Understanding the Role of Parents in Reinforcing Inequalities Over the Life Course

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Abstract

There is no consensus on whether parents reinforce or compensate birth endowments. In this paper, we use multiple shocks- exposure to Ramadan and rainfall in utero- to study the broader relation between endowments and parental responses over the life course across multiple dimensions using data from Indonesia. We find parents make reinforcing investments in health and nutrition in early childhood but do not make further reinforcements in late childhood. We investigate the external validity of the results by using DHS data from 50 countries. We find parents reinforce initial inequalities in poorer countries relative to richer countries.

Keywords: Birth Endowment, Intra Household Resource Allocation, Ramadan, Rainfall.

JEL Classification: I1, J1, O12

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1 Introduction

Human development begins in utero. More than 2000 studies in epidemiology and economics document that the fetal environment is a key predictor of later life disease, cognition and labor market success, suggesting serious long-run consequences of neglect of pregnant women today (Almond and Mazumder, 2011; Almond et al., 2017; Behrman and Rosenzweig, 2004; Crookston et al., 2013; Cunha et al., 2006; Currie and Vogl, 2013; Heckman et al., 2006; Hoddinott et al., 2008a,b; Majid, 2015; Maluccio et al., 2009; Richter et al., 2017).[1] Despite the fact that economists have long recognized the importance of parental investments in formation of human capital and wellbeing, surprisingly little is empirically understood about the role of parental investments in mapping fetal inequalities to later life wellbeing. Neither theory (Becker and Tomes, 1976; Behrman et al., 1982) makes unequivocal predictions about parental response nor has the empirical evidence any consensus on whether parents compensate or reinforce inequalities in endowments. As Yi et al. (2015) state "How parents invest in children with different endowments is not well-studied and there is no consensus in the literature."

In this paper, we ask the following questions: Do parents tend to reinforce or compensate inequalities in fetal endowments? If they do, when over the life course do parental investments respond to child endowments and through which dimensions- health, education or non human capital transfers- do parents respond? How similar are parental responses across countries with different cultures and genetic endowments? To answer these questions, we take a life course perspective and study parental responses to a fetal shock during pregnancy, on several dimensions of investments in utero and between ages 0-5, 5-15 and adulthood.

We use the overlap of Ramadan fasting with pregnancies for Muslims as a natural experiment to study parental response to fetal under nutrition. Though pregnant women are exempted from fasting, estimates from diverse societies show between 70 to 90 percent of Muslim pregnant women fast during Ramadan (Almond and Mazumder (2011)). Fasting during pregnancy leads to decline in maternal glucose levels, leads to biochemical changes and affects total calorie intake. We build on earlier studies which have shown overlap of pregnancies with Ramadan have long run consequences on birth weight, academic performance, cognitive ability and labour market outcomes (Almond and Mazumder, 2011; Almond et al., 2015; Majid, 2015). Since Ramadan follows a lunar calendar, the month of

[1]We conducted broad searches in PubMED for manuscripts published in any language from database inception to Jan 26, 2018, using the following search terms: "fetal origins", "Barker hypothesis" "developmental origins" (PubMed) and search terms "fetal", "Barker" OR "in-utero" OR "prenatal" in EconLit. The searches yielded more than 2000 papers.

Ramadan moves by 11 days every year on the Gregorian calendar. We use this feature to identify the effects of Ramadan and isolate it from season of birth, a potential confounder. In summary, our identification strategy compares parental investments over several dimensions and over a life course for children exposed to Ramadan in utero with unexposed children.

Our main context of study is Indonesia, the largest Muslim majority country in the world. We use data from all fives waves of the Indonesian Family Life Survey (1993, 1997, 2000, 2007 and 2014) to study parental investments.[2] We study effects of fetal exposure to Ramadan in Indonesia on three primary health investments-vaccines, dietary intakes, and expenditures on acute health care (such as over the counter medicines). For education, we study total expenditure on primary and secondary education and for non-human capital we study gifts received/dowries at time of marriage. We also measure parental beliefs in late childhood about future success of their children. In addition, we also study spillover effects on siblings and parental response on their subsequent fertility.

We find that for children between 0-5 years, parents make reinforcing investments in vaccinations, diet as well as for acute health care. Exposed children are 3 percent less likely to have completed all vaccinations, 3 percent less likely to have consumed any protein in the last week of the survey and parents spend 40 percent less on medicines for exposed children.[3] There are no systematic effects on human capital investments during late childhood (aged 5-15). We do not find any under-investment for exposed children on education, food or medicine in late-childhood. However, we find fathers have lower expectations about general future wellbeing of the exposed children. In later life i.e when they are adults, parents compensate the undernourished child by giving them non-human capital transfers in the form of larger dowries at the time of marriage. We do not find any evidence of parental response in ante-natal care. It is also possible parents may reallocate resources among siblings (Abufhele et al., 2017; Nerlove et al., 1984; Rosenzweig and Wolpin, 1995). However, we don't find any spillover effects on biological siblings suggesting that parents are not reallocating resources to non-exposed children instead. We also do not find any effects on number of children born after the exposed child, implying parents do not respond by altering fertility.

We perform several robustness checks. One concern with the exogenity of Ra-

[2]This is in contrast with previous work on Ramadan in Indonesia (Van Ewijk, 2011; Majid, 2015) which mostly use a single wave of the IFLS. This has two advantages over previous work a) by using 5 waves we can use data from several cohorts thus reducing the possibility of being confounded by season of birth. b) it provides large enough sample sizes to compare with non-Muslims.

[3]We use completion of BCG, Measles, Polio and DPT vaccinations.

madan is mothers may selectively time birth with Ramadan (Ahsan, 2014; Karimova, 2015). We do not find any systematic evidence of parents timing their birth with Ramadan. First, we check if the overlapping of pregnancy with Ramadan is associated with parental socio-economic status. We find, exposure to Ramadan is not associated with mother's education, father's education, per capita household consumption expenditure and household size. Second, we use data from the 2010 Indonesian census and construct cohort sizes at month of birth-year of birth level. If parents were selectively timing their birth with respect to Ramadan, we would see cohort sizes of month of birth-year of birth pairs whose in utero period overlapped with Ramadan to be less than cohort sizes of month of birth-year of birth pairs whose in utero period did not overlap with Ramadan.[4] We find no such effect and the effects are largely unchanged over time. Third, we repeat our analysis by comparing siblings, i.e by controlling for biological mother fixed effects. Our results remain broadly similar. [5]

Several factors like parental preferences, inequality aversion, costs of investments, knowledge of the human capital production process, credit constraints may affect decisions about parental investments. Thus the effects from Indonesia may not be generalizable to other contexts as these factors may vary across different contexts and cultures. To determine external validity of the results we use the Demographic and Health Survey (DHS) data from 50 countries around the world with a significant Muslim population and see the effects of exposure to Ramadan in utero on parental investments, specifically on vaccinations.[6] We find significant heterogeneity in parental investments in response to exposure to Ramadan in utero. In 14 countries we find significant reinforcement behavior. In these countries, children exposed to Ramadan are less likely to have completed their vaccination course. On the other hand, in 11 countries we find compensating investments by parents. In these countries, children exposed to Ramadan are more likely to have completed their vaccination course. In the remaining 25 countries we find no statistically significant effects of exposure to Ramadan on completion of vaccination. We then explore patterns of this heterogeneity. We combine data

[4] If exposure to Ramadan had any effects on mortality the effects will be even stronger.

[5] Another concern with Ramadan exposure is misclassifying pre-term births. Since we do not have exact dates of conception and if Ramadan exposure happens early on in the pregnancy, some pre-term births (unrelated with Ramadan) who did not experience Ramadan in their first trimester will be erroneously classified as exposed. If under investments are systemically related with pre-term births then we will be overstating the effects. However, such systematic misclassification will also be present for non-Muslims. If these systematic relations were driving the results we would also observe similar effects on non-Muslims. We do not find any similar effects on non-Muslims.

[6] We focus on vaccinations in the DHS because of comparability with IFLS. In addition, the DHS data covers a range of 32 years, thus covering the full cycle of Ramadan.

from all 50 countries and interact exposure with several macro economic indicators, like per capita GDP and proportion of Muslim population in the country. We find that parents are more likely to reinforce in poor countries than in richer ones. Proportion of Muslim population in the country does not explain the heterogeneity.

Another concern of external validity is the generalizability to other kinds of fetal shocks, as different types of fetal shocks may effect different dimensions of endowment at birth and consequently lead to different parental responses. We address this by studying the effects of another shock in utero namely, rainfall shocks on the same dimensions of parental investments, using the same five waves of IFLS data from Indonesia.[7] Rainfall shocks affect income and consumption of households. We establish this by comparing per capita consumption of households within a same IFLS community over different survey years and relating this with the rainfall shock in the community in the survey year. We then use information from different waves of IFLS and determine the district of birth of individuals and then combine this with monthly level rainfall data. We then compare parental investments of individuals born in the same district-month but over different years, and relate this with rainfall shocks they received in utero. We find comparable results with Ramadan with a same reinforcing behavior in vaccinations and a compensation in dowry.

