Can the Major Public Works Policy Buffer Negative Shocks in Early Childhood?
Evidence from Andhra Pradesh, India

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Abstract

The study examines the role of the largest public works program in the world-the National Rural Employment Guarantee Scheme (NREGS) - in buffering the negative effects of early childhood exposure to rainfall shocks on long-term health outcomes. Exploiting the spatial and temporal variation in NREGS coverage, the study estimates the extent to which nutritional insults in early childhood can be offset in the presence of the policy. The study employs a unique identification strategy by integrating detailed administrative records of drought shock and phase-wise roll-out information of NREGS with a household level panel data-the Young Lives survey- conducted over three waves (2002, 2006-07 and 2009-10) in the state of Andhra Pradesh, India. Using child fixed effects estimation the study finds that while the policy does not help correct long term past health deficiencies it is useful in buffering recent drought shocks, which varies by policy relevant sub-groups.

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I. Introduction

Exposure to negative shocks in early childhood is known to significantly affect the health and educational outcomes of the population, more so in developing countries. Increased climatic variability over time poses special challenges for child nutrition especially among subsistence farmers depending on rain-fed agriculture. Additionally, there is no operational practice to forecast drought (Gore et al., 2010) where such an event may often lead to adverse outcomes of loss of land rights against debt and declining nutrition levels for the poorer majority of population. With a large proportion of households depending on agriculture -a highly volatile source of subsistence- the effects may be worse for the rural poor who often lack formal credit markets to smooth consumption. In such a setup, rainfall shocks can lead to substantial reduction in household income, which can significantly reduce investments in children often compromising their calorie intake. This is a serious concern as the investments in early childhood can have significant impact on the human capital attainments and achievements as adults (Hoddinott and Kinsey (2001); Maccini and Yang (2009)). While the long term consequences of malnutrition during childhood are well established in the literature little is known about the extent to which individuals are able to mitigate some of the deficits in health outcomes under the availability of social protection schemes.

Although stunting might be permanent when nutritional deficits begin early, nutritional remediation can still take place as long as the critical period for growth remains open. Therefore, it is important to study the vulnerability that a child faces when exposed to shocks that risks child nutrition and health by a decline in household income/food availability. Further it would be very important to identify the extent to which individuals are able to compensate and offset these negative effects when a social safety net is in place and examine additionally whether the mitigation varies by policy-relevant demographic subgroups.

Employment generating programs are expected to support vulnerable households assuring nutrition security during economic downturns. In the context of the major public-works policy in India, earlier studies have mainly focused on targeting of the scheme and labor market impacts as opposed to examining its role in social protection. In this study we examine the effects of negative rainfall shocks on children's long-term health outcome in rural Andhra Pradesh, India and shed light on the impact of the access to the National Rural Employment Guarantee Scheme (NREGS) on health. Using panel data from the Young Lives Survey following children over eight years and linking them with very detailed administrative records of both rainfall shocks and the policy availability, the estimates indicate that while drought has significant and strong negative impact on height-for-age of the children, the availability of this program proves significant in mitigating the negative impacts from the very recent drought shocks but are unable to correct for longer-term past deficiencies.

This paper contributes to the existing literature on a number of aspects. First, utilizing a rich set of detailed data on weather shocks and policy coverage the study is one of the first few ones to examine causal impacts of a policy in being able to correct for past deficiencies relevant to child health in the long-run. While there exists a body of literature exploring the effects of early childhood shock on human capital outcomes, the issue of how its effect can be mitigated under a public intervention is relatively under studied. Examining this mitigating effect on child growth requires sufficiently integrated data sets to deal with the methodological difficulties in addressing the bias from self selection into the program. Unlike past studies, I collected[2] and used very detailed information at the mandal-level (sub-district level) for rainfall shocks, program availability, and community level measures of health-infrastructure that varies with time. This enabled to control for a host of factors that influence child health independently, thus accounting for any unobservable inherent differences in families who participate and reducing the selection problem. Second, the existing literature for developing countries has mostly focused on a rather extreme health outcome - child mortality, while we are able to focus on malnutrition/child-stunting among survivors. Furthermore, we are able to use anthropometric measures of the same child at different ages and control for inherent healthiness as opposed to using self-reported health outcomes. Third, while existing studies on the major public-works policy in India mainly focused on targeting of the scheme this paper is one of the very first few ones to examine its causal impact on catch-up growth in children, following them over eight years. Finally, we are able to comment on the differential impact of the mitigation across the demographic features of the child by age, gender, caste, her mother's education, which again is crucial for policy insights.

In the next section, we discuss the background and implementation of the NREGS in India. In section III we outline the conceptual framework for the study and discuss our estimation strategy to find how long-term health evolves under shocks and its potential mitigation under social protection policy. In section IV we lay out the empirical specification followed by a discussion on the datasets we use and the relevant descriptive statistics in section V. Section VI presents the main empirical results along with brief discussion of the policy insights.

[2] I collected and complied the mandal-level information of rainfall and health facility over time from various years of Handbook of Statistics for each sample district in Andhra Pradesh by visiting the Directorate of Economics and Statistics, Government of Andhra Pradesh in Hyderabad.

II. The Program: National Rural Employment Guarantee Scheme (NREGS)[3]

The National Rural Employment Guarantee Scheme (NREGS), which is now the largest public works program in the world, came into force in February 2006 under the legislative framework of the National Rural Employment Guarantee Act (2005). It provides a legal guarantee for 100 days of employment in every financial year to adult members of any rural household willing to do unskilled manual work at the statutory minimum wage of Rs.120[4] (US$2.64) per day in 2009 prices. Employment is given within fifteen days of application for work, if it is not then daily unemployment allowance is paid (GOI, 2008). Wages are required to be disbursed generally on a weekly basis but it cannot be beyond a fortnight[5] after the work has taken place. By 2010, the National Rural Employment Guarantee Act (NREGA) reached 52 million households across the country, making it by far the largest social protection program in the world (Vij, 2010). During 2010–11 Andhra Pradesh provided 274.8 million person days of employment (Galab et al. 2011). We discuss several important features of the policy important for our empirical strategy.

(i) Public-works as a safety net

NREGS was introduced in India with an aim of improving the purchasing power of the rural people, primarily providing semi or unskilled work to people living in rural India, whether or not they are below the poverty line. The purpose of this scheme is to create strong social safety net for the vulnerable groups, increase female labor-force participation, create durable and

[3] NREGA is now known as MGNREGA (Mahatma Gandhi National Rural Employment Guarantee Act)
[4] In comparison, farm wage typically hovers around of 100-150 rupees depending on agricultural season.
[5] Although according to the PACS-CSO survey(2007) , the majority of workers received their wages within 30 days for the aggregate sample of Indian state

productive assets[6] in rural areas that encourage sustainable development and reduce rural-urban migration. The evaluation report from Ministry of Rural Development (2011) finds the policy resulted in reduction in the distress-migration of labor and a rise in expenditure on food and non-food items, which again can have strong associations with child growth.

Zimmerman (2012) finds NREGS has led to a substantial increase in the private-sector casual wage for women, the effects being concentrated in the main agricultural season. A number of studies point that women's independent income benefits household nutrition and child health, both through increase in household income as well as through an increase in women's status, autonomy and decision-making power specially those relating to nutrition, immunization and feeding practices(Smith, 2001). Uppal (2009) reports positively about the self-targeting mechanism under the NREGS and notes that poorer and 'lower' caste households are more likely to register for this work which had significantly reduced the likelihood of children in the household being required to work. There is evidence by Dutta et al.(2012) pointing that it is often difficult for poorer states to meet with the demand for job under this program thereby limiting availability of the scheme where it could benefit the most.

While there have been some recent studies on NREGS reflecting on issues of its targeting, increased participation benefits accruing to women (Khera et al. 2009, Azam 2011), or to scheduled caste (SC) and scheduled tribe (ST) households (Drèze et al. 2009), there has been little empirical evidence exploring its potential role in serving as buffer against negative shocks. Most of the existing literature on workfare schemes evaluate targeting outcomes in terms of average incidence across income sub-groups. Specifically, this paper extends this current debate

[6] However as have pointed out by the recent World Bank report (2011) the objective of asset creation runs a very distant second to the primary objective of employment generation, it has been the case that the policy has only been successful in generating employment but not so in terms of asset creation.

in India on the role of NREGS as a safety net and finds causal evidence that supports preliminary findings of positive benefits of NREGS on households.

(ii) Gender-sensitive component of NREGS

The scheme promotes women's participation in the labor force through a one-third quota for women in each state and also guarantees equal wages to both men and women workers. According to the official records for NREGS, the share of women workers was found to be greater in Andhra Pradesh than nationally in 2011(National average share for women being 50.1 %, while in Andhra Pradesh it is 57.5 %). Since the prospects are typically worse for women in private casual wage work in India the provision of equal wages should have positive impacts on female participation. As argued by Azam (2011) and Imbert et al. (2011) , using NREGS has a sharper impact on female labor force participation[7] than that of males. In order to encourage participation from mothers with very young children, the program makes the presence of child care facilities mandatory[8] at all sites where more than five children under the age of six are present.

(iii) Implementation of NREGS

The Government has implemented the scheme in phase-wise manner making use of a 'backwardness index' -comprising agricultural productivity per worker, agricultural wage rate, and Scheduled Caste/Scheduled Tribe population, developed by the Planning Commission. Figure 1 illustrates the phase-wise[9]rollout of NREGS in the state of Andhra Pradesh.

[7] Khera et al(2009) points that NREGA wages implied a substantial jump in the earning potential for women at the national level.

[8] In spite of this provision the program has only 8.74% of registered respondents reporting the availability of on-site child-care center (Galab, 2008).

The first phase of the scheme was rolled out in 200 districts of the country from February 2006. In phase two, additional 130 districts were included from April 2007 (total 330 districts). From April 2008, in phase three, it has been universalized and extended to all 596 rural districts in the country. For Andhra Pradesh the program roll out expansion across all its districts is shown in Table 1- first of all to 13 districts in 2005, then to a further six districts in 2007 and three more districts in 2008, to cover all 22 districts in the state. Two out of six rural districts covered by Young Lives fell within the second and third phases, and in these two districts a large proportion of the Scheduled Tribe households are covered. Importantly for our identification strategy, four of the Young Lives sample districts (comprising of 11 mandal sites) were covered by the NREGS in the first phase of implementation in 2005-06 (Anantapur, Mahaboobnagar, Cuddapah, Karimnagar), with the addition of one more sample district(comprising of 4 mandal sites) –Srikakulam- in 2007, coinciding with second phase of implementation, and lastly the district of West Godavari(two mandal sites)was included in 2008- coinciding with phase three of the program expansion.

III. Conceptual Framework: Shocks, Child Vulnerability and Remediation

In order to discuss the potential impacts of the employment guarantee scheme on child outcomes in a simple analytical framework, the underlying hypothesis examined in this study is that direct positive income from wages earned from public work can feed into child investments in an otherwise situation of crises protecting the long-term health status. This is expected to be even more beneficial in a situation of drought in a rural setting with very limited outside opportunities to fall back on.

'Drought' in most cases refers to receiving lower than long-term average rainfall extending over weeks, months or even years. The Indian Meteorological Department declares rainfall as 'deficient' if the rainfall is 20% below its long-term average (IMD, 2002). In 2009, around half of the districts were declared to be drought affected in Andhra Pradesh[10], the state -where over 80 per cent of the population depends on agriculture. Stunting[11], or low height-for-age, is a measure of chronic malnutrition and is generally considered a long-term indicator for health status. Earlier studies have pointed that stunting might be permanent when nutritional deficits begin early and are prolonged. Hoddinott and Kinsey (2001) find that droughts in rural Zimbabwe occurring between the ages of 0 and 12 months lead to reductions in child height when measured 12 months later. Maccini and Yang (2009) find a strong relationship between rainfall in the birth year and adults' health and socio-economic outcomes for women but not for men in Indonesia. Almond et al.(2011) points that even relatively mild prenatal exposures can result in lower birth weights, which can have persistent effects.

However, the medical literature evidence points that there exists biological potential for catch-up in response to clinical interventions, which is explored in some studies focusing on catch-up growth (Deolalikar, 1996; Fedorov and Sahn, 2005; Alderman et al, 2006; Mani, 2008). Martorell et al. (1994) survey evidence from medical literature and find evidence of catch up growth when living conditions are improved, especially for younger children. Outes et al.(2012) point that nutritional remediation can take place and catch up growth can be achieved as long as

[10] Andhra Pradesh is the fifth largest state in India, with a total population of 84.66 million – 73 % of whom live in rural areas (Census 2011).

[11] The rate of stunting is severely high in developing countries including India -having the highest number of stunted children below the age of 5 in the world (Unicef 2009). In Andhra Pradesh, according to National Family Health Survey (NFHS-3, 2006) prevalence of malnutrition among children (0-59 months) is very high (32.5% underweight 42.7 % stunted and 12.2% wasted).

the critical period for growth remains open. Few studies in this regard point the potential for early nutritional intervention in accelerating growth. Schroeder et al. (1995) find that nutritional supplementation has a significant impact on growth for kids under 3 year olds in Guatemala. Yamano et al. (2005) emphasize in the context of rural Ethiopia, that food aid can compensate the negative effects of early shocks, but that inflexible targeting, endemic poverty and low maternal education often keep stunting at high levels despite such interventions. In Mexico, it was found that conditional cash transfer protects education, particularly that of girls, and thus fosters the formation of human capital, offsetting shocks such as parental unemployment or illness (de Janvry et al., 2006).In terms of the evidence base of social protection policies, a recent systematic review of Hagen-Zanker, et al. (2011) points out that there are significantly more studies available on cash transfers than employment guarantee programs, indicating further need for more studies on the impacts of the later.

In this context, it is immensely important to see to what extent the recent large scale public-works intervention in India- in the form of provision of an employment guarantee scheme for rural households in India- is enabling the individuals to buffer negative shocks and correct nutritional deficiencies in early childhood.

IV. Empirical Specification and Identification

The outcome variable in our current analysis is Height-for-age z-score[12] which is a standardized measure of health status and is a well established long run indicator of individual health status

[12] This analysis uses height-for-age z-score as an indicator of catch-up growth following the rationale pointed by Cameron, Preece and Cole (2005). First, they note the correlation between baseline and follow-up height is dependent on the ratio of height standard deviations of the two measurements, which in contrast, z-scores are not subject to, as they already take into consideration reference groups of equal age and sex. The second justification is that demonstration of catch-up growth needs to be compared with growth in a control group, which z-score measurement fulfills but a single height measurement does not. Third, the authors note that by using z-score measurements, catch-up growth may be separated from correlations predicted by regression to the mean.

especially among children (Martorell, 1999). It shows the height of the child relative to an international reference group of healthy children. Since height is a stock variable that reflects all past inputs into child health including the impact from past shocks and effect of the child level unobservables, it gives a cumulative picture of the child's overall growth status.

We define drought shocks depending on the timing and severity of the event: first, we capture a cumulative measure for past rainfall shocks (cumulated from birth year till the point in survey). Second, we have a drought measure capturing recent shock as having drought in the year prior to the survey. We have two severity measures for both of these measures of drought, 'Severe Drought' is constructed by the fraction of years where rainfall is below 20% [13] than the long-term average at mandal level and 'Drought' is receiving lower rainfall than long term average for a mandal in the previous year to the survey.

We primarily use the policy access information from administrative records. Since the policy is first targeted to the poorer districts and also involves voluntary participation from households, there can be potential selection bias in estimates arising from individual specific unobservables influencing both the outcome variable and treatment. By including child fixed effects we could reasonably reduce these individual-specific but time invariant unobservable heterogeneities and address the selection bias. Besides genetic factors, the fixed effects approach helps explore the dynamics related to the persistence of shocks across individuals controlling unobserved heterogeneity between families that influences height. Thus we model the determinants of long-term child health as reflected by height-for-age z-scores status as follows:

(1) $H_{it} = \beta_1 \text{ Drought}_{it} + \beta_2 \text{ Coverage}_{it} + \beta_3 (\text{Drought}_{it} * \text{Coverage}_{it}) + \Sigma\, \beta_j X_{jit} + \alpha_{it} + \varepsilon_{it}$

where t=survey rounds 1,2,3; i= 1,..,N

[13] The Indian Meteorological Department declares rainfall as 'deficient' if it is 20% below its long-term average

H_{it} is the child's height-for-age z-score measured at time t(survey rounds). Drought $_{it}$ is a measure of negative rainfall shock affecting the location of the i^{th} child. Coverage is access to NREGS. While we do not focus on the independent impact of coverage on households, the key variable in our analysis is the interaction term which permits us to analyze how effective is the program in protecting child health during shocks, where it is expected to be all the more beneficial. Thus the parameter of interest is β_3. Precisely, a positive and significant β_3 would indicate that the negative effect of drought exposure on child health status is mitigated by the policy access. We saturate the regression equation (1) with all the relevant controls which can change over time and have independent influence on health status like receiving external food supplement and community health infrastructure. X_j's are time-varying regressors which include age of the child, health inputs, community resources. α_{it} represents the child fixed effects. The time-invariant regressors like sex of the child, mother's schooling, ethnicity of the household gets washed away in the child fixed effects specification.

While there is agreement that the make-up of health is highest in early childhood, estimates of mitigation can differ widely by a number of factors, such as severity, duration of the shock exposure, stage of development of the child at the start of malnutrition, gender of the child, household level demographics like education of the mother/caregiver, caste of the household. Thus we separately explore whether the program has differential impacts by the policy-relevant population sub-groups.

In estimating the effect of employment scheme in buffering health outcomes there can be a potential serious problem of selection that arises at two levels, first from the targeted roll-out of the program and secondly from the self-selection mechanism[14] by which the scheme operates giving rise to potential econometric issues. The issue of self-selection cannot be simply done away by using administrative records of roll-out as the phases were determined according to the backwardness index of the district. Also, within a particular mandal if the most poor households self-select into the scheme, then simple regression estimates without the individual fixed-effects might give under-biased estimates. In contrast if the more informed and well-connected households (among the poor households who are aware of the scheme) take advantage of the scheme first then estimates without fixed effects might over-bias the impacts of the scheme. Investment decisions about the amount of inputs to use may depend on, among other things, the health endowment of the child. It might be that a weak child may attract more attention and inputs from parents in an attempt to ensure his or her survival. Additionally, the overall level and mix of inputs depends on the parental preferences for health, which if not controlled can result in biased estimates. By using child-fixed effects estimation we could reasonably reduce these individual-specific but time invariant unobservable heterogeneities. Besides genetic factors, the fixed effects approach also neutralizes additive effects of other unobserved heterogeneity between families, like heterogeneity in terms of location, family structure, traditions, values norms, habits, wealth and household practices that influences height. However accounting for time varying characteristics across households would be more challenging. Here as we are identifying coverage from administrative records rather than self-reported measures of participation, the analysis is based on the treatment that the households were intended to receive and not on actual participation. Thus, based on the intent to treat approach

[14] Uppal (2009) finds that households hit by drought are 10.7% more likely to register for the NREGS than other households.

and not the household choice to take up this opportunity, we evaluate our research questions. Moreover by identifying drought at the mandal level (rather than measuring the intensity of the drought reported at the household-level), we have mitigated the reporting bias and some selection bias(from family-specific unobservables related with exposure variables) but have also introduced a source of measurement error and caused a potential attenuation bias in the estimates. Even though droughts are categorized as covariate shocks which simultaneously affect households over large geographical areas (and in spite of the fact that we do have very detailed mandal-level rainfall data), they are unlikely to affect all households equally in a given community. Precisely the household-level impact of a drought will depend on the occupation type among household members, availability of alternative irrigation sources, availability of alternative livelihood, access to safety nets, etc.

