposed and not exposed to Ramadan can be analyzed. Under the assumption that parents do not selectively time birth relative to Ramadan, it gives causal estimates of the impact of experiencing *in utero* nutrition shock. Moreover, unlike most of the epidemiological studies, the study of Almond and Mazumder (2011) is also based on a large number of cohorts.

Almond and Mazumder (2011) use data from Michigan, Iraq and Uganda. They study the impact of nutrition compromise due to exposure to Ramadan on birth weight using data from Michingan, and on various forms of disabilities using census data from Iraq and Uganda. They find children who were exposed to Ramadan have lower birth weights and are more likely to be disabled. Following Almond and Mazumder (2011), Ewijk (2011) finds that exposure to Ramadan increases the chances of developing health problem such as coronary heart disease and type 2 diabetes. Majid (2015) finds that exposure to Ramadan leads to fewer hours worked and self-employment in later life[8]. Ewijk (2011) and Ewijk et al. (2013) use the wave 3 of the IFLS. On the other hand, Majid (2015) primarily use the wave 4 of the IFLS. Using the English register data, Almond et al. (2014) finds that maternal fasting during Ramadan leads to lower test scores. Karimi (2014) uses 80 Demographic Health Surveys (DHS) from 35 countries spanning 27 birth cohorts and finds that exposure to Ramadan *in utero* leads to shorter stature for children. Ewijk et al. (2013) also finds negative association between Ramadan exposure and height using the IFLS wave 3. Unlike Karimi (2014), they focus on adult height.

[8]In this context, self-employment is regarded as a poor labor market outcome.

Although the studies in economics literature highlight the importance of identifying impact of maternal fasting during Ramadan on relevant outcomes allowing for selection, they do not find any evidence on parental selection in Ramadan exposure. The key limitation in these studies is that they do not examine parental selection in Ramadan exposure allowing for time varying changes. The above mentioned studies also have data limitations. In their Michigan and Iraq data, Almond and Mazumder (2011) could not identify the religion of the mother. They used Arab as proxy for Muslims in their Michigan data. The birth data from Uganda, Iraq, Indonesia and the DHS data sets were also self-reported. This could be a serious problem as misreported birth dates will lead to wrong classification of exposure to Ramadan. In comparison to the some of the past data sets used in this literature, in the MHSS data the religion of the mother can be clearly identified as well as reliable information on birth date can be extracted for a large number of cohorts. Unlike the past studies which have used the IFLS, this paper uses the birth information from the pregnancy histories of the mothers from all four waves of the IFLS and restricts the recall period up to 5 year maximum to minimize the recall bias. The benefit of using birth data from pregnancy histories is that it includes all the live births.

Compare to these earlier studies, this paper documents the effect of an intensive family planning program on selection in birth timing. The time varying component under study here is the family planning program in Matlab, as the paper uses birth data of the birth cohorts who were born before and after the program. This paper illustrates that if the timing and placement information of the family planning program are ignored, we might incorrectly conclude that there is no presence of parental selection in Ramadan exposure. Moreover, this paper shows that presence of selection may produce biased estimate of the impact of the exposure to Ramadan. Thus, this paper contributes to a set of literature that documents importance of taking selection into account for causal identification. Thomas (2009), and Brown and Thomas (2011) document selection in exposure to 1918 influenza epidemic in USA. Prior to their papers, Almond (2006) shows that exposure to Influenza epidemic have implications for human capital formation. Brown and Thomas (2011), however, find that fathers of those who were exposed were from lower SES, were older, had higher fertility, less likely to be white and less likely to be veteran of WW1. Controlling for these observables and using similar exposure definition as it was defined by Almond (2006), they do not find any effect of Influenza on child human capital formation and labor market outcomes. Buckles and Hungerman (2012) document variation in maternal characteristics with respect to season of birth. They find that winter births disproportionately realized by teenagers, unmarried women, less educated mothers, and white women. Their findings have implications for using season of birth as an instrument for schooling in estimating returns to schooling. Prior to their paper, Lam et al. (1994), using data from USA, show that parents are more likely avoid conceptions during extreme summer heat and seasonal variation in births differs between two races namely the white and the black. Thomas (2009) documents selection in fertility during famine in Bangladesh. Using the MHSS 1996, he finds that during the famine in 1974 parents who gave birth were different in observable characteristics than who did not. Pitt (1997) describes the importance of taking into account fertility and mortality selection in estimating determinants of child health. Following Pitt (1997), Maitra and Pal (2008) document the importance of fertility selection in estimating relation between birth spacing and child mortality.

3 Matlab Family Planning and Child Health

Matlab is a *thana* (sub-district) in Chandpur District in Bangladesh. It is located 55 kilometers of South-East of Dhaka. The International Center for Diarrhoeal Disease Research, Bangladesh (ICDDR,B) established a demographic surveillance system (DSS) in 1966. In the DSS areas, the record of birth, death, and migration (in and out) are collected from the start of the project. In October 1977, the DSS areas were contracted to 149 villages by excluding 84 villages. The family planning and health project was launched in 70 villages (treatment areas) and the remaining villages were comparison areas. No report of using randomization mechanism has been found (Schultz 2009). The figure (1) in appendix also shows that the treatment areas were grouped into clusters[9]. Schultz (2009) argues that the clustering of villages into the treatment areas retain the spillover effect. Table 3 in appendix presents 1974 census data which shows that the treatment and the comparison area were very similar except for few observable characteristics such as sources of drinking water, number of cows, and age of both household head and his/her spouse.

Based on several studies, Joshi and Schultz (2013) point out the contraceptives use went up from 7% to 33 %. Sinha (2005) finds that the family planning program in Matlab lead to 14% reduction in life time fertility. Barham (2012) also describes the other treatments added to the treatment areas which are documented in Bhatia et al. (1980), Phillips et al. (1984) and Koenig et al. (1990). In October 1977, the family planning program began in the treatment areas through the provision of modern contraceptives. From June 1978, pregnant women received tetanus toxoid vaccination, and also pregnant women in their last trimester pregnancy received iron and frolic acid tablets. From March 1982, the children aged from 9 months to 59 months in the treatment area 1 received measles vaccine. This program was expanded to treatment area 2 on November 1985. From January 1986, DPT, polio, and tuberculosis immunization were given to children under age 5. Later in 1986, Vitamin A supplementation for children under age 5 and nutritional rehabilitation for those who were nutritionally risky were added to the treatment areas. In appendix section, I reproduced the table 1 from Barham (2012) which gives a summary of the programs introduced in the treatment areas and age cohorts the programs have affected.

4 Data

Matlab Household and Socioeconomic Survey

The first source of data used in this study is the Matlab Household and Socioeconomic Survey (MHSS) 1996 which was funded by National Institute of Aging and was collaborative effort of RAND, the Harvard School of Public Health, the University of Pennsylvania, and the University of Colorado at Boulder. The primary sample was drawn from a probability sample of 2,883 *baris* from 7,440 *baris* in the DSS 1994 sample frame. *baris* usually consists of cluster of households in close physical proximity. In all *baris*, interviews were completed in 2,781 out of 2883 eligible *baris*. Within each bari, up to two households were randomly selected. For each *baris*, one household was randomly chosen and designated as primary household[10]. If there are more than two households, the second household was randomly chosen and termed as secondary household[11]. Otherwise, the second household was designated as the secondary household. Out of the 2,781 *baris*, 94 *baris* were inappropriately interviewed and therefore disregarded from analysis which leaves us with 2,687 *baris*. Out of these *baris*, 656 *baris* consisted of one household, and rest of them had two or more households. Ideally, there should be 2,013 households but the survey team could find only 1,677 households. The remaining secondary households are purposive sample based on relationship to the first household. In this paper, I limit my analysis only to primary households.

[9] Figure 1, table 2 and 3 are reproduced from Barham (2012).

[10] In the data set, the primary households are denoted as $status = 1$.

[11] In the data set, the secondary households are denoted as $status = 2$.

In the survey mothers were asked about birth dates of each of their children. Later the birth dates were matched with the DSS data sets for their consistency and accuracy. Although the DSS started in 1966, the events (i.e. birth, marriage) which took place beginning from 1974 were linked to computerized system of DSS. During the data collection process of the MHSS, the consistency and accuracy of the dates of the events (i.e. birth, marriage) was only checked for the data which was computerized. Therefore, I have reliable birth dates for 22 birth year cohorts from 1974 to 1995. All birth dates before 1974 are self reported (Rahman et al., 1999). There are also some other limitations with this data. I obtained the birth data from the pregnancy history of the women interviewed in the MHSS 1996. This is a limitation because I can know about births prior to 1996, only if the women living in sampled household survived until 1996.

Another limitation of the data is that for some births only month and year of birth are known, and the birth dates are replaced with zero. It also varies considerably between the treatment areas and the control areas. There are 2086 births which had date *zeros* out of 8573 from 1974 to 1995. Out of the 2086 births, the treatment areas had 856 births and the control areas had 1230 births[12]. Moreover, among the births which have birth date information, 11 % of births were reported to take place on date 1. I drop these births which report date 1 to avoid wrong classification of births relative to the time of Ramadan they were born. The difference in probability of reporting date 1 is not statistically significant between the treatment and control areas.

After dropping the unknown birth dates, months and years, the final data set consists of 5754 births for birth year cohort from 1974 to 1995. Out of 5754 births, 2638 births are from the treatment areas and 3116 births are from the control areas. Out of 2638 births in the treatment areas, 2290 were born to Muslim mothers and 348 were born to Hindu mothers. Out of 3509 births in the control areas, 2981 were born to Muslim mothers and 135 were born to Hindu mothers.

For the height analysis, I restrict the sample to the birth cohorts born between 1986 and 1995. Therefore, the oldest child in the height sample was less than 10 years old in 1996. This limits the concern that the children in the sample may have migrated away as well the concern regarding adolescent growth spurt (Falkner and Tanner, 1986). In the MHSS 1996, height was not measured for all children but only 2 randomly selected out of all the children present in the household (Rahman et al., 1999). After limiting sample to children whose birth dates which are not missing or reported date 1, there are 669 males and 655 females.

To study the association between exposure to Ramadan and child height, I match the birth dates from the mother's pregnancy with the birth month and year of the individuals surveyed in the Matlab. I later match anthropometric data for each individual. I limit this study to only single birth. I found only 28 twins in this data.

In the MHSS, the village level facility information was collected for 140 out of the 141 villages. Therefore, when the village level time varying characteristics are included in the regression specification, the sample would be based on 140 villages.

Another concern is the defining for years of education. The years of education is only known if the household member went to a formal school. An overwhelming majority of the household members, if educated, go to a formal school. However, some went to Madrasa, BRAC school or Maktab [13]. Some Madrasa and BRAC schools also provide educations very similar to formal schools and curriculum approved by the government. It is not clear whether the household

[12]This could be due to the fact that family planning lead to decrease in total number of births in the treatment areas. Hence missing births also declined in treatment areas.

[13]Less than 3.5 percent of all the births can be attributed to women who went to these schools.

member went to a Madrasa or BRAC school which provide formal education or not. I define zero years of education if the household member went to Madrasa, BRAC and Matab to avoid missing data.

Table 1 summarizes the data from Matlab. The mothers on the average have 2.17 and the fathers on the average have 3.28 years of education. The average age of mothers at the survey is 37.40. About 13 % of the total births were not exposed to Ramadan at the time of pregnancy and 8 % the total births were born during Ramadan. This implies 79% of the births were exposed to Ramadan at some point during the pregnancies[14]. In the sample, 8 % of total births are Hindu births. 52 % of the total births resulted in male births which is close to the natural sex ratio at birth.

Indonesian Family Life Survey

In this study, the second source of data comes from all four waves of the Indonesian Family Life Survey (IFLS) which were conducted on 1993, 1997, 2000 and 2007 respectively. At the time of the first survey 1993–94, the sample drawn was representative of 83% of the population residing in 13 provinces out of 27 provinces of Indonesia's. Within each of the 13 the provinces, the enumeration areas (EA) were randomly selected for inclusion in the final survey. In the first wave, 7224 households were interviewed and detailed individual level information were also collected including the age and education of the household members. The later waves sought to follow up the same household and the re-contact rates of households from first wave were 94.4%, 95.1% and 93.6% in the second, third and fourth wave respectively. The low attrition rate is one of the appealing features of IFLS. The attrition rate is lower because the migrant households and individuals were carefully tracked in the later waves. Thomas et al. (2012) point out that the migrant and non-migrant individuals are different in observable characteristics. The design of IFLS to follow up on the migrant households and individuals reduces the concern of any selection bias due to migration and disappearance from the original households.