This paper makes five important contributions. First, unlike previous work studying effects of Ramadan (Almond and Mazumder, 2011; Almond et al., 2015; Van Ewijk, 2011; Majid, 2015) or rainfall shocks (Adhvaryu and Nyshadham, 2015; Maccini and Yang, 2009; Shah and Steinberg, 2017) in utero and their long-term effects on human capital and labor market outcomes, we are primarily interested in parental investment response to fetal shocks to better understand the mechanisms through which these shocks play out over the life course. Second, existing studies of parental investments focus on either late childhood (Rosenzweig and Zhang, 2009) or during early life (Adhvaryu and Nyshadham, 2016; Hsin, 2012). Parents may choose to make reinforcing investments in early life based on efficiency concerns but compensate later as children get more agency or due to inequality aversion. We contribute to the existing literature by studying parental investments over a life-course. Third, as Yi et al. (2015) argues most of the current literature focuses on a single dimension of parental response. We complement our life cycle perspective on parental responses by studying multiple dimensions of health

[7] There is a large literature that studies rainfall shocks in utero and the long and short human capital effects. This literature also does not directly look into parental investments. See Maccini and Yang (2009) and Shah and Steinberg (2017).

investments and educational investments. In addition, Becker and Tomes (1976) predicts substitutability between human and non-human capital transfers. To the best of our knowledge, we are the first paper to study parental responses to fetal shocks in adulthood, particularly non-human capital transfer. Fourth, existing studies on parental investments do not determine external validity of the results. We contribute to the existing literature by studying external validity of our results across different countries and also across different kinds of in utero shocks. Fifth, our paper contributes to the better understanding of cultural and religious practices on economic development (Campante and Yanagizawa-Drott, 2015; Clingingsmith et al., 2009; Kuran, 2004, 2014; Kuran and Rubin, 2018; Meyersson, 2014). Though, some studies have focused on the effect of Islam on human capital, not much is known about the effect of Islamic cultural practices on parental investments (Ashraf et al., 2016). We add to this literature by studying the effects of Ramadan on parental investments using data from more than 50 lower-middle income countries across Asia, Middle East and Africa.

The paper is organized in the following fashion. Section 2 provides a background to Ramadan, section 3 provides a conceptual framework, section 4 describes the data, section 5 describes the empirical strategy, section 6 describes the results and section 7 concludes.

2 Background on Ramadan

Ramadan is a holy month in Islam (9^{th} month in Islamic calendar). Practicing Muslims are required to fast from dawn to dusk during the month of Ramadan. They are required not to consume any water or food during the fasting period from dawn to dusk. Since the Islamic calendar follows a lunar calendar, the exact one month of Ramadan is not fixed in the Gregorian calendar. The exact one month moves 11 days backwards every year in the Gregorian calendar. Thus over approximately 33 years Ramadan covers all seasons and months.

A natural question is whether pregnant mothers fast during Ramadan? Islam does not require pregnant women to fast. However, pregnant women who exempt themselves from fasting are required to compensate for it by fasting later or under some interpretations of Koran by paying alms to the poor. Comprehensive data on fasting during pregnancy does not exist. In a survey from Iraq, 71 percent of $4,343$ pregnant mothers observed fast for at least one day in the one month period (Arab and Nasrollahi, 2001). In another study from Jakarta, Indonesia of the 187 pregnant women interviewed, 80 percent fasted during Ramadan (van Bilsen et al., 2016). Evidence from several other countries like Singapore, Yemen, Gambia, England and the U.S suggests between to 70 to 90 percent of pregnant women observe fasting (Almond and Mazumder, 2011). There are several possible reasons for observing the fast. Pregnant women want to avoid fasting alone and they fast with their family. Also, fasting during Ramadan is associated with several community or family level events like breaking the fast together, praying etc. Women do not want to feel alienated from these community activities. In addition, in many developing countries women are often not aware about their pregnancies in the first trimester. However, to the extent individuals choose to fast during Ramadan our estimates should be interpreted as the reduced form (or Intent to Treat (ITT)) estimates of the pregnancy overlapping with Ramadan.

There are several plausible mechanisms through which Ramadan affects fetal health. a) Fasting during Ramadan can reduce calorie intake. In a study from Iran, (Arab, 2004) shows most pregnant women have over 500 calorie deficiency due to fasting. In a study from Gambia authors find, a 1 Kg weight loss due to fasting from Ramadan (Strickland and Ulijaszek, 1993). Reduced calorie intake may effect fetal growth. b) Fasting also leads to reduced glucose level in the blood and this may also lead to reduced fetal growth and lower birth weight (Scholl et al., 2001; ter Braak et al., 2002). c) Maternal fasting may also lead to a set of biochemical changes known as “accelerated starvation". “Accelerated starvation" is associated with diminished cognition and neurological impairment (Metzger et al., 1982; Moore et al., 1989; Rizzo et al., 1991).

We build on earlier studies in economics that have shown long run effects of pregnancies overlapping with Ramadan. Our paper is closely related to two papers. Using the same data (IFLS) as ours and in the same context of Indonesia, Van Ewijk (2011) finds individuals who were exposed to Ramadan in utero are more likely to report poorer general health and Majid (2015) finds individuals who were exposed to Ramadan in utero score less on cognitive and math test and also work fewer hours. Two other important studies also find long run negative effects of fasting during Ramadan. Using data from Michigan, Uganda and Iraq, Almond and Mazumder (2011) shows that the overlap of Ramadan with the pregnancy leads to lower birth weight, less number of male children, increased learning disability. In another study using data on academic performance for children of Bangladeshi and Pakistani families in England, Almond et al. (2015) finds the overlap of pregnancy with Ramadan reduces the academic outcomes at age 7. In this paper, we do not study the direct effects of fasting during Ramadan on human capital formation. Instead, we focus on the subsequent parental investment responses to better understand the behavioral response and coping strategies of parents to endowment shocks on their children.

3 Conceptual Framework

In this section we provide a conceptual framework to understand the role of parental response to the Ramadan shock in utero. We mostly borrow from the frameworks presented in (Almond et al., 2017; Cunha and Heckman, 2007). Following Cunha and Heckman (2007) and Almond et al. (2017), we consider a two period Constant Elasticity of Substitution (CES) function as a production technology of human capital. This can be written as

$$h = A[\gamma(\bar{I}_1 + \mu_{1g})^{\phi} + (1 - \gamma)(I_2 + \mu_{2g})^{\phi}]^{1/\phi}$$

Let h denotes health or human capital as assessed over the life course. A represents factor productivity, and and $\bar{I}_1$ and I_2 are the parental investments made in the prenatal period and in the period after birth (upto adulthood). The first childhood period is denoted with subscript 1 (e.g., in utero) and the second period (say from preschool to adulthood) with subscript 2. A bar superscript indicates that the first period investment is already set, and what is under consideration is the second period investment.

μ_{1g} represents Ramadan exposure in utero . If we hold I_2 to be fixed (unresponsive to μ_{1g}), then the impact of Ramadan exposure in utero is purely biological however if I_2 does change, then the effects of Ramadan may not be entirely biological but driven by parental responses as well. The parameter γ , where $\gamma \in [0, 1]$, represents the weight each childhood period receives in the production of adult health (or more generally, adult human capital). The parameter ϕ , where $\phi \in (\infty, 1]$, denotes the extent to which investments in different periods are substitutes or complements.

We assume that parents make the investment decisions for their child. In addition, we assume the investment decision on one child, is independent of their siblings. Investments are costly and they are valuable insofar as they improve h. Parents maximize their utility, and they care about their consumption as well as health of their children. The budget constraint of the parents can be expressed as

$$Y = p_c C + p_I I_1 + p_I I_2/(1 + r)$$

where Y denotes family income, p_c and p_I are the market prices of consumption and investment, and r is the interest rate. We assume parents have a Cobb-

Douglas utility function.

$$U \ = \ (1-\alpha) log \ C \ + \alpha log \ h$$

To understand how a negative shock in utero can alter parental investment, it is helpful to consider two extreme cases of health production functions. First, consider a case where investment across two periods are perfect substitutes, i.e $\phi = 1$. Optimal investment response I_2^* will be a function of all the parameters, i.e α , γ and μ_{1g} apart from prices, income and interest rate. Solving the optimization problem gives us,

$$\frac{\delta I_2^*}{\delta \mu_{1g}} \ = \ -\frac{(1-\alpha)\gamma}{(1-\gamma)} \ < \ 0$$

As Ramadan exposure is a negative shock (makes children sick), the above derivative implies investments in the second period increases in response to Ramadan shock in utero. That is, period 2 investments are compensatory. When investment responses are compensatory, reduced form analyses of the impact of Ramadan exposure in utero on long run human capital formation will tend to understate the biological effects (Royer, 2009). In addition, we will also find increased investments in the early childhood period in response to the Ramadan shock.

The other extreme example is when investments in two periods are perfect compliments (Cunha and Heckman, 2007). In this case the health (or human capital) production function will be

$$h \ = \ A \ Min[\gamma(\bar{I}_1 \ + \ \mu_{1g}) \ , (1- \ \gamma)(I_2 \ + \ \mu_{2g})]$$

Solving for the optimal investment response gives,

$$\frac{\delta I_2^*}{\delta \mu_{1g}} \ = \ \frac{\gamma}{(1-\gamma)} \ > \ 0$$

The period 2 investment response is now reinforcing with respect to the initial shock. Attempting to ameliorate Ramadan exposure later in life is completely ineffective, so it is optimal to match period 1 investments (subject to weighting by γ) and consume the rest. Thus we will find parents reduce investment in early childhood in response to the Ramadan shock.