V. Data and Descriptive Statistics

The current study uses a unique household panel data set: Young Lives Survey from Andhra Pradesh, India- which is a longitudinal data set collected through household surveys conducted over three waves (2002, 2007 and 2009-10). For our study we use the longitudinal information of children who were aged 6 to 18 months in 2002. The sample comprises of 20 sub-districts or mandals from seven districts spread across the state. The sampling strategy was based on randomly selecting 150 children within 20 clusters or mandals spread across Andhra Pradesh[15]. The sample consists of 7 districts (including 103 villages) from the state to represent the

[15] Andhra Pradesh is divided into 23 administrative districts that are further subdivided into 1,125 mandals and 27,000 villages.

different regions[16] and income levels within the state. Overall attrition by the third round was 2.2%[17] (with attrition rate of 2.3 per cent for the younger cohort) over the eight-year period. The information on coverage of the scheme is obtained again from the administrative database, which has a detailed information of the NREG scheme expansion (month-wise mandal-wise records of the average number of days of employment provided, projects undertaken, percentage of women participation, number of years the program has been running in that administrative division etc). We construct a variable 'Coverage' which measures the average number of work days under NREGS per household for a particular mandal. We also have self-reported measures of program participation and use that to create a finer measure for program coverage .We refine our coverage variable(average number of days provided under NREGS per household in a mandal) using information on actual participation from the household survey data and construct variable 'NREGS'. We declare it to be zero where participation from a mandal is less than 5 percent.

Four of the Young Lives sample districts comprising of 11 sub-districts/mandal sites were covered by NREGS in the first phase of implementation in 2005-06, with the addition of four mandal sites in 2007, coinciding with second phase of implementation, and lastly with two more mandal sites were included in 2008- coinciding with phase-III of the program expansion. So, essentially in round two of the survey only phase-I districts were 'treated' while both phase-II and III were not covered. By the third round, all the sample districts were covered, although there remains variation in the program intensity as number of employment days available by

[16] Andhra Pradesh has three distinct agro-climatic regions: Coastal Andhra, Rayalseema and Telangana. The sampling scheme adopted for Young Lives was designed to identify inter-regional variations with a uniform distribution of sample districts across the three regions to ensure full representation.

[17] Attrition in the Young Lives sample is low in the international comparison with other longitudinal study (Outes and Dercon, 2008)

mandal is different. We restrict the sample to 4289 observations keeping households that are present in all the survey rounds and complete information on all control variables and excluding potential outlier cases with Height-for-age z score beyond the [−5, +5] range. Since, the employment guarantee policy is only relevant for the rural sector we focus on rural sample comprising 17 mandals and use the urban sample for robustness check.

We saturate the regression equation (1) with all the relevant controls which can change over time and have independent influence on health status like external food supplement (Food), community health infrastructure(Health Facilities). We include the following time varying observables that can be controlled- the exact age of the child at the time of interview, number of health care units in the community (mandal-level), whether child has been a part of supplemental food program in ICDS[18] centre/mid-day meal[19]. Both of these food supplement programs were universalized across the country much ahead of the NREGS policy implementation and are not associated with the availability of the employment guarantee scheme in a mandal. For household education we construct the variable 'Primary' measuring whether the caregiver has completed primary school. The 'Food Supplement' is a binary variable constructed from self-reported measures that takes value 1 if the child received food under the ICDS[20] scheme between round 1 and round 2 or if the child availed mid-day meal[21] scheme between round 2 and round 3 (i.e. when the kids are school going age). There exists

[18] Launched in year 1975, Integrated Child Development Scheme (ICDS) supplementary feeding is supposed to provide support to all children 0-6 years old for 300 days in a year (25 days a month).

[19] The Midday Meal Scheme is a school meal program in India which started in the 1960s was universalized by 2002. It involves provision of lunch free of cost to school-children

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variation in terms of health infrastructure across communities which might be related with health outcomes of child. We therefore control for the community health infrastructure which we proxy by the number of health facilities (both government and private hospitals) present in the mandal. This information on health facilities is obtained from the administrative records of Directorate of Economics and Statistics, Government of Andhra Pradesh. This information was collected and complied from handbook of statistics for different districts in Andhra Pradesh for different years.

Table1. Descriptive Statistics

Variable	**Phase I**		**Phase II and III**	
	Mean	**Std. Dev.**	**Mean**	**Std. Dev.**
Outcome Variables				
Height-for-age	-1.62	1.24	-1.63	1.09
Stunting	0.38	0.12	0.36	0.10
Program Variable/Shocks				
Coverage (Average Days)	26.77	22.41	13.99	20.92
Participation Percent	0.44	0.33	0.22	0.31
NREGS	26.77	22.41	13.26	21.04
Drought	0.56	0.50	0.67	0.47
Severe Drought	0.39	0.49	0.20	0.40
Cumulated Drought	0.45	0.25	0.65	0.17
Cumulated Severe Drought	0.30	0.22	0.25	0.17
Child Level Variables				
Food Supplement	0.43	0.49	0.62	0.49
Age	4.82	2.88	4.84	2.90
Male	0.53	0.50	0.52	0.50
Household Characteristics				
Primary Education of Household Head	0.25	0.43	0.54	0.49
Caste	0.16	0.37	0.10	0.30
Community Characteristics				
Health Facilities	1.88	1.18	3.63	1.23
Observations N=4289	**2831**		**1458**	

We show the descriptive statistics in Table 1 by phase-wise sites (phase II and III have been clubbed together as none of these received the program by the second round and can be treated as controls). We use annual rainfall data from the administrative records and health facilities disaggregated at the mandal level obtained from the Directorate of Economics and Statistics, Government of Andhra Pradesh.

We find that the anthropometric status of children – as measured by height-for-age – deteriorates between the time of birth (round1) and 5 years of age (round 2) for all phases-wise locations(Figure2,3,4). We have 66 % of our total rural sample from the phase I locations. As discussed earlier, the phase I mandals got access to coverage by April 2006, phase II mandals by April 2007, and Phase III mandals by April 2008.

In round 1 of the survey the average height-for-age z-score in phase I mandals was -1.20, which substantially went down to -1.84 in round 2 and recovered slightly to -1.81 in the third round. It should be noted that the urban locations from all the districts were dropped from the current analysis, however the calculation of backwardness index on the basis of which coverage was rolled out in a particular district included these locations. Thus, it is not surprising, in spite of being slightly higher in rank in the backwardness index as a district, for the remaining rural sample locations under phase II, the average height-for age was slightly worse off than that of phase-I. However, for phase-II, the mean height-for-age z score went down from -1.50 to -1.70, which again went up to -1.66 in the third round. Unlike the other two phases, for Phase III, the mean height-for-age z score went down for all the rounds from -1.55 to -1.74 between the first two rounds and then to -1.84 in the third round. We present briefly the discussion of the findings in the following section.

VI. Discussion of the Findings and Policy Insights

All regression specifications with height-for-age as outcome variable includes child fixed effects, and regressions with average stunting percent at the mandal level include mandal fixed effects. Table 2.1 shows the regression estimates of cumulative drought shock, program availability and their interaction on Height-for-Age for child-fixed effects specifications. In both specifications (1) and (2) we include cumulated past drought shocks, with different degrees of severity. We find that regardless of how we specify severity of drought, it has significantly strong negative effect on height-for-age. In both specifications we control for child age, supplementary food intake and community health facility. We use robust boot strapped standard errors clustered at the level of treatment -here at the level of mandal. The interaction term of program and drought although positive (suggestive of mitigation) is not significant in either model (1) or (2). However, we find the food supplement variable to have a positive and significant effect on height-for-age in both the specifications, reiterating the importance of nutrition. In Table 2.2 we include the recent exposure to drought [22]to examine whether the policy is at least able to serve as buffer in this case. We find that while even recent exposure of mild drought significantly affects the height-for-age around .4 standard deviations, the program serves as a significant buffer against these shocks, increasing the height-for-age z-score by around .26 standard deviations for those who suffered from the shock, thereby mitigating some of its negative impact. We use the refined measure 'NREGS' (corrected for low participation) and find similar impacts as specification (1). As a further robustness check we repeat specification (1) for urban sites (the idea being that the availability of the program will not be affecting the urban households) in specification (3) and find no buffering effect of program

[22] Drought is receiving less than the long term average rainfall at mandal in the year prior to survey

availability as per our expectation.

In Table 2.3 we carry out a similar exercise with the outcome variable of average stunting[23] defined at the mandal level to see the impact of cumulative shocks and recent shocks and the program mitigation. Specification (1) and (2) include cumulative drought shocks and specification (3) includes recent drought. We find similar results compared to that of height-for-age. We find the level of stunting increases by around 8% with exposure to even recent mild drought. We run a robustness check for the main findings on stunting level in Table 2.4. We find program access leads to .3 standard deviation improvement in stunting for locations suffering from drought last year.

In Table 2.5 while there is similar impact of drought and program impact by gender, we find coefficient of food supplement although positive for both gender groups, is highly significant for female children with a .17 increase in standard deviation for height-for-age, significant at 1%.

In Table 2.6 we examine the impacts by caste groups. While there is a greater negative impact of drought exposure for the backward caste children we find the availability of program is significant in serving as buffer for these groups. Also, notable is the fact that food supplement is positive and significant for lower caste children as per our expectation. In Table 2.7 disaggregating the results by education level of the caregiver we find a strong significant negative impact of drought exposure on children for whose caregiver's education level is below primary level. Also, notable is the fact that it is only for this group that the food supplement variable is significant as well. The impact of drought although negative is not found to be

[23] Stunting is a dummy variable with Height-for-age less than -2 standard deviations

significant for those kids where caregivers have higher than primary education. However there seems to be similar impact of program availability across these groups.

In general we find the access to program per se is not significant across specifications, but significant for those with drought, as per our expectation. The coefficient of program variable although statistically insignificant has a negative sign indicating the possibility of negative selection for participation in the program. It may be possible that people who lost jobs/ had a decline in household income joined the program. Also, notable is the fact that when we exclude the fixed effects the OLS results (not reported here) understates the impact of both drought and the interaction. Although, we find the health facility variable to be positive and significant in the OLS specifications, we find it insignificant with the fixed effects. The estimated coefficient on 'Age' is always negative and significant across all specifications in rural sites signifying worsening of z-score with the age. A one year increase in age decreases height-for-age z-scores by 0.09 standard deviations in the fixed effects estimation. Food supplement is found to be positive and significant for all rural specification highlighting the beneficial impact of supplementary nutrition on health outcome. Even when we interact the food supplement variable with drought shocks we find its significant beneficial impact for mitigating negative impact of drought, especially for the girl child. Hence, we find the estimated coefficient on the food variable to be positive and significant in almost all specifications confirming our prior expectation about the role of nutrition in child health.

Thus to summarize our results for policy insights we find while there is long-run impact of early-life conditions on health several years later, access to coverage helps tackle only for recent shocks but not correct for longer-term past deficiencies. However the results indicate that access to coverage seems to help compensate poor child nutrition and growth, thus helping poor vulnerable individuals to cope with the very recent drought shocks. Hence, it is important to note here that social safety nets available later on life cannot mitigate past deficiencies that carry forward later on life. Further, the analysis underscores the importance of food supplement in this whole set up, especially pronounced for female children, children from backward castes and for households with lower education level. Hence there is much room which the policy can address by working on ensuring food security issues of the household. The analysis once again brings out the vulnerability of these households in the face of increasing climatic variability. Hence this calls for policy dialogue on focusing more on developing the nutritional aspect of the policy, timely delivery of wages and catering to the unmet demand in the poorest districts so that it may serve as an effective safety-net.

Table 2.1

Dependent Variable :Height For Age		
	(1)	(2)
Drought_Cumulated	-0.975***	
	(0.323)	
Coverage	-0.00700	-0.00505
	(0.0113)	(0.00725)
DroughtC*Coverage	0.0118	
	(0.0208)	
Food Supplement	0.0953**	0.0862*
	(0.0391)	(0.0471)
Health Facility	0.00765	0.0141
	(0.159)	(0.165)
Age	-0.0627***	-0.0530**
	(0.0224)	(0.0243)
Severe Drought_Cumulated		-1.071**
		(0.512)
Severe DroughtC*Coverage		0.0132
		(0.0211)
Observations	4289	4289

Robust boot-strapped Standard errors (clustered at the mandal) in parentheses
Note:* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
a) HAZ indicates Height for Age, adjusted for both Age and Sex
b) Coverage is Average number of Days available under NREGA in the mandal
c) DroughtC is a fraction of years having low drought less than the long term average rainfall at mandal cumulated from birthyear defined at mandal level
d) Severe DroughtC is a fraction of years having less than 20% rainfall from the long term average at mandal cumulated from birthyear
e) Specifications include child fixed effects

Table 2.2

Dependent Variable: Height For Age

	(1) Rural	(2) Rural	(3) Urban
Drought	-0.403***	-0.399***	0.0322
	(0.139)	(0.142)	(0.257)
Coverage	-0.00727		-0.00613
	(0.00586)		(0.00887)
Drought*Coverage	**0.0127***		**0.00489**
	(0.00467)		(0.00782)
Food Supplement	0.134***	0.134***	0.0751
	(0.0432)	(0.0454)	(0.0480)
Health Facility	0.0692	0.0686	0.106
	(0.0873)	(0.117)	(0.154)
Age	-0.0851***	-0.0876***	-0.0201
	(0.0255)	(0.0312)	(0.0306)
NREGS		-0.00671	
		(0.00444)	
Drought*NREGS		**0.0124***	
		(0.00412)	
Observations	4289	4289	1376

Robust boot-strapped Standard errors (clustered at the mandal) in parentheses
Note:* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
a) HAZ indicates Height for Age, adjusted for both Age and Sex
b) Coverage is Average number of Days available under NREGA in the mandal
c) Drought is receiving less than the long term average rainfall at mandal in the year prior to survey
d) Specifications include child fixed effects
e) NREGS is Coverage (Average days per household under the program)corrected for participation

Table 2.3

Dependent Variable : Stunting

	(1)	(2)	(3)
Drought_Cumulated	0.246***		
	(0.0775)		
Coverage	0.00243	0.00158	0.00235**
	(0.00216)	(0.00192)	(0.000969)
DroughtC*Coverage	-0.00422		
	(0.00430)		
Food Supplement	-0.0103**	-0.00828*	-0.0144***
	(0.00428)	(0.00482)	(0.00409)
Health Facility	0.00236	-0.000529	-0.0114
	(0.0410)	(0.0341)	(0.0251)
Age	0.0131***	0.0109*	0.0158***
	(0.00476)	(0.00624)	(0.00609)
Severe Drought_Cumulated		0.270**	
		(0.123)	
Severe DroughtC*Coverage		-0.00437	
		(0.00489)	
Drought			0.0780**
			(0.0319)
Drought*Coverage			**-0.00325*****
			(0.000913)
Observations	4289	4289	4289

Robust boot-strapped Standard errors (clustered at the mandal) in parentheses:
* p < 0.10, ** p < 0.05, *** p < 0.01
a) Dependent variable stunting is dummy variable, takes 1 if Height-for-age < -2
b) Coverage is Average number of Days available under NREGA in the mandal
c) Drought is receiving less than the long term average rainfall at mandal in the year prior to survey
d) DroughtC is a fraction of years receiving less than the long term average rainfall at mandal cumulated from birth
e) Severe DroughtC is a fraction of years receiving less than 20% rainfall below the long term average at mandal

Table 2.4

Dependent Variable: Stunting

	(1) Rural	(2) Rural	(3) Urban
Drought	0.0821**	0.0785**	-0.0292
	(0.0349)	(0.0352)	(0.0439)
Coverage	0.00238**		0.000236
	(0.00117)		(0.00155)
Drought*Coverage	**-0.00341*** **		**0.0000705**
	(0.000843)		(0.00128)
Food Supplement	-0.0217***	-0.0219***	-0.00195
	(0.00794)	(0.00816)	(0.00148)
Health Facility	-0.0116	-0.0113	-0.0222
	(0.0284)	(0.0315)	(0.0258)
Age	0.0166**	0.0166**	0.00546
	(0.00777)	(0.00697)	(0.00714)
NREGS		0.00233**	
		(0.000921)	
Drought*NREGS		-0.00333***	
		(0.000810)	
Observations	4289	4289	1376

Robust boot-strapped Standard errors (clustered at the mandal) in parentheses:
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
a) Dependent variable stunting is dummy variable, takes 1 if Height-for-age < -2
b) Coverage is Average number of Days available under NREGA in the mandal
c) Drought is receiving less than the long term average rainfall at mandal in the year prior to survey
d)Specifications include mandal(sub-district) fixed effects
e) NREGS is Coverage (Average days per household under the program)corrected for participation

Table 2.5

Dependent Variable: Height For Age (Results by Gender)

	(1) Male	(2) Female
Drought	-0.401***	-0.401***
	(0.137)	(0.121)
NREGS	-0.00876*	-0.00428
	(0.00453)	(0.00368)
Drought*NREGS	0.0133***	0.0114***
	(0.00358)	(0.00382)
Food Supplement	0.0957	0.178***
	(0.0613)	(0.0463)
Health Facility	0.0707	0.0689
	(0.115)	(0.0928)
Age	-0.0807***	-0.0969***
	(0.0297)	(0.0233)
Observations	2272	2017

Robust boot-strapped Standard errors (clustered at the mandal) in parentheses
Note:* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
a) HAZ indicates Height for Age, adjusted for both Age and Sex
b) Coverage is Average number of Days available under NREGA in the mandal
c) Drought is receiving less than the long term average rainfall at mandal in the year prior to survey
d) Specifications include child fixed effects
e) NREGS is Coverage (Average days per household under the program)corrected for participation

Table 2.6

Dependent Variable: Height For Age (Results by Caste)

	(1) General Caste	(2) Backward Caste
Drought	-0.300*	-0.413***
	(0.156)	(0.145)
NREGS	-0.00517	-0.00679
	(0.00790)	(0.00519)
Drought*NREGS	0.00960	0.0127***
	(0.00639)	(0.00390)
Food Supplement	0.187	0.126***
	(0.137)	(0.0454)
Health Facility	0.126	0.0678
	(0.148)	(0.0692)
Age	-0.0957***	-0.0871***
	(0.0336)	(0.0250)
Observations	600	3689

Robust boot-strapped Standard errors (clustered at the mandal) in parentheses
Note:* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
a) HAZ indicates Height for Age, adjusted for both Age and Sex
b) Coverage is Average number of Days available under NREGA in the mandal
c) Drought is receiving less than the long term average rainfall at mandal in the year prior to survey
d) Specifications include child fixed effects
e) NREGS is Coverage (Average days per household under the program)corrected for participation