During each wave of the survey the reproductive age women were asked about their pregnancy histories. These pregnancy histories includes the birth date, birth outcome, gender of the child, age of the mother at birth for all the births. All the information regarding birth dates and outcomes are self-reported. For the purpose of the analysis of this paper, I restrict the births which took place within the last five years from the survey wave year to reduce the recall bias. This gives us 12,331 births from all four waves of the IFLS. Out of these births, 1430 births have some missing birth dates and months. The information on mother education is missing for about 59 births. Moreover, the community information is also missing for about 144 births. After dropping these births, the final sample consist 10,753 [15]. One key difference between this paper and other studies which used IFLS is that the other studies do not use the data from the pregnancy histories of women to examine selection in Ramadan exposure. Ewijk (2011) and Ewijk et al. (2013) use the third wave of the IFLS and Majid (2015) use the fourth wave of the IFLS. However, the advantage of using pregnancy history is that pregnancy history contains information on all births.

One advantage of the IFLS over the MHSS is that the selection in maternal mortality is less of a concern. In the MHSS, I have information on pregnancy history for last 22 years for women who were alive during the survey year. With all four waves of the IFLS, I can restrict the pregnancy histories of the women for up to five years and still manage to get birth information

[14] Some births may still get classified as being exposed. However, past literature have used a very similar exposure definition.

[15] Majid (2015), Ewijk (2011) and Ewijk et al. (2013) also take similar approach to missing birth dates. However, they loose a much higher percentage of sample due to missing birth dates.

for 16 birth year cohorts[16].

Table 2 summarizes the sample from IFLS. The mothers on the average have 7.45 years of education which is far more than the average years of education of mothers in MHSS. The mothers on the average are 28.89 years old at the time of survey. Interestingly, the proportion of children who are not exposed to Ramadan *in utero* is 13% which is very similar to MHSS. Moreover, the proportion who are born during Ramadan is again 0.08%. The non-Muslim accounts for 11% of the births. About 51.2% of the total births in IFLS resulted in male births.

5 Ramadan Measures

For a given year, the dates of the Western calender which overlaps with the Ramadan Month of the Islamic calender can be matched [17]. From the birth data only birth date of an individual is known but gestation period is unknown. However, it can be observed that how many months after Ramadan an individual 'i' was born. Generally, the gestation period for human is about 280 days long. For each date of birth, I construct century day code (CDC) following Almond and Mazumder (2011). Here, $ramadan_0$ is the month of the month of Ramadan and takes a value of 1 if the individual 'i' was born during the month of Ramadan and 0 otherwise. Similarly, $ramadan_1$ would be the next month after the month of Ramadan and takes a value of 1 if the individual 'i' was born next 30 days after Ramadan and 0 otherwise. Thus, $ramadan_X$ would imply X month after Ramadan and would take a value of 1 if the individual 'i' was born during the X month after Ramadan and 0 otherwise. Based on the definition of these variables, for $X < Y$, $ramadanXtoY$ can be constructed where $ramadanXtoY$ takes a value of 1 if the individual 'i' was born exact X to Y months after Ramadan and 0 otherwise.

Given that the gestation period of human is approximately 9 month long, the individuals who were born during $ramadan_{10}$ and $ramadan_{11}$ are not exposed to Ramadan *in utero*. A variable *nonexposed* is constructed that takes a value of 1 if an individual is born 10 to 11 months after Ramadan and 0 otherwise.

6 Empirical Strategy

The discussion on empirical strategies is divided in three subsections. First subsection describes the potential empirical challenges. Second subsection discusses the empirical frameworks for examining the impact of the family planning on change in birth time relative to Ramadan and change in parental selection in Ramadan exposure. Third subsection discusses the empirical frameworks for analyzing association between Ramadan exposure and the child height.

Empirical Challenges

Endogenous program placement — as described earlier, the family planning program in Matlab was not randomly allocated between the treatment and control areas. Although the treatment and control areas were very similar in observable characteristics in 1974 (Barham, 2012; Joshi

[16] I have only 16 birth year cohorts due to the fact that IFLS 2 and 3 were conducted in less than five years from the previous wave. The birth history was restricted up to the last wave.

[17] Following Almond and Mazumder (2011), we construct the Ramadan month from Institute of Oriental Studies at the University of Zurich using their website www.oriold.uzh.ch/static/hegira.html

and Schultz, 2013), they could potentially differ in unobservable dimensions. In the regression equations village level fixed effects are included which will control for all the village level time invariant unobservables. As long as the placement of the family planning program are not correlated with time varying unobserved factors, the difference-in-differences strategy will provide us with unbiased estimate.

Selection in birth — the MHSS birth sample comes from pregnancy histories of the women. One key issue, in the case of the MHSS, is that because of family planning program some women may choose to avoid giving birth completely. For example, family planning program may allow some parents to delay births who may not have done so in the absence of the program. These parents may also not have avoided birth relative to Ramadan, had they chosen to have given births. Thus, in the data only those parents who have given birth and avoid birth relative to Ramadan might be observed.

To address this concern, the proportions of women who had ever given a live birth between the control and treatment areas are compared in Table 3. Panel A of Table 3 shows about 93–94% of the women have at least one birth and women in the treatment areas are 1% less likely to give birth but the difference between two areas is not statistically significant. Among those who had not given a birth yet (panel B), the years of education of the women, education of their husbands, their ages at the time of survey are compared. The married women who had not given a birth yet in the treatment areas are older, less educated and has husband with less education than their counterparts in the control areas. However, these differences are not statistically significant. A similar analysis is performed on the Muslim married women who had not given a birth yet (Table 4) and the conclusions remain the same. It is also important to note that with the MHSS, I analyze birth cohort spanning over 22 years and in Matlab where eventually almost every woman gives birth to a child. Given that there exists a large number of birth cohorts and the fact more than 90 percent of women had given one live birth at some point her reproductive age, selection in live birth is of less concern in this context.

Maternal mortality and migration attrition — the birth sample in the MHSS comes from the pregnancy histories of the women observed in Matlab in 1996. The family planning program and maternal health interventions may affect the mortality of the women in the treatment areas. It is not clear how this will affect the bias. If women from the lower SES are more likely to benefit from these programs and they are less likely to avoid births relative to Ramadan, then we will observe a downward bias in the estimation of the program effect. On the other hand, if women from higher SES are more likely to benefit from these programs and they are more likely to avoid birth relative to Ramadan, we will observe an upward bias. Ronsmans et al. (1997) find that maternal mortality from all causes declined in both the treatment and control areas from 1976 to 1993, but the difference is not statistically significant between the treatment and the control areas.

Migration may affect the results, if the migration pattern differs between the treatment and control areas because of the program placement. Joshi and Schultz (2013) point out that the relation between fertility outcomes and female migration in and out from the Matlab study areas is not statistically significant. Barham and Kuhn (2014) find that male and female out migration was higher in the control areas between 1983 and 1988. They attribute the lower out migration in the treatment areas compared to that in the control areas to the health interventions implemented in the treatment areas during 1983–88 period. The results of female out migration in the study of Barham and Kuhn (2014) is not robust to inclusion of village fixed effects. As mentioned earlier, village fixed effects are included in all regression equations employed in this paper. In addition to this, one advantage of this analysis here is that program effect is studied both for Muslims and non-Muslims. If migration is responsible for the birth pattern relative to Ramadan, this may show for non-Muslims as well— as the data for both Muslims and non-Muslims are available from both the treatment and control areas. Furthermore, Joshi and Schultz (2013) show that pre-family planning program fertility rate is very similar between the treatment and the control areas using the MHSS data. Barham (2012) shows that the levels of cognitive functioning, in the MHSS data, are very similar between the individuals from the treatment and control areas who were born prior to the health interventions in the treatment areas. In the context of Ramadan exposure, this study also do not find any difference in Ramadan exposure between the two areas prior to the implementation of the family planning program.

Another concern is that the information on birth location of the births are missing in the data. In the paper, it is assumed that the location of the mother at the time of survey as the birth location of the individuals born in the Matlab. Joshi and Schultz (2013) point out that female migrations to the control areas from the treatment areas and vice-versa are very low. Moreover, Joshi and Schultz (2013) and Sinha (2005) also consider that the location of the mother at the time of the survey as the birth location of the children born in Matlab.

In the case of the IFLS, selection due to maternal mortality is less of a concern as the subsequent waves were conducted at sufficient short intervals and pregnancy histories from at most last five years of each survey wave are used for the analysis of this paper. Moreover, selection due to migration is even lesser of a concern as the survey team carefully followed the migrant households and individuals in the subsequent waves.

Trend difference — another key concern in any difference-in-differences strategy is that the treatment and control villages may experience different time trends and establishment of other facilities. The difference-in-differences point estimate may capture the inherent trend differences between the treatment and control areas. As long as the trend is similar for both Muslims and Hindus, the point estimates give us the causal effect of the family planning program. I also control other time varying characteristics such as timing of electricity connection, village tube-well establishment, satellite clinic establishment, and timing of the family planning program interacted with the large market[18]. Electricity may allow parents to access information about contraceptives from television and radio which in turn may affect the use of contraceptives. Satellite health clinic may also provide provide contraceptives or information on using contraceptives. Proximity of a large market may reflect the cost of getting contraceptives. Nearness of tube-well will affect cost of getting water for women.

Birth Timing

To examine the impact of the family planning program in Matlab on the fertility selection relative to Ramadan, birth data is needed both before and after the initiation of the program. Recall that reliable birth data from the MHSS is available for 22 birth year cohorts born from 1974 to 1995 for both the treatment and control areas, and the family planning was initiated in October, 1977. It could be tempting to think that a difference-in-differences strategy can be applied on Muslim births to get the causal estimates of the effect of the family planning program on fertility selection relative to Ramadan controlling for fixed seasonal variation in births. However, one key concern still remains for causal identification. Ramadan completes the full circuit of the western calender in 33 years, but the MHSS has reliable birth data for only 22 birth cohorts. As a result, Ramadan is not balanced with respect to seasonality. To analyze whether this is a concern, a difference-in-difference strategy can be applied on the Hindu births similar to that applied on the Muslim births. The underlying assumption is that both

[18]In MHSS 1996, there is only information whether the village has large market but no information on from what year it has large market.

Muslim and Hindus are affected by residual seasonality, if there is any, and only Muslims have incentive to respond to the family planning program in timing birth relative to Ramadan. If the difference-in-differences estimate on Hindu births is not statistically different from zero but on Muslim births it statistically different from zero, this would imply that Muslims are indeed timing birth relative to Ramadan and the difference-in-differences estimate provides us a causal estimate.

I estimate the following linear probability model for individual 'i' whose mother lives in village 'j' and was born in month m and year t:

$$R_{ijmt} = \beta_1 Post_{mt} + \beta_2 Post_{mt} \times Treated_j + \beta_3 X_{ijmt} + \beta_4 \theta_{jt} + \gamma_m + \delta_t + \theta_j + \epsilon_{ijmt} \quad (1)$$

The dependent variable R takes various measures of birth in months from Ramadan. These measures take a value of 1 if true and 0 otherwise. For example, if R represents the variable $ramadan8to9$, in equation (1) $ramadan8to9$ would take a value of 1 if an individual 'i' born whose mother lives in j in month m and year t were born 8 to 9 months after Ramadan and 0 otherwise. It is assumed that the individuals are born in the same locations their mothers live at the time of the survey[19]. $Post$ takes a value of 1 from August from 1978 and 0 otherwise. Although the family planning program started in October, 1977 (Joshi and Schultz, 2013), the female village workers started working in November, 1977 (Bhatia et al., 1980)[20]. $Post$ is identified because the year fixed effects do not absorb the coefficient of *post*, as the program did not start from the beginning of a year. The $Treated$ variable takes a value of 1 for villages which received the family planning program and 0 otherwise. The interaction term $Post_{mt} \times Treated_j$ captures the effect of contraceptive program on birth timing. In the regression equation (1), the village fixed effect θ_j absorbs the main effect of $Treated$ as well as controls for the village level unobservables. X_{ijmt} is set of mother level observables like mother education in years and mother age at the time of survey. The mother ages at the survey are specified as spline functions with knots at every 5 year interval starting from age 15 to allow flexibly for non-linearities. θ_{jt} is set of village level time varying characteristics such as from what year the village has tube-well, electricity, satellite health clinics, and post interacted with whether the village has a large market. I regress the equation (1) by splitting the sample by Muslim and Hindu and compare the coefficient of $Post_{mt} \times Treated_j$ for both Muslims and Hindus. Since only Muslim population is affected by fasting during Ramadan, it allows us disentangle whether or not the change in birth relative to Ramadan after the initiation of the intensive family planning program is due to Ramadan or any other event which may affect both Muslims and Hindus[21]. In a full sample combining the birth sample of both Hindu and Muslims, I can recover the point estimates for Muslims and Hindus by interacting $Post_{mt} \times Treated_j$ and other relevant variables with Hindu and compare it with $Post_{mt} \times Treated_j$. The results will not differ in a split sample and full sample as long as same fixed effects and controls are included in the regression equations.