Since the exact nature of the human capital production function is not known, it is not possible to make unambiguous theoretical predictions about parental investments in response in utero shock. It thus becomes an empirical question that we attempt to answer in this paper.

Most of the current literature that uses mild shocks (weather shocks, alcohol/tobacco , Ramadan, etc) to test for the fetal origins hypothesis aims to capture the reduced form effects of early-childhood shocks without explicitly assuming that the second period parental investments are responsive to these shocks (Almond et al., 2017). The above framework highlights the role of parental investments in determining the effects of these shocks on long term human capital formation.

The above framework can easily be extended to incorporate multiple dimensions of investments and multiple periods of investments after birth. We look at multiple dimensions of investments over a life course in the empirical analysis. By taking a flexible life course perspective and by considering multiple dimensions of investments we provide evidence on two critical questions. First, if parents do respond (reinforce or compensate the effects of initial fasting) when in the life of a child do they do so? What are the most sensitive and critical periods in a person's life when parents are most likely to respond? Second, how do they do it? On which dimensions of investments do parents respond? Thus, the empirical analysis not only sheds light on the underlying technology of skill formation- i.e. which dimensions of skills are malleable to change from fetal insults, but also provide indirect evidence on which investments are most likely to matter in explaining the cognition and labor market effects of Ramadan fasting documented before (Almond and Mazumder, 2011; Majid, 2015).

4 Data

4.1 Indonesian Family Life Survey (IFLS)

In this paper we use data from an exhaustive set of five waves of the Indonesian Family Life Survey (IFLS). IFLS is a nationally representative survey, it covers half of Indonesia's provinces and represents 83 percent of the population. IFLS data was collected for five waves in 1993, 1997, 2000, 2007 and 2014. This is a panel data set which tracks households over five waves. The attrition rate in IFLS is particularly low with 90 percent followup. IFLS has several features which makes it particularly suitable for our study. First, Indonesia is the largest Muslim majority country in the world and around 88 percent of individuals in IFLS are Muslims. This makes it particularly useful as the effect of Ramadan should particularly be concentrated for Muslims and thus we will have a large enough sample. Second, IFLS collects very credible data on exact date of birth, which is important for determining the exposure status of individuals. Third, IFLS collects detailed data on parental investments. It also collects data for individuals of all ages. This helps us in finding effects on multiple dimensions of parental investments and over a life course. Fourth, we build on earlier work that has already shown the long run effects of fasting during pregnancy on cognitive health and labour market outcomes using the IFLS (Van Ewijk, 2011; Majid, 2015).

We use data from several modules of IFLS. For information on vaccinations, medicinal expense, food and educational expense we use data from the children module of IFLS. [8] We then combine data from all the five waves for these measures. Since we are combining the waves, we will have some individuals who were interviewed in more than waves and asked the same question, like education expense, food consumption. Since the answers may vary across waves, we count those individuals twice and cluster our standard errors at the individual level. We use the dowry information from the adult module [9] and include information on all marriages, if the individual married more than once. We also use the adult module for information on expectations about their children and we include all children about whom expectation answers were given and were also in the household roster. In addition, we also use the pregnancy history from the women's module [10] to get information on use of pre-natal care and fertility. And finally, we use the household roster to get information on siblings, religion, mother's education and father's education.

Since IFLS is a panel dataset, we have date of birth information about the same individual in many waves. Within a wave the date of birth information is also available in multiple files.[11] There are at times inconsistencies in the date of birth information both across waves and also within a wave. We have eliminated observations which have inconsistencies in exposure status across waves. This eliminates individuals for whom we do not have a consistent information on date of birth and thus reduces measurement error.[12]

In Table 1 we present descriptive statistics of our sample for Muslims by exposure status. This table provide a preliminary picture of differential investments on children particularly on children less than 5 years. As the table indicates, Muslim exposed children are less likely to be vaccinated, parents spend less on their medication and they also consume less nutritious food.

[8] Book 5 for individuals 0-15.

[9] Book 3A for adults.

[10] Book 4 for women.

[11] In the cover files of the modules, in the AR file of the wave and in the US file for anthropometric measures.

[12] See the Data Appendix for a detailed description.

4.2 Demographic Health Surveys

We use data from the Demographic Health Surveys (DHSs) from 50 countries and restrict our sample to Muslims only. [13] [14] We use the children's recode file to construct measures of vaccinations and get information on date of birth. Most DHS's do not have exact date of birth and have information only on month of birth and year of birth. Thus we only use month of birth and year of birth information to construct our measure of Ramadan exposure. Since DHSs only ask information on vaccination for children younger than five years, we limit our sample to cohorts born five years prior to each survey. Though this limits the number of cohorts we can use, it decreases measurement error by reducing recall bias of mothers.

4.3 Rainfall Data

We use monthly level rainfall data which are collected by University of Delaware.[15] The data is available from 1900 to 2014 on a 0.5 X 0.5 latitude longitude grid across the globe. We take the latitude longitude of a district centroid and match it with all the grid points within 275 kms radius of the district centroid.[16] We then take an average of the rainfall from the matched grids within the radius and weight it by the inverse distance from the grid to the district centroid. Thus we obtain monthly rainfall for each district from 1900 to 2014.

We define in utero rainfall shock in several ways. a) We calculate total rainfall in the in utero period including the birth month. b) We calculate log of mean rainfall for each district-month from 1900-2014. We then take the difference between log of rainfall for each month in utero and the log of mean rainfall for that district-month. We then add all these deviations for the 10 months in utero and the birth month. c) We calculate median rainfall for each district-month from 1900-2014. We then create a dummy which takes the value one if the rainfall in a particular

[13] Of all the countries surveyed in the DHS, we include only those countries which have a Muslim population of 1 percent and above in the census. In some DHS waves, information on religion is missing. For these countries-waves in the DHS (that have individual level data on religion missing) we do the following: If the country has a significantly large proportion of Muslim population (above 90 percent in the census), we include the entire country-wave and treat everybody in that DHS wave as Muslims. If the country has less than 90 percent of the population as Muslims in the census, we exclude that entire country-wave. In addition, Angola, Sao Tome and Principe, Swaziland are excluded because they have very few observations for Muslims in the DHSs.

[14] Afghanistan and Nepal are excluded because of inconsistencies with the date of birth of information. Trinidad and Tobago is excluded because of missing information on BCG vaccinations.

[15] The data are available from `http://climate.geog.udel.edu/~climate/html_pages/download.html`. This data have been used in several studies of rainfall and human capital, see Shah and Steinberg (2017) and Adhvaryu and Nyshadham (2015).

[16] We choose 275 kms to make sure every district has atleast one grid within this radius.

month in utero is less than the median rainfall for that district-month. We then add these dummies for all the ten months and thus create a variable that is the number of months in utero, rainfall was below the median for that district-month. d) To understand if more extreme shocks have a different impact, we calculate the bottom 20^{th} percentile rainfall for each district-month from 1900-2014. We then create a dummy that takes the value one if the rainfall in a particular month in utero is less than the 20^{th} percentile rainfall for that district-month. We then add these dummies for all the ten months and thus create a variable that calculates number of months in utero rainfall was below the 20^{th} percentile for that district-month. e) To understand if positive shocks are different from negative shocks, we calculate the top 80^{th} percentile rainfall for each district-month from 1900-2014. We then create a dummy that takes the value one if the rainfall in a particular month in utero is more than the 80^{th} percentile rainfall for that district-month. We then add these dummies for all the ten months and thus create a variable that calculates number of months in utero rainfall was above the 80^{th} percentile for that district-month.

Finally, we also construct rainfall data at the IFLS community level. We repeat the same process as IFLS districts, except we match the data to latitude and longitude of IFLS communities.[17] We use a simple measure of a dummy that takes the value one if the total rainfall in the survey year was below the long run yearly median of that community.

5 Empirical Strategy

5.1 Ramadan

Our empirical strategy compares parental investment on individuals born with their in utero period overlapping with Ramadan, with individuals whose in utero period did not overlap with Ramadan. We limit our sample to Muslims only as the effect of Ramadan should only be on them. In particular we estimate the following equation.

$$Y_i = \alpha + \beta\ Exposure_i + \gamma\ X_i + \delta_m + \epsilon_i$$

Here, Y_i is measures of various parental investments. These are spending on health care, food consumption, education and transfer. $Exposure_i$ is a dummy variable which takes the value one if the individual's in utero period overlapped

[17]We choose 250 kms to make sure every community has atleast one grid within this radius.

with Ramadan. We use the exact date of birth with day, month and year to define exposure. We assume an average pregnancy lasts for 266 days and calculate the date of conception using the date of birth. We define an individual as exposed to Ramadan if the individual was exposed for the whole month of Ramadan in utero, and individuals who were not exposed at all as control group. X_i includes controls like the age of the individual and its square, gender, wave fixed effects. Since we get the date of birth information from several different files in IFLS, we also control for source of the date of birth information. δ_m is the month of birth fixed effect. In addition, since we use data from five waves, we also control for wave fixed effects.