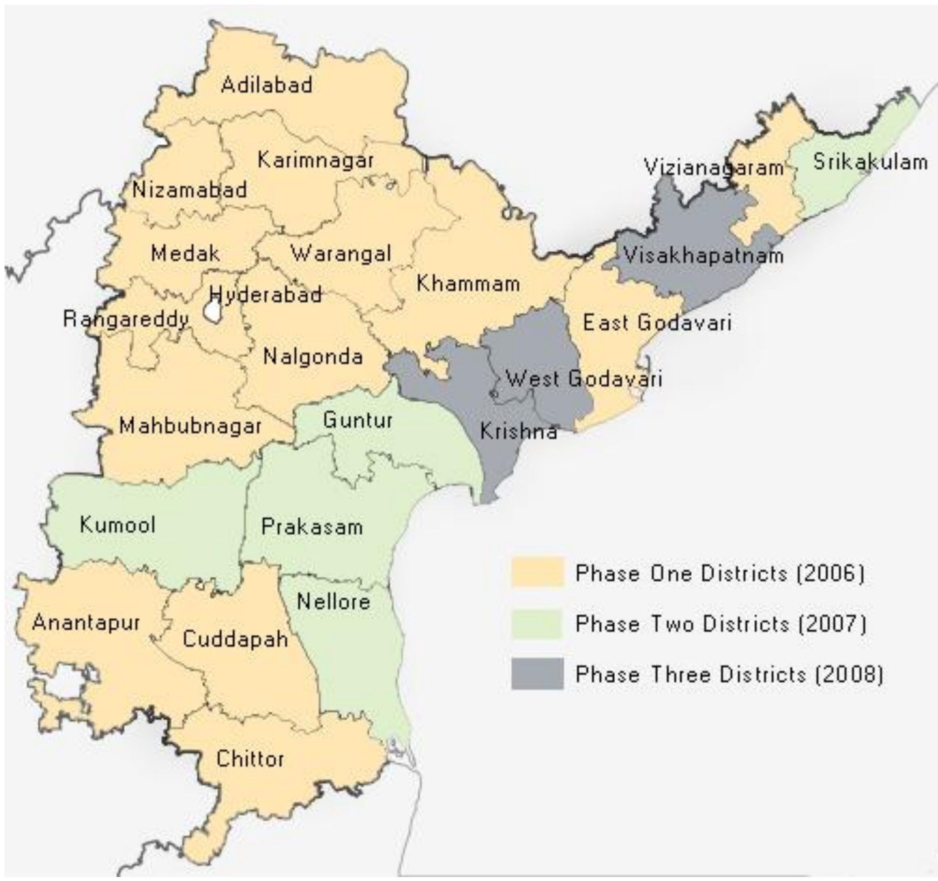
Table 2.7

Dependent Variable :Height For Age (Results by Caregiver's Education Level)

	(1) Primary	(2) Below Primary
Drought	-0.385	-0.412***
	(0.252)	(0.132)
Coverage	-0.0118	-0.00566
	(0.00808)	(0.00510)
Drought*Coverage	0.0177**	0.0110***
	(0.00726)	(0.00350)
Food Supplement	0.0799	0.164***
	(0.0649)	(0.0545)
Health Facility	0.0434	0.0863
	(0.114)	(0.0940)
Age	-0.0756**	-0.0911***
	(0.0368)	(0.0274)
Observations	1229	3057

Robust boot-strapped Standard errors (clustered at the mandal) in parentheses
Note:* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
a) HAZ indicates Height for Age, adjusted for both Age and Sex
b) Coverage is Average number of Days available under NREGA in the mandal
c) Drought is receiving less than the long term average rainfall at mandal in the year prior to survey
d) Specifications include child fixed effects
e) NREGS is Coverage (Average days per household under the program)corrected for participation

Figure 1: Map of phase-wise expansion of NREGS across Young Lives Sample



Phase-wise Coverage across districts in Andhra Pradesh

Phase - I	Phase - II	Phase – III
VIZIANAGRAM	EAST GODAVARI	WEST GODAVARI
CHITTOOR	GUNTUR	KRISHNA
CUDAPPAH	KURNOOL	VISHAKHAPATNAM
ANANTPUR	NELLORE	
MAHBUBNAGAR	PRAKASAM	
MEDAK	SRIKAKULAM	
RANGA REDDY		
NIZAMABAD		
WARRANGAL		
ADILABAD		
KARIMNAGAR		
KHAMMAM		
NALGONDA		

*The colored districts imply the sample ones from the survey.

Figure 2: Mean Height-for-Age by Survey Rounds

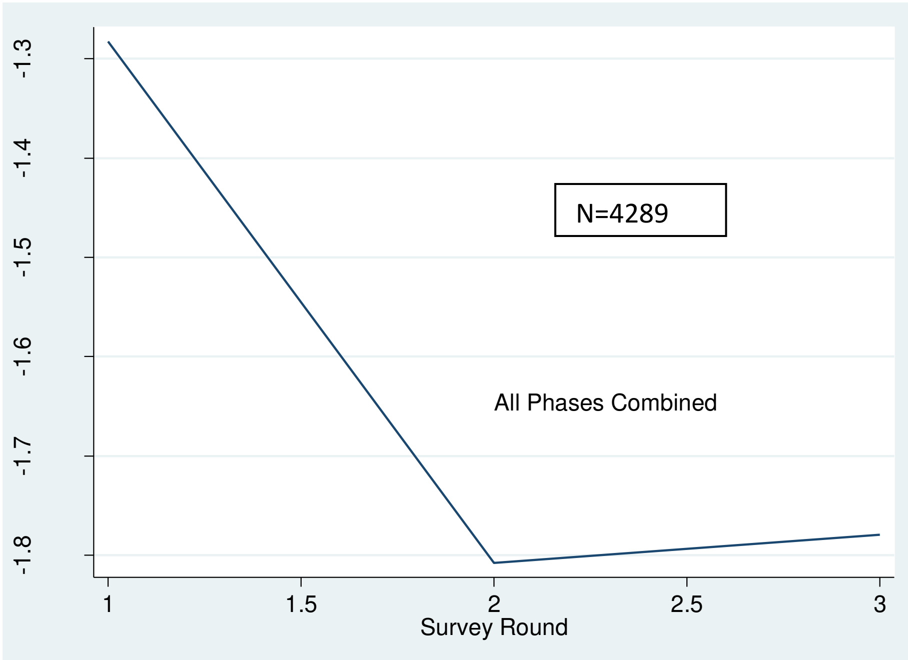


Figure 3: Mean Height-for-Age by Rounds and Phases

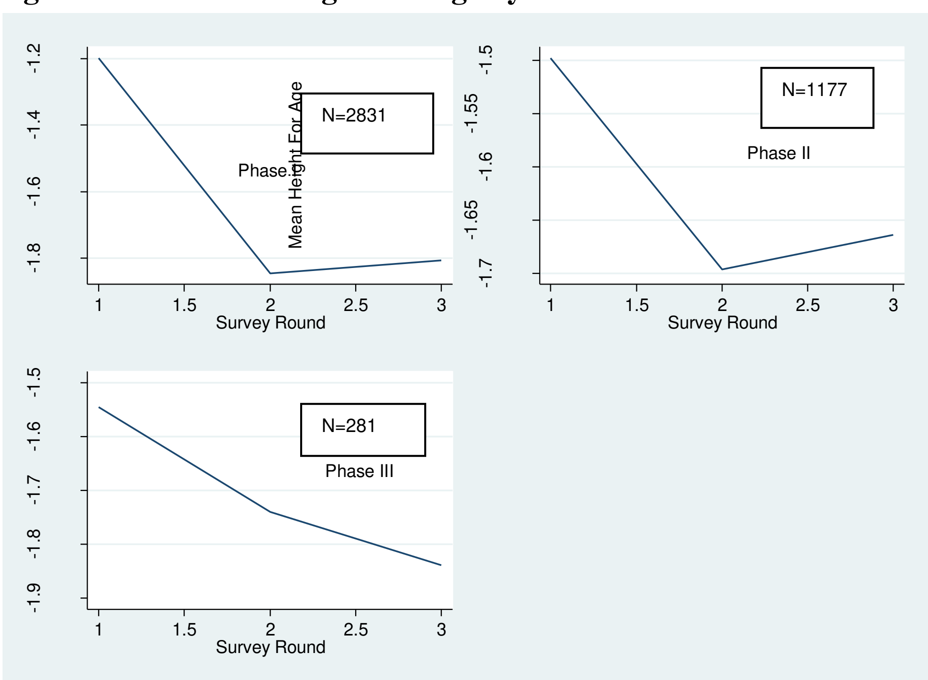


Figure 4: Mean Height-for-Age by Caregiver's Education

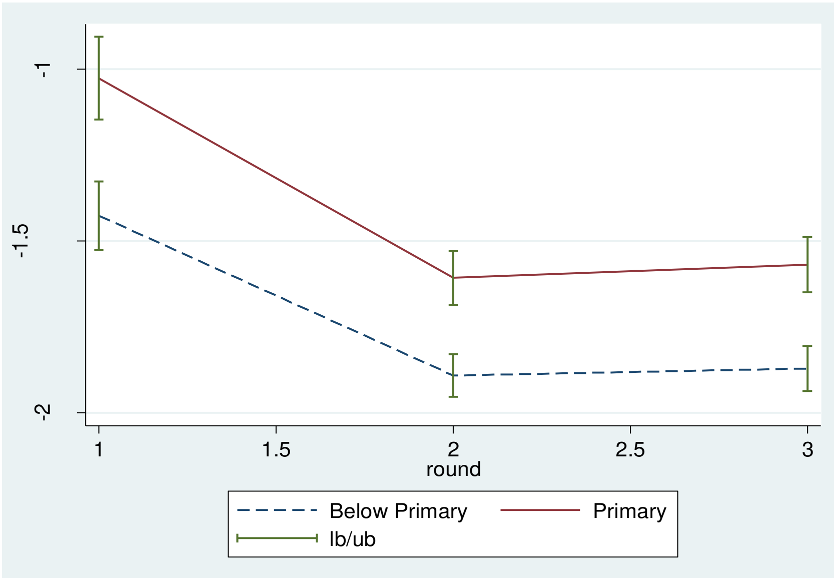


Figure 5: Mean Height-for-Age by Gender

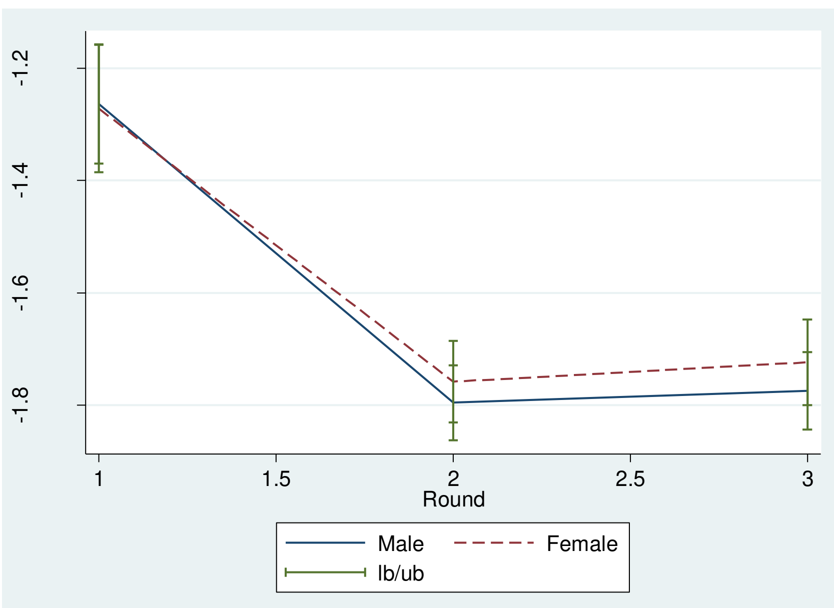
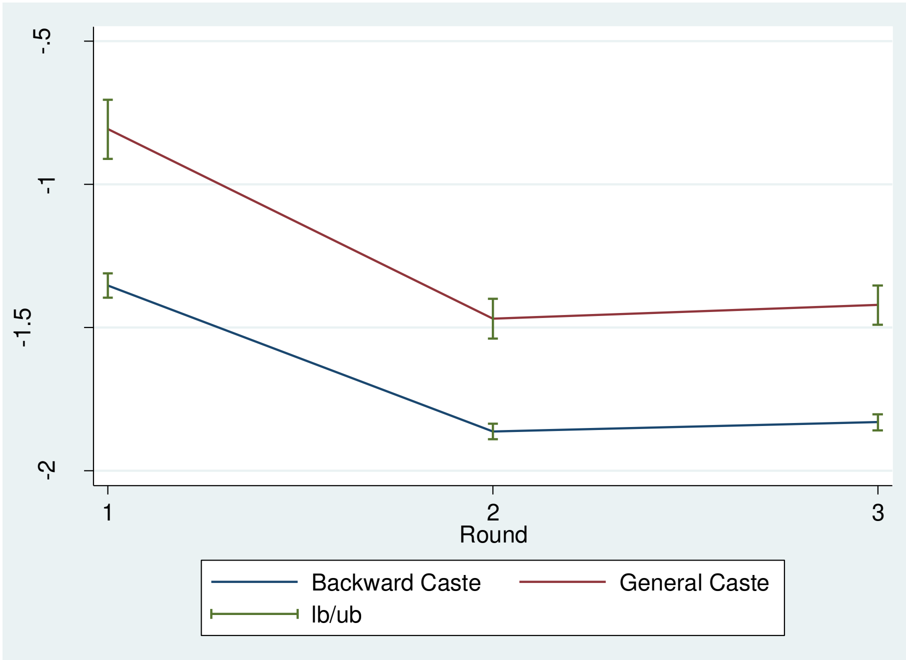


Figure 6: Mean Height-for-Age by Caste



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Heat Waves at Conception and Later Life Outcomes*

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Abstract

This paper asks whether children conceived during heat waves have better health and educational outcomes later in life. We hypothesize that during heat waves, sexual activity not intended to result in a conception decreases, implying that children conceived during heat waves are on average more planned than otherwise. Using Rwandan Census data, we show that children conceived during heat waves have higher literacy rates and more years of schooling by age 16. We also show, using a combined AIS, DHS, and MIS data set from Africa, that infant mortality is lower for children conceived during heat waves. We provide evidence that these effects most likely run through two channels: wantedness, and selection of lower quality parents out of conceiving during heat waves.

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1 Introduction

What drives differences in educational and health outcomes in children, and later adults? A large body of evidence points to the importance of early life and in utero conditions, especially health conditions, in determining later life outcomes. Exogenous variation in climatic conditions are increasingly being used as proxies or instruments for variation in health conditions in infancy or in utero. However, we believe that variations in temperature could affect another important determinant of outcomes: child wantedness.

During heat waves, sexual activity decreases. Insomuch as the reduction in sexual activity during heat waves is because sex is less enjoyable when it is hot, the disproportionate share of this reduction in fertility should happen among unwanted or mistimed pregnancies. The key idea is that the timing of purposefully conceived children should not be affected by something as trivial as a heat wave, but accidental pregnancies could. As a result, the fraction of children who are wanted (or planned) should rise during heat waves.

In this paper, we estimate the effect of heat waves at conception, in utero, and in infancy on a series of educational and health outcomes. In this version of the paper, we focus on five African countries (Guinea, Malawi, Rwanda, and Uganda) as well as Spain, but have plans to expand that to 12 additional countries distributed across the world and in both high, middle, and low income countries. Since we are especially interested in the effect of child wantedness on outcomes, we focus on heat waves at the time of conception. We find that children conceived during heat waves in Rwanda are more literate later in life, obtain more schooling, and have lower rates of infant mortality.

There may be other reasons besides wantedness that temperature at the time of conception may affect outcomes. In this paper, we propose several reasons why heat waves at conception may affect outcomes, and provide evidence for or against each hypothesis. We divide these potential reasons into two categories: behavioral (including wantedness) and biological.

Beyond wantedness, a second behavioral reason we think temperature at the time of conception may effect outcomes is closely related to the first – perhaps lower quality mothers (quality meaning in terms of education or health) select out of having children during heat waves. As stated before, if the timing of a woman's pregnancies are based on something as trivial as a heat wave, perhaps that may be correlated with the woman having a low education, being careless, etc., which would then affect the child's well being.

Another potential cause of differences in the outcomes of children conceived during heat waves is a biological one: perhaps heat affects the probability of conception differentially across different types of mothers. For example, if it is harder to conceive when it is hot for biological reasons, perhaps more fertile (or healthy) mothers would still be able to conceive during heat waves, while less fertile mothers would not. If there is a correlation between fertility of the mother and the outcomes for the child, we would expect to see children conceived during heat waves have better outcomes.

A second biological cause could be that children conceived during heat waves face different conditions during early pregnancy than children conceived during periods of normal temperatures. There is a large literature on the effect of fetal stress due to famines, malarial conditions, or other adverse maternal health shocks on child outcomes. It may be the case that above average temperatures constitute such a shock. As a result, high temperatures in utero may adversely affect outcomes later in life.

Estimating the effect of temperature at conception, in utero, and in infancy is not trivial. There are many reasons why absolute temperature should be correlated with outcomes even if no causal effect exists. For example, temperature could be correlated with the seasonality of birth and conception, or geography (to name a few), which in turn could affect child outcomes. In this paper, we use within-region deviations from month-specific average temperatures as our measure of heat. This way, only the exogenous variation in temperature due to heat waves and cold snaps is used to identify the effect of temperature on later life outcomes. This method is similar to Lam and Miron's (1996) paper on heat waves and fertility rates, and Kudamatsu et. al.'s (2012) paper on malarial conditions and infant mortality.

Our paper contributes to the literature in several important ways. First, while other papers use weather shocks as a proxy for malarial or drought conditions to assess the impact of malaria or malnutrition at birth and during infancy on child health (see Kudamatsu et. al. 2012), we are the first paper to focus solely on temperature. We believe there can be other interesting effects of temperature on selection into pregnancy, biological effects on health and cognition, and the like besides just through the channel of malaria or malnutrition. Second, while Kudamatsu et. al. have identified the effect of malarial conditions (including temperature) at birth and infancy on infant mortality, we are the first paper to focus on the effect of temperature at conception and in utero. Third, while we do estimate the effect of heat waves at conception and later infant mortality, our main focus is the effect of heat waves at conception on other health and educational variables such as disability, literacy, and years of schooling. Fourth, while our focus in this paper is on heat waves at conception, we also provide the first estimates of the effect of heat waves at birth and in infancy on educational and health outcomes later in life, rather than just infant mortality. Finally, we are the first paper to our knowledge to show that sexual activity decreases in heat waves.

Using weather data from the National Climatic Data Center (NCDC) at the National Oceanic and Atmospheric Administration (NOAA), and outcomes data from census in IPUMS , we find that children conceived during heat waves have higher rates of literacy, more years of education, and lower rates of disability as adults. Using data from a combined AIS, DHS, and MIS data set for all of Sub-Saharan Africa, we find lower infant mortality for children conceived during heat waves.

We further investigate which of the possible channels is most likely responsible for our results. We find evidence that higher quality mothers are more likely to conceive during heat waves. Literacy rates and years of schooling are significantly different for mothers who conceived during heat waves as opposed to during normal temperatures. In addition, we find no evidence of adverse effects of temperature in utero, ruling out the hypothesis that heat alone is an adverse health shock to the fetus. We also find that when controlling for parental characteristics, the positive effect of temperature goes away in Spain, but persists for Africa.