The provision of a family planning program may also change the observed characteristics of the children who are exposed to Ramadan and not exposed to Ramadan *in utero*. Here, the observed characteristic of interest is mother years of education, as based on several studies Schultz (2002) point out mother years of schooling has more effect on child health, schooling, and adult productivity than same years of father's education. Moreover, there is a substantial

[19]This is not a serious concern, as Joshi and Schultz (2013) point out migrations from the treatment to the control areas and vice-versa are very low.

[20]The start date of the program and the post date is different because those who were conceived before November 1977 were not exposed to the program. Thus post starts from August, 1978 nine months from the start of the program.

[21]During the month of Ramadan the prices of goods and services and number of national holidays may change and the mothers may want to time birth based on that. The underlying assumption is that both Hindus and Muslims are affected by the changes in prices and other events like number of national holidays.

literature which documents that the better mother education is positively associated with better child height and BMI (Behrman and Deolalikar, 1988; Behrman and Wolfe, 1989, 1984; Strauss and Thomas, 1995, 1998).

To analyze selection in Ramadan exposure, I limit the sample to birth cohorts born from 1974 to 1982. The family planning program may affect the female education who in future will be mothers. Thus, limiting the sample to a smaller birth cohorts reduces the concern that the education of the mothers, who are giving birth from 1974 to 1982, will be affected by family planning program. I divide this sample in three categories: Muslim births in the control areas, Muslim births in the treatment areas, and Hindu births in the treatment areas; and I estimate the following linear probability model for individual 'i' whose mother lives in village 'j' and was born in month m and year t:

$$nonexposed_{ijmt} = \alpha_1 motherage_{ijmt} + \alpha_2 mothereduy_{ijmt} + \alpha_3 Post_{mt} \times mothereduy_{ijmt} + \nu_m + \rho_t + \vartheta_j + \varepsilon_{ijmt} \quad (2)$$

The variable *nonexposed* takes a value of 1 if children were not exposed to Ramadan during pregnancy and 0 otherwise. The variable *mothereduy* is measured as years of education and *motherage* is measured as age at the survey. ρ_t , ϑ_j and ν_m are year fixed effects, village fixed effects, and month fixed effects respectively. With equation (2), the consequence of ignoring placement and timing information of the family planning program can be analyzed. For that, the variable $Post \times mothereduy_{ijmt}$ will be omitted and equation (2) will be regressed adding the Muslim birth sample from both the treatment and control areas for the same birth cohorts.

Recall that association between Ramadan exposure and child height is studied for children who are less than 10 years old. Therefore, the oldest children in the sample are born in 1986 when the family planning is already in place in the treatment areas. Therefore, a variant of equation (2) will be used where instead of post $treated_j$ will be interacted with *mothereduy* and *Block* will interacted with ρ_t to capture the additional interventions implemented in the treatment areas.

With all four waves of pregnancy history data from the IFLS, whether exposure to Ramadan varies by mothers' level of educations is examined. For that purpose I use a variant of equation (2) where instead of village fixed effects, I apply community fixed effects. In addition to month and year fixed effects, birth date fixed effects are included in the model[22]. The standard errors are clustered at community level instead of village level. It is important to reiterate that with the MHSS data we are examining selection in Ramadan exposure due to family planning program but with the IFLS we are only examining association between Ramadan exposure and mother education.

In utero Ramadan Exposure and Child Height Correlations

If we find that parents are selectively timing birth relative to Ramadan, it is important that we study the effect of maternal fasting on child health controlling for selection. Although the family planning program is exogenous to households in Matlab, it may affect the child health through the child quality quantity trade off. Therefore, due to exclusion restriction violation the family planning program can not be used as an instrument for birth time relative to Ramadan

[22]Since the IFLS data is based on self-reported birth dates, this would reduce the concern if there is any systematic relation between date of birth and Ramadan exposure during pregnancy.

in child health outcome function. However, the correlations between Ramadan exposure and child height can be examined in both the treatment areas and control areas. If a mother with higher SES avoids pregnancy overlapping with Ramadan, we may find the correlations between pregnancy overlapped with Ramadan and child height is negative. On the other hand, if a mother with higher SES does not time birth relative to Ramadan in control areas because of the absence of the family planning program, we may not find any correlation between child height and pregnancy overlapped with Ramadan. Under this scenario we should be worried that perhaps selection is driving the results rather than maternal fasting during Ramadan.

I regress following equation for individual 'i' whose age is 'a' living in village j and born in month 'm' :

$$\begin{aligned} H_{ijma} = \Gamma_1 Exposed_{ijma} + \Gamma_2 BornduringRamadan_{ijma} + \Gamma_3 Hindu_{ijma} + \\ \Gamma_4 Hindu_{ijma} \times Exposed_{ijma} + \Gamma_5 Hindu_{ijma} \times BornduringRamadan_{ijma} + \\ \Gamma_6 malec \times Hindu \phi_m + malec \times \pi_a + \varphi_j + \xi_{ima} \quad (3) \end{aligned}$$

When the outcome of interest is child height (H). Currie and Vogl (2013) argues that child height is a good proxy for child birth weight. Height represents long run nutritional status (Strauss and Thomas, 2007). The variable *Exposed* takes a value of 1 if the child was born between 1 and 9 months after Ramadan and 0 otherwise. The variable *BornduringRamadan* takes a value of 1 if the child was born during the month of Ramadan and 0 otherwise. *BornduringRamadan* is defined separately from *Exposed* because individuals who were born during Ramadan were partially exposed to Ramadan *in utero*. The omitted category in the regression equation (3) are the individuals who are not exposed to Ramadan. ϕ_m, δ_a and φ_j are birth month, age in months fixed effects and village fixed effects respectively. The *malec* takes a value of 1 if the child is a male and 0 otherwise. The interaction term $malec \times \pi_a$ absorbs the main effect of *malec*. Moreover, it takes into account differential growth pattern for male and female children. The standard errors in all regression equations are clustered at village level.

7 Results

Family planning program and Birth Timing

To study selective timing of birth, regression of $ramadan_X$ on *post*, $post \times treated$ are performed, where X takes the values 0 to 11, controlling for birth month and year fixed effects as well as village fixed effects. The results are presented in Table 5. The cohorts under study were born from year 1974 to 1995. It shows women living in the treatment areas are around 3 percentage points less likely to give birth 8 and 9 months after Ramadan after being exposed to the family planning program. This demonstrates that women in the treatment areas are avoiding conceptions when Ramadan is one month away or in the month of Ramadan. On the other hand, the coefficient of $post \times treated$ is positive for $ramadan_4$ to $ramadan_7$ and except for $ramadan_6$. For $ramadan_6$, the coefficient of $post \times treated$ is negative but close to zero. For $ramadan_0$ to $ramadan_2$ the $post \times treated$ coefficients are positive but close to zero. It is important to note that in Table 5, the effect of family planning program is examined on the birth for each separate month relative to Ramadan. Grouping the months would allow us to gain more statistical power as well as be more informative. Based on the sign of coefficients, I construct variables $ramadan4to7$ and $ramadan8to9$ where for example $ramadan8to9$ takes a value of 1 if individual 'i' was born either during $ramadan_8$ or $ramadan_9$ and 0 otherwise. Each of $ramadan8to9$ and $ramadan4to7$ are then regressed on the same independent variables including maternal years of education and spline fucntion of mother age at the survey with several knots. To examine that these results are not driven by any other event other than Ramadan, similar empirical strategies are applied on same Hindu birth cohorts. Biological mother fixed effects, which absorb the mother time invariant preference for fasting, mother education, mother age at survey, time invariant health and nutritional status such as height and time invariant asset level, are also applied as a further robustness check.

In Table 6, the impacts of the family planning program on the births during $ramadan8to9$ for both Muslims (column 1–3) and Hindus (column 4–6) are presented. It shows that Muslims women—living in the treatment areas— are 6.2 percentage points less likely to give birth 8 and 9 months after Ramadan (column 1) following the family planning program. After the inclusion of other village level time varying characteristics the magnitudes drops slightly but still remain significant (column 2). The inclusion of biological mother fixed effect in contrast increases the coefficient magnitude (column 3). On the other hand, for Hindus living in the treatment areas, no change in birth during $ramadan8to9$ is found following family planning program in all three specifications (column 4–6).

Similar to Table 6, the effect family planning program on birth during $ramadan4to7$ is analyzed in Table 7. Here, it shows that provision of a family planning program actually increased the Muslim births by 8.6 percentage points during $ramadan4to7$ (column 1). For Hindus, no change in birth during $ramdan4to7$ (column 4) is found. The inclusion of village level time varying characteristics do not change the result for Muslims (column 2) that much and for Hindus it gives slightly positive estimate (column 5) but still much less than that for Muslims. Even after the inclusion of biological mother fixed effects (column 3), the coefficient magnitude are very similar for Muslims. For Hindus, however, the coefficient magnitude is -0.048. It should be noted that when biological mother fixed effects are applied, the coefficient of Post× Treated are only identified within the same mothers who gave birth before and after the family planning program. The sensitivity of the coefficient of $Post \times Treated$ for Hindus, when biological mother fixed effects are applied, could be due to small number births by the same mother before and after the family planning program. To examine results are not sensitive to splitting the sample in Hindu and Muslim births, for the same dependent variables as in Table 6 and Table 7, Hindu is interacted with $Post \times Treated$ and other relevant variables, and regressions are employed on the full sample. The results are presented in Table A1 which show that the same results as in Table 6 and Table 7.

Birth Relative to Ramadan and Child Height

Based on the evidence provided by earlier literature, it becomes important to examine the effect of exposure to Ramadan on child health. One advantage in this study is that the association between Ramadan exposure and child health outcomes can be studied for both the treatment and control areas. As mentioned earlier, child height is used in this paper as a measure of child health, as height represents long term nutritional status (Strauss and Thomas, 2007). The results are presented in Table 8. Pooling the height data of Muslim children from treatment and control areas who are less than 10 years old at the time of survey, it is found that Ramadan exposure is associated with about 2.78 centimeter decrease in child height (column 1) and it is also significant at the 10 percent level. However, when the analysis is done separately for control and treatment areas, the results are striking. In control areas (column 2) exposure is associated with decrease in height by 1.3 centimeter whereas in treatment areas exposure is associated with decrease in height by 4.42 centimeters (column 3). To explore the possibility that anything other than Ramadan is responsible for this association in column (3), the association of Ramadan exposure with child height are analyzed for both Muslim and Hindu children in treatment ares (column 4). The coefficient of *Exposed* for Muslims in column (4) is similar to that in column (3) but for Hindus it is positive and joint F-test is statistically insignificant. This implies the negative association between Ramadan exposure and height is only present for Muslim children in the treatment areas. These results on height are puzzling. Suppose we had height data of the children only from the treatment areas and produced the results in column (3) and (4) in Table 8 following the empirical strategy of the earlier studies (Almond and Mazumder, 2011; Ewijk, 2011; Almond et al., 2014; Majid, 2015; Karimi, 2014; Ewijk et al., 2013) we might have incorrectly concluded that exposure to Ramadan leads to shorter stature. However, when we have data height data from both the treatment and control areas, we can not draw the same conclusion even after using the same empirical strategies.

One explanation for observing negative effects in the treatment areas but no effects in control areas is that survival bias could be different between the two areas. If children exposed to Ramadan, who might have been shorter on average, did not survive till the survey period in the control areas, we might have observed similar relation between exposure to Ramadan and child height. However, statistical difference in mortality of children between the treatment and control areas is not found in the data. Moreover, it should be noted that mortality is also an extreme event.