We take two steps to account for measurement error in date of birth that may lead to misclassification of exposure. First, we drop individuals who were exposed for less than one month to Ramadan in utero. This helps us to account for misclassification due to preterm deliveries. To understand this, consider a preterm delivery where the pregnancy lasted for 240 days. Since we do not know the exact date of conception, we will consider the pregnancy lasted for 266 days. Suppose, Ramadan exposure happens during the first 26 days. In this case we would misclassify the birth as exposed if we were to include individuals who were exposed for less than one month to Ramadan. To minimize this problem we drop individuals who were partially exposed. In addition, this also helps to get more precise effects of Ramadan. Second, some pregnancies may last for more than 266 days and we may misclassify somebody as unexposed when they were actually exposed. Though very few pregnancies last for more than 266 days, we control for those pregnancies that were conceived within three weeks after the end of Ramadan, to account for this kind of misclassification.

We also follow a similar identification strategy for the DHS data with some small modifications. Since we do not have the exact date of birth in the DHS, we define a child as exposed using the month and year of birth and only using the month and year of Ramadan. We define a child as exposed if Ramadan both started and ended between and including the conception and birth month. We drop a child from the regressions if the Ramadan started on the birth month, but end the month after. We drop them because there is exposure after utero in the post natal period, which may also have an effect on subsequent parental investment. Like the regressions from IFLS we control for gender, age, age squared, month of birth and wave fixed effects in cases when we use multiple waves from a country. We also pool data from all countries and run a single regression to get at world wide effects. In those regressions, we include country-wave fixed effects and country-month of birth fixed effects to account for time varying country specific unobservables like measurement error, cultural factors etc and also country-specific seasonal effects.

5.2 Rainfall

As mentioned before, we also use in utero rainfall shocks to establish broad based external validity of our results. We use the following equation to estimate the effect of in utero rainfall shocks on parental investments.

$$Y_{idmy} = \alpha + \beta \; (In\, Utero\, Rainfall\, Shock)_{idmy} + \gamma \; X_i + \delta_{dm} + \phi_{my} + \epsilon_{idmy}$$

Here Y_{idmy} is the measure of parental investment of an individual i, born in district d, month m and year y. $(In\;\; Utero\, Rainfall)_{idmy}$ measures the rainfall received in utero of an individual i, born in district d, month m and year y. As discussed before we use several different measures of rainfall in utero to capture effects of moderate to extreme negative shocks as well as positive shocks. X_i are individual level controls like age at the time of the survey, age squared and gender, wave fixed effects and source of the date of birth information. Cross sectional comparisons of individuals born in different districts will yield biased results. This is because several unobserved factors vary at district level that can impact parental investments. Similarly, since season of birth is often related with parental SES and rainfall is seasonal, comparing individuals born in different seasons would also yield biased results. Thus we control for district-month of the birth fixed effects in δ_{dm}. This will control for all time invariant district level seasonal factors. Cohorts born in different years can be affected by factors specific to those years like political shocks, conflicts etc. ϕ_{my} controls for month of birth-year of birth fixed effects. This accounts for time varying shocks that are common across Indonesia. Thus our identification relies on comparing parental investments of individuals who were born in a same district-month but over different years. The identification assumption is within a same district season, the rainfall an individual receives in utero is random and unpredictable from before.

6 Results

6.1 Children less than five years

In this section we explain in detail our findings on parental investment for children of less than five years. To do this we use information from vaccination cards wherever available and if not available self reported data is used. Vaccination questions were asked for all individuals aged 0-5. Since, we are only information from the last five years before the survey, this limits chances of recall biases. We find compared to unexposed children of same age, parents are less likely to invest in vaccination for their exposed children. As Table 2 indicates, parents are 3 percent less likely to complete all vaccinations.[18] These results can be interpreted as parents underinvesting in preventative health care of children in response to their initial endowment. These results are important for two reasons. First, despite improvements in vaccination coverage across the world, WHO estimates 1.5 million deaths per year can be avoided with improved coverage of vaccinations and an estimated 19.5 million infants are missing out on basic vaccines. Though supply side and informational constrains are considered as an important impediments to vaccination access, our results suggest under investments in vaccinations could be an strategic parental response. Second, our results also has important implications for public policy at large. Community level immunity against diseases can only be achieved, with a critical amount of immunization. Underinvesting in immunization thus has significant negative externality.

Second, we also find compared to unexposed children of same age parents also invest less in the diet of the exposed children. Detailed information on the diet of the child was asked. Information was collected on the several kinds of food items consumed in the last week preceding the survey. Table 2 illustrates the results. In column 3 we show the effects on whether the child consumed any protein in the last week.[19] We focus on the consumption of protein because it is relatively more expensive than carbohydrates and also important for a balanced nutrition. As the table indicates, we find exposed children are 3 percent less likely to have consumed any protein in the last week preceding the survey. Our results suggest parents reinforce the initial endowment by investing less in food quality. These results could potentially have important long run consequences on health and economic productivity. Nutrition in early life is shown to have long run effects on wages and on health (Hoddinott et al., 2008a; Hoynes et al., 2016). Hoddinott et

[18] Here, we consider completing one dose BCG, one dose of Measles, atleast one dose of Polio, atleast one dose of DPT and atleast one dose of Hepatitis B as completing all vaccinations.

[19] Consumption of eggs, meat or fish is considered as having any protein.

al. (2008a) shows provision of a nutritional supplements to children of less than three years old in Guatemala improves their hourly wages several decades later. Moreover, as our results indicate, effects of early life shocks can be exacerbated by systematic underinvestment by parents in nutrition.

Third, we show results on parental investments in medicine and acute care, particularly on self treatment. IFLS collects information on medical spending of the last week preceding the survey. Information was collected on expenditures on over the counter drugs, traditional medicines and topical medicines. Table 2 show the results for spending on medicine. As the results show, we find a consistent pattern of underinvestment even in self treatment for exposed children in early life. As Table 2 shows, parents spend 44 percent less on medicine for treatment of their exposed children.

6.2 Children older than five years and Adults

In this section, we present our findings on parental investment for children older than five years. Table 3 presents the result. First, we show the effects on educational investments. IFLS collects detailed data on educational expenses. It has information on the amount spent on registration, exams, books, uniforms, transportation, housing etc in the academic year preceding the survey. We take log of aggregate expenditure on all these categories as our dependent variable. As the table indicates, we find no evidence of parents investing less on educational expenses of children exposed to Ramadan in utero, compared to unexposed children of same age. Thus, we find no evidence of parents responding to initial endowment in terms of investment in education. Second, we show results on consumption of medicine. This is the expenditure on same set items like over the counter drugs, traditional medicine etc as mentioned in the previous section. As with children of 0-5 years, we use log of aggregate expenditures on these categories as our dependent variable. This expenditure can be interpreted as investment in acute care. Investment responses to acute and preventative care is often different, with individuals showing more inelastic response to acute care. Thus it is important to study parental response separately for acute and preventative care. Column 2 of Table 3 shows results on spending on medicine in the last four weeks prior to interview. As the results show parents do not underinvest in acute care of exposed children when they grow older. In column 3 of Table 3 we show the effects on consumption of any protein in the last week preceding the survey. We do not find any evidence of parents reinforcing or compensating the initial birth endowment.

These results highlights the importance of focusing on a) multidimensionality of investments and b) on taking a life cycle approach to study parental investments. When contrasted with the investment response on younger kids (0-5), it shows a) parents may reinforce in one dimension like vaccinations while they may not make any reinforcing or compensating investments in other dimensions like education. b) It also shows on the same dimension of investment like spending on medicine parents reinforce in early life but may not make the same reinforcing investments in later life. Several factors may explain this pattern. First, it is possible that children get some agency as they grow old and it is more difficult for parents to make such reinforcements for older children than for younger children. Second, since we do not know the true human capital production function, the nature of relationship between endowments at birth and parental investments may vary with age and also with different dimensions.

We also show results on parental expectations. It is important to understand parental expectations as parents are responding to their subjective expectations and as researchers it may not be possible to capture all the dimensions of parental investments and parental expectations can serve as an important proxy. In column 4 and column 5 of Table 3 we show expectations of mothers and fathers separately (for the same set of individuals) about future wellbeing of their children. Parents were asked, when their children will be of their age whether they expect them to have a better life than them. As results in column 4 and column 5 indicates fathers expect exposed children are less likely to have a better life when their children will be of their age. However, mothers do not form lower expectations for exposed children about their future life. The contrast, between mothers and fathers expectation highlights an important area of future research. If parents are responding to their subjective beliefs and if mothers and fathers have different expectations, the bargaining power of each parent will matter in terms of investment. Though we do not have data, it will be important to study mother and father specific investments on children.