To investigate the wantedness channel, we explore three different metrics: data on the effect of heat on sexual activity, the effect of heat on reported wantedness, and the effect of heat waves on child spacing.[1] Using DHS data, we find that heat waves reduce the likelihood a woman will report being sexually active. In addition, using data on Google searches on adult websites and other online sexual material in Rwanda as a proxy for sexual activity, and we again find that sexual activity decreases during heat waves. Since we previously found that heat waves improved the average education levels of women who conceive, this suggests that women with lower education levels are reducing their fertility faster than highly educated women during heat waves. This is consistent with our story of wantedness, inasmuch as educated women are less likely to make mistakes. We also find that children conceived during heat waves are more likely to be reported as wanted than those conceived during normal temperatures directly from DHS data. We also find that child spacing for children conceived in heat waves is longer. All three of these results suggest wantedness is a main driver of the effects of heat on later outcomes.

The paper proceeds as follows: Section 2 establishes that there indeed is a causal link between temperature at the time of conception and later life outcomes. Section 3 provides evidence that these effects run through the channel of child wantedness, rather than other behavioral or biological channels. Section 4 concludes.

2 Heat Waves at Conception and Outcomes

Methodology

Estimating the effect of temperature at conception, in utero, and in early childhood on outcomes is not trivial. There are many reasons why absolute temperature should be correlated with outcomes even if no causal effect exists.

[1] Child spacing may be indicative of unwanted or mistimed births, since if an accident occurs spacing will be shorter than otherwise desired.

For example, temperature is related to seasonality. There is a large and old literature analyzing the effect of seasonality and outcomes. Many births (and conceptions) are timed around the seasons, the school year, holidays, summer vacations, etc. In the United States, the months of August and September have more births per day than any other month. In addition, the parents of children born in the spring tend to have higher education levels and have higher wages than of those born in the fall or winter.

Another reason why temperature may be related to outcomes is spatial in nature: absolute temperature may be correlated with the level of development in a region. A quick view around the globe reveals the interesting fact that locations closer to the poles tend to be more developed than those close to the equator. However, almost all of the explanations of that correlation have nothing to do with temperature, but rather historical population densities, institutions, technological progress, and disease environments, to name a few. In addition, topographical characteristics could be correlated with weather and development. For example, places along the coasts tend to have smaller between-month temperature deviations than landlocked regions, and coastal regions also have higher economic development due to lower transportation costs via naval shipping. Or locations at higher elevations could both be colder and have lower levels of economic development.

In this paper, we use within-month and within-region temperature deviations to identify the effect of heat waves on later life outcomes. By comparing the temperature in a given region in a given month against average temperatures for the same month and region, we isolate the variable component of temperature while stripping away the permanent component. We assume that these weather shocks are uncorrelated to geography, seasonality, and other similar variables which affect later life outcomes.

Formally, we estimate the following regression equation to test whether temperature at conception affects later life outcomes:

$$Y_i = \alpha_y + \theta_r + \gamma_m + \sum_{j=t-15}^{t+15} \beta_j T_{j,i} + \psi X_i + \epsilon_i, \tag{1}$$

where Y_i is the outcome of interest for individual i, α_y is a year fixed effect, θ_r is a dummy variable for being born in region r, and γ_m is a dummy variable for being born in month m. In addition, there are 31 temperature variables T_j, corresponding to the deviation from the month-specific average temperature in the region of birth for each month from 15 months before birth to 15 months after birth. For example, if $t-0$ (the time of birth) falls in July, then $T_{t-0,i}$ is the deviation of temperature in that July in which the child is born from the average temperature over all other July's in that region. This way, we are left with just deviations in temperature from the average temperature at that time of year, and we strip out the part of temperature which varies with the season. This is important because the seasonality of birth affects outcome for other reasons besides temperature, such as timing of births around specific holidays, the school year, summer vacation, etc.[2] Finally, X_i is a vector of other explanatory variables. In our baseline specification of the model, we do not include any other explanatory variables. Later we control for parental characteristics. Theoretically, if the temperature data is truly exogenous as we believe it is, then the coefficients on the β_j should be unaffected by the exclusion or inclusion of extra explanatory variables.

The main variable of interest is β_{t-9}. If $\beta_{t-9} > 0$ as we hypothesize, that implies heat waves at conception are correlated with better outcomes. However, the other βs may be interesting as well. For example, we can test whether heat waves at the time of birth, or shortly after birth, affect later life outcomes. This may occur due to changes in the disease environment the infants are subjected to with increased heat, for example. The variables for temperature deviations before conception provide a placebo test for our result, since there are few, if any, theoretical reasons heat before conception should affect later life outcomes.[3]

The key identifying assumption in this section is that the deviation from the average monthly temperature in a given region at the time of conception (or later) is exogenous to outcomes later in life, and therefore the effects of

[2]See Lam and Miron (1996) for a more detailed discussion.
[3]See Section 3 for a more detailed discussion.

heat at conception on outcomes are causal. This assumption seems fairly straightforward. However, interpreting why heat may affect outcomes is more difficult. In section 3, we analyze several possible channels for the effect of heat on outcomes and conclude that the most likely cause is child wantedness.

One potential source of bias to our estimates is migration. Since we only observe the location of birth, and not the location of conception, we cannot be exactly sure that the individuals in our sample were exposed to the heat deviations at the time of conception (or in utero) in the region of their birth. This measurement error would bias our results towards zero. However, given the rates of migration observed in the census, we believe that the fraction of children who were conceived in a location different from their birth is small. In addition, since we will find significant results on the effects of temperature at conception on outcomes, this only serves to strengthen our findings.

Another problem with assuming that children are conceived precisely nine months before birth is that the timing of birth is a random variable, whose mean is around 40 weeks, or close to nine months. Some children, however, are born prematurely or late. As a result, heat waves which occur at $t-8$ or $t-10$ may actually be the true heat waves at conception for some children. As a result, the βs immediately surrounding β_{t-9} may also show significant results if heat waves at conception affect outcomes. In addition, each of the coefficients β_{t-8}, β_{t-9}, and β_{t-10} will be attenuated towards zero, and therefore underestimate (in terms of absolute value) the true impact of heat waves at the time of conception.

Data

Our data on temperature comes from the National Climatic Data Center (NCDC) at the National Oceanic and Atmospheric Administration (NOAA). The NCDC publishes a large set of climate data from each weather station globally from 1900, including data on temperature, rainfall, wind. For example, in Rwanda there are five weather stations, in Butare, Gisenyi, Kamembe, Kigali, and Ruhengeri. All stations have been in operation since August of

1985, with Kigali being in operation since June of 1973.

Each station reports weather conditions every few hours. From these hourly observations, NOAA constructs daily temperature data set, from which we construct a data set of average temperatures for each month for each station. Then we link these monthly temperatures for each weather station to the 12 provinces of Rwanda contained in the census data.[4] Since there are 12 provinces in Rwanda, but only 5 weather stations, we omit provinces which do not have a station. We repeat the process for each of the four African countries in our sample and Spain. Finally, we detrend the temperature data using the average temperature across each individual month to get deviations in temperature from the average temperature, providing us with a measure of heat waves. For example, for a given July, the temperature variable will show the deviation from the average temperature in that specific region compared with all other Julys.

We then link the data on heat waves to the national censuses available from IPUMS. The census contains data on the region, month, and year of birth, as well as a variety of outcomes such as literacy, educational attainment, and disability. Using the region and timing of birth, and assuming that people are conceived in the same region they are born in, we can find the temperature at conception (or soon before or after) for each individual in our sample. Specifically, we create 31 different temperature variables, corresponding to the detrended temperature in his region of birth for each month from 15 months previous to birth to 15 months after birth for each individual. Then we can regress the outcomes on these temperature variables.

[4] In 2006 the 12 provinces of Rwanda were collapsed into 5 larger provinces. Since the census data (which we describe later) was from 2002, the old province classification was used.

Results

Rwanda

We estimate equation (1) using three different outcomes: a literacy dummy, years of schooling, and a disability dummy.[5] The results for Rwanda are presented in Table 1, and in Figures 1, 2, and 3 for literacy, years of schooling, and disability respectively. The three figures plot the estimated β coefficients for the effect of the deviation from average temperature for every month between 15 months prior to birth to 15 months after birth on literacy . The 95% upper and lower confidence intervals are also shown.

From Figure 1, the we see that there is is a positive effect of being conceived during a heat wave on literacy. The coefficient on β_{t-9} of 0.0083 implies that if the average temperature during the month of conception is one degree (Celsius) hotter than normal, those individuals are 0.83% more likely to be literate as an adult. This result is significant at the one percent level. Surprisingly, if a heat wave occurs one month after birth, then those babies are 0.87% more likely to be literate as an adult, also significant at the one percent level. Stated differently, a one standard deviation increase in temperature at the time of conception increases the literacy rate 0.132%, and by 0.139% for a one standard deviation increase one month after birth. Heat waves at any other time in utero or in early childhood have no statistically significant effect on literacy later in life.

One reason we included variables for heat waves up to six months before conception is to provide a placebo test. It is hard to think why heat waves before conception would affect later life outcomes.[6] Since we do not find any affect of heat waves before conception, it lends credibility to our empirical specification.

[5]The disability dummy takes a value of 1 if the individual is *not* disabled, meaning a positive correlation with this variable implies a healthier population. The literacy dummy is standard, meaning the variable takes a value of 1 if the individual is literate.

[6]One possible exception is an income effect. For example, if higher temperatures are correlated with harvest size or income, then temperature shocks before conception could in affect nutrition for the mother leading up to pregnancy, and thereby affect the child's health or cognitive ability in utero. We find no evidence of such an effect. See Section 3 for details.

The results for years of schooling are similar to the results for literacy: Heat waves at conception and one month after birth increase the years of schooling completed. Specifically, if the average temperature during the month of conception is one degree hotter than normal, those individuals attain an additional 0.055 years of schooling. For one month after birth, the coefficient is 0.069 years of schooling. Both of these results are significant at the one percent level. Stated differently, a one standard deviation increase in temperatures would increase years of schooling by 0.088 if they occurred at the time of conception, and by 0.109 years for one month after birth.

Interestingly, we find no effect of heat waves on disability rates.[7] This could be the case for two reasons: There may be no effect of heat waves on disability rates, or there may be an effect but we do not capture it because we are only looking at young adults, and disabilities would likely show up later in life. The only thing we can say with confidence currently is that heat waves at conception, in utero, or in early childhood do not seem to be correlated with disability in young adults.

Africa and Spain

The results for Rwanda are especially clean – however not all countries look the same. For example, Uganda and Malawi exhibit a similar pattern of the effect of heat on outcomes, but Guinea has a very small sample size (due to poor weather data) and looks very different. In addition, in some countries temperature for some months becomes significant – but those results are not significant across countries. To get a better picture of what is going on in Africa as a whole, we decided to pool all the censuses together and estimate

[7] While some of the beta coefficients in the disability regression are marginally statistically significant, the results follow no consistent pattern and occur at months which do not make economic sense. We attribute these results to the fact that, given the definition of statistical significance at the X percent level, X out of every 100 coefficients should be statistically significant even if there is no underlying effect. We find it comforting to find that out of 93 estimated temperature coefficients in the three regressions, precisely five are significant at the 5% level and ten significant at the 10% level. The only exceptions are the coefficients on the temperature at time of conception and one month postpartum, which are consistently large, positive, and significant at the one percent level for the education regressions.

equation (1). The results are reported in Table 8. We only report the results for β_{t-9}, but the full regression results can be provided upon request. We find that for these four African countries, years of schooling and educational attainment are higher for children conceived during heat waves, while we do not find significant results on literacy and disability at the 10% level, but we do at the 15% level.

We report the results for Spain in table 9. Similar to Africa, we find significant results on the effect of heat waves at conception on years of schooling, educational attainment, literacy rates, and disability. In general, the estimated coefficients on the human capital variables tend to be smaller than those for Africa, but more precisely estimated. The coefficient on disability in Spain is larger than that for Africa, although we are not very confident about this result because of the smaller sample size.

Infant Mortality

We also look at the effect of heat waves in utero on infant mortality. In this case, we cannot use data from the IPUMS censuses, since children who have died are no longer in the sample. Instead, we create a very large data set which contains data from the Demographic and Health Surveys (DHS), Malaria Indicator Surveys (MIS) and AIDS Indicator Surveys (AIS) for countries in Sub-Saharan Africa since 1999. These surveys are large and nationally representative for a number of Sub-Saharan Countries. The DHS, MIS and AIS do not contain any information on the temperature, but they so contain the region of residence of each individual. Therefore, we can simply merge the temperature data from the British Atmospheric Data Centre with this data set and determine the temperature at conception for each individual. Using this data, we can assess whether heat at the time of conception is related to infant mortality.[8]

[8] The DHS' goal is to monitor the population and health situations of the target countries. The DHS, MIS and AIDS are part of the MEASURE DHS project which is partially funded by USAID. DHS, AIS and MIS use the same basic questions, making comparisons between and within countries using these surveys straightforward. These data sets contain detailed

However, the data do not contain information about the migration history of households, so we cannot be sure that children living in one given region at the time of the interview were actually conceived and born in that region. To overcome that difficulty, we make the reasonable assumption that all children born in the 12 months preceding the interview were conceived and born in the household region of residence at the time of the interview. We restrict the analysis sample to these children born over the 12 months preceding the interview.

We investigate whether the temperature at conception is correlated with the probability of the child dying before being one year of age. Specifically, the dependent variable is a dichotomous variable for whether the child is dead at the time of the interview. The regression specification is similar to (1), except that we only look at heat in utero instead of after birth. Specifically, we use temperature lags $t-12$ to $t-1$ instead of $t-15$ to $t+15$ as before. We also control for region fixed effect and a series of month birth dummies.

The regression results are given in Table 2. They indicate a negative association between temperature at conception and death. This result suggests that children conceived during heat waves are healthier than other children during their first year of life. For example, an increase in average temperatures exactly nine months before birth reduces the probability a child will die before her first birthday from 5.64% to 5.43%, or a 3.7% decrease in child mortality. Table 2 also indicates that temperature 7 months before birth is negatively associated with death in infancy. In contrast, there is no evidence that temperature from 6 months preceding birth matters.

Less importantly, the regression only includes controls for the temperature before conception, as a falsification test. If one were to find that temperature before conception is correlated with death, then this would indicate that the econometric model may be misspecified. As expected, the results in Table 2 indicate no correlation between temperature before conception and death in infancy.

information on reproductive behaviors of women and health of women and children. A detailed list of the data sets used in this analysis is provided in the appendix of the article.

It is interesting to note that child mortality is affected not only at the month of conception, but from the month before conception to two months after conception. We may find these results for two reasons. First, the timing of conception is actually unknown, since children may be born late or early. We do know that the mean time of conception should nine months prior to birth. As a result, the coefficients surrounding $t-9$ could also be due to heat waves at conception. The second reason is that heat in the first trimester, not just the time of conception, may matter. If this were the case, our results are surprising since we would have expected higher heat in utero to be positively correlated with infant mortality due to a worse disease environment. However, it is important to remember that we are just looking at the effect of heat, not malarial conditions, which are the intersection of heat, rainfall, and other climatic and geographic factors.

3 Channels

In the previous section we established that heat waves at the time of conception matter – children conceived during hotter weather have better educational outcomes and lower rates of infant mortality. In this section, we seek to answer why this happens. First, we establish a fact proposed earlier in the paper: that sexual activity decreases during heat waves. Using this fact, we then provide a mechanism by which heat waves can affect wantedness and the selection of different types of mothers into pregnancy (intended or not). We then test for these mechanisms, as well as look at biological and other channels which may be correlated with temperature and affect outcomes.

Heat Waves and Sexual Activity

A key assumption to our wantedness story is that sexual activity not intended to result in a pregnancy falls during heat waves. While this assumption seems logical to us, we are not aware of any study which has established such a result. Therefore, it is imperative to the paper that we investigate the relationship between heat and sexual activity. There is suggestive evidence that this may be the case, such as Lam and Miron (1996) finding that fertility rates fall during heat waves. Their finding is not sufficient, however, to establish that sexual activity declines during heat waves. For example, it may be the case that it is biologically harder to conceive when it is hot outside.

To test whether heat waves affect sexual activity, we look at two different sources of data. The first is the combined AIS, DHS, and MIS data set described earlier, which contains information on reported sexual activity in the last four weeks. Because the interview is done on any day of the month, the four weeks preceding the interview correspond to the interview month and the last week(s) of the month preceding the month of the interview. When examining the relationship between sexual activity and temperature, we thus quantify the correlation between sexual activity and both the temperature deviations of the month of the interview and the temperature deviations of the month before the interview. Specifically, we regress a dummy for sexual activity on these two temperature variables, region dummies, month dummies and Country*Year dummies.

The results of this regression are given in Table 3. We find that sexual activity decreases in heat waves. For example, our results imply that a one-standard deviation increase in temperatures reduces the likelihood of a woman reporting being sexually active in the last four weeks by 1.57%.

Secondly, we look at the frequency of internet searches in Rwanda for a series of sexually-themed words, and how the search frequency correlates with heat waves. The assumption is that if heat is correlated with sexual desire, we should find that demand for other sexual activities besides intercourse vary with heat, such as looking at pornography online. This method is also informative since while the DHS only asks the women about their sexual activity, there is no information about the sexual activity of men. Insomuch as the preponderance of online searches for pornography are probably made by men, this may be a good proxy for men's sexual desire which may translate into increased frequency of intercourse, and thereby more pregnancies.

Using data on web searches from Google Insight, we regress the frequency of searches for the word "sex" on a series of month dummies and the deviation in temperature from the average in Kigali.[9] The data is reported monthly from Nov. 2006 to Dec. 2011, and not all months are reported. As a result, our sample size is somewhat small – only 55 observations. Our results are reported in Table 4.

In spite of the small sample size and the inclusion of 11 month dummies, we still find a negative and statistically significant relationship between heat and searches for "sex". We also use data on the frequency of searches for a broader the set of sexually-themed words, and again find significant results. Since Google Insight does not report the absolute number of searches, but rather a number normalized between 0 and 100, the interpretation of our reported coefficient is somewhat unclear. In addition, we transform this index to be a daily average rather than a monthly index, complicating interpretating the coefficient even further.[10] In order to understand the coefficient, we report the effect of an increase of one standard deviation in temperature on the percentage change in searches. We find that a one standard deviation increase in temperature correlates with a 6.5% decrease in the frequency of searches for "sex", and an 8.6% decrease in the frequency of searches for the broader set of sexually-themed words.

Wantedness and Heat Waves

Now that we have provided evidence that heat waves affect sexual activity, we turn to wantedness. As previously stated, it is unlikely that planned pregnancies are planned around the incidence of a heat wave. Therefore, if sexual activity varies with heat, it is likely that any reductions in fertility due to increased temperature are due to reductions in unplanned or mistimed pregnancies.

We test this hypothesis using two sets of data. First, we look at reported wantedness and heat waves the combined AIS, DHS, and MIS surveys for all

[9] Google Insight only reports searches from Kigali.