It could be also possible that that the pregnant women in the treatment areas are more likely to fast during Ramadan compared to the control areas. It is impossible to rule out such possibility, as I do not have information on the fasting behaviors of women residing in the treatment and control areas. However, such differences might seem unlikely due to the geographical proximity of both areas.

Apart from these possibilities, we might observe the results presented in column (3) and (4) of Table 8, if there is parental selection in Ramadan exposure. If parents with higher SES background are less likely to overlap pregnancies with Ramadan, we might observe the difference in child health outcomes between children who are exposed and not exposed to Ramadan. Even though the treatment and control areas are very similar in observable dimensions in 1974, reproductive age women, in the treatment areas in Matlab, were provided with an intensive family planning program starting from October,1977. Following the family planning program, mothers from different SES backgrounds may respond differently in relation to overlapping their pregnancies with Ramadan. Among the SES characteristics, mother education is considered to be an important determinant of child health outcome (Schultz, 2002). The possibility whether more educated mothers in the treatment areas are less likely to overlap their pregnancies with Ramadan is considered in the next subsection.

Selection in Ramadan Exposure

Recall the child height were studied for children who are less than 10 years old at the time of survey or possibly birth cohorts born between 1986 and 1995, and the family planning program was initiated in October, 1977 much before these children were born. Before examining parental selection in Ramadan exposure for the same birth cohorts whose heights were studied in Table 8, it is important to examine parental selection in Ramadan exposure before the placement of the family planning program. It could be possible that Muslim mothers, living in the treatment areas, selectively overlap their pregnancies to Ramadan even before the start of the family planning program. If that happens and we also find parental selection for the birth cohorts who were born after the program, we might incorrectly attribute the observed selection in Ramadan exposure to the family planning program. In Table 9, the likelihood of parental selection in Ramadan exposure before the program placement is examined. It shows that there is no difference between the treatment and control areas in terms of Ramadan exposure and parental SES measured by mother years of education, father years of education, and mother age.

Table 10 shows association between exposure to Ramadan and mother years of education in the treatment and control areas for birth cohort born between 1982 and 1995. The oldest individual in this sample is 14 years old and it also encompasses the birth cohort born between 1986 and 1995. Joshi and Schultz (2013) also study these births cohorts to analyze the impact of the family planning program on the child health and education outcomes. The results in column (1) show that in the control areas there is no difference in mother years of education between children exposed and not exposed to Ramadan. On the other hand, the results in column (2) show that in the treatment areas more educated Muslim mothers are less likely to expose their children to Ramadan at the time of pregnancy. In contrast to the results in column (2), the results in column (3) shows that in the treatment areas more educated Hindu mothers are more likely to overlap pregnancies with Ramadan, but the association is not statistically significant.

Table 11 documents the consequence of ignoring placement information of a program, for instance a family planning program, that can induce selection in Ramadan exposure. The first two columns are based on the birth cohorts born between 1982 and 1995 and last two columns are based on the birth cohorts born between 1986 and 1995. Only Muslim births are considered for this analysis. In column (1), *treated* is not interacted with *mothereduy*, and it shows that more years of mother education has zero or no relation with Ramadan exposure. In contrast, when *treated* is interacted with *mothereduy*, the coefficient of the interaction term is positive and larger than that of *mothereduy* and the joint F test of *mothereduy* is statically significant (p-value= 0.02). For birth cohorts born between 1986 and 1995, the results in column (3) and (4) reflect findings in column (1) and (2) respectively. Given that mothers in Matlab, on the average has very low level of education, it could be possible that the relations observed in column (3) and column (4) are driven by outliers. To check that it is not the case, a similar analysis is performed replacing *mothereduy* with *motheredu* where *motheredu* takes a value of 1 if mother has atleast 1 year of formal education and 0 otherwise. The results, presented in Table A2, are very similar to that of Table 11. Therefore, the conclusions remain the same.

One concern in empirical frameworks presented in Table 10 and Table 11 is that the family planning program may affect the female education. Thus, it could be possible that the family planning is affecting the selection in Ramadan exposure and mother years of education is an intermediary variable. To address this concern, an analysis similar to that in Table 10 and Table 11 is performed restricting the birth cohorts born between 1974 and 1982. This gives about four and half years of birth sample before and after the initiation of the family planning program. The results are presented in Table 12. Column (2) shows, no change in selection in Ramadan exposure post initiation of the family planning program in the control areas which is as expected, since the control areas did not receive the family planning program. However, column (3) shows that in the treatment areas more educated mothers are less likely to expose their children to Ramadan following the family planning program even after controlling for month fixed effects which captures seasonality. However, there could still be residual seasonality which may not have captured by the month fixed effects. It could be still possible that parents are timing birth against the any other factor instead of Ramadan[23]. If that is the case, similar selection pattern would be observed for more educated Hindu mothers living in the treatment areas. Column (4) in Table 12 shows no change in birth pattern for more educated Hindu mothers. This result validates the findings that more educated Muslim mothers are less likely to overlap pregnancies overlapping with Ramadan, if they are provided with a family planning

[23]For example, parents may face both high temperature and Ramadan at the same time but parents may want to time conceptions based on temperature rather than Ramadan.

program. With Table 12, the consequence of ignoring timing and placement of family planning program can be analyzed. In column (1), the birth data from the treatment and control areas are pooled and it shows that there is no selection. In contrast, column (3) of Table 12 shows substantial selection in Ramadan exposure.

To explore whether or not selection in Ramadan exposure is limited only to Matlab, Bangladesh, data from Indonesia—the IFLS is also used. As discussed in the data section, previous studies Ewijk (2011); Majid (2015); Ewijk et al. (2013), which also used IFLS, did not examine the selection in Ramadan exposure using the data from pregnancy histories of the reproductive age women. The results are presented in Table 14 and Table 15. In column (1) of Table 14, the results are presented for all the births which took place in the last five years of the survey wave. The results show that more educated Muslim mothers are less likely to overlap their pregnancies to Ramadan. However, for non-Muslim mothers, the years of education has almost zero correlation with being exposed to Ramadan. In column (2), as a robustness check, the sample is restricted to mothers whose highest education is up to 12 years [24], the results do not change. In Table 15, we define mother education as dummy variable where it takes a value of 1 if educated and 0 otherwise. Again, results essentially remain the same.

8 Discussion

In the previous section three main results are presented. First, it is shown that the provision of a family planning program leads to a decrease in births eight to nine months after Ramadan but an increase in births four to seven months after Ramadan. Given that the length of human gestation is about 9 months, it implies that Muslim mothers are avoiding conceptions when Ramadan is one month away and in the month of Ramadan. On the other hand, they are increasing conceptions when it is two to five months away. Secondly, it is shown that significant negative association between Ramadan exposure and child height only exists for Muslim children in the treatment areas but not for Muslim and Hindu children living in the control and treatment areas respectively. Thirdly, it is also shown that in the treatment areas more educated Muslim mothers are less likely to have pregnancies overlapped with Ramadan. The second and third results together implies that perhaps the observed negative relation between Ramadan exposure and the height of the Muslim children in the treatment areas is probably due to parental selection in Ramadan exposure. Moreover, it is shown that the lower likelihood between mother years education and Ramadan exposure is not limited to Matlab, Bangladesh but also is pertinent to Indonesia.

It is puzzling to observe that mothers are avoiding conceptions when Ramadan is imminent or taking place and shifting conceptions a few months ahead. If mothers are at all concerned about avoiding pregnancies with Ramadan, they should choose a time period which do not overlap with Ramadan. Then why do we also find that mothers in the treatment areas are more likely to give birth 4 to 7 months after Ramadan. I point out two hypotheses for this behavioral pattern but can not test them because of the data limitation. Firstly, it could be possible that women, who are pregnant for longer periods of time, are less likely to be asked to fast during Ramadan. Thus, if she relaxes the use of contraceptives few months ahead of Ramadan and possibly get pregnant, she may may seek exemption from fasting. On other hand, if she is pregnant for one month, she might be more likely to be asked to fast during Ramadan. In the USA, it has been found most pregnancies are not acknowledged until after the first month of gestation (Floyd et al., 1999). Unfortunately, we do not have any information on the fasting behavior of the Muslim

[24] Some mothers in IFLS who report that they went to college or beyond college but has missing years of education.

women residing in Matlab at the time of their pregnancies to test this hypothesis. Secondly, it could be possible that household expenditure for Muslims may increase during the period of Ramadan to celebrate both Ramadan and Eid al-Fitr. After the end of Ramadan, on the first day of the next Islamic month *Shawwal*, Muslims celebrate Eid al-Fitr which is one of the major festivals in the Muslim world. It could be possible that Muslim parents are avoiding conceptions when Ramadan is imminent to avoid pregnancies at the time when the household face increase in expenditure due to Ramadan and Eid al-Fitr, and this would be consistent with the findings in the earlier literature. For example, Alam and Pörtner (2013) find an increase in the use of contraceptives at the time of crop loss for agricultural households in Tanzania. Similar to their findings, Eckstein et al. (1984) find that favorable price, weather and wage shock lead to increase in fertility. Galloway (1987), using historic data from France, shows that increase in price of wheat lead to a decline in fertility of the urban poor. Therefore, increase in household expenditure can potentially explain avoidance of conceptions when Ramadan is imminent. However, it does not properly explain why they are shifting the conceptions a few months ahead. One possibility is that by the time the households recover from the phase of elevated expenditure due to Ramadan, Eid al-Fitr, and Eid al-Adha, if the women gets pregnant she can not avoid her pregnancies overlapping with the Ramadan in the following year. Recall if a woman conceives in a narrow period of about 74 days after Ramadan is over, she may completely avoid pregnancies overlapping with Ramadan[25]. This would explain why they are avoiding conceptions when Ramadan is imminent and why they are shifting conceptions a few months ahead. This hypothesis can not be tested as well because of the data limitation.

In the birth timing results, it was also found that the results are robust to inclusion of biological mother fixed effects. This result has a methodological implication. Ewijk (2011) and, following Ewijk (2011), Majid (2015) motivate to solve the problem of selection in Ramadan exposure by controlling biological mother fixed effects. Controlling for biological mother fixed effects this paper finds that time varying changes in the community affect timing of births. Griliches (1979) in his seminal paper discusses identification limitation of using biological mother fixed effects. He argues that parents may intervene based on discrepancy in endowments among siblings. Indeed, using data from Colombia, Rosenzweig and Wolpin (1988) find that intra-family health heterogeneities exist and parents also responds to those heterogeneities. They further show that application of family fixed effects are ineffective in addressing such heterogeneities, and such strategy may fail to produce consistent estimates in the presence of heterogeneities.

The second and third results have implications for empirical methodlogies employed in the existing studies which examine the impact of exposure to Ramadan on various outcomes. As mentioned earlier, existing studies rely on the assumption that parents do not time birth relative to Ramadan. Although these studies recognize that selection in Ramadan exposure can bias the estimates of interests, they do not examine selection allowing for time varying changes. If the empirical strategies of the earlier studies were followed and placement information of the family planning program were ignored, we would have incorrectly concluded there is no selection in Ramadan exposure (see Table 11 column (1) and (3)). Moreover, based on the results shown in column (1) in Table 8, we would have falsely concluded that the negative association between Ramadan exposure and the child height as causal. However, column (2) and (4) of Table 11 and column(2) of Table 10 show substantial selection in Ramadan exposure when placement information of the family planning program were taken in to account. Mother education is regarded as an important determinant of child health and also in Matlab, a positive association between mother years of education and child height is found (Table 13). As a result, it is not surprising to find that the difference in height between exposed and non-exposed Muslim children in the treatment areas are larger than that of in the control areas, once it is known

[25]Eid al-Adha and Eid al-Fitr are two biggest religious festivals in the Muslim world. Eid al-Adha takes place about 70 days after Eid al-Fitr.

that in the treatment areas more educated mothers are less likely to expose their children to Ramadan.

It is important to point out that results on the child height should be interpreted carefully. In the control areas, selection in Ramadan exposure is not found and the exposure to Ramadan is not also associated with the child height. However, this should not be interpreted as—Ramadan exposure has no effect on child height or health outcomes. The reason is that the coefficient of the variable *exposed* is an ITT (Intent to Treat) estimate which takes average impact of those who fast and who do not fast. It could be still possible that there is an effect on child health for those who fast. Moreover, health has a complex and multidimensional nature (Strauss and Thomas, 2007). It could be possible that fasting does not affect child height but affects other relevant health outcomes.