In addition to investments on human capital we also study non-human capital transfers to adults. Such differential investment may be a result of inequality aversion preferences. On the other hand, it is also possible parents enjoy some monetary or social returns to non-human capital investments. For example, when transferring productive assets parents may prefer the productive child. Moreover, parents may consider human capital and non-human capital investments as substitutes. To understand this we study the effect of exposure to Ramadan on the value of dowry and gifts received at the time of marriage. Column 6 of Table 3 shows the results. As the results indicate, adults who were exposed to Ramadan in utero get 8 percent more gifts and dowry for marriage. The results highlight parents also make complimentary investments on non human capital as transfer to adults. Thus in addition to investments on human capital, studying non-human capital transfers are important to understand the parental investment behaviors over the life course. Our results are consistent with the wealth model (Behrman et al., 1990, 1995)

6.3 Siblings, Fertility and Pre-Natal Care

The allocation of resources between siblings may depend on each sibling's endowment at birth. Thus the lower endowment of a child can have effects on investments on his/her siblings. If parents reinforce and redistribute, then lower endowment of a child will lead to higher investment on his/her siblings. On the other hand if parents compensate, the higher investment in the child may crowd out investment on his/her siblings. However, it is also possible, the human capital of each child enters separately in the parent's utility function and thus the endowment of a child may have no impact on investment on his/her siblings. Thus ex-ante there is no clear prediction of the direction of the effect of an endowment shock on sibling's investment. In Table 4 we present the results on investments due to sibling's exposure to Ramadan after controlling for own exposure. Since, an individual can have more than one sibling, we define sibling's exposure as proportion of siblings exposed to Ramadan in utero. As the results indicate, we find no effect on investments due to sibling's exposure in any of the investment measures we considered before. The results thus indicate perhaps the human capital of each child enters separately in the parent's utility function and thus parents do not reinforce with respect to sibling's endowment at birth.[20]

Another dimension of parental response to child endowment can be subsequent fertility decisions. If parents want to achieve a certain amount of human capital combing all their children, they can decide to have more children in response to a negative endowment shock on the child. It is also possible they decide to compensate the child and reduce their subsequent fertility. However, it is also possible parents do not respond by altering fertility. Thus again ex-ante the effect of an endowment shock on subsequent fertility is ambiguous. To answer this question we use the pregnancy history of women and calculate the subsequent fertility to every child born. Table 5 presents the results on subsequent fertility decisions of parents. In column 1 the dependent variable is number of children

[20]We do not present result on expectation as expectations are unlikely to be affected by sibling's endowment. We also omit the results on dowry as we do not have information on the siblings of adults.

born after the child was born and in column 2 the dependent variable number of alive siblings after the child. As the results indicate in utero exposure to Ramadan has no effect on subsequent fertility decisions of their parents.

Finally, we also study the effect of in utero exposure on pre-natal investments and some additional post-natal investment like breastfeeding. We use the pregnancy history of women and construct several measures of pre-natal and post-natal care like pregnancy checkup, whether had iron pills, tetanus injection, whether delivered at home, whether breastfeed and how long was the child exclusively breastfeed. In Table 6 we present the results on these pre-natal and post-natal investments. As the results indicate, we do not find any difference in parental investments between exposed and non-exposed children.

6.4 Robustness

In this section we address the concern about robustness of our identification strategy. One concern is parents may selectively time their pregnancies so as to avoid the overlap of Ramadan. This may bias our results if parents with higher SES or other unobservable characteristics time their birth away from Ramadan. However this is unlikely as Ramadan occurs for a period of one month and thus it leaves very little chance of avoiding the overlap of Ramadan with a nine month pregnancy. It is however possible that individuals avoid the overlap of certain trimesters with Ramadan. However, this is less likely to bias our results, since we are considering any overlap during the pregnancy and are not particularly focusing on the effects of any particular trimester.

However, in this section we provide two sets of evidence to show parents do not selectively time their pregnancies with Ramadan. First, we regress a set of parental and household characteristics on Ramadan exposure, after controlling for same set of variables as in our main regression. In particular, we look at mother's education, father's education, per capita household consumption and household size. If the selective fertility (if any) of parents are associated with these set of characteristics then we would observe exposure to Ramadan is associated with these variables. In Table 7 we show the results. As the results indicate, we find no evidence of selection into exposure on these observables. Second, If individuals are indeed timing their birth with Ramadan, the cohort sizes of individuals born with their in utero period overlapping with Ramadan will be less compared to individuals born with their in utero period not overlapping with Ramadan. To answer this question, we use, the Indonesian census data of 2010 and calculate cohort sizes for each month-year. We then regress log cohort size of individuals born in a particular month-year on whether Ramadan overlapped with the nine-months preceding that month-year. In Table 8 we show the results. We try several limitations on the sample, to understand if selective fertility changed over time. First we restrict the sample to individuals born after 1978 as these individuals will be 15 years or younger at the time of IFLS 1. Similarly, we try restricting the sample to individuals born after 1988, as these individuals will be 5 years or younger at the time of IFLS 1 and also to individuals born after 1992 as these individuals will be 5 years or younger at the time of IFLS 2. As the results indicate we find no evidence of change in cohort size, indicating parents do not time their birth with respect to Ramadan.[21]

Finally, we use the sample of non-Muslims as a placebo. This will help us to address the concern of measurement error driving our results. As mentioned before, since we do not know the exact date of conception we may misclassify some pre-term births with a probable exposure in early pregnancy as exposed when they were not. If there is systematic underinvestment on pre-term births, this will bias our results. However, if our results are driven by systematic underinvestment on pre-term births and not due to Ramadan exposure, we would observe a similar effect on non-Muslims. In Table 9 we present results on the same set of investment measures for non-Muslims. We run the same specification, except we limit our sample to non-Muslims. As the results indicate, we do not find any similar effect on non-Muslims.

6.5 Global Evidence

In this section we present results on effects of exposure to Ramadan on investments in vaccinations from 50 other countries with a significant Muslim population.[22] For this purpose we use the Demographic Health Survey data from 50 countries and combined several waves within each country. The main purpose of presenting results from 50 countries apart from Indonesia, is to understand the external validity of our results. As explained before, the effect of endowment at birth on parental investments may depend on several factors like parental preference over inequality, their knowledge of the human capital production function, macro economic conditions like returns to human capital etc. These factors may

[21] If Ramadan also affected mortality, which will also affect cohort size. But the direction of the effect of selective fertility and mortality is the same. The fact that we find no effect on cohort sizes, implies that the effect of selective fertility and mortality is null.

[22] We chose only vaccination as this is directly comparable with results from IFLS. Here completion of all vaccinations implies completion of one dose of BCG, atleast one dose of polio, atleast one dose of DPT and one dose of Measles.

vary across countries and thus the external validity of the results from Indonesia is not obvious. Ramadan shock has an unique advantage for understanding external validity, i.e its geographical spread. Since the observance of Ramadan is an important part of Islam in all parts of the world it renders comparability of outcomes across the world. In addition, DHS is a standardized survey implemented across several countries and over many waves adding to the ease of comparability.

In Table 10 we present the results. As the table indicates, there is no clear pattern of either reinforcement or compensation. The results show in 13 countries there is reinforcement, i.e parents invest less in vaccinations of children exposed to Ramadan. These countries are Albania, Benin, Egypt, Ethiopia, Gambia, Guyana, India, Maldives, Mali, Niger, Pakistan, Rwanda, and Sudan. On the other 11 countries show a compensating behavior, i.e parents compensate by investing more on children exposed to Ramadan in utero. These countries are Burkina Faso, Comoros, Guinea, Jordan, Kenya, Morocco, Sierra Leone, Tajikistan, Thailand, Tunisia and Yemen. In the rest of the countries we find no clear indication of reinforcement or compensation with respect to in utero endowment.[23] Thus there emerges no clear prediction of parental investments in response to a inutero shock.

To discern a pattern of parental investment we then pool the data from all 50 countries and try to find an association with respect to observable macro or individual characteristics. We run a similar regression as before, except we control for country-wave fixed effects and country-month fixed effects. Table 11 presents the results. In column 1 we show the pooled results. We find a negative coefficient of exposure on vaccination, though it is not statistically significant. This is not surprising, given the large amount of heterogeneity observed in Table 10. In column 2 we explore heterogeneity with respect to per capita GDP of the country at the year of survey.[24] As the results show, a clear patter emerges. Poorer countries have more reinforcing behaviors while richer countries have a more compensating behavior. We also explore heterogeneity with respect to the proportion of Muslims in the country at the time of the survey and also with individual wealth index.[25] These do not explain the heterogeneity across the world. In column 4 we

[23]These countries are Bangladesh, Nigeria, Chad, Burundi, Cameroon, Central African Republic, Congo, Democratic Republic of Congo, Gabon, Ghana, Ivory Coast, Kazakistan, Kyrgyz Republic, Liberia, Madagascar, Malawi, Mozambique, Philippines, Senegal, Sri Lanka, Tanzania, Togo, Turkey, Uganda and Uzbekistan.

[24]The data is available from `https://data.worldbank.org/indicator/NY.GDP.PCAP.KD`

[25]Proportion of Muslim population is calculated from the each DHS wave. Wealth index is a variable calculated by DHS. It takes the value 1 to 5 with 1 being in the poorest decile in the distribution of wealth of the country and 5 being at the top Some DHS waves do not have wealth index, so in regressions with wealth index we drop them.

put all these factors together and find only per capita GDP explains the variation across the world. Thus these results indicate, in poorer environments reinforcement is more common while in more richer environment compensation is more common. There can be several factors that can explain the difference across poor and rich countries. Education, information, norms and several other factors can vary. However, we cannot identify the mechanism behind this heterogeneity.

6.6 Rainfall

In this section we present evidence on parental investments from another kind of shock, namely rainfall shock. The intuition is rainfall shocks in utero affects income, which affects nutrition and other pre-natal investments which in turn affects endowment at birth. We identify the effects by comparing individuals born in the same district-seasons but over different years. Thus we take care of time invariant spatial unobservables and seasonal observables and only make comparisons within a district-season.