[10] We do this because different months have a different number of days in each month, and therefore the number of searches would vary with the number of days in each month. Although this effect would be presumably taken out with the month fixed effects, we find taking the average number of searches per day to be a more econometricaly sound method.

of sub-Saharan Africa. Specifically, the surveys ask whether the most recent birth was wanted, wanted but mistimed, or unwanted. We regress a dummy for being wanted on a series of region and month fixed effects, and our variable for heat waves. For robustness, we also create dummies for just being mistimed, and the union of mistimed and unwanted, and run similar regressions. The results are reported in Table 6. We find that the probability a woman will report a child as wanted is no different for children conceived in a heat wave compared with children conceived during normal temperatures. However, we are somewhat suspicous of this result since there is likely to be a large amount of misreporting of wanted status in the DHS.

The second method of testing the wantedness hypothesis is by looking at birth spacing. Birth spacing may be smaller for children who are mistimed or unwanted, since they will be conceived before a woman would optimally space her children. We again use the combined AIS, DHS, and MIS surveys for all of sub-Saharan Africa to test this hypothesis. Our results are reported in Table 7. We find no evidence that children conceived during heat waves are spaced longer than children conceived during periods of normal temperatures.

Heat Waves and Parental Characteristics

One potential explanation of our results is related to our story of wantedness, but with a twist. Perhaps women who conceive during heat waves are not more or less likely to want their children, but they just have different characteristics. For example, perhaps the type of woman whose pregnancy timing would be affected by heat waves are, on average, lower quality than those who plan pregnancies independent of the weather. If this were the case, we would expect to see that women who conceived during heat waves would themselves have better outcomes, such as higher educational attainment. We may also expect them to be better off in other ways, such as having better health or earning higher wages.

To test whether mothers who conceive during heat waves have different characteristics than those who don't, we return to the census data. In the census, individuals in a household are linked, making it possible (in some cases) to determine the characteristics of parents. Specifically, we look at whether wealth, literacy rates, years of schooling, or disability rates differ between women who conceive in heat waves vs. during periods of normal temperatures. We also do the same for the fathers.

We regress each parental characteristic on our temperature variable at the time of conception of the child, along with month and region dummies. The results are reported in Tables 12 and 13. We find that educational outcomes of mothers of children who were conceived during heat waves are higher than of children conceived during normal temperatures, both for Africa and Spain. For example, we find that children born during a one-degree increase in average temperatures in Africa have mothers who are 0.146% more likely to be literate and have 0.0276 additional years of schooling. In addition, we find that mother's who conceived during heat waves in Africa are 0.75% more likely to be classified as wealthy. We find similar results for Spain.

Biological Channels

Thus far, we have only considered how heat waves may effect parental conception behavior. However, it may be the case that the effects we found in section 2 have nothing to do with parental behavior, but rather the biology of conceiving during heat waves. There are several biological reasons children conceived in heat waves may have different outcomes than those conceived during times of normal temperature.

First, there is a large literature on the effect on in utero and early childhood conditions and later life outcomes. Inasmuch as temperature is a negative health shock to the fetus or early in life, this may negatively affect outcomes later on. There are several reasons we do not think this is the case. First, from our regressions in section 2, we find no evidence of temperature negatively affecting child quality in utero or postpartum, except for a positive effect at the time of conception, not a negative one. Second, as shown above, temperature around the time of conception and in the first trimester decreases child mortality, not increases it. Finally, there is a growing literature in the medical field which hypothesizes that temperature in utero may be negatively correlated with outcomes via natural selection. The basic idea is that cold weather is an adverse shock to the fetus, and therefore weaker fetuses will be spontaneously aborted, only leaving stronger fetuses surviving. Our findings are the exact opposite, implying that, if anything, the positive effect we find is the net affect of the negative biological channels and the positive behavioral ones.

Second, heat may affect conception by affecting the quality of sperm. It has been well established that sperm quality deteriorates during the summer months as temperatures rise. However, there is no evidence that sperm quality translates into worse outcomes for children, higher rates of miscarriage, or any other aspect of fetal health. It only affects the probability of conception. Inasmuch as heat affects the sperm of all men the same, there should be no reason to think heat waves should affect child quality either directly or indirectly (by affecting the fertility of educated men differently than uneducated men, for example) via differences in sperm quality.

Other Channels

A few additional theories of how heat at conception may affect outcomes are worth mentioning.

First, one may think that heat waves may be correlated with income. For example, heat waves may ffect the agricultural productivity of a region, which in turn could affect income, especially in developing countries. If heat waves at conception are correlated with parental income at conception, then there could be a differential effect on maternal nutrition and health, leading to different in utero conditions for the fetus and thereby affect later life outcomes.

If this were the case, then we would expect heat waves in the months preceding conception to also affect outcomes. Since income can be smoothed, an income shock before conception would affect consumption at the time of conception as well. Since we find no effect of heat waves before conception, we conclude that heat's affect on income is not the main driver of our results. Also, we usually would think of heat waves in Rwanda having a negative affect on incomes, leading to worse nutrition at conception and therefore worse outcomes later in life, the opposite of what we find.

Second, if Lam and Miron (1996) find that heat waves reduce fertility, this implies that cohort sizes of children conceived during heat waves are smaller. These smaller cohorts could lead to higher educational attainment (due to smaller class sizes and higher investments per child, for example), and higher wages in the labor market since labor supply in the cohort is smaller. While we cannot strictly rule out this effect, it seems implausible as the main driver of the effects we find for three reasons. First, the heat waves in this study are monthly deviations, which is too short of a time frame to significantly alter the demographic structure of society. For example, a small monthly cohort conceived during a heat wave would be put into the same school class as eleven other month cohorts, some of which would be born during normal temperatures and some born during below average temperatures. It is hard to believe that monthly temperature deviations would significantly alter the size of an annual school class. This leads us to the second reason we do not believe different cohort sizes drives our results – the magnitudes of the fertility effect from Lam and Miron are much too small to significantly alter cohort size. Finally, labor is highly substitutable between cohorts, meaning we should not expect to find a significant difference in wages simply due to reduced labor supply in one monthly cohort.

4 Conclusion

Using Rwandan Census data and weather data from NOAA, we have shown that heat waves at conception affect later life outcomes, including literacy rates, years of schooling, and infant mortality in Rwanda. In addition, using a large combined data set of African DHS, AIS, and MIS data, we have provided evidence that these effects come through the channel of child wantedness and the behavioral selection of lower quality mothers out of conception during heat waves, rather than purely biological or other channels. We have also shown that sexual activity decreases in heat waves.

We contribute to the literature on the disparities in health and educational outcomes in several ways. This paper is the first paper to our knowledge to assess the impact of heat at the time of conception on later life outcomes. In addition, we are the first paper to look solely at the effect of heat, rather than malarial or drought conditions, in the context of infant mortality. This is true both for heat at conception and heat in utero and in infancy. We are the first paper to look at the effect of just heat in infancy on outcomes. Finally, we are the first paper to show that sexual activity decreases in heat waves. In future versions of the paper, we hope to get better weather data, as well as expand the analysis to 15 other countries across the globe.

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[4] Google insight data

[5] Weather data

[6] DHS and other data

Add more references – finish this later

Appendix

List of the AIS, DHS, and MIS data sets used in the analysis:

- AIS: Cote d'Ivoire/2005; Tanzania/2007-08.

- DHS: Benin/2001; Benin/2006; Burkina Faso/1998-99; Burkina Faso/2003; Burundi/2010; Cameroon/2004; Chad/2004; Congo Democratic Republic/2007; Ethiopia/2005; Ethiopia/2011; Ghana/1998-99; Ghana/2003; Ghana/2008; Guinea/1999; Guinea/2005; Kenya/2003; Kenya/2008-09; Lesotho/2004-05; Lesotho/2009-10; Liberia/2006-07; Madagascar/2003-04; Madagascar/2008-09; Malawi/2000; Malawi/2004-05; Mali/2001; Mali/2006; Mozambique/2003-04; Namibia/2000; Namibia/2006-07; Niger/2006; Nigeria/1999; Nigeria/2003; Nigeria/2008; Rwanda/2000; Rwanda/2005; Rwanda/2007-08 (DHS-Intermediate); Rwanda/2010 (DHS-Special); Senegal/2005; Senegal/2010-11 ; Sierra Leone/2008 ; Swaziland/2006-07; Tanzania/1999; Tanzania/2004-05; Tanzania/2009-10; Uganda/2000-01; Uganda/2006; Zambia/2001-02; Zambia/2007; Zimbabwe/1999; Zimbabwe/2005-06; Zimbabwe/2010-11.

- MIS: Angola/2006-07; Angola/2011; Liberia/2008-09; Liberia/2011; Madagascar/2011; Malawi/2010; Nigeria/2010; Senegal/2006; Senegal/2008-09; Uganda/2009-10.

Table 1: Heat Waves and Literacy -- Rwanda

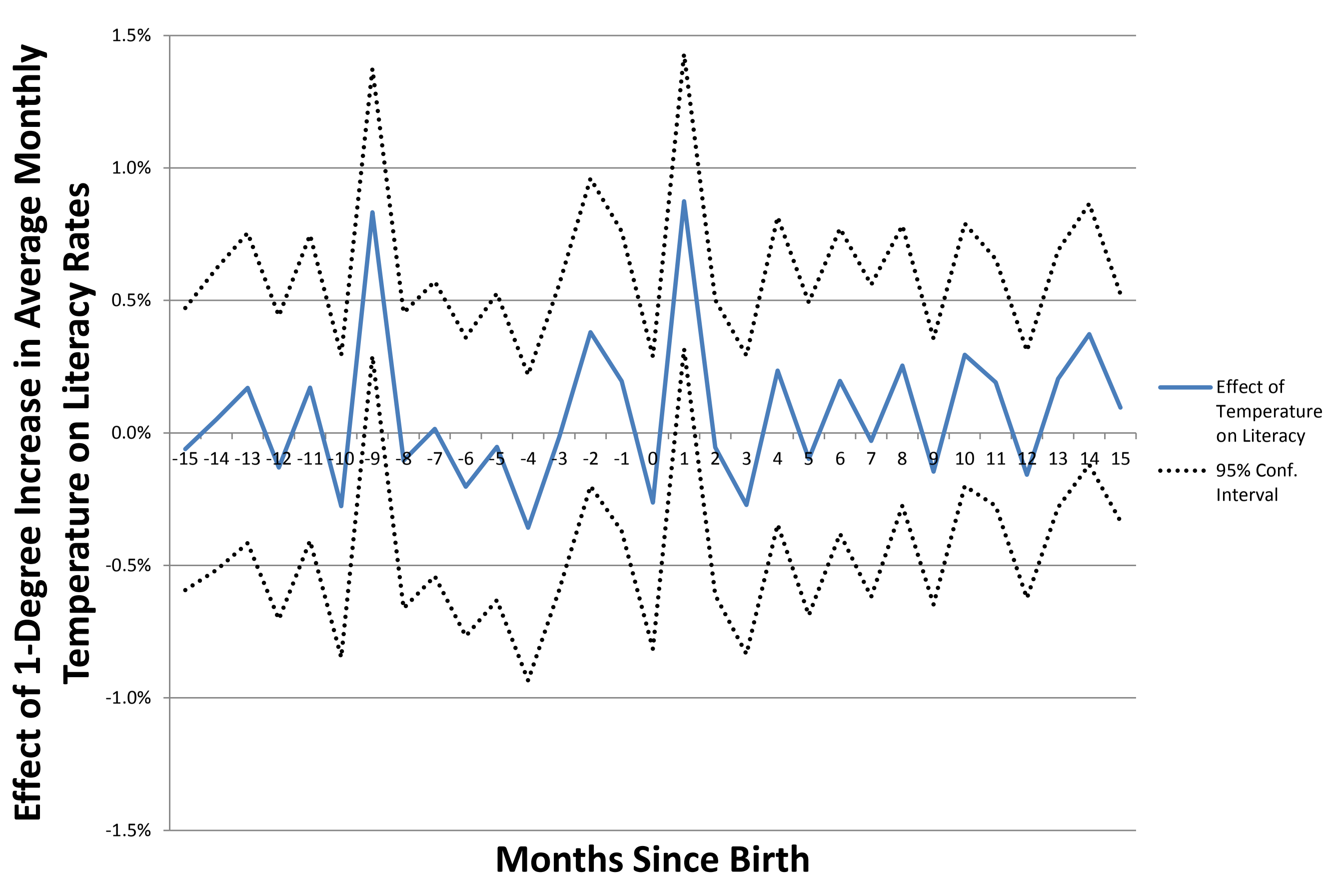
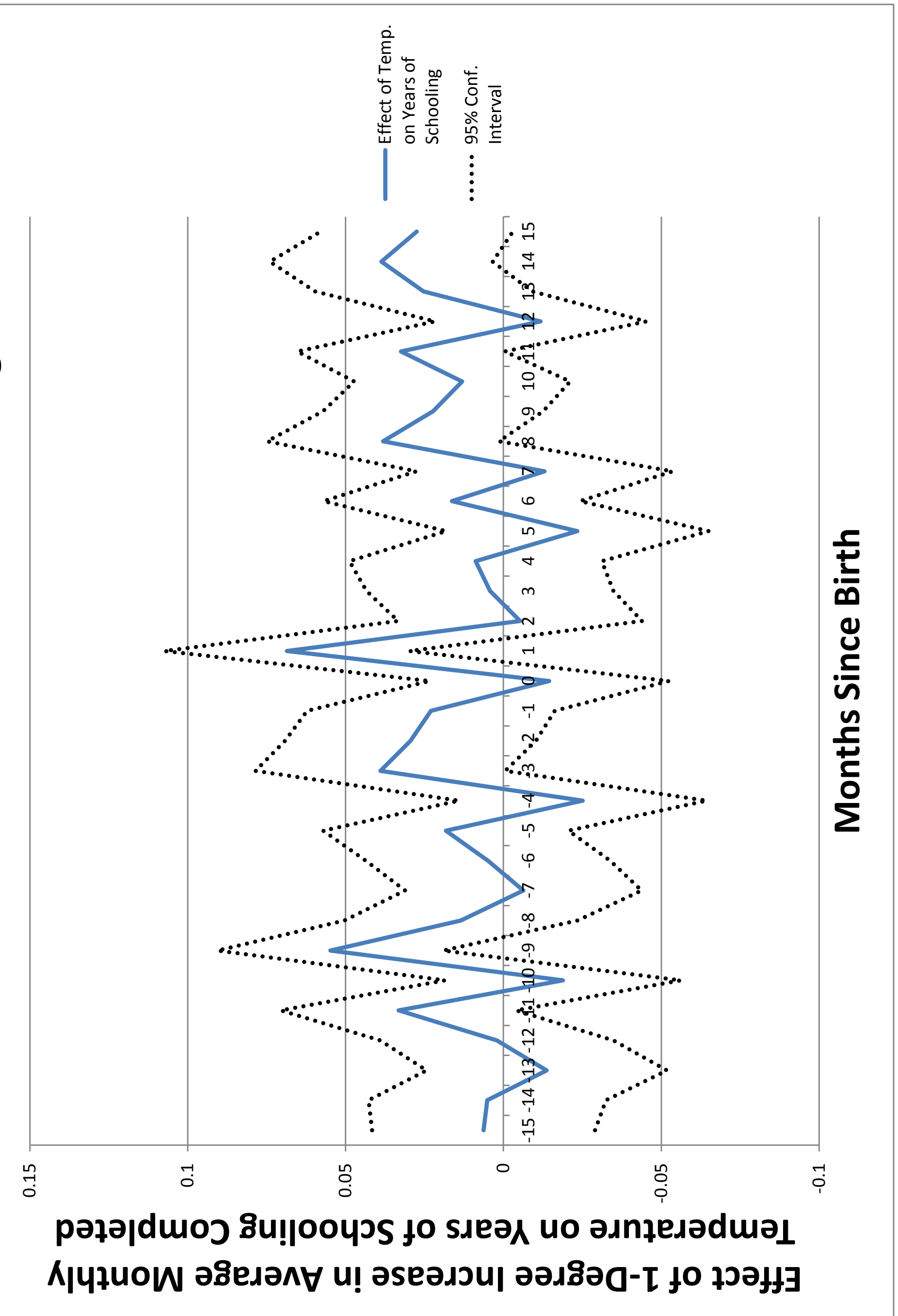


Table 2: Heat Waves and Years of Schooling -- Rwanda



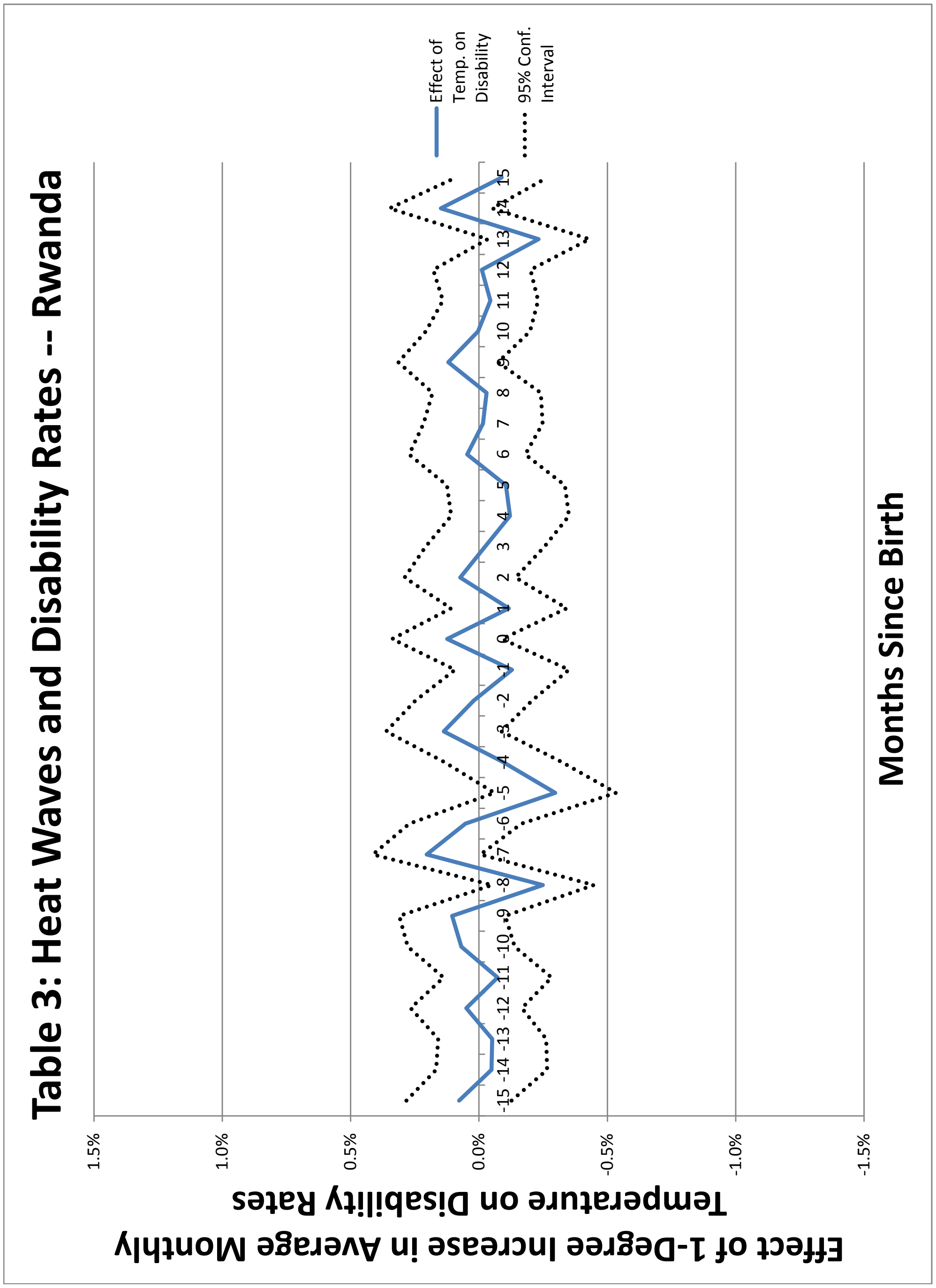


Table 3: Heat Waves and Disability Rates -- Rwanda
Effect of 1-Degree Increase in Average Monthly Temperature on Disability Rates
1.5%
1.0%
0.5%
0.0%
-0.5%
-1.0%
-1.5%
-15 -14 -13 -12 -11 -10 -9 -8 -7 -6 -5 -4 -3 -2 -1 0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15
Months Since Birth
Effect of Temp. on Disability
95% Conf. Interval