Recall that one limitation in this data set is that only birth dates of the children born in Matlab are known but their gestation periods are unknown. Therefore, it is important to evaluate whether premature births of babies can potentially confound the estimates. For premature and post-term births to explain these results, two things should also take place. Let's say that the observed decrease in births after 8 to 9 months after Ramadan was due to premature births and the conceptions of those births actually took place after Ramadan was over. To premature births to explain this pattern, mothers in the treatment areas should give birth prematurely within 7 months after Ramadan after getting the family planning program. Secondly, only the Muslim births in the treatment areas should be affected. Chances of both taking place are very rare. If anything we should observe more premature births in the control areas than in the treatment areas because the mothers living in the treatment areas some maternal health care along with the family planning program that was not available to mothers in the control areas. Therefore, the treatment areas should have less premature births relative to the control areas and the results should have, if anything, downward bias. Moreover, if any health shocks affecting mothers and leading to premature births in the treatment areas, it should affect both Muslims and Hindus living in the treatment areas. The fact that I do not observe similar birth pattern of Hindus relative to Ramadan attests to the results that health shocks leading to premature births are not responsible for this result.

Another concern is that male fragility may explain the decrease in births 8 to 9 months after Ramadan in the treatment areas after the initiation of the family planning program. Almond and Mazumder (2011) finds exposure to Ramadan in first two months of gestation leads to lower male births. In this data, I do not observe such pattern. The results are presented in appendix Table A3 birth relative to Ramadan and male birth for the control areas only because I do not observe selection in birth timing in control areas. Moreover, male fragility also less likely to be an explanation because the treatment areas received other maternal health interventions which might increase the chances of male birth. Thus, male fragility can be ruled out as a likely explanation.

9 Conclusion

There is little doubt about the welfare implications of the impact of adverse environments and nutrition shocks *in utero* on health outcomes. We have seen proliferation of studies which have documented the consequences of *in utero* shocks. In the context of nutrition shock due to maternal fasting during Ramadan, prior studies have treated Ramadan exposure as a natural experiment and attributed the negative effects of exposure to Ramadan on health and other outcomes to maternal fasting during Ramadan. Although the prior studies have highlighted the importance of identifying exposure effect of Ramadan allowing for parental selection in Ramadan exposure, they do not find any evidence of such selection. The examination of parental selection in Ramadan exposure in prior studies is inadequate, as selection in Ramadan exposure is not examined allowing for time varying changes.

Using two data sets from two countries, this paper shows that mothers with more education are less likely to overlap their pregnancies with Ramadan. As mother education is one of the most important determinants of child health, such selection in Ramadan exposure can lead to unequal outcomes between exposed children and non-exposed children. Therefore, comparing the mean outcomes of exposed children with that of non-exposed children, we may falsely conclude that the effects are due to maternal fasting during Ramadan. This paper emphasizes that parental selection in Ramadan exposure should be examined allowing for time varying changes, as Ramadan is a recurring and a predictable event. It also documents that ignoring time varying changes may lead to falsely concluding that there is no evidence of parental selection. It further shows presence of parental selection may produce biased estimates of the effects of Ramadan exposure. In the light of these findings, this paper recommends that it is important to examine the nature of selection in Ramadan exposure appropriately, and any evaluation of the impact of maternal fasting during Ramadan on relevant outcomes should be performed allowing for selection.

Moreover, this paper also documents a beneficial aspect of provision of a family planning program. Although the earlier studies have documented negative consequences of Ramadan exposure on health, labor, and education outcomes, there was no evidence what kind of policy may help mothers to avoid their pregnancies overlapping with Ramadan. The evidence from Matlab suggests that provision of a family planning program may allow mothers to time birth relative to Ramadan.

References

Alam, Shamma Adeeb and Claus C Pörtner, "Income Shocks, Contraceptive Use, and Timing of Fertility," Technical Report, Working paper. Seattle, WA: Seattle University, Seattle 2013.

Almond, Douglas, "Is the 1918 Influenza Pandemic Over? LongTerm Effects of In Utero Influenza Exposure in the Post1940 U.S. Population," *Journal of Political Economy*, August 2006, *114* (4), 672–712.

_ **and Bhashkar Mazumder**, "Health Capital and the Prenatal Environment: The Effect of Ramadan Observance During Pregnancy," *American Economic Journal: Applied Economics*, October 2011, *3* (4), 56–85.

_ **and Janet Currie**, "Killing me softly: The fetal origins hypothesis," *The journal of economic perspectives: a journal of the American Economic Association*, 2011, *25* (3), 153.

_ **, Bhashkar Mazumder, and Reyn van Ewijk**, "In Utero Ramadan Exposure and Children's Academic Performance," *The Economic Journal*, September 2014, pp. n/a–n/a.

Barham, Tania, "The Effects of Child Health Interventions on Later Life Cognitive Functioning: Evidence from Matlab in Bangladesh," *American Economic Journal: Applied Economics*, 2012, *4* (1), 245–73.

_ **and Randall Kuhn**, "Staying for Benefits.," *Journal of Human Resources*, 2014, *49* (4).

Behrman, Jere and Anil Deolalikar, "Health and nutrition," in Hollis Chenery and Srinivasan T.N., eds., *Handbook of Development Economics*, 1 ed., Vol. 1, Elsevier, 1988, pp. 631–711.

Behrman, Jere R. and Barbara L. Wolfe, "More evidence on nutrition demand," *Journal of Development Economics*, January 1984, *14* (1), 105–128.

_ **and** _ , "Does More Schooling Make Women Better Nourished and Healthier? Adult Sibling Random and Fixed Effects Estimates for Nicaragua," *The Journal of Human Resources*, October 1989, *24* (4), 644–663.

Bhatia, Shushum, W Henry Mosley, Abu SG Faruque, and Jyotsnamoy Chakraborty, "The Matlab family planning-health services project," *Studies in family planning*, 1980, pp. 202–212.

Brown, Ryan and Duncan Thomas, *On the long term effects of the 1918 US influenza pandemic*, unpublished manuscript, 2011.

Buckles, Kasey S. and Daniel M. Hungerman, "Season of Birth and Later Outcomes: Old Questions, New Answers," *Review of Economics and Statistics*, July 2012, *95* (3), 711–724.

Cooper, Cyrus, Avan A Sayer, Elaine Margaret Dennison, P Gluckman, M Hanson et al., "The developmental environment: clinical perspectives on effects on the musculoskeletal system.," *Developmental origins of health and disease*, 2006, pp. 392–405.

Cross, JH, J Eminson, and BA Wharton, "Ramadan and birth weight at full term in Asian Moslem pregnant women in Birmingham.," *Archives of disease in childhood*, 1990, *65* (10 Spec No), 1053–1056.

Currie, Janet, "Healthy, wealthy, and wise: Is there a causal relationship between child health and human capital development?," *Journal of Economic Literature*, 2009, *47* (1), 87–122.

— **and Tom Vogl**, "Early-Life Health and Adult Circumstance in Developing Countries," *Annual Review of Economics*, 2013, *5* (1), 1–36.

Dikensoy, Ebru, Ozcan Balat, Bahar Cebesoy, Ayhan Ozkur, Hulya Cicek, and Gunay Can, "The effect of Ramadan fasting on maternal serum lipids, cortisol levels and fetal development," *Archives of Gynecology and Obstetrics*, May 2008, *279* (2), 119–123.

DiPietro, Janet A., Marc H. Bornstein, Chun-Shin Hahn, Kathleen Costigan, and Aristide Achy-Brou, "Fetal Heart Rate and Variability: Stability and Prediction to Developmental Outcomes in Early Childhood," *Child Development*, November 2007, *78* (6), 1788–1798.

Eckstein, Zvi, T Paul Schultz, and Kenneth I Wolpin, "Short-run fluctuations in fertility and mortality in pre-industrial Sweden," *European Economic Review*, 1984, *26* (3), 295–317.

Ewijk, Reyn, "Long-term health effects on the next generation of Ramadan fasting during pregnancy," *Journal of Health Economics*, December 2011, *30* (6), 1246–1260.

Falkner, Frank and James Mourilyan Tanner, *Methodology Ecological, Genetic, and Nutritional Effects on Growth*, Vol. 3, Springer, 1986.

Floyd, R. Louise, Pierre Decoufl, and Daniel W Hungerford, "Alcohol use prior to pregnancy recognition1," *American Journal of Preventive Medicine*, August 1999, *17* (2), 101–107.

Galloway, Patrick R, "Differentials in demographic responses to annual price variations in pre-revolutionary France," *European Journal of Population/Revue européenne de Démographie*, 1987, *2* (3-4), 269–305.

Gluckman, Peter D and Mark A Hanson, *The fetal matrix: evolution, development and disease*, Cambridge University Press, 2004.

Griliches, Zvi, "Sibling models and data in economics: Beginnings of a survey," *The Journal of Political Economy*, 1979, pp. S37–S64.

Hunter, E. S. and T. W. Sadler, "D-()-beta-hydroxybutyrate-induced effects on mouse embryos in vitro," *Teratology*, October 1987, *36* (2), 259–264.

Jaddoe, Vincent W. V. and Jacqueline C. M. Witteman, "Hypotheses on the Fetal Origins of Adult Diseases: Contributions of Epidemiological Studies," *European Journal of Epidemiology*, February 2006, *21* (2), 91–102.

Joshi, Shareen and T Paul Schultz, "Family planning and womens and childrens health: Long-term consequences of an outreach program in Matlab, Bangladesh," *Demography*, 2013, *50* (1), 149–180.

Kapoor, Amita, Elizabeth Dunn, Alice Kostaki, Marcus H. Andrews, and Stephen G. Matthews, "Fetal programming of hypothalamo-pituitary-adrenal function: prenatal stress and glucocorticoids," *The Journal of Physiology*, April 2006, *572* (1), 31–44.

Karimi, Seyed M, "The Effect of Malnutrition In Utero on Height: Ramadan as a Natural Experiment," 2014.

Koenig, Michael Alan, Mehrab Ali Khan, Bogdan Wojtyniak, John D Clemens, Jyotsnamoy Chakraborty, Vincent Fauveau, James F Phillips, Jalaluddin Akbar, and Uday S Barua, "Impact of measles vaccination on childhood mortality in rural Bangladesh.," *Bulletin of the World Health Organization*, 1990, *68* (4), 441.

Lam, David A, Jeffrey A Miron, and Ann Riley, "Modeling seasonality in fecundability, conceptions, and births," *Demography*, 1994, *31* (2), 321–346.

Maitra, Pushkar and Sarmistha Pal, "Birth spacing, fertility selection and child survival: Analysis using a correlated hazard model," *Journal of Health Economics*, May 2008, *27* (3), 690–705.

Majid, Muhammad Farhan, "The Persistent Eff ects of in Utero Nutrition Shocks Over the Life Cycle: Evidence From Ramadan Fasting," *Journal of Development Economics*, November 2015, *117*, 48–57.

Malhotra, Ashok, P. H. Scott, J. Scott, H. Gee, and B. A. Wharton, "Metabolic changes in Asian Muslim pregnant mothers observing the Ramadan fast in Britain," *British Journal of Nutrition*, May 1989, *61* (03), 663–672.

Martorell, Reynaldo and Jean-Pierre Habicht, "Growth in early childhood in developing countries.," 1986.

Meis, Paul J., James C. Rose, and Melissa Swain, "Pregnancy Alters Diurnal Variation of Plasma Glucose Concentration," *Chronobiology International*, January 1984, *1* (2), 145–149.

Metzger, BoydE, RitaA Vileisis, Veronica Ravnikar, and Norbert Freinkel, ""ACCELERATED STARVATION" AND THE SKIPPED BREAKFAST IN LATE NORMAL PREGNANCY," *The Lancet*, March 1982, *319* (8272), 588–592.

Mirghani, Hisham M and Omer A Hamud, "The effect of maternal diet restriction on pregnancy outcome.," *American journal of perinatology*, 2006, *23* (1), 21–24.

Mirghani, Hisham M., Sarath Weerasinghe, Shamsaa Al-Awar, Lolowa Abdulla, and Mutairu Ezimokhai, "The Effect of Intermittent Maternal Fasting on Computerized Fetal Heart Tracing," *Journal of Perinatology*, November 2004, *25* (2), 90–92.