First, we establish rainfall shocks affect household consumption. To do this we compare household consumption measured in the survey year within a IFLS community but over different waves. In particular we regress per capita household consumption in the survey year on whether there was rainfall shock in the survey year after controlling for community fixed effects and survey year fixed effects. We use a dummy variable denoting whether total rainfall in the survey year and in the IFLS community where the household was interviewed was below the historic median of that community. We use three measures of consumption, log per capita food consumption, log per capita non-food consumption and log per capita total consumption. The results are presented in Table 12. As the results indicate a rainfall shock in the survey year leads to lower consumption of food as well as non food items. A bad year of rainfall leads to 5 percent reduction in per capita food consumption expenditure and a 8 percent reduction in non food expenditure. Thus, these results establish that rainfall shocks are a potential channel to effect endowment at birth.

Finally, we present the results of effects of rainfall shock in utero on the same set of parental investment measures as Ramadan. This renders comparability of the effects across two different kind of shocks. As mentioned before, we use several measures of rainfall shocks. These are a) log total rainfall received in utero b) summation of log deviations of rainfall from historical mean c) Number of months in utero was rainfall below the historical median d) Number of months in utero was rainfall below the twentieth percentile e) Number of months in utero was rainfall above the eightieth percentile. Table 13 presents the results. As the results indicate we find parents neither reinforce nor compensate in food, medicine and education either in the young ages (0-5) or the in older ages (5-15). However, we find consistent evidence of parents reinforcing by underinvesting in vaccinations. This is consistent regardless of the measure of shock we use. In the same vein as the effects of Ramadan, we do find parents compensate by providing more in terms of non human capital transfers in terms of more dowry, though the result is not consistent across all measures of rainfall shocks. Overall, we find some similarities between the effects of Ramadan shock in utero and rainfall shocks in utero specifically on vaccinations.

7 Discussion and Conclusion

A large set of studies have consistently found strong gradients between markers of early life environments and later life outcomes. But the causal processes that underlie these relationships remain poorly understood, particularly the distinction between the direct biological effect and subsequent parental investments (Aizer and Cunha, 2010; Baker and Stabile, 2011; Bleakley and Ferrie, 2016; Cesarini et al., 2016; Heckman and Mosso, 2014; Shah and Steinberg, 2017; Yi et al., 2015).

We contribute to this literature by estimating parental responses to fetal shocks over the life course (prenatal, early childhood, late childhood and adulthood) and over multiple dimensions- human capital as well as non human capital transfers in adulthood by parents through multiple credible exogenous shocks (Ramadan and rainfall exposure in utero) in Indonesia using 5 waves of the Indonesian Family Life Survey. We also explore external validity by estimating responses in the Demographic and Heath Surveys (DHS) across 50 countries for vaccinations in early childhood.

Our first set of analysis exploits the overlap of Ramadan with pregnancies for Indonesian Muslims in the 5 waves of the IFLS as an adverse fetal shock. We find that for exposed children under five years, parents are less likely to get them vaccinated, invest less in their diet and spend less on their medicine. For children between six and fifteen, we find no evidence of parents underinvesting in education or on medicines. However, fathers have a lower expectation about their exposed children's future general welfare. For adult children, parents give more dowry to exposed children. Overall our results suggest, parents mostly reinforce the initial inequalities, particularly by underinvesting in crucial health care in early life.

A second set of analyses studies external validity of our estimates on Ramadan exposure in utero by using DHS data on child immunizations from 50 other countries. We establish two new facts. First, there is evidence of substantial heterogeneity in parental immunization of their children among Muslims across countries in response to Ramadan exposure in utero so that in some societies either there is no response or there is evidence of remediation. Second, despite the heterogeneity, we establish new robust evidence that parents are less likely to postnatally immunize their lower endowed children in poorer countries relative to rich ones.

Finally, to explore generalizability of our findings from Ramadan exposure, we study parental responses to Rainfall shocks in utero in the 5 waves of the IFLS and find evidence of reinforcement in vaccines and remediation in dowries, similar to our estimates using Ramadan as a natural experiment.

That parents choose to alter investments in their children's medical expenses, diets and vaccination uptake is important given the fundamental importance of these investments in child and adult wellbeing. In 2008 a distinguished panel of economists, part of the Copenhagen Consensus declared combating malnutrition as the world's best investment. In fact, five of the top 10 solutions involved addressing malnutrition and expanded vaccination coverage early in life. Bloom et al. (2012) find that childhood vaccination significantly increases cognitive test scores by about half a standard deviation. Similarly, there is increasing evidence that early life nutritional investments between 0-5 affects educational attainment, adult cognitive skills, wages, and other important life outcomes (Ager et al., 2017; Behrman et al., 2009; Bhalotra and Venkataramani, 2015; Bharadwaj et al., 2013; Geoffard and Philipson, 1997; Hoddinott et al., 2008a,a; Maluccio et al., 2009; Victora et al., 2008).

Majid (2015) and Almond et al. (2015) show that Ramadan exposure in utero reduced child test scores in Indonesia and the UK. Shah and Steinberg (2017) finds that children who experienced a positive rainfall shock in the first 1000 days of life had higher test scores. Maccini and Yang (2009) documents increases in adult height, schooling attainment, and labor market outcomes. Together these findings provide suggestive evidence that in utero shocks (Ramadan and Rainfall) impact test scores, adult height, and labor market outcomes by inducing parents to invest less in critical vaccines and nutritional investments during early childhood rather than through changes in late childhood investments.

Similar to Adhvaryu and Nyshadham (2016) we don't find that parents respond to fetal shocks during the prenatal period. This is consistent with parents responding to observed changes in endowments of their children later in infancy rather than to expected changes in birth endowments due to fetal shocks. Our findings that parental investments respond most between 0-5 rather than 6-15, even for the same investment (medicine and diet) suggests that early life heath investments are complimentary to fetal investments, but later life health and educational investments are not, a finding consistent with evidence that there is a greater malleability of skills in early versus later in a child's life cycle (Attanasio et al., 2015; Cunha et al., 2010).

Our findings that parents reinforce in early childhood but compensate through dowries in adulthood, is consistent with the wealth model (Becker and Tomes, 1976). Parents in poorer countries have limited access to public health care infrastructure, imperfect credit markets and poor social security in old ages. This makes them care for efficiency of their investments than for equity. Our findings that parents from poorer countries tend to reinforce more relative to richer ones can be interpreted in this light.

There are limitations to our study as well. First, we did not have detailed child level parental time investment data in IFLS or in DHS. More generally, we can only study the set of parental responses which are measured and observed in our surveys, so that we don't know much about how parents respond in unobserved manners. Second, if parent's subjective beliefs about the technology of skill formation deviate from the true production function, our reduced form estimates may not be informative about the true technology of skill formation (Cunha et al., 2013). That said, we do exploit a question about parental expectation of future wellbeing of exposed children in the IFLS. Our results suggests parents correctly believe (qualitatively) that the exposed children will have lower wellbeing in the future as shown in (Van Ewijk, 2011; Majid, 2015). Third, we do not estimate the ensuing effects of the parental responses on long-term skills/capabilities.

Future work should collect data on time investments along with data on subjective beliefs regarding the technology of skill formation to better understand the role of these factors in shaping parental responses to fetal shocks. Exploiting exogenous shocks to parental investments in addition to fetal shocks in a reduced form or structural stetting may help us to highlight the effect of parental responses we measure on long term outcomes as documented in the literature (Maccini and Yang, 2009; Majid, 2015).

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8 Tables

TABLE 1: DESCRIPTIVE STATISTICS

VARIABLES	Muslims		
	Exposed	Unexposed	Difference
All Vaccinations (0-5 years)	.613	.630	-.02
	(.005)	(.010)	(.011)
Log Spending Medicine (0-5 years)	3.55	3.87	-.32
	(.036)	(.076)	(.084)
Log Spending Medicine (5-15 years)	3.28	3.32	-.03
	(.025)	(.053)	(.058)
Any Protein (0-5 years)	.866	.879	-.01
	(.004)	(.007)	(.008)
Log Educational Spending (5-15 years)	12.60	12.60	.00
	(.018)	(.035)	(.039)
Log Dowry (Adults)	10.89	10.92	-.03
	(.021)	(.045)	(.050)

All variables are in proportion except Log Education Expense, Log Spending on Medicine and Log dowry. The sample only includes individuals who were either exposed to Ramadan fully or not at all. Partially exposed individuals are excluded.

Table 2: Children Less than 5 Years

VARIABLES	(1) All Vaccinations	(2) Expenditure on Medicine	(3) Any Protein
Exposed	-0.0260	-0.442	-0.0269
	(0.0128)	(0.103)	(0.00890)
Observations	12,174	15,250	10,325

Exposed takes the value one if the child was exposed to Ramadan during pregnancy. The sample excludes partially exposed. The sample is adults aged less than five years at the time of the survey (waves 1, 2, 3, 4 and 5). Vaccinations is available only from waves 2 to 5 and protein from waves 3 to 5. The regressions control for gender, age, age squared, wave fixed effects, source of the date of birth fixed effects and month of birth fixed effects. In addition, it also controls for if the child was conceived 21 days after Ramadan. Vaccinations is completion of at least one dose of all kinds of vaccination, expenditure is log of expenditure on medicine and protein is whether consumed eggs, fish or meat in the last week preceding the survey. Clustered Standard Errors at the level of individual in Parenthesis.