Table 1: The Effect of Heat Waves on Outcomes

Months After Birth	Dep. Var.	Literacy	Yrs Schooling	Disabled	Months After Birth	Dep. Var.	Literacy	Yrs Schooling	Disabled
Regression:		(1)	(2)	(3)			(1)	(2)	(3)
-15		-0.0006	0.0063	0.0008	0		-0.0026	-0.0145	0.0012
		(0.0027)	(0.0180)	(0.0010)			(0.0028)	(0.0193)	(0.0011)
-14		0.0005	0.0051	-0.0005	1		0.0087***	0.0686***	-0.0012
		(0.0029)	(0.0192)	(0.0011)			(0.0028)	(0.0198)	(0.0012)
-13		0.0017	-0.0137	-0.0005	2		-0.0006	-0.0053	0.0007
		(0.0030)	(0.0194)	(0.0011)			(0.0028)	(0.0197)	(0.0011)
-12		-0.0013	0.0021	0.0005	3		-0.0027	0.0043	-0.0002
		(0.0029)	(0.0189)	(0.0011)			(0.0029)	(0.0200)	(0.0012)
-11		0.0017	0.0332*	-0.0007	4		0.0023	0.0088	-0.0012
		(0.0029)	(0.0190)	(0.0011)			(0.0030)	(0.0204)	(0.0012)
-10		-0.0028	-0.0188	0.0007	5		-0.0010	-0.0234	-0.0010
		(0.0029)	(0.0190)	(0.0011)			(0.0030)	(0.0212)	(0.0012)
-9		0.0083***	0.0548***	0.0010	6		0.0020	0.0163	0.0005
		(0.0028)	(0.0182)	(0.0011)			(0.0029)	(0.0207)	(0.0012)
-8		-0.0010	0.0135	-0.0025**	7		-0.0003	-0.0130	-0.0002
		(0.0028)	(0.0188)	(0.0010)			(0.0030)	(0.0207)	(0.0012)
-7		0.0002	-0.0063	0.0020*	8		0.0025	0.0382**	-0.0003
		(0.0028)	(0.0191)	(0.0011)			(0.0027)	(0.0187)	(0.0011)
-6		-0.0020	0.0049	0.0005	9		-0.0015	0.0223	0.0012
		(0.0029)	(0.0198)	(0.0011)			(0.0026)	(0.0178)	(0.0010)
-5		-0.0005	0.0182	-0.0030**	10		0.0029	0.0131	0.0000
		(0.0030)	(0.0198)	(0.0012)			(0.0025)	(0.0174)	(0.0010)
-4		-0.0036	-0.0251	-0.0009	11		0.0019	0.0324*	-0.0004
		(0.0029)	(0.0200)	(0.0012)			(0.0024)	(0.0168)	(0.0009)
-3		-0.0002	0.0390*	0.0014	12		-0.0016	-0.0118	-0.0001
		(0.0029)	(0.0202)	(0.0011)			(0.0024)	(0.0171)	(0.0010)
-2		0.0038	0.0295	0.0002	13		0.0020	0.0253	-0.0023**
		(0.0030)	(0.0203)	(0.0012)			(0.0025)	(0.0175)	(0.0010)
-1		0.0019	0.0229	-0.0013	14		0.0037	0.0387**	0.0015
		(0.0029)	(0.0200)	(0.0011)			(0.0025)	(0.0180)	(0.0010)
					15		0.0010	0.0274*	-0.0009
			Continued in next column				(0.0022)	(0.0155)	(0.0009)
							33,713	33,491	33,999

p-val<1%***, <5%**, <10%*. All regressions include region and month fixed effects. A positive coefficient on disability indicates less disability. Heat waves are measured as the deviation in Celsius degrees from the average for each month.

Table 2: Infant Mortality and Temperature

VARIABLES	(1) dead
lag12_detrended_temp_birth	-0.00169
	(0.00119)
lag11_detrended_temp_birth	-0.000546
	(0.00122)
lag10_detrended_temp_birth	**-0.00257****
	(0.00123)
lag9_detrended_temp_birth	**-0.00208***
	(0.00123)
lag8_detrended_temp_birth	**-0.00298****
	(0.00121)
lag7_detrended_temp_birth	**-0.00391*****
	(0.00122)
lag6_detrended_temp_birth	0.00165
	(0.00126)
lag5_detrended_temp_birth	-0.000167
	(0.00128)
lag4_detrended_temp_birth	-0.000633
	(0.00130)
lag3_detrended_temp_birth	0.000846
	(0.00130)
lag2_detrended_temp_birth	9.64e-05
	(0.00128)
lag1_detrended_temp_birth	0.000261
	(0.00130)
detrended_temp_birth (birth)	-0.00198
	(0.00127)
Constant	0.0564***
	(0.00244)
Birth month dummies	Yes
Region dummies	Yes
Observations	106,153
R-squared	0.008

***, **, * represents significance at the 1%, 5%, and 10% level respectively.

Table 3: Heat Waves and Sexual Activity in the AIS, DHS, and MIS Dataset

	Sexually active recently
Temperature the month of the interview	-0.00192
	(0.00167)
Temperature the month before the interview	-0.00702***
	(0.00160)
Constant	0.696***
	(0.00357)
Region dummies	Yes
Month of interview dummies	Yes
Country * Year of interview dummies	Yes
Observations	373,200
R-squared	0.041

Notes: The model is a linear probability model. Standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Internet Search Activity and Heat

Dep. Var.	Words Searched: "Sex"	Combination
Temperature	-0.324**	-0.414**
	(0.157)	(0.203)
1-SD increase	-6.5%	-8.6%
Month Dummies	Yes	Yes
Obs	55	44
R-sqrd	0.367	0.516

** implies significant at the 5% level. 1-SD increase signifies the effect of a one standard deviation increase in temperature on the dependent variable in terms of percentage increase. Combination refers to the average search volume of a set of sexual words.

Table 5: Heat at Conception and Parental Quality

	Literacy		Years of Schooling		Disability	
Dep. Var.	Mother	Father	Mother	Father	Mother	Father
Temp. at conception of	0.00269**	.00461***	.0234***	.0315***	0.00070	0.00143
	(0.00117)	(0.00147)	(0.0071)	(0.0096)	(0.00060)	(0.00092)
Month Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Region Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Obs	140,530	88,912	134,793	85,384	141,955	89,631
R-sqrd	0.058	0.033	0.108	0.124	0.003	0.003

***, **, * implies significant at the 1%, 5%, and 10% level respectively.

Table 6: Heat waves and child wantedness in the AIS/DHS/MIS data

	(1) 1="Wanted then" or "later" Versus 0="Wanted no more"	(2) 1="Wanted then" Versus 0="Wanted later" or "no more"	(3) 1="Wanted then" Versus 0="Wanted later" (excluding "Wanted no more")	(4) 1="Wanted then" Versus 0= "Wanted no more" (excluding "Wanted later")
Temperature at t-12	-0.000111	-0.00107	-0.000760	8.13e-05
	(0.00146)	(0.00236)	(0.00234)	(0.00178)
Temperature at t-9	0.000212	-0.00200	-0.00247	-0.000433
	(0.00142)	(0.00230)	(0.00228)	(0.00174)
Temperature at t-6	-0.00236	-0.00273	-0.000790	-0.00298*
	(0.00145)	(0.00234)	(0.00231)	(0.00176)
Temperature at t-3	-0.000511	-0.00198	-0.00178	-0.000458
	(0.00141)	(0.00229)	(0.00226)	(0.00171)
Temperature at t	0.00188	0.00278	0.00172	0.00256
	(0.00144)	(0.00233)	(0.00229)	(0.00175)
Temperature at t+3	0.000326	-0.000645	-0.00119	0.000331
	(0.00150)	(0.00243)	(0.00240)	(0.00184)
Constant	0.905***	0.707***	0.785***	0.878***
	(0.00536)	(0.00868)	(0.00839)	(0.00630)
Observations	37,041	37,041	33,866	29,231
R-squared	0.100	0.112	0.081	0.144

Table 7: Heat waves and birth spacing in the AIS/DHS/MIS data

	(3) Short birth interval
Temperature at t-12	0.000719
	(0.00148)
Temperature at t-9	-0.00201
	(0.00145)
Temperature at t-6	0.00240*
	(0.00146)
Temperature at t-3	0.000890
	(0.00144)
Temperature at t	0.000740
	(0.00149)
Temperature at t+3	0.00157
	(0.00152)
Constant	0.103***
	(0.00560)
Observations	37,445
R-squared	0.019

Table 8: Heat Waves and Outcomes

Africa

	(1)	(2)	(3)	(4)
VARIABLES	Yrs. Schl.	Ed. Attain.	Literacy	Disable
tempdev_l9	0.0363***	0.0276***	0.00146	0.000313
	(0.00601)	(0.00419)	(0.000950)	(0.000221)
Constant	4.916***	2.713***	0.894***	1.993***
	(0.0511)	(0.0353)	(0.0109)	(0.00113)
Observations	194,438	194,396	138,846	284,685
R-squared	0.458	0.273	0.134	0.008

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

Table 9: Heat Waves and Outcomes:

Spain

	(1)	(2)	(3)	(4)
VARIABLES	Yrs. Schl.	Ed. Attain.	Literacy	Disable
tempdev_l9	0.0645**	0.00965***	0.000361***	0.00365***
	(0.0280)	(0.00246)	(0.000123)	(0.000518)
Constant	1.880***	3.328***	0.986***	1.858***
	(0.271)	(0.0369)	(0.00264)	(0.0189)
Observations	8,640	684,425	684,438	11,292
R-squared	0.386	0.566	0.883	0.177

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

Table 10: Heat waves and Outcomes

Controlling for Mother's Characteristics -- Africa

	(1)	(2)	(3)
VARIABLES	Ed. Attain.	Literacy	Disable
tempdev_l9	**0.0102****	**0.00393*****	**0.000204**
	(0.00444)	**(0.00147)**	**(0.000265)**
Constant	1.577***	0.741***	1.990***
	(0.0438)	(0.0214)	(0.00147)
Observations	95012	49027	175587
R-squared	0.444	0.241	0.010

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

Table 11: Heat waves and Outcomes:

Controlling for Mother's Characteristics -- Spain

VARIABLES	(1) Ed. Attain.	(2) Literacy	(3) Disable
tempdev_l9	**0.000553**	**3.20e-05**	**0.00432*** **
	(0.00238)	**(0.000139)**	**(0.000662)**
Constant	-1.051***	0.834***	1.836***
	(0.0652)	(0.00628)	(0.0199)
Observations	546530	546495	7344
R-squared	0.656	0.894	0.250

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

Table 12: Heat Waves and Mothers' Characteristics

Africa

Dependent Variable	(1) Mother's Wealthy (0 or 1)	(2) Mother's Years of Schooling	(3) Mother's Literacy	(4) Mother's Disability
Survey	DHS	Census	Census	Census
Temperature from t-10 to t-8	**0.00750****	**0.0276*****	**0.00146**	**0.000313**
	(0.00295)	**(0.00419)**	**(0.00095)**	**(0.000221)**
Constant	0.365***	2.713***	0.894***	1.993***
	(0.00770)	(0.0353)	(0.0109)	(0.00113)
Observations	48,388	194,396	138,846	284,685
R-squared	0.242	0.273	0.134	0.008

Table 13: Heat Waves and Mothers' Characteristics

Spain

Dependent Variable	(1) Mother's Years of Schooling	(2) Mother's Literacy	(3) Mother's Disability
Survey	Census	Census	Census
Temperature from t-10 to t-8	**0.00965***** **(0.00246)**	**0.000361***** **(0.000123)**	**0.00365***** **(0.000518)**
Constant	3.328*** (0.0369)	0.986*** (0.00264)	1.858*** (0.0189)
Observations	684,425	684,438	11,292
R-squared	0.566	0.883	0.177

Long Term Consequences of Early Life Health Shocks: Evidence from the 1980s Peruvian Crisis*

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Preliminary version

Abstract

During the Peruvian economic crisis of the late 1980s, infant mortality significantly increased, which evidence a severe health shock for young children. This paper investigates the long term consequences of this shock on health and education for those infants who survived that period. Because no longitudinal data are available, the estimation of causal effects is performed combining a difference-in-differences estimator with a two-sample instrumental variable approach. Results from a pooling of repeated cross sections indicate a higher prevalence of chronic illnesses, and lower levels of education at age 15 among those whose health was more affected by the crisis in their early childhood. Two-sample instrumental variable estimates suggest that the detrimental health conditions associated with an additional percentage point in infant mortality makes children who survived the crisis 2.2 percentage points more likely to suffer a chronic illness and 2 percentage points less likely to attend secondary school at age 15.

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Keywords: crisis, Peru, long-term consequences, early childhood, disease, nutrition

1 Introduction

Gestation and infancy constitute a crucial period in life. The body and the brain grow and develop at an exceptionally fast rate. It is also a period when the health of a person is particularly vulnerable. Insufficient nutrition and diseases tend to increase infant mortality and may impede those who survive childhood from developing their full potential when they reach adulthood. This problem is particularly relevant in developing countries and potentially exacerbated during economic downturns. This paper explores the long term consequences of health shocks experienced in early childhood by those born in the late 1980s in Peru as a consequence of a severe economic crisis.

The medical literature indicates a positive correlation between early childhood health and chronic illnesses later in life (Ericksson et al. [2000], Eriksson et al. [2001], Rich-Edwards et al. [1999], Singhal et al. [2001], Walker et al. [2001], WHO [2003]). However, most of these studies fail to establish a causal connection because they do not control for genetic and family background determinants. It is possible that the same genetic traits that make a person less healthy during childhood make him more likely to suffer a chronic illness later in life. The correlation between health and unobservable family background characteristics is similarly problematic. A child who receives better care during his first year of life, probably because his mother has good knowledge of disease prevention, is more likely to receive better health care during the rest of his life. For these reasons, the simple correlation between child health and chronic illness does little to answer the policy-relevant question of whether improving health during childhood improves health outcomes later in life.

This paper makes use of an exceptionally severe economic downturn to study the long term consequences of early childhood health shocks. As a consequence of heterodox policies and a previous mismanagement of the debt crisis (Glewwe and Hall [1994]), Peru experienced a sharp economic contraction in late 1980's. Between 1987 and 1990, the Peruvian GDP fell 30%, real wages in Lima decreased 80%, and inflation reached four digits. Previous evidence indicates that a consequence of this severe crisis was an increase in infant mortality resulting in 17,000 excess deaths (Paxson and Schady [2005]). Although the exact causes of the increased infant mortality cannot be completely identified due to lack of data, the evidence suggests that the decline in private and public health expenditure played an important role in explaining the facts (Paxson and Schady [2005]).

In this paper, I focus on those who were born during the crisis and survived childhood. The same adverse conditions that increased infant mortality, may have affected the health of survivors, not just temporarily but permanently, and with it the outcomes that depend on health, such as education and employment. Combining information from eight repeated cross sections (ENAHO survey from 2004 to 2011) together with health information during the economic downturn (DHS 1996 and 2000), I exploit the within cohort heterogeneous impact of the crisis and the between differences in cohort exposure to determine if the nutritional deficiencies and diseases during infancy had any consequence on health and education at age 15. The intensity of the health shock depends on the year when the person was born (i.e., during or after the crisis) and the level of education of his mother. After controlling for mother's education and birth cohort effects, I use the interaction between a variable that indicates if the person was born during the crisis and the maximum level of education of his mother as an instrument in a two-sample intrumental variable approach (TSIV) (Angrist and Krueger [1992]); the first stage is computed using the DHS when the person was born, and the second stage is computed using a series of ENAHO surveys 15 years later. This is a novel strategy that combines a difference-in-differences estimator with a two-sample instrumental variables approach. It is an extension of Duflo [2001] who uses a school construction program to estimate returns to education in Indonesia.

Results indicate that those whose health was affected the most by the crisis during their infancy are more likely to report a chronic illness and are less likely to attend secondary school at age 15. If infant mortality is interpreted as a measure of the severity of the health shock (Bozzoli et al. [2009]), the heterogeneous impact of the crisis in relation to mother's education can be used as an instrument to estimate the long-run causal effect of early childhood disease on health, education, and employment at age 15. Results of this strategy indicate that the additional detrimental health conditions during the crisis associated with a 1% increase in infant mortality made survivors of the same cohort 2.2 percentage points more likely to suffer a chronic illness and 2 percentage points less likely to attend secondary school by age 15. There is no difference in the employment rate at age 15 between those born during the crisis and those born afterwards.

This paper contributes to the literature on the long term consequences of fetal and infant health. Behrman and Rosenzweig [2004] identify the returns to birthweight. Using a sample of identical twins from the U.S., they eliminate the influence of genetic endowments and family background characteristics. They find a strong impact of birthweight on school attendance and adult height. Behrman and Rosenzweig [2004] identify the returns to birthweight arguing that the in-the-womb nutritional differences between monozygotic twins is random. In contrast to Behrman and Rosenzweig, I use the heterogeneous impact of the crisis as an exogenous shock to fetal and infant health to identify its long term consequences. In a cross country study, Bozzoli et al. [2009] show that postneonatal mortality correlates with adult height. However, in contrast to my paper, they do not show if posneonatal mortality affects other dimensions of health and outcomes that depend on health.

There are other papers that study how economic downturns affect child health (Paxson and Schady [2005] on infant mortality in Peru, and Bozzoli and Quintana-Domeque [2010] and Cruces et al. [2011] on low birthweight in Argentina). Nonetheless, none of these studies provide clear evidence on the long term consequences of these shocks. An exception is Hidrobo [2011] who studies how the Ecuador crisis hurt the cognitive ability of children. She finds that 5 year-olds exposed to the crisis during their first 3 years of life got significantly lower vocabulary test scores. Nonetheless, she cannot estimate how this reduction in cognitive skills affects education and labor market outcomes.