Moore, D. C. P., M. Stanisstreet, and C. A. Clarke, "Morphological and physiological effects of -hydroxybutyrate on rat embryos grown in vitro at different stages," *Teratology*, September 1989, *40* (3), 237–251.

Phillips, James F., Ruth Simmons, J. Chakraborty, and A. I. Chowdhury, "Integrating Health Services into an MCH-FP Program: Lessons from Matlab, Bangladesh," *Studies in Family Planning*, July 1984, *15* (4), 153–161.

Pitt, Mark M., "Estimating the Determinants of Child Health When Fertility and Mortality Are Selective," *The Journal of Human Resources*, January 1997, *32* (1), 129–158.

Rahman, Omar, Jane Menken, Andrew Foster, and Paul Gertler, "Matlab [Bangladesh] Health and Socioeconomic Survey (MHSS), 1996," *Ann Arbor, Michigan: Inter-university Consortium for Political and Social Research (ICPSR 2705)*, 1999, *13.*

Reeves, Jane, "Pregnancy and fasting during Ramadan.," *BMJ: British Medical Journal*, 1992, *304* (6830), 843.

Rizzo, Thomas, Boyd E Metzger, William J Burns, and Kayreen Burns, "Correlations between antepartum maternal metabolism and intelligence of offspring," *New England Journal of Medicine*, 1991, *325* (13), 911–916.

Robinson, Trinka and Jeanne Raisler, "Each one is a doctor for herself: Ramadan fasting among pregnant Muslim women in the United States," *Ethn Dis*, 2005, *15* (1 Suppl 1), S1–99.

Ronsmans, Carine, Anne Marie Vanneste, Jyotsnamoy Chakraborty, and Jeroen Van Ginneken, "Decline in maternal mortality in Matlab, Bangladesh: a cautionary tale," *The Lancet*, 1997, *350* (9094), 1810–1814.

Rosenzweig, Mark R and Kenneth I Wolpin, "Heterogeneity, intrafamily distribution, and child health," *Journal of Human Resources*, 1988, pp. 437–461.

Schultz, T. Paul, "Why Governments Should Invest More to Educate Girls," *World Development*, February 2002, *30* (2), 207–225.

Schultz, T Paul, "How Does Family Planning Promote Development? Evidence from a Social Experiment in Matlab, Bangladesh19771996," *Yale University, Economic Growth Center, New Haven, Conn*, 2009.

Seckl, Jonathan R and Megan C Holmes, "Mechanisms of disease: glucocorticoids, their placental metabolism and fetal'programming'of adult pathophysiology," *Nature clinical practice Endocrinology & metabolism*, 2007, *3* (6), 479–488.

Sheehan, E. A., F. Beck, C. A. Clarke, and M. Stanisstreet, "Effects of -hydroxybutyrate on rat embryos grown in culture," *Experientia*, February 1985, *41* (2), 273–275.

Sinha, Nistha, "Fertility, child work, and schooling consequences of family planning programs: Evidence from an experiment in rural Bangladesh," *Economic Development and Cultural Change*, 2005, *54* (1), 97–128.

Strauss, John and Duncan Thomas, "Human resources: empirical modeling of household and family decisions.," 1995.

_ **and** _ , "Health, Nutrition, and Economic Development," *Journal of Economic Literature*, June 1998, *36* (2), 766–817.

_ **and** _ , "Health over the life course," *Handbook of development economics*, 2007, *4*, 3375–3474.

Thomas, Duncan, "The causal effect of health on social and economic prosperity: Methods and findings," *Health Economic Growth Framework Paper*, 2009.

_ **, Firman Witoelar, Elizabeth Frankenberg, Bondan Sikoki, John Strauss, Cecep Sumantri, and Wayan Suriastini**, "Cutting the costs of attrition: Results from the Indonesia Family Life Survey," *Journal of Development Economics*, May 2012, *98* (1), 108–123.

van Ewijk, Reyn J. G., Rebecca C. Painter, and Tessa J. Roseboom, "Associations of Prenatal Exposure to Ramadan with Small Stature and Thinness in Adulthood: Results From a Large Indonesian PopulationBased Study," *American Journal of Epidemiology*, April 2013, *177* (8), 729–736.

10 Tables

Table 1: MHSS Summary Statistics

Panel A : Parental Characteristics	
Variables	Mean
Mother years of education	2.17
Father years of education	3.28
Mother age	37.40
Panel B: Birth Characteristics	
nonexposed	0.13
Born during Ramadan	0.08
Male	0.52
Hindu	0.08

Notes: This table describes summary characteristics from the MHSS for birth cohorts born between 1974 and 1995. In panel A summary characteristics of parents are presented. $Motherage$ is measured at the time of survey. In Panel B Birth characteristics are presented. *nonexposed* takes a value of 0 if pregnancy overlapped first, second and third trimester and 1 otherwise. The trimesters are calculated from the birth date based on the assumption that pregnancy lasts for 9 months. Born during Ramadan takes a value of 1 if the individual was born during the month of Ramadan 0 otherwise. The calculation of exposure to Ramadan is based under the assumption that pregnancy lasts for 9 months.

Table 2: IFLS Summary Statistics

Panel A : Parental Characteristics	
Variables	Mean
Mother years of education	7.45
Mother age	28.89
Panel B: Birth Characteristics	
nonexposed	0.14
Born during Ramadan	0.08
Male	0.51
Non-Muslim	0.11

Notes: This table describes summary characteristics from the IFLS restricting pregnancy history to maximum 5 years. In panel A summary characteristics of parents are presented. $Motherage$ is measured at the time of survey. In Panel B Birth characteristics are presented. *nonexposed* takes a value of 0 if pregnancy overlapped first, second and third trimester and 1 otherwise. The trimesters are calculated from the birth date based on the assumption that pregnancy lasts for 9 months. Born during Ramadan takes a value of 1 if the individual was born during the month of Ramadan 0 otherwise. The calculation of exposure to Ramadan is based under the assumption that pregnancy lasts for 9 months.

Table 3: Selection in live birth

	Panel A			
VARIABLES	Control	Treatment	Diff	p-value
Live birth	.94	0.93	0.01	0.39
	Panel B			
Women Age	23.55	27.90	3.35	0.11
Women Yedu	3.89	3.13	0.77	0.41
Husband Yedu	3.06	2.67	0.39	0.67

Notes: This table describes whether there is selection in live birth because of the family planning program between the treatment and control areas. The first two columns show means in the treatment and control areas. The variable $Yedu$ is education in years. age is age at survey. The means are calculated using the survey weights. Panel A compares the probability of at least one birth between the treatment and control areas. Panel B compares observable characteristics of women who did not give birth yet between the treatment and control areas . This table is based on data from the MHSS.

Table 4: Selection in live birth : Muslim Married Women

	Panel A			
VARIABLES	Control	Treatment	Diff	p-value
Live birth	0.95	0.93	0.02	0.19
	Panel B			
Women Age	24.09	26.99	1.90	0.33
Women Yedu	3.76	3.22	0.49	0.46
Husband Yedu	3.06	2.67	0.39	0.67

Notes: This table describes whether there is selection in live birth because of the family planning program between the treatment and control areas. The first two columns show means in the treatment and control areas. The variable $Yedu$ is education in years. age is age at survey. The means are calculated using the survey weights. Panel A compares the probability of at least one birth between treatment and control areas. Panel B compares observable characteristics of women who did not give birth yet between the treatment and control areas . This table is based on data from the MHSS.

Table 5: Family Planning Program and Birth Timing Relative to Ramadan for Muslim-Birth Cohort 1974-95

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
post× treated	0.016	0.009	0.002	-0.010	0.024	0.042*	-0.007	0.024	-0.033*	-0.035	-0.008	-0.025
	(0.025)	(0.021)	(0.016)	(0.023)	(0.022)	(0.023)	(0.019)	(0.017)	(0.017)	(0.021)	(0.022)	(0.017)
post	0.260***	0.171***	0.178***	0.158***	0.072	-0.282***	-0.192***	-0.226***	-0.119***	-0.066*	0.048	-0.005
	(0.041)	(0.031)	(0.038)	(0.046)	(0.052)	(0.047)	(0.030)	(0.038)	(0.036)	(0.037)	(0.029)	(0.030)
R2	0.103	0.092	0.079	0.087	0.062	0.084	0.061	0.083	0.068	0.073	0.084	0.085
Observations	5271	5271	5271	5271	5271	5271	5271	5271	5271	5271	5271	5271
Mean Y	0.086	0.068	0.076	0.088	0.104	0.104	0.093	0.096	0.079	0.075	0.076	0.057
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Village FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Standard errors are clustered at the village ID level.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: The dependent variables are based on birth months from Ramadan. ramadan_0 is the month of Ramadan. ramadan_1 is first 30 days after the month of Ramadan, ramadan_2 is the second 30 days after the month of Ramadan and so on. For x=0,1,11 $ramadan_x$ takes a value of 1 if the individual was born x months after Ramadan and 0 otherwise. Post takes a value of 1 from August,1978 and 0 otherwise. ramadan_11 is the last 24 days based on calender year starting from the month of Ramadan. Post takes a value of 1 if the birth took place from August, 1978 and 0 otherwise. The variable *treated* takes a value of 1 if mother lives in the treatment areas and 0 otherwise. The analysis in this table are based on pregnancy histories of the Muslim mothers only. This table is based on data from the MHSS.

Table 6: Family Planning Program and Birth Timing Relative to Ramadan: Birth Cohort 1974-95

	Muslim Birth Sample			Hindu Birth Sample		
VARIABLES	ramadan8to9	ramadan8to9	ramadan8to9	ramadan8to9	ramadan8to9	ramadan8to9
	(1)	(2)	(3)	(4)	(5)	(6)
post× treated	-0.062**	-0.057**	-0.066*	0.004	0.005	0.067
	(0.028)	(0.025)	(0.037)	(0.095)	(0.101)	(0.208)
post	-0.183***	-0.169***	-0.159*	0.020	0.012	0.053
	(0.046)	(0.048)	(0.082)	(0.168)	(0.167)	(0.305)
R2	0.111	0.112	0.427	0.306	0.310	0.534
Observations	5166	5166	5166	466	466	466
Mean Y	0.154	0.154	0.154	0.182	0.182	0.182
Month FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Village FE	Y	Y	Y	Y	Y	Y
Village TVC	N	Y	Y	N	Y	Y
Mother FE	N	N	Y	N	N	Y

Standard errors are clustered at the village ID level.
*** p<0.01, ** p<0.05, * p<0.1

Notes: The dependent variable ramadan8to9 takes a value of 1 if the individual was born 8 to 9 months after Ramadan and 0 otherwise. Post takes a value of 1 from August, 1978 and 0 otherwise. The variable *treated* takes a value of 1 if the village received the family planning program and 0 otherwise. All regressions include spline function of mother age at 5 year interval from age 15 onward and mother years of education. Village T.V.C are village time varying characteristics are large market, electricity, tube-well and satellite clinic. Post is interacted with whether the village has large market. For electricity, tube-well and satellite clinic post takes a of value 1 from the year after these were established and 0 otherwise. This table is based on data from the MHSS.

Table 7: Family Planning Program and Birth Timing Relative to Ramadan: Birth Cohort 1974-95

	Muslim Birth Sample			Hindu Birth Sample		
VARIABLES	ramadan4to7	ramadan4to7	ramadan4to7	ramadan4to7	ramadan4to7	ramadan4to7
	(1)	(2)	(3)	(4)	(5)	(6)
post× treated	0.086***	0.080**	0.082*	-0.017	0.026	-0.048
	(0.032)	(0.033)	(0.049)	(0.218)	(0.212)	(0.333)
post	-0.627***	-0.623***	-0.611***	-0.681*	-0.755**	-0.888*
	(0.063)	(0.062)	(0.094)	(0.352)	(0.366)	(0.529)
R2	0.191	0.191	0.497	0.342	0.356	0.550
Observations	5166	5166	5166	466	466	466
Mean Y	0.396	0.396	0.396	0.337	0.337	0.337
Month FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Village FE	Y	Y	Y	Y	Y	Y
Village TVC	N	Y	Y	N	Y	Y
Mother FE	N	N	Y	N	N	Y

Standard errors are clustered at the village ID level.
*** p<0.01, ** p<0.05, * p<0.1

Notes: The dependent variable ramadan4to7 takes a value of 1 if the individual was born 4 to 7 months after Ramadan and 0 otherwise. Post takes a value of 1 from August, 1978 and 0 otherwise. The variable *treated* takes a value of 1, if the village received the family planning program and 0 otherwise. All regressions include spline function of mother age at 5 year interval from age 15 onward and mother years of education. Village T.V.C are village level time varying characteristics are large market, electricity, tube-well and satellite clinic. Post is interacted with whether the village has large market. For electricity, tube-well and satellite clinic post takes a value of 1 from the year after these were established and 0 otherwise. This table is based on data from the MHSS.