Table 3: Children 5-15 Years and Adults

VARIABLES	(1) Education Expense	(2) Expenditure on Medicine	(3) Any Protein	(4) Father Expectation	(5) Mother Expectation	(6) Dowry Value
Exposed	-0.0134	0.0181	0.000624	-0.0168	-0.00146	0.0857
	(0.0356)	(0.0730)	(0.00357)	(0.00853)	(0.00828)	(0.0440)
Observations	22,193	27,961	19,170	12,222	12,222	32,852

Exposed takes the value one if the child was exposed to Ramadan during pregnancy. The sample excludes partially exposed. The sample is adults aged five and fifteen at the time of the survey (waves 1, 2, 3, 4 and 5). Information about protein is available only in waves 3 to 5. The sample for expectation is 7-24 and is only available in waves 4 to 5. The sample for dowry includes individuals aged 15 to 101. The regressions control for gender, age, age squared, wave fixed effects, source of date of birth and month of birth fixed effects. In addition, it also controls for if the child was conceived 21 days after end of Ramadan. Education expense is log of education expense in the last year, medicine is log of expenditure on medicine, protein is whether consumed eggs, fish or meat in the last week preceding the survey, father's and mother's expectation is whether they expect their children to have a better life than them and dowry is log of dowry value received. Clustered Standard Errors at the level of individual in Parenthesis.

TABLE 4: SIBLINGS

VARIABLES	(1) Medicine Young	(2) Any Protein Young	(3) All Vaccinations Young	(4) Educational Expense	(5) Medicine Adult	(6) Any Protein Adult
Exposure	-0.420	-0.0242	-0.0366	-0.00608	0.0589	0.00144
	(0.113)	(0.00990)	(0.0144)	(0.0379)	(0.0776)	(0.00379)
proportion of siblings exposed	-0.0624	0.00736	-0.00542	-0.0392	-0.00918	-0.00438
	(0.0945)	(0.00760)	(0.0113)	(0.0360)	(0.0711)	(0.00284)
Observations	12,400	8,011	9,685	19,697	24,655	16,731

Exposed takes the value one if the child was exposed to Ramadan during pregnancy. The sample excludes partially exposed. Proportion of siblings exposed is total number of siblings exposed/number of siblings. The regression thus excludes individuals who do not have a sibling. The regressions control for gender, age, age squared, wave fixed effects, source of date of birth and month of birth fixed effects. In addition, it also controls for if the child was conceived 21 days after Ramadan. Young refers to 0-5 and Adult refers to 5-15. Vaccinations is completion of at least one dose of all kinds of vaccination, medicine is log of expenditure on medicine, protein is whether consumed eggs, fish or meat in the last week preceding the survey and education expense is log of education expense in the last year. Clustered Standard Errors at the level of individual in Parenthesis

TABLE 5: FERTILITY

VARIABLES	(1) Number of Pregnancy	(2) Number of Siblings
Exposure	0.0179	0.0200
	(0.0356)	(0.0296)
Observations	24,397	24,397

Exposed takes the value one if the child was exposed to Ramadan during pregnancy. The sample excludes partially exposed. The regressions control for gender, mother's age at the time of birth, mother's age at the time of squared and month of birth fixed effects. Number of pregnancy is number of times the mother was pregnant after the child was born and Number of Siblings is number of siblings born after the child. In addition, it also controls for if the child was conceived 21 days after Ramadan. Robust standard error in parenthesis.

TABLE 6: PRE-NATAL CARE

VARIABLES	(1) Pregnancy Checkup	(2) Iron Pills	(3) Tetanus Injection	(4) Birth at Home	(5) Breastfeed	(6) Exclusive Breastfeeding
Exposure	0.00609	-0.00450	-0.00968	0.0104	-0.00186	-0.349
	(0.00556)	(0.0105)	(0.0117)	(0.0125)	(0.00456)	(2.991)
Observations	14,527	12,092	12,088	14,271	14,326	12,825

Exposed takes the value one if the child was exposed to Ramadan during pregnancy. The sample excludes partially exposed. The regressions control for gender, mother's age at the time of birth, mother's age at the time of squared, wave fixed effects, year of birth fixed effects and month of birth fixed effects. In addition, it also controls for if the child was born 21 days after Ramadan. Column 1 is whether had a pregnancy check up, column 2 is whether had Iron Pills during pregnancy and column 3 is whether had tetanus Injection, column 4 whether delivered at home, column 5 is whether breastfed and column 6 is how many months exclusively breastfeed. Clustered Standard Errors at the level of mother in Parenthesis

TABLE 7: SELECTIVE FERTILITY

VARIABLES	(1) Less Than Primary (Mother)	(2) Less Than Primary (Father)	(3) Log Percapita Consumption	(4) Household Size
Exposure	0.00275	0.0113	-0.00660	0.00372
	(0.00859)	(0.00917)	(0.0139)	(0.0369)
Observations	26,337	24,462	86,200	86,529

Exposed takes the value one if the child was exposed to Ramadan during pregnancy. The sample excludes partially exposed.The sample is adults aged between zero and fifteen at the time of the survey (waves 1, 2, 3, 4 and 5). The regressions control for gender, age, age squared, wave fixed effects, source of date of birth and month of birth fixed effects. In addition, it also controls for if the child was conceived 21 days after Ramadan. Column 1 and column 2 is whether mother and father had less than primary schooling, column 3 is log per capita household consumption and column 4 is household size. Clustered Standard Errors at the level of individual in Parenthesis

TABLE 8: COHORT SIZE

VARIABLES	(1) Full Sample	(2) Born After 1978	(3) Born After 1988	(4) Born After 1992
Exposure	0.0105	0.0693	0.127	0.0974
	(0.0259)	(0.0609)	(0.102)	(0.128)
Observations	1,006	335	224	180

Exposed takes the value one if the month-year of birth of the cohort overlapped with Ramadan. The sample excludes partial post natal exposure. The regressions control for month of birth fixed effects and year of birth fixed effects. Column 2, 3 and 4 includes samples born after 1978, 1988, 1992. Robust standard errors in Parenthesis

TABLE 9: NON MUSLIMS

VARIABLES	(1) Medicine Young	(2) Any Protein Young	(3) All Vaccinations Young	(4) Educational Expense	(5) Medicine Adult	(6) Any Protein Adult	(7) Expectation Father	(8) Expectation Mother	(9) Dowry
Exposure	0.279	-0.0168	0.0424	0.0559	-0.255	0.0148	-0.0232	-0.0421	-0.404
	(0.283)	(0.0254)	(0.0370)	(0.0920)	(0.186)	(0.0108)	(0.0247)	(0.0225)	(0.216)
Observations	1,780	1,180	1,382	2,847	3,555	2,312	1,557	1,557	2,309

Exposed takes the value one if the child was exposed to Ramadan during pregnancy. The sample excludes partially exposed. The regressions control for gender, age, age squared, wave fixed effects, source of date of birth and month of birth fixed effects. In addition, it also controls for if the child was conceived 21 days after Ramadan. Young refers to 0-5 and Adult refers to 5-15. The sample for dowry is age 5 to 101. Vaccinations is completion of at least one dose of all kinds of vaccination, medicine is log of expenditure on medicine, protein is whether consumed eggs, fish or meat in the last week preceding the survey, education expense is log of education expense in the last year, father's and mother's expectation is whether they expect their children to have a better life than them and dowry is log of dowry value received. Clustered Standard Errors at the level of individual in Parenthesis.

TABLE 10: VACCINATION COMPLETION

	(1) Bangladesh	(2) Kenya	(3) Nigeria	(4) Mali	(5) Pakistan	(6) India	(7) Chad	(8) Albania	(9) Azerbaijan	(10) Benin
Exposure	-0.00720	0.0456	-0.00980	-0.0440	-0.0164	-0.0138	-0.0162	-0.109	-0.134	-0.0634
	(0.00581)	(0.0188)	(0.00699)	(0.00805)	(0.00888)	(0.00614)	(0.00998)	(0.0381)	(0.0639)	(0.0167)
Observations	32,544	4,961	33,733	27,171	22,899	51,269	15,435	1,202	1,633	7,075
	Burkina Faso	Burundi	Cameroon	CAR	Comoros	Congo	DR Congo	Egypt	Ethiopia	Gabon
Exposure	0.0443	0.0266	0.00664	0.138	0.0964	-0.122	0.0468	-0.0169	-0.0473	-0.0750
	(0.00953)	(0.0445)	(0.0216)	(0.331)	(0.0260)	(0.0959)	(0.0791)	(0.00422)	(0.0114)	(0.0767)
Observations	15,777	522	4,194	243	3,647	160	402	59,898	11,570	362
	Gambia	Ghana	Guinea	Guyana	Ivory Coast	Jordan	Kazakstan	Krygz Republic	Liberia	Madagascar
Exposure	-0.0829	7.89e-05	0.0376	-1.525	0.0167	0.0164	0.0661	-0.00279	0.0436	-0.125
	(0.0215)	(0.0218)	(0.0141)	(0.149)	(0.0200)	(0.00587)	(0.0617)	(0.0147)	(0.0365)	(0.0871)
Observations	7,209	2,638	11,794	75	4,753	34,597	1,014	4,680	1,420	190
	Malawi	Maldives	Morocco	Mozambique	Niger	Phillippines	Rwanda	Senegal	Sierra Leone	Sri Lanka
Exposure	0.00929	-0.181	0.0367	-0.00237	-0.0211	0.0125	-0.0904	0.00767	0.109	0.0187
	(0.0127)	(0.0275)	(0.0104)	(0.0223)	(0.00765)	(0.0259)	(0.0476)	(0.00837)	(0.0153)	(0.266)
Observations	5,339	3,474	13,134	2,910	27,072	2,379	567	23,919	11,239	152
	Sudan	Tajaksthan	Tanzania	Thailand	Togo	Tunisia	Turkey	Uganda	Uzbekistan	Yemen
Exposure	-0.0872	0.0702	-0.0204	0.456	0.0423	0.123	0.0204	0.0214	0.109	0.0418
	(0.0384)	(0.0262)	(0.0130)	(0.145)	(0.0349)	(0.0325)	(0.0138)	(0.0182)	(0.0799)	(0.0113)
Observations	3,413	4,305	7,374	64	1,709	3,868	7,900	3,940	1,077	19,299