In section 2, I briefly discuss the relevant stylized facts about the late 1980s Peruvian crisis. In section 3, I present the identification strategy. It is a difference-in-differences estimator embedded in a two-sample instrumental variable approach. In section 4, I describe the different surveys I use, and present descriptive statistics. In section 5, I analyze the heterogenous impact of the Peruvian crisis on infant health. These results constitute the first stage of the TSIV. In section 6, I use pooled cross sections to show whether those whose health was more affected during the crisis were more likely to suffer from a chronic illness and a less likely to complete primary education at age 15. These results correspond to the second stage reduced form of the TSIV. In section 7, I use results from previous sections to compute a two-sample instrumental variable approach. In section 8, I disentangle selection from the scarring effect. In section 9, I perform robustness analysis and discuss results. Finally in section 10, I conclude.

2 The Peruvian crisis

The 1980s were a difficult period for Latin America, and Peru was not an exception. At the beginning of the decade, the *debt crisis* created strong external pressures on the economy. The increase in international interest rates made the already onerous debt service difficult to afford and took a larger portion of public sector revenues. Concurrently, the value of export goods declined, which hurt the balance of payments even more. In response to this situation, in 1985 Peru implemented a series of heterodox stabilization policies. The most notable policy was the suspension of foreign debt payments and the use of these resources to stimulate the economy. As Glewwe and Hall [1994] indicate, the plan was successful in the short run and boosted consumer demand, but unsustainable due to strong inflationary pressures and severe fiscal deficits. In September 1988, the government could not continue on this path and announced a series of new policies that inevitably involved a sharp contraction of government expenditure. This fiscal policy led to a sudden contraction in the economy. Figure 1 shows real expenditures of the

Peruvian central government from 1980 to 2000. After the 1987 peak, government expenditures fell 43% in four years. Figure 2 shows real GDP per capita (PPP) for the same years. The figure shows a dramatic 28% contraction from 1987 to 1990.

The impoverishment of the population during the crisis had a negative impact on the health of children. Figure 3 shows infant mortality from 1984 to 1998. It is clear that the percentage of children dying in their first year of life significantly increased during the economic downturn. In 1990, infant mortality reached its peak at 7.8%. A year later it fell to 4.9%. Paxson and Schady [2005] suggest that a sharp decline in private and public health expenditures could be an important cause of this increase in infant mortality.

Although the economic downturn hit the entire economy, the negative impact on the population was not homogeneous. Generally, the poor and less educated are more vulnerable to economic crises. This is because their ability to maintain consumption levels is constrained. Their incomes are low, sometimes close to subsistence. Consequently, they have little or no savings, lack good access to credit markets, and have limited ability to smooth consumption with assistance from friends and relatives in the face of aggregate economic shocks.[1]

Glewwe and Hall [1994] present evidence that the 1980s Peruvian crisis affected less educated people more severely. The total expenditure of households with heads who completed primary education or less decreased almost 60%, in contrast to a 52% decline among households with more educated heads. Accounting for the fact that less educated people tend to have lower income in normal times, a sharp decline during the crisis makes them more likely to cut on basic needs in comparison to educated people. This may involve food and health services for children. In this paper, the heterogeneity in the way the crisis affected infants is used to identify the long term consequences of the early childhood diseases and malnutrition in a context when

[1] There is a vast literature in Development Economics about the importance of family-and-friends networks to smooth consumption among the poor in developing countries. Theoretically and empirically, it is clear that these networks are particularly relevant to insure idiosyncratic shocks, but not aggregate shocks. See Townsend [1994]

no panel data are available.

3 Identification

In the context of the Peruvian crisis, the severity of nutritional deficiencies in the womb and the exposure to diseases during infancy depend on the date of birth of the child (during or after the crisis) and the level of education of his mother. Children born to low-educated mothers suffered the most during the crisis. Low education is associated with low income and possibly insufficient mechanisms to smooth consumption. As mentioned in the previous section (Glewwe and Hall [1994]), there is evidence that total expenditure fell more in households where the head completed only primary education or less. Also, the decline in public health spending that occurred during the crisis (Paxson and Schady [2005]) more likely affected children from poor and low educated households since the rich could afford private health care. Additionally, highly educated mothers may have a better understanding of disease prevention and take better care of their babies.

Identification relies on the plausible exogeneity of the interaction between the mother's education and the year when the person was born. Table 1 illustrates the idea. Panel A shows infant mortality of children born during the crisis (years 1988-1990) and after the crisis (years 1991-1993). In both cases, infant mortality is computed conditional on the level of education of the mother. Classifying what years correspond to a crisis is arbitrary. In Peru, it seems clear that the crisis began in 1988. Less clear is the exact date when it finished. I define crisis as the years for which infant mortality sharply increased. These years correspond to the period when GDP sharply declined.[2]

Table 1 panel A shows no significant change in infant mortality among children born to high educated mothers. The 0.3% change is small and statistically not different from zero. On the other hand, children from low educated mother died more frequently during the crisis.

[2] Paxson and Schady [2005] p.206 also identify the years 1988-1990 as those when child health was affected.

Among mothers with primary education or less, infant mortality was 1.9% higher for children born during the crisis in relation to those born after the crisis. In the absence of a crisis, the gap in infant mortality between high educated and low educated mothers was 3.2%. If this gap had prevailed during the crisis, the 1.6% corresponding to the double difference, in relation to mother's education and year born, identifies the excess infant mortality caused by the crisis among children born to low educated mothers.

The assumption that the infant mortality gap between low and high educated mothers would have not changed in the absence of the crisis cannot be taken for granted. Secular improvements in health tend to benefit most those who are more exposed to diseases, in this case children from low educated mothers. For this reason, in table 1 panel B, I repeat the exercise performed in panel A, but I compare children born in the years 1991-1993 to children born in the years 1994-1996. Because no child in either group was born during a crisis, the difference in difference should be zero if the assumption that the infant mortality gap between low and high educated mother remains constant in the absence of an economic downturn. The results indicate that the double difference in the absence of a crisis is not statistically different from zero. Following Duflo [2001], I take panel A as the true experiment and panel B as the control experiment.

From table 1, it is clear that the crisis created specially adverse conditions for children born to low educated mothers. If there is any long term effect among survivors, the health gap between children born to low educated mothers in relation to those born to high educated mothers should persist over time. This gap can be estimated by computing the diff-in-diff as in table 1 for the same cohorts but for the prevalence of chronic illnesses at age 15 instead of infant mortality. The diff-in-diff can also be computed for outcomes that are not health indicators but depend on how healthy the person is, such as education and employment. The health gap during the first year of life and the gap in variables of interest 15 years later can be combined to a two-sample instrumental variable approach as follows.

$$y_{ib} = \delta(b) + \gamma_1 h^0_{ib} + \gamma_2 E^m_{ib} + \epsilon_{ib} \tag{1}$$

Equation (1) is the relationship of interest. The coefficient γ_1 measures the impact of early childhood health (h^0_{ib}) on some outcome y_{ib} when person i reaches age 15. This outcome alternates between chronic illness, education, and employment. Equation (1) includes birth cohort fixed effects ($\delta(b)$) that capture any time trend in the variable of interest. It also includes characteristics of the person and her family (E^m_{ib}). To illustrate the methodology, I assume E^m_{ib} contains only one characteristic, the level of education of the mother.

Equation (1) cannot be directly estimated, there is no sample that contains y_{it} and h^0_{it} for the same person. This is because there are no panel data in Peru that follow people from the moment they are born until they become teenagers. Even if a sample had all the variables required to estimate (1), a simple OLS regression would yield biased results because the error term contains genetic traits and unobserved family background characteristics correlated with h^0_{it}. However, γ_1 can be consistently estimated in a two-step procedure. The first step uses a sample that contains information about infant health during and immediately after the crisis to estimate the following equation:

$$h^0_{ib} = \zeta(b) + \alpha_1 E^m_{ib} + \alpha_2 (r_b * E^m_{ib}) + \epsilon^1_{ib} \tag{2}$$

where r_b is a dummy variable that takes the value 1 if the person was born during the crisis and zero otherwise. The coefficient α_2 in (2) is the double difference in table 1 panel A with the caveat that a set of birth year dummy variables is included to better control for cohort fixed effects $\zeta(b)$.[3]

The second step uses a different sample carried out many years later when the cohorts included in (2) reached their teen years. Because of data limitations, equation (1) cannot be

[3]The regression version of table 1 is $h^0_{ib} = \alpha_0 r_b + \alpha_1 E^m_{ib} + \alpha_2(r_b * E^m_{ib}) + \epsilon^1_{ib}$, where E^m_{ib} takes the value 1 if the mother completed primary school or less and zero otherwise.

estimated, but a reduced form substituting (2) in (1) is possible if r_b and E^m_{ib} are observed.

$$y_{ib} = \psi(b) + \beta_1 E^m_{ib} + \beta_2(r_b * E^m_{ib}) + \epsilon^1_{ib} \tag{3}$$

From the second step, $\beta_2 = (\gamma_1 * \alpha_3)$ is identified but not γ_1. Nonetheless, when E^m_{ib} contains only one variable the ratio of $\widehat{\beta_2}$ and $\widehat{\alpha_2}$ yields the TSIV estimator.

$$\widehat{\gamma_1} = \frac{\widehat{\beta_2}}{\widehat{\alpha_2}} \tag{4}$$

The numerator and the denominator in (4) are estimated with different samples. The numerator is estimated with the eight ENAHO rounds and the denominator with the two DHS surveys. This is a particular case of the Angrist and Krueger [1992] estimator when the model is just identified.[4]

4 Data and variable definition

This study uses ten surveys, two rounds of the Demographic and Health Survey (*DHS*), and eight rounds of the Encuesta Nacional de Hogares Actualizada (*ENAHO-Actualizada*). I use the *DHS* to analyze the contemporaneous effect of the crisis on infant health, and the *ENAHO-Actualizada* to study its long term impact by tracking those who were born during the crisis 15 years later.

The *DHS* is a nationally representative survey. It provides basic information for each member in the household, including age, sex, and education. The survey collects rich information about women aged 15 to 49 years old. Each woman is asked when her children were born, if they are still alive and, for those children who died, how old they were when they passed away. I focus on children born between years 1988 and 1996.

Table 2 presents summary statistics after pooling the DHS-1996 and the DHS-2000. In total, the sample contains 47,757 observations. Each one corresponds to a birth between the

[4] Dee and Evans [2003] use a similar a approach. They also compute a TSIV as the ratio of two estimates.

years 1988 and 1996. I calculate statistics separately from the cohorts of interest. Following Paxson and Schady [2005], I compute infant mortality as the fraction of children who died at age 12 months or younger. To eliminate problems of censored data, children born within 23 months of the survey are discarded. To minimize recall bias, children born more than 12 years before the survey are also discarded. Additionally, births to mothers younger than 15 years of age and older than 44 are excluded from the sample. For this reason, each observation in DHS-1996 corresponds to a child born between 1988 and 1994, and each observation in 2000 is a child born between 1989 and 1996. To avoid oversampling the birth years for which the two rounds of the DHS overlap, I divide the sample weights for children born between 1989 and 1994 by two.

Table 2 shows that infant mortality is clearly higher for the crisis years (6.3% for 1988-1990 in contrast to 4.8% and 4.1% for the non-crisis years). On average 54% of the children in the sample has a mother with primary education or less. During the crisis this number is slightly higher at 59%. The age of the mother at the moment of birth is 26.8 years old. During the crisis mothers were half a year younger. There is no difference in the proportion of male babies born during and after the crisis.

Similar to the DHS, the ENAHO-Actualizada is also a nationally representative survey. It is regularly conducted by the National Institute of Statistics of Peru (INEI). Since 2004, approximately 90,000 individuals were interviewed each year. Each new round of the ENAHO-Actualizada is comparable to previous rounds. The sample design is the same each year and the framing of questions used in this study have not changed since 2004. Before 2004, the ENAHO had a different design with a smaller set of questions. This survey is named ENAHO-Anterior. To avoid comparability problems due to methodological changes, the ENAHO-Anterior is not used in this study.

The ENAHO-Actualizada covers a wide range of topics such as education, consumption, dwelling characteristics, employment, and income. The health section is almost entirely focused on the use of health services and not on the health conditions of the people. Nonetheless, one important question about chronic illnesses is included in the survey.

Table 3 presents summary statistics for the relevant variables obtained from the ENAHO survey. The sample consists of people born between 1988 and 1996, the same birth cohorts used in the DHS (table 2), but now when all the members of the cohorts are 15 years old. To compare people of different cohorts at the same age, I pool all the available ENAHO-Actualizada rounds. Since the first round was in 2004 and the latest available was carried out in 2011, 15 years old is the only common age for which all the cohorts are observed.

The first four columns of table 3 present summary statistics for the time-invariant characteristics also observed in the DHS sample (table 2). The education of the mother, the age of the mother at the moment of birth and the sex of the person are very similar in the ENAHO sample and in the DHS sample.

The last four columns of table 3 present summary statistics for the outcomes of interest. They show important differences across cohorts. People born later are more likely to be enrolled in school and have achieved more education. This may correspond to secular improvements in education. A counter intuitive trend is the one observed in chronic illness. Cohorts born later within the sample show higher prevalence of chronic illness at age 15 in relation to cohorts born early when they reach the same age. Since the specific illness is not reported, it is possible that some of them are associated with improvement in standards of living such as obesity, hypertension and type II diabetes.

5 First stage: The crisis and the health of infants

This section shows how the Peruvian crisis had a negative and heterogeneous impact on infant health. This relation is the first step to identify the long term consequences of early childhood health shocks.

There are a variety of channels through which the economic downturn may affect infant health. Some of these channels are expected to have a negative impact and others a positive impact on health. During a crisis, the decline in income may decrease the quantity or quality of food consumed in the household. If this is the case and a child is malnourished, her body's ability to fight diseases decreases, and her growth and development are retarded (dug [2008]).

On the other hand, nutrition may actually improve during a crisis. If the mother spends more time at home, possibly due to unemployment or involuntary reduced working hours, she can breastfeed her infant longer. There is evidence that breastfeeding not only improves child's nutrition but also fortifies the baby's immune system (MRabet et al. [2008]). Moreover, if the economic crisis hurts the nutrition of the mother, her ability to breastfeed is not affected (Frigerio et al. [1991],Spring et al. [year]) Changes in the time allocation of the mother induced by the crisis may also affect the health of children through other channels. When the mother is at home, she can more closely care for the baby.

Another channel through which the economic downturn may impact child health is the increase in the exposure to disease. If an adult in the household gets sick then the child is more likely to get sick even if she is well nourished. The immune system of a newborn is weaker. A disease that only displays has mild symptoms in an adult may have serious consequences for young children (MRabet et al. [2008]). On top of this, if the quality of health services declines, the prevalence of disease may significantly increase. This seems to be the case for Peru. As mentioned before, Paxson and Schady [2005] point out that private and public health expenditures collapsed during the crisis.

5.1 Rationale for the double difference and results on the crisis and infant mortality

The rationale for the first step (equation (2)) of the methodology is the following. The health of child i, born in year b, during his first year of life can be decomposed into two terms: potential health h_i and detrimental elements of health v_{ib}.

$$h_{ib}^0 = h_i - v_{ib} \tag{5}$$

The first term h_i is the healthiness of the child based on her genetic traits at the moment of conception. The second term, v_{ib}, is a compound of elements that prevent the child from reaching his potential health. Bozzoli et al. [2009] consider that v_{ib} is the exogenous disease burden common to everyone born in the same year. Here, v_{ib} is a function of all the elements that affect the health of the child, including but not restricted to the exposure to disease, which transform (5) in a standard health function (Rosenzweig and Schultz [1983], Strauss and Thomas [1998]).

$$v_{ib} = v(C_{ib}(r_b), l_{ib}^m(r_b), B_{it}(r_b), D_b(r_b), E_i^m) \tag{6}$$

More specifically, v_{ib} depends on the quantity and quality of nutrients (C_{ib}), the amount of time the mother spends with the baby (l_{ib}^m), exposure to disease (D_b), availability of health services (B_{ib}), and the level of education of the mother (E_i^m). For the reasons explained above, all these variables except the mother's education may depend on whether the child was born during the crisis or not (r_b). Then, the reduced form of (5) and (6) is:

$$h_{ib}^0 = h_i - v(r_b, E_i^m) \tag{7}$$

Since r_b and E_i^m are both binary variables, the functional form of $v(.)$ is not a concern. Including both variables in levels and their interaction saturates the model. The estimating equation is:

$$h_{it}^0 = \alpha_0 + \alpha_1 r_b + \alpha_2 E_i^m + \alpha_3 (r_b * E_i^m) + h_i \tag{8}$$

Coefficient α_3 is the double difference reported in table 1. Replacing r_b with a series of dummy variables for the year of birth yields equation (2). The health of the child (the dependent variable) is an indicator of whether the child died during her first year of life. The coefficient of interest, α_3, indicates the excess mortality of children born to low educated mothers during the crisis.

Table 4 presents results from estimating (8) together with alternative specifications for the pooled DHS data. Column 1 is the regression version of the two-by-two matrix in table 1, panel A. The gap between children born to low educated and high educated mothers increased 1.6 percentage points as a consequence of the crisis. In columns 2 to 4, I replace the variable 'crisis' with a series of year of birth dummies to better control for cohort effects. In column 3, I include the age of the mother when the child was born and the sex of the child as additional regressors. As expected, there is a U-shape relation between infant mortality and mother's age. The probability that a child dies in his first year of life is relatively high when the mother is a teenager. It reaches its minimum when the mother is in her twenties and increases when she is in her thirties and early forties.

Infant mortality in Peru is one percentage point higher for boys (table 4, column 3). This result is consistent with others in the literature (see Pham et al. [2012] for Vietnam). More importantly, the interaction of male and crisis is not statistically different from zero (column 4), which suggests that during the economic downturn there was not (additional) intrahousehold sex discrimination. Resources in the household were allocated independently of the child's sex. Also in column 4, the interaction of 'crisis' and the age of the mother is not statistically different from zero which suggests that the economic downturn affected mothers of all ages equally.

Table 5 is the control experiment. The sample contains children born to 1991 to 1996. None of these children was born during the crisis. Nonetheless, I label those born in years 1991-1993 as 'placebo' crisis and estimate equation (8). The interaction of mother's education and 'placebo crisis' is statistically equal to zero. The contrasts in the results in tables 4 and 5 suggest that the increase in infant mortality during the crisis was particularly severe for children born to low educated mothers. The gap between these two groups increased by 1.6 percentage points (table 4).

6 Reduced form: health, education, and employment at age 15

6.1 Chronic illnesses

The World Health Organization (WHO [2003]) enumerates different types of associations between fetal and infant health, and chronic conditions that appear later in life. For example, intrauterine growth retardation is associated with increased risk of diabetes, heart disease, and raised blood pressure (Godfrey and Barker [2000], Rich-Edwards et al. [1999]). Insufficient growth during the first year of life is associated with coronary diseases independently of the birth weight (Barker et al. [1989], Eriksson et al. [2001]). Although the evidence suggests that there is a link between fetal and infant health, and chronic diseases, these studies only capture correlation but not causation.