Table 8: Exposure to Ramadan and Child Height

VARIABLES	Muslim Children Only height(C+T)	height(C)	height(T)	Muslim+Hindu Children height(T)
	(1)	(2)	(3)	(4)
BornduringRamadan	-1.219	-0.011	0.165	-0.070
	(1.496)	(2.578)	(2.789)	(2.296)
exposed	-2.782*	-1.384	-4.428	-4.516*
	(1.490)	(1.927)	(2.813)	(2.294)
Hindu				-4.627*
				(2.329)
BornduringRamadan× Hindu				5.388
				(8.008)
exposed×Hindu				6.797**
				(3.106)
R2	0.876	0.889	0.938	0.924
Observations	1174	655	519	611
Mean Y	100.5	100.8	100.1	100.7
Month FE	Y	Y	Y	Y
Malec× Age in Month FE	Y	Y	Y	Y
Village FE	Y	Y	Y	Y
F:exposed+exposed × Hindu				0.6
P-Value				0.5

Standard errors are clustered at the village ID level.
*** p<0.01, ** p<0.05, * p<0.1

Notes: The height sample is based on children who are less than 120 months old at the time of survey. The dependent variable height is measured in centimeters. Exposed takes a value of 1 if pregnancy overlapped first, second and third trimester and 0 otherwise. The trimesters are calculated from the birth date based on the assumption that pregnancy lasts for 9 months. BornduringRamadan takes a value of 1 if the individual was born during Ramadan and 0 otherwise. The omitted category is the pregnancies which did not overlap with Ramadan. All regression specifications include mother age, mother years of education. C and T denotes control are treatment areas respectively. C+T imply the samples from the control and treatment areas are pooled. This table is based on data from the MHSS.

Table 9: Parental SES and Ramadan Exposure for cohort 1974-July,1978

VARIABLES	Control	Difference	p-value
motherage	-0.002	0.002	0.38
mothereduy	0.001	-.004	0.21
fathereduy	0.001	-.002	0.39

Notes: This table presents coefficients of regressions *nonexposed* on *motherage*, *fathereduy* and *mothereduy* controlling for month and year fixed effects. The column named *Difference* shows difference in coefficient between treatment and control areas for pre-program birth cohorts. The third column shows *p-value* of of the difference. The dependent variable *nonexposed* takes a value of 0 if pregnancy overlapped first, second and third trimester and 1 otherwise. The variable *mothereduy* and *fathereduy* is mother and father education in years respectively. *motheage* is age at survey. Post takes a value of 1 from August,1978 and 0 otherwise. This table is based on data from the MHSS.

Table 10: Exposure to Ramadan and Mother Education: Birth Cohort 1982-1995

	Muslim Birth Sample		Hindu Birth Sample
	nonexposed(C)	nonexposed(T)	nonexposed(T)
	(1)	(2)	(3)
motherage	0.001	-0.001	0.002
	(0.001)	(0.001)	(0.005)
mothereduy	-0.004	0.006*	-0.012
	(0.003)	(0.003)	(0.011)
R2	0.240	0.272	0.668
Observations	1990	1432	215
Mean Y	0.146	0.138	0.153
Month FE	Y	Y	Y
Block× Year	Y	Y	Y
Village FE	Y	Y	Y

Standard errors are clustered at the village ID level.
*** p<0.01, ** p<0.05, * p<0.1

Notes: The dependent variable *nonexposed* takes a value of 0 if pregnancy overlapped first, second and third trimester and 1 otherwise. The trimesters are calculated from the birth date based on the assumption that pregnancy lasts for 9 months. All regression specifications include mother age and mother years of education. mother years of education. C and T denote the control areas and the treatment areas respectively. Column 1 and 2 are Muslims from the control areas and the treatment areas only. Column 3 is Hindus from the treatment areas. This table is based on data from the MHSS.

Table 11: Birth Relative to Ramadan and Mother Education

	Muslim Birth Cohort 1982-1995		Muslim Birth Cohort 1986-1995	
	nonexposed	nonexposed	nonexposed	nonexposed
	(1)	(2)	(3)	(4)
motherage	0.000	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
mothereduy	0.000	-0.005*	-0.002	-0.008**
	(0.002)	(0.003)	(0.003)	(0.004)
treated× mothereduy		0.011***		0.014***
		(0.004)		(0.005)
R2	0.248	0.250	0.359	0.362
Observations	3422	3422	2356	2356
Mean Y	0.143	0.143	0.160	0.160
Month FE	Y	Y	Y	Y
Block× Year FE	Y	Y	Y	Y
Village FE	Y	Y	Y	Y
F:mothereduy+treated×mothereduy		5.36		2.98
P-Value		0.02		0.09

Standard errors are clustered at the village ID level.
*** p<0.01, ** p<0.05, * p<0.1

Notes: The dependent variable *nonexposed* takes a value of 0 if pregnancy overlapped first, second and third trimester and 1 otherwise. The trimesters are calculated from the birth date based on the assumption that pregnancy lasts for 9 months. The treated takes a value of 1 if the villages received the family planning program and 0 otherwise. All regression specifications include mother age and mother years of education. The sample includes Muslim births both from treatment and control areas. This table is based on data from the MHSS.

Table 12: Exposure to Ramadan and Mother Education: Birth Cohort 1974-1982

	Muslim Birth Sample			Hindu Birth Sample
VARIABLES	nonexposed(C+T)	nonexposed(C)	nonexposed(T)	nonexposed(T)
	(1)	(2)	(3)	(4)
motherage	0.000	-0.001	0.002	-0.006
	(0.001)	(0.001)	(0.001)	(0.006)
mothereduy	0.003	0.003	-0.003	-0.018*
	(0.002)	(0.005)	(0.003)	(0.010)
post		-0.045	-0.139**	-0.143
		(0.049)	(0.054)	(0.104)
post×mothereduy		-0.002	0.013**	0.014
		(0.007)	(0.005)	(0.017)
R2	0.402	0.421	0.406	0.684
Observations	2012	1080	932	146
Mean Y	0.117	0.111	0.123	0.185
Month FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Village FE	Y	Y	Y	Y
F:mothereduy+post×mothereduy		0.034	6.258	0.065
P-Value		0.855	0.015	0.801

Standard errors are clustered at the village ID level.
*** p<0.01, ** p<0.05, * p<0.1

Notes: The dependent variable *nonexposed* takes a value of 0 if pregnancy overlapped first, second and third trimester and 1 otherwise. The trimesters are calculated from the birth date based on the assumption that pregnancy lasts for 9 months. The treated takes a value of 1 if the villages received the family planning program and 0 otherwise. Variables *motherage* is mother age at survey and *mothereduy* is mother years of education at survey. Post take a value of from August, 1978 and 0 otherwise. C and T denote the control areas and the treatment areas respectively. C+T imply the samples from the treatment and control ares pooled. Column 2 and 3 are based on Muslim sample from the control areas and treatment areas respectively. Column 4 is based on Hindu sample from the treatment areas. This table is based on data from the MHSS.

Table 13: Mother Education and Child Height

	All	Control Areas	Treatment Areas
	(1)	(2)	(3)
motherage	-0.009	0.003	0.014
	(0.047)	(0.075)	(0.070)
mothereduy	0.378***	0.295*	0.495***
	(0.099)	(0.158)	(0.132)
R2	0.865	0.883	0.921
Observations	1299	688	611
Mean Y	100.7	100.8	100.7
Month FE	Y	Y	Y
Malec× Age in Month FE	Y	Y	Y
Village FE	Y	Y	Y

Standard errors are clustered at the village ID level.
*** p<0.01, ** p<0.05, * p<0.1

Notes: The height sample is based on children who are less than 120 months old at the time of survey. The dependent variable height is measured in centimeters. The sample includes all the children from the treatment and control areas. All imply sample from both the treatment and control areas are pooled. This table is based on data from the MHSS.

Table 14: Exposure to Ramadan and Mother Education

VARIABLES	(All Education) nonexposed	(Less than Equal to 12 Years) nonexposed
motherage	0.00125**	0.00134**
	(0.000571)	(0.000603)
mothereduy	0.00221*	0.00303*
	(0.00130)	(0.00163)
nonm	0.0412	0.0377
	(0.0313)	(0.0334)
nonm×mothereduy	-0.00384	-0.00259
	(0.00287)	(0.00372)
Constant	0.197***	0.200***
	(0.0324)	(0.0340)
Observations	10,753	9,853
R-squared	0.142	0.141
Birthday FE	Y	Y
Month FE	Y	Y
Year FE	Y	Y
commid93	Y	Y
F: mothereduy+nonm×mothereduy	0.40	0.02
$P > F$	0.54	0.90

Standard errors clustered at community ID level.
*** p<0.01, ** p<0.05, * p<0.1

Notes: The dependent variable *nonexposed* takes a value of 0 if pregnancy overlapped first, second and third trimester and 1 otherwise. The trimesters are calculated from the birth date based on the assumption that pregnancy lasts for 9 months. Variables *motherage* is mother age at survey and *mothereduy* is mother years of education at survey. The nonm (non-Muslim) takes a value of 1 if the mother is non-Muslim and 0 otherwise. Commid93 represents original IFLS communities. *mothereduy* is defined as years of mother education. The results in the table are based on all four waves of the IFLS restricting pregnancy history to maximum 5 years from the survey year.

Table 15: Exposure to Ramadan and Mother Education

VARIABLES	(Birth cohort-last five year from survey) nonexposed	(Birth cohort-1989-2007) nonexposed
motherage	0.00130**	0.00137***
	(0.000578)	(0.000504)
motheredu	0.0423**	0.0323*
	(0.0191)	(0.0166)
nonm	0.0340	0.0456
	(0.0385)	(0.0401)
nonm×motheredu	-0.0269	-0.0220
	(0.0380)	(0.0416)
Constant	0.170***	0.112***
	(0.0369)	(0.0331)
Observations	10,753	13,351
R-squared	0.142	0.144
Birthday FE	Y	Y
Month FE	Y	Y
Year FE	Y	Y
commid93	Y	Y
F: mothereduy+nonm×motheredu	0.21	0.07
$P > F$	0.64	0.79

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The dependent variable *nonexposed* takes a value of 0 if pregnancy overlapped first, second and third trimester and 1 otherwise. The trimesters are calculated from the birth date based on the assumption that pregnancy lasts for 9 months. Variables *motherage* is mother age at survey and *motheredu* takes a value of 1, if mother had atleast 1 year of education and 0 otherwise. The nonm (non-Muslim) takes a value of 1 if the mother is non-Muslim and 0 otherwise. Commid93 represents original IFLS communities. *motheredu* takes a value of 1 if mother is educated and 0 otherwise. The results in the table are based on all four waves of the IFLS.