Exposed takes the value one if the child was exposed to Ramadan during pregnancy. The sample excludes partial post natal exposure. The sample is adults aged less than five years at the time of the survey. The regressions control for gender, age, age squared, wave fixed effects and month of birth fixed effects. Vaccinations is completion of at least one dose of all kinds of vaccination. CAR is Central African Republic and DR Congo is Democratic Republic of Congo. Robust standard errors in parenthesis.

TABLE 11: EFFECTS FROM OTHER COUNTRIES

VARIABLES	(1) All Countries	(2) All Countries	(3) All Countries	(4) All Countries	(5) All Countries
Exposure	-0.00157	-0.0560	-0.00822	-0.00218	-0.0849
	(0.00178)	(0.0150)	(0.00468)	(0.00467)	(0.0228)
Exposure*Log PercapitaGDP		0.00815			0.0115
		(0.00221)			(0.00322)
Exposure*Proportion of Muslim			0.00895		0.00423
			(0.00572)		(0.00784)
Exposure*Wealth Index				0.000308	0.000599
				(0.00137)	(0.00138)
Observations	506,201	506,030	506,201	352,299	352,128
R-squared	0.305	0.305	0.305	0.327	0.326

The sample is Muslims from 50 countries. Log per capita GDP is GDP at the survey year. Proportion of Muslims is calculated at the country-wave level. Wealth Index is a measure from 1 to 5, with 1 being the poorest. The regressions controls for gender, age at the time of survey, age squared, country-wave fixed effects, country-month of birth fixed effects. In the regressions with Wealth Index it also controls for Wealth index FE. Vaccinations is completion of at least one dose of all kinds of vaccination. Robust standard errors.

TABLE 12: EFFECT ON HOUSEHOLD CONSUMPTION

VARIABLES	(1) PC Food	(2) PC NonFood	(3) PC Food Nonfood
Rainfall Below Median in Survey Year	-0.0452	-0.0812	-0.0700
	(0.0195)	(0.0361)	(0.0270)
Observations	38,267	38,327	38,330
R-squared	0.685	0.601	0.676

The dependent variable in column 1 is log of per capita food consumption at the household level, column 2 is log of per capita non food consumption and column 3 is log of food and non food consumption. Rainfall Below Median in Survey year takes the value one if rainfall in the community is below historical median in the survey year. The regression controls for community fixed effects and wave fixed effects. Standard errors are clustered at the community level.

TABLE 13: RAINFALL AND INVESTMENTS

VARIABLES	(1) Protein	(2) Medicine Less than Five	(3) Vaccinations	(4) Education	(5) Medicine More than Five	(6) Protein Above Five	(7) Dowry
Log In Utero Rainfall	0.0172	-0.209	0.105	-0.168	-0.564	0.00228	-0.289
	(0.0236)	(0.353)	(0.0395)	(0.135)	(0.235)	(0.00909)	(0.156)
Observations	11,832	17,599	14,023	25,796	32,029	21,678	35,901
Log of In utero Rainfalldev	-0.000296	0.00232	0.00694	-0.00796	-0.0198	0.000658	-0.00836
	(0.00166)	(0.0259)	(0.00282)	(0.0101)	(0.0159)	(0.000703)	(0.00814)
Observations	11,832	17,599	14,023	25,796	32,029	21,678	35,901
Number of Months In Utero Below Median	-0.00114	0.0237	-0.00974	0.00838	0.0334	7.99e-05	0.0194
	(0.00218)	(0.0262)	(0.00330)	(0.00858)	(0.0160)	(0.000913)	(0.0108)
Observations	11,832	17,599	14,023	25,796	32,029	21,678	35,901
Number of Months In Utero Below Twenty	0.000153	-0.0227	-0.00687	0.00687	0.0189	-0.000304	0.0104
	(0.00198)	(0.0283)	(0.00317)	(0.0121)	(0.0216)	(0.000917)	(0.0135)
Observations	11,832	17,599	14,023	25,796	32,029	21,678	35,901
Number of Months In Utero Above Eighty	0.00160	-0.0118	0.0152	-0.00904	0.00575	-0.000315	-0.0179
	(0.00379)	(0.0432)	(0.00579)	(0.00975)	(0.0243)	(0.00163)	(0.0131)
Observations	11,832	17,599	14,023	25,796	32,029	21,678	35,901

The regressions controls for age at the time of survey, age squared, wave FE, source of the IFLS file of birth information, gender, religion of the person, month of birth-year of birth FE and district of birth-month of birth FE. The sample includes both Muslims and non-Muslims. Standard errors are clustered at the district level.

9 Data Appendix

9.1 Date of Birth Construction

To get the date of birth information we followed the following steps.

- First, we use the date of birth information from the anthropometric files of each wave.[26] We drop the invalid date of birth informations.[27] We then calculate Ramadan exposure status using this date of birth information. Since we are using five waves, there can be inconsistencies in the date of birth of the same individual across waves. We drop those observation whose date of birth is inconsistent to such an extent that the exposure status varies across waves.

- We then use the date of birth information as given in the cover files of the modules from the all the waves. [28]. We then follow a similar process as with the anthropometric files, i.e drop the invalid date of birth information and also drop those observations which have inconsistencies in the date of birth information to such an extent that the exposure status varies across waves.

- An exact similar process is followed with the date of birth information in the AR files, which are household rosters. [29]

- We then combine the date of birth information from all these three sources after the cleaning of the files mentioned above. We give preference to the anthropometric files first, then to the cover files and then to the AR files. This implies we take the date of birth information (and consequently the exposure status associated with it) from the anthropometric files first, if available. Then we use the cover files only for those whose information is not available in the anthropometric files and then finally we use the information from the AR files for those information is not available in the cover files. 86.19 percent of all information is available from the anthropometric files, remaining 6.41 percent from cover files and 7.40 percent from AR files.

[26]The US files in waves 2-5 and the CA in the first wave

[27]Those whose day of birth is greater than 31, month of birth is greater than 12 and year of birth is invalid.

[28]Book 3a for adults and Book 5 for children. However, day of birth information in the cover files is only available from waves 3 to 5

[29]There is no date of birth information in the AR files for waves 1 to 3

9.2 District of Birth Harmonization

Since we are using data from 5 different waves, we have to harmonize the district of birth information available across different waves. This is because the district boundaries have changed from the time of first wave in 1993 to the fifth wave in 2015. So we harmonized all the district of birth information to 1993 boundaries. However, a consistent set of crosswalk is not available for mapping all the district boundaries to the 1993 boundaries. Following steps are taken to achieve this.

- We do not directly know the district of birth for children, but we do know migration histories of their mothers.[30]. So as a first step we create a dataset containing migration history of all individuals separately for each wave, because the district codes in the migration history is not harmonized yet.

- IFLS 5 provides a crosswalk for mapping districts across waves until 1998. So we can map the district codes given in the migration files across waves until 1998. However, this does not solve the entire problem as we will not be able to use information from 1993 wave, i.e the first wave. To solve this we use another file, i.e the htrack file of IFLS 2. This file gives a crosswalk of districts between between 1993 and 1999. However, this crosswalk is limited to only those districts where IFLS households in the first wave were interviewed. So we have a crosswalk (spanning all waves) of all districts where atleast one household was interviewed in 1993. However, the downside is districts where IFLS did not do any interviews in 1993 cannot be mapped to a common set of district boundaries. Thus we could convert all the migration histories from all the waves into 1993 codes, except for those part of the migration history where an individual was in a non-IFLS district and recorded in the 2^{nd} to 5^{th}.

- We then take all the individuals who ever completed the book 5. We then obtain their date of birth using the same date of birth file we created in the previous subsection. This way we drop all those observation who have a invalid date of birth for Ramadan exposure. This helps to maintain comparability with the Ramadan analysis. We then link them to their mothers, i.e obtain their mother's pidlink. Then we link this to the migration history of the mother which as mentioned above was created separately from all the waves and then harmonized with 1993 districts. We then obtain the district of birth information of the child from the migration history, by identifying

[30]This however only true if the mother was also interviewed

the district the mother was when the child was born. Some of these districts are those which could not be harmonized with the 1993 districts, so we drop them.