In this section, I study if the Peruvian crisis had any permanent effect on those who were more exposed to it. Since the crisis had a particularly detrimental effect on the health of children born to low educated mothers during the crisis, then the prevalence of chronic illness should be higher for them if health shocks during early childhood had a long-lasting effect on health. Nonetheless, there is the possibility of a "positive" effect of early childhood disease on teenagers' health if the selection effect, and probably also a compensation effect, dominates the scarring effect. The following equation helps illustrate these three effects.

$$h_{it}^{T} = \phi h_{ib}^{0} + (1-\phi)h_i - g_{ib} \tag{9}$$

Equation (9) indicates that the health h_{it}^{T} of teenager i, born in year b, is a weighted average of her health when she was a child h_{ib}^{0} (equation (5)) and her potential health given by her genetic traits h_i. The coefficient ϕ indicates how persistent the health shock in early childhood is. If $\phi = 0$, it indicates that the child fully recovers from illness, i.e., no sequela. $\phi > 0$ indicates that an early childhood health shock affects health permanently. g_{it} is a compound of elements that affect the health of the teenager contemporaneously. It includes variables that the person controls, such as diet, and others the person does not control, such as air pollution. The conditional expectation of (9) yields:

$$E[h_{ib}^T | h_{ib}^0] = \phi h_{ib}^0 + (1-\phi) E[h_i | h_{ib}^0 > z] - E[g_{ib} | h_{ib}^0] \tag{10}$$

The first term in (10) is the scarring effect. It indicates the persistence of early childhood health shocks. The second term is the selection effect. Diseases in the first year of life tend to kill the most unhealthy children (i.e., children whose health was below a threshold z). Consequently, those who reach teenage years tend to have a better health endowment h_i. Finally, the third term is the set of variables that compensate or exacerbate the scarring effect. For chronic illnesses, it usually entails actions to mitigate symptoms (e.g., appropriate diet for diabetes). But, because of the nature of these medical conditions, these actions do not generally cure or cause these illnesses in the short-run. Since the dependent variable is an indicator of whether the person has a chronic condition, the third term in (10) is expected to be zero. Nonetheless, latter in the paper I will study outcomes that are potentially affected by general health conditions. Then, the three components may be important.

To determine if the Peruvian crisis permanently affected the health of those born during that period, I repeat the analysis performed in the previous section but I focus on the prevalence of chronic illnesses when those who survived the crisis reached age 15. he late 1980s economic crisis created particularly adverse conditions for the health of children born to low educated mothers (tables 4 and 5). If there is a permanent effect on health, the same pattern should be observed later in life: a higher prevalence of chronic illnesses for those born to low educated mothers during the crisis. The estimating equation for chronic illnesses is:

$$h_{ib}^T = \psi(b) + \beta_2 E_i^m + \beta_3 (r_b * E_i^m) + \epsilon_{ib}^1 \tag{11}$$

Equation (11) is the reduced form (equation (3) in section 3). The specification is identical to (8), but, the dependent variable in (11) is an indicator of whether person i born in period b

has a chronic illness. The coefficient β_3 measures the excess prevalence of chronic illness that people born to low educated mothers during crisis experienced at age 15. It contains the three components in (10): scarring, selection, and compensation.

I estimate equation (11) by pooling eight repeated cross sections (ENAHO 2004-2011). The reason to use multiple surveys is to compare different cohorts at the same age. I estimate (11) including only teenagers that are 15 years old at the moment of the survey. Given the rounds of the ENAHO I have (years 2004-2011), this is the only age for which I observe all the cohorts needed to estimate (8). The sample used to estimate the impact of the crisis on infant mortality, tables 4 and 5, consists of people born to 1988 to 1996. To make a valid comparison, I include the same cohorts here to estimate the impact on chronic illnesses at age 15.

Table 6 shows the results from the estimation of equation (11). The specifications are identical and the sample represents the same cohorts as those in table 4. The interaction of the mother education and the indicator of whether the person was born during the crisis suggests that the economic downturn increased the prevalence of chronic illnesses at age 15 by 3.4 percentage points among teens born to low educated mothers in relation to those born to high educated mothers during the crisis. The age of the mother when the child was born seems not to be an important determinant of chronic illness (columns 3 and 4), but, the sex of the children seems to be important. Boys have a 2.6 percentage point lower probability of suffering a chronic illness. Since boys are more likely to die in their first year of life (tables 4 and 5), this negative number is consistent with a strong selection effect. But, we have to be cautious with this interpretation. The crisis had no differential effect on boys (table 4 column 4), so, this hypothesis cannot be tested.

Table 7 is the control experiment. The effect of the interaction of the 'pseudo crisis' variable and mother's education on chronic health is much lower in magnitude and not statistically different from zero.

6.2 Education and employment

The results in tables 6 and 7 suggest that health shocks during the first year of life had a long-lasting effect that is evidenced in the manifestation of chronic disease. It is also important to analyze if the diminished health affected the ability of the person to generate income. It is not obvious that chronic illnesses should impair the normal functioning of a person. Medications and healthy habits may reduce or eliminate the symptoms that could affect work. In other words, the compensation effect in (10) may offset the scarring effect. On the other hand, some health issues that may reduce the work capacities of the person are not associated with chronic illnesses such as the normal development of the brain. The medical literature suggests that malnutrition during infancy may harm cognitive ability for life (MK [year]). During the first three years of life, the brain of a well nourished child grows 300% and reaches 80% of its final weight (AS. [1978]). If during this period the brain does not develop normally, the possibilities of making up later are limited.

Since the child's progression through formal education depends on the health of the student and on her cognitive ability, the Peruvian crisis may have hurt the education of children that were exposed the most to disease during that period. To analyze this possibility, I estimate equation (11) but replace the dependent variables with educational outcomes.

Table 8, columns 1 to 4, shows the results for school enrolment. A child born during the crisis has a lower relative probability of school enrolment at age 15 of around 3.7 percentage points if the mother was low educated. Nonetheless, this result is only statistically different from zero in column 4. Columns 5 to 8 show another dimension of education. The dependent variable takes the value one if the maximum level of education is primary school or less. At age 15 a person should be in secondary school. So, if the child school progression is good, the dependent variable takes the value zero. The interaction of mother's education and 'crisis' indicates that teen born to low educated mother during the 1980s crisis are 3% more likely to have completed only primary school or less at age 15 that other teens.

The last four columns of table 8 show results for the same econometric specifications but with employment as the dependent variable. The interaction of 'mother's education' and 'crisis' suggests that the probability of working at 15 was not affected among those who were born during the late 1980s economic downturn.

Finally, for comparison reasons, table 9 is analogous to table 8 for the control experiment. In this case, the interaction of 'mother's education' and 'placebo crisis' is not statistically different from zero in any of the regressions.

7 Long term consequences of early childhood disease: two-sample instrumental variable approach

The question of how much early childhood malnutrition and diseases affect long term outcomes can be answered by combining the results from sections 5 and 6. If infant mortality is taken as a measure of the prevalence of disease and malnutrition for a group of people in early life, its impact on chronic illness, education, and employment at age 15 is measured using the interaction of mother education and whether the child was born during the crisis as an instrument in a two-sample instrumental variable approach.

The ratio of β_3 from (11) and α_3 from (8) identifies the causal effect of an increase in the disease and nutritional burden associated with an additional 1% infant mortality on the prevalence of chronic illnesses, education, and employment of survivors at age 15. Table 10 presents the TSIV results. Standard errors are computed using the Delta method. The first column uses the sample containing only 15-years-olds. This is the same sample I use to compute tables 6 and 8. The results suggest that the malnutrition and disease burden that increased infant mortality by 1% during the crisis generated a 2.2 percentage point higher prevalence of chronic illness among survivors 15 years later. Nonetheless, this result is not statistically different from zero at conventional levels. To improve the power of the test, I use a larger sample that contains teenagers from 15 to 18 years old. The second stage reduced form results used to compute the third column in table 10 are in table 11. The TSIV results from the 15 to 18-year-olds are similar to those using the sample containing only 15-year-olds. They indicate that the crisis increased the prevalence of chronic illness by 2.36 percentage points. With more precision obtained from a larger sample, this estimate is now statistically different from zero at a 5 percent level.

The other results using the lager sample (ages 15 to 18) are similar to those from the smaller sample (age 15 only). Exposer to nutritional deficiencies and diseases in early childhood that increase infant mortality by 1% increases the probability of having low levels of education (primary education or less) by 2 percentage points. Results from the first and third columns of table 10 agree that there is no impact on employment.

8 Disentangling scarring from selection effect

9 Some robustness

10 Conclusions

The economic history of Latin America is characterized by severe macroeconomic crises, but little is known about their long term consequences on human capital. This paper investigates the possibility that sharp contractions in economic activity hurt the health of children not just temporarily, but permanently. In spite of not having panel data, I incorporate a diff-in-diff estimator in a two-sample instrumental variable approach TSIV to combine information from two rounds of the Demographic and Health Survey (DHS 1996, 2000) and eight rounds of the Encuesta Nacional de Hogares (ENAHO 2004-2011).

My results indicate that children more exposed to diseases and nutritional deficiencies during the late 1980s Peruvian crisis are more likely to suffer a chronic health condition and less likely to achieve high levels of formal education 15 years later. The TSIV results suggest that an increase in adverse health conditions during the crisis associated with each additional 1 percentage points in infant mortality exacerbates on average the prevalence of chronic illnesses by 2.2 percentage points among survivors 15 years afterwards, and reduces by 2 percentage points the probability of completing more than primary education at age 15.

The findings have important implications for policy. They suggest that the implementation of safety nets by governments can improve the well-being of the poor during macroeconomic shocks, but also they may significantly improve the health of new generations. Another policy implication in on how to allocate resources not only during economic downturns. Investing in the health of children creates a more skilled labor force that is potentially more productive.

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11 Figures

Figure 1:

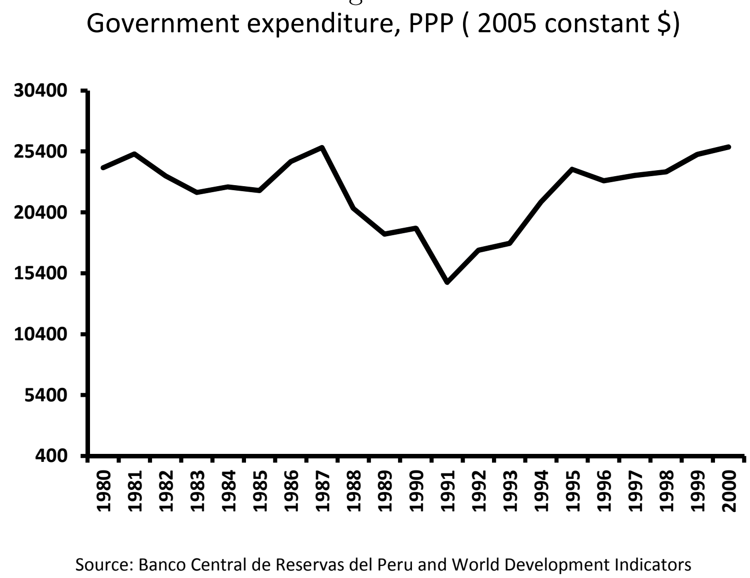


Figure 2:

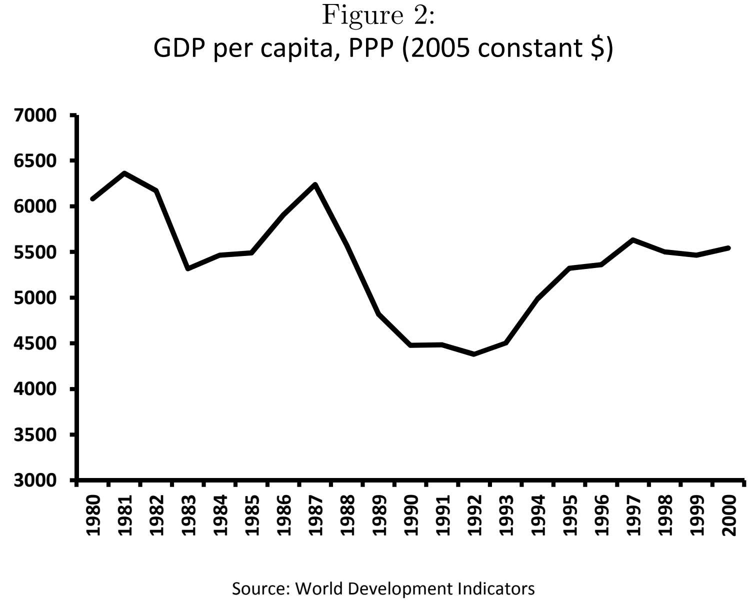
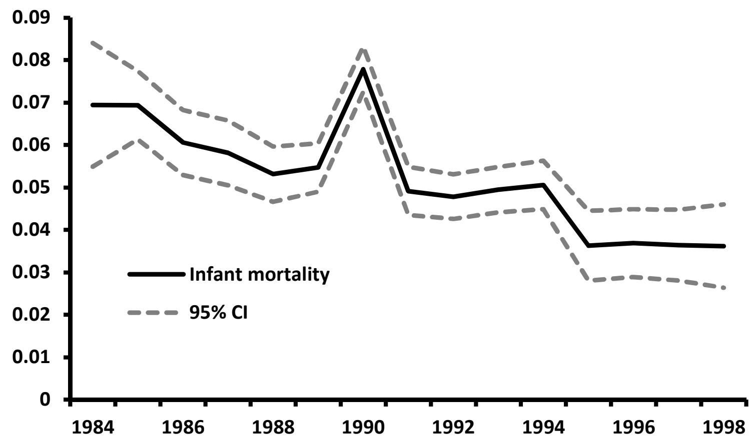


Figure 3:
Infant mortality



Source: DHS-1996 and DHS-2000

Table 1:
Infant mortality by year of birth and education of the mother

Panel A: Experiment

	mother's education		
	low	high	difference
born in years 1988-1990 (crisis)	0.082	0.035	0.048
	(0.003)	*(0.002)*	*(0.004)*
born in years 1991-1993 (post-crisis)	0.063	0.031	0.032
	(0.002)	*(0.002)*	*(0.003)*
difference	0.019	0.003	0.016
	(0.003)	*(0.003)*	*(0.005)*

Panel B: Control

	mother's education		
	low	high	difference
born in years 1991-1993	0.063	0.031	0.032
	(0.002)	*(0.002)*	*(0.003)*
born in years 1994-1996	0.054	0.027	0.027
	(0.003)	*(0.002)*	*(0.004)*
difference	0.009	0.004	0.005
	(0.003)	*(0.003)*	*(0.005)*

s.e. in parenthesis

Table 2:
Summary statistics: pooled data DHS-1996 and DHS-2000

year born	obs	Infant mortality	mother primary school or less	mother age	male
Panel A: experiment					
1991-1993	19,643	0.048	0.544	26.747	0.508
		(0.21)	*(0.50)*	*(6.43)*	*(0.50)*
1988-1990	16,414	0.063	0.590	26.306	0.506
		(0.24)	*(0.49)*	*(6.20)*	*(0.50)*
Panel B: control					
1994-1996	11,700	0.041	0.531	26.807	0.515
		(0.20)	*(0.50)*	*(6.57)*	*(0.50)*
1991-1993	19,643	0.048	0.544	26.747	0.508
		(0.21)	*(0.50)*	*(6.43)*	*(0.50)*

standard deviations in parenthesis

Table 3:
Summary statistics: pooled data (ENAHO 2004-2011)

year born	obs	mother primary school or less	mother age	male	chronic illness	school enrollment	primary school or less	employed
Panel A: experiment								
1991-1993	5,482	0.527	27.260	0.506	0.127	0.777	0.154	0.485
		(0.50)	*(6.58)*	*(0.50)*	*(0.33)*	*(0.42)*	*(0.36)*	*(0.49)*
1988-1990	4,168	0.572	27.273	0.502	0.096	0.663	0.181	0.491
		(0.49)	*(6.65)*	*(0.50)*	*(0.29)*	*(0.47)*	*(0.39)*	*(0.49)*
Panel B: control								
1994-1996	4,832	0.498	27.633	0.512	0.167	0.817	0.130	0.505
		(0.50)	*(6.73)*	*(0.50)*	*(0.37)*	*(0.39)*	*(0.34)*	*(0.50)*
1991-1993	5,482	0.527	27.260	0.506	0.127	0.777	0.154	0.485
		(0.50)	*(6.58)*	*(0.50)*	*(0.33)*	*(0.42)*	*(0.36)*	*(0.49)*

standard deviations in parenthesis

Table 4:
Impact of Peruvian crisis on infant mortality

EXPERIMENT
Sample: children born in years 1988-1993
Dep. Variable: child died in his/her first year of life

VARIABLES				
mother educ.	0.032***	0.032***	0.031***	0.032***
	(0.0035)	*(0.0035)*	*(0.0036)*	*(0.0036)*
mother educ. * crisis	0.016***	0.016***	0.016***	0.014**
	(0.0057)	*(0.0057)*	*(0.0057)*	*(0.0058)*
mother age			-0.0055***	-0.0055**
			(0.0020)	*(0.0023)*
mother age sqr			0.00010***	0.000094**
			(0.000036)	*(0.000041)*
male			0.011***	0.014***
			(0.0029)	*(0.0036)*
mother age * crisis				-0.00057
				(0.0041)
mother age sqr * crisis				0.000028
				(0.000075)
male * crisis				-0.0068
				(0.0059)
crisis	0.0035			
	(0.0038)			
Constant	0.031***	0.027***	0.092***	0.095**
	(0.0023)	*(0.0048)*	*(0.027)*	*(0.044)*
year of birth fixed effects	NO	YES	YES	YES
Observations	36,057	36,057	36,057	36,057
R-squared	0.009	0.010	0.011	0.011

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

Table 5:
Control experiment: impact of Peruvian crisis on infant mortality

CONTROL
Sample: children born in years 1991-1996
Dep. Variable: child died in his/her first year of life

VARIABLES				
mother educ.	0.027***	0.026***	0.025***	0.025***
	(0.0044)	*(0.0044)*	*(0.0045)*	*(0.0045)*
mother educ. * p-crisis	0.0053	0.0057	0.0060	0.0068
	(0.0056)	*(0.0056)*	*(0.0056)*	*(0.0058)*
mother age			-0.0066***	-0.0076**
			(0.0020)	*(0.0032)*
mother age sqr			0.00011***	0.00014**
			(0.000036)	*(0.000058)*
male			0.0097***	0.0046
			(0.0028)	*(0.0044)*
mother age * p-crisis				0.0021
				(0.0039)
mother age sqr * p-crisis				-0.000042
				(0.000071)
male * p-crisis				0.0097*
				(0.0057)
scrisis	0.0040			
	(0.0038)			
Constant	0.027***	0.031***	0.11***	0.10***
	(0.0031)	*(0.0037)*	*(0.026)*	*(0.032)*
year of birth fixed effect	NO	YES	YES	YES
Observations	31,343	31,343	31,343	31,343
R-squared	0.005	0.006	0.007	0.007

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

Table 6:
Impact of Peruvian crisis on chronic illness (15 years afterwards)

EXPERIMENT
Sample: children born in years 1988-1993
Dep. Variable: chronic illness

VARIABLES	(1)	(2)	(3)	(4)
mother educ.	-0.092***	-0.092***	-0.094***	-0.094***
	(0.012)	(0.012)	(0.013)	(0.013)
mother educ. * crisis	0.034*	0.034*	0.032*	0.032
	(0.019)	(0.019)	(0.019)	(0.020)
mother age			0.0031	0.0095
			(0.0053)	(0.0071)
mother age