11 Appendix

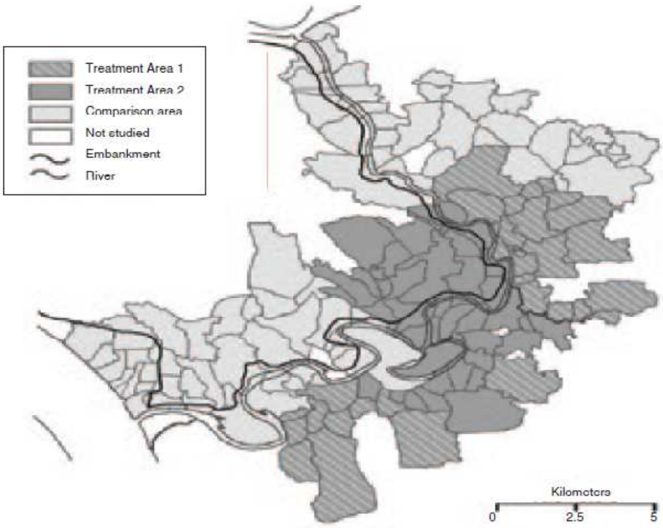

Figure 1. Map of Matlab Study Area

Table 3—1974 Baseline Characteristics

	Treatment Area			Comparison Area			Difference in Means		
	Mean	SD	Obs.	Mean	SD	Obs.	Mean	T-stat	Mean/SD
Panel A. Full Sample									
Family size	7.01	(5.15)	2,124	6.82	(4.20)	2,548	0.18	1.34	0.04
Owns a lamp (=1)	0.65	(1.18)	2,124	0.61	(0.92)	2,548	0.04	1.37	0.04
Owns a watch (=1)	0.16	(0.94)	2,124	0.16	(0.62)	2,548	0.00	0.05	0.00
Owns a radio (=1)	0.08	(0.63)	2,124	0.08	(0.47)	2,548	0.00	0.15	0.00
Wall tin or tinmix (=1)	0.32	(1.08)	2,124	0.31	(0.78)	2,548	0.01	0.27	0.01
Tin roof (=1)	0.83	(0.66)	2,124	0.84	(0.69)	2,548	0.00	−0.10	0.00
Latrine (=1)	0.83	(0.78)	2,124	0.85	(0.86)	2,548	−0.03	−1.22	−0.03
Number of rooms per capita	0.21	(0.15)	2,124	0.21	(0.18)	2,548	0.00	0.39	0.01
Number of cows	1.55	(3.30)	2,124	1.37	(3.05)	2,548	0.19	2.02	0.06
Number of boats	0.68	(1.50)	2,124	0.68	(1.42)	2,548	−0.01	−0.20	−0.01
Drinking water, tubewell (=1)	0.31	(1.41)	2,124	0.16	(0.93)	2,548	0.15	4.35	0.11
Drinking water, tank (=1)	0.38	(1.72)	2,124	0.33	(1.68)	2,548	0.05	1.01	0.03
Drinking water, other (=1)	0.31	(2.21)	2,124	0.51	(1.84)	2,548	−0.20	−3.39	−0.09
HH age	47.8	(22)	2,124	46.5	(22)	2,548	1.28	1.97	0.06
HH years of education (edu.)	2.52	(6.72)	2,124	2.34	(5.25)	2,548	0.17	1.25	0.02
HH works in agriculture (=1)	0.61	(0.89)	2,124	0.59	(0.99)	2,548	0.02	0.79	0.02
HH works in fishing (=1)	0.05	(0.53)	2,124	0.06	(0.49)	2,548	−0.01	−0.57	−0.02
HH spouse's age	37.0	(17)	2,124	36.2	(16)	2,548	0.86	1.72	0.05
HH spouse's years of edu.	1.14	(3.58)	2,124	1.21	(2.56)	2,548	−0.07	−0.95	−0.02
Panel B. Age 8–14									
Family size	6.59	(3.09)	188	6.87	(3.20)	304	−0.27	−0.97	−0.08
Owns a lamp (=1)	0.63	(0.52)	188	0.60	(0.69)	304	0.03	0.63	0.05
Owns a watch (=1)	0.16	(0.71)	188	0.17	(0.43)	304	−0.01	−0.13	−0.01
Owns a radio (=1)	0.09	(0.51)	188	0.09	(0.29)	304	0.01	0.14	0.01
Wall tin or tinmix (=1)	0.29	(0.72)	188	0.32	(0.43)	304	−0.03	−0.49	−0.05
Tin roof (=1)	0.81	(0.45)	188	0.88	(0.52)	304	−0.07	−1.57	−0.14
Latrine (=1)	0.85	(0.39)	188	0.85	(0.52)	304	−0.01	−0.13	−0.01
Number of rooms per capita	0.21	(0.08)	188	0.21	(0.18)	304	0.00	0.09	0.01
Number of cows	1.52	(2.51)	188	1.38	(2.22)	304	0.14	0.60	0.06
Number of boats	0.67	(0.98)	188	0.67	(0.92)	304	−0.01	−0.07	−0.01
Drinking water, tubewell (=1)	0.27	(0.71)	188	0.13	(0.58)	304	0.14	2.28	0.21
Drinking water, tank (=1)	0.37	(0.84)	188	0.31	(0.99)	304	0.06	0.75	0.07
Drinking water, other (=1)	0.36	(1.12)	188	0.56	(0.97)	304	−0.21	−2.09	−0.18
HH age	48.9	(17)	188	46.3	(18)	304	2.58	1.61	0.15
HH years of education	1.92	(3.84)	188	2.10	(3.15)	304	−0.18	−0.61	−0.05
HH works in agriculture (=1)	0.67	(0.45)	188	0.55	(0.67)	304	0.12	2.29	0.19
HH works in fishing (=1)	0.07	(0.35)	188	0.07	(0.38)	304	0.00	0.11	0.01
HH spouse's age	37.6	(15)	188	35.8	(14)	304	1.82	1.37	0.13
HH spouse's years of edu.	1.00	(1.39)	188	1.20	(1.58)	304	−0.20	−1.58	−0.12

Table 1—MCH-FP Program Eligibility by Birth Year

Birth cohorts	Birth cohort label[a]	Program eligibility[b]
October 1947–September 1972	25–49	*Pre-intervention group*
October 1972–September 1977	20–24	*No interventions, potential sibling competition* i. Not eligible for child health interventions and unlikely to use family planning and maternal health interventions. ii. Potentially affected by the program through sibling competition.
October 1977–February 1982	15–19	*Intensive family planning and maternal health interventions* i. Mother eligible for family planning, tetanus toxoid vaccine, folic acid, and iron in last trimester of pregnancy. ii. Children under age five eligible for mainly late measles vaccination in Treatment Area 1. iii. Potentially affected by sibling competition from younger groups.
March 1982–December 1988	8–14	*Child health interventions added*
March 1982–October1985	12–14	*Child health interventions added in Treatment Area 1* i. Mother eligible for family planning, tetanus toxoid vaccine, folic acid, and iron in last trimester of pregnancy. ii. Children under age five eligible for on-time measles vaccination in Treatment Area 1, but for late DPT, polio, and tuberculosis vaccination in entire treatment area.
November 1985–December 1988	8–11	*Child health intervention extended to entire treatment area* i. Mother eligible for family, tetanus toxoid vaccine, folic acid and iron in last trimester of pregnancy. ii. Children under age five eligible for on-time vaccination (measles, DPT, polio, tuberculosis) and vitamin A supplementation. iii. Nutrition rehabilitation for children at risk.

Table A1: Family Planning Program and Birth Timing Relative to Ramadan: Birth Cohort 1974-95

	ramadan8to9			ramadan4to7		
	(1)	(2)	(3)	(4)	(5)	(6)
post×treated	-0.062**	-0.057**	-0.066*	0.085***	0.082**	0.083*
	(0.028)	(0.025)	(0.037)	(0.032)	(0.032)	(0.049)
post×treated×Hindu	0.076	0.070	0.161	-0.101	-0.100	-0.178
	(0.092)	(0.096)	(0.184)	(0.230)	(0.228)	(0.323)
post	-0.180***	-0.165***	-0.158*	-0.620***	-0.615***	-0.600***
	(0.046)	(0.048)	(0.083)	(0.063)	(0.063)	(0.093)
post×Hindu	0.119	0.105	0.133	-0.094	-0.100	-0.277
	(0.136)	(0.136)	(0.226)	(0.320)	(0.317)	(0.435)
R2	0.126	0.128	0.435	0.199	0.199	0.499
Observations	5632	5632	5632	5632	5632	5632
Mean Y	0.156	0.156	0.156	0.391	0.391	0.391
Month FE	Y	Y	Y	Y	Y	Y
Hindu× Year FE	Y	Y	Y	Y	Y	Y
Hindu × Village FE	Y	Y	Y	Y	Y	Y
Village TVC	N	N	N	N	N	N
Mother FE	N	N	N	N	N	N
F:	0.026	0.020	0.270	0.005	0.006	0.086
P-Value	0.873	0.889	0.604	0.944	0.939	0.770

Standard errors are clustered at the village ID level.
*** p<0.01, ** p<0.05, * p<0.1

Notes: The dependent variable ramadan4to7 takes a value of 1 if the individual was born 4 to 7 months after Ramadan and 0 otherwise. The dependent variable ramadan8to9 takes a value of 1 if the individual was born 8 to 9 months after Ramadan and 0 otherwise. Post takes a value of 1 from August, 1978 and 0 otherwise. The variable *treated* takes a value of 1, if the villages received the family planning program and 0 otherwise. The variable *post × treated* is the interaction of post and treated. All regressions include spline function of mother age at 5 year interval from age 15 onward and mother years of education. Village T.V.C are village time varying characteristics are large market, electricity, tube-well and satellite clinic. Post is interacted with whether the village has large market. For electricity, tube-well and satellite clinic post takes a value 1 from the year after these were established and 0 otherwise. This table is based on data from the MHSS.

Table A2: Ramadan Exposure and Mother Education

	Muslim Birth Sample				Hindu Birth Sample			
	Birth Cohort 1982-1995		Birth Cohort 1986-1995		Birth Cohort 1982-1995		Birth Cohort 1986-1995	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
motherage	0.000	0.000	0.000	0.000	0.006	0.006	0.005	0.005
	(0.001)	(0.001)	(0.001)	(0.001)	(0.005)	(0.005)	(0.004)	(0.004)
motheredu	0.002	-0.017	-0.007	-0.028	-0.068	-0.081	-0.091	-0.050
	(0.013)	(0.017)	(0.018)	(0.025)	(0.061)	(0.164)	(0.074)	(0.107)
treated× motheredu		0.045*		0.052		0.017		-0.052
		(0.024)		(0.032)		(0.174)		(0.132)
R2	0.248	0.249	0.359	0.360	0.633	0.633	0.775	0.776
Observations	3422	3422	2356	2356	317	317	223	223
Mean Y	0.143	0.143	0.160	0.160	0.142	0.142	0.152	0.152
Month FE	Y	Y	Y	Y	Y	Y	Y	Y
Block× Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Village FE	Y	Y	Y	Y	Y	Y	Y	Y
F:motheredu+treated ×motheredu		3.02		1.49		1.11		1.34
P-Value		0.08		0.22		0.30		0.25

Standard errors are clustered at the village ID level.
*** p<0.01, ** p<0.05, * p<0.1

Notes: The dependent variable *nonexposed* takes a value of 0 if pregnancy overlapped first, second and third trimester and 1 otherwise. The trimesters are calculated from the birth date based on the assumption that pregnancy lasts for 9 months. The treated takes a value of 1 if the villages received the family planning program and 0 otherwise. Variables *motherage* is mother age at survey and *motheredu* takes a value of 1 if mother has any formal education and 0 otherwise. Post takes a value of 1 from August, 1978 and 0 otherwise. *treated* takes a value of 1 if the areas received family planing program and 0 otherwise. Column 1 and 2 are Muslims from the control areas and the treatment areas only. Column 3 is Hindus from the treatment areas. This table is based on data from the MHSS.

Table A3: Birth Relative to Ramadan and Male Fragility

VARIABLES	(1) malec	(2) malec
	(1)	(2)
ramadan_0	0.038	0.036
	(0.043)	(0.042)
ramadan_1	0.005	-0.003
	(0.054)	(0.056)
ramadan_2	0.017	0.016
	(0.049)	(0.050)
ramadan_3	0.072	0.067
	(0.050)	(0.053)
ramadan_4	0.033	0.035
	(0.044)	(0.045)
ramadan_5	-0.015	-0.018
	(0.043)	(0.043)
ramadan_6	0.064*	0.068**
	(0.034)	(0.031)
ramadan_7	0.021	0.019
	(0.044)	(0.046)
ramadan_8	0.043	0.045
	(0.053)	(0.053)
ramadan_9	0.045	0.048
	(0.039)	(0.040)
R2	0.039	0.043
Observations	2981	2937
Mean Y	0.528	0.529
Month FE	Y	Y
Year FE	Y	Y
Village FE	Y	Y
F	0.921	0.818
P>F	0.341	0.369

Standard errors are clustered at the village ID level.
*** p<0.01, ** p<0.05, * p<0.1

Notes: The dependent variable *malec* takes a value of 1 if the individual born is a male. $ramadan_0$ is the month of Ramadan. Each $ramadan_x$ x=1-9 takes 30 days from the time Ramadan ends. $ramadan_x$ takes a value of 1 if true and 0 otherwise. Omitted category is the births which were not exposed to Ramadan. Column 1 does not include any mother level observables. Column 2 include mother age defined as spline function with knots at 5 year interval from 15 onward and mother years of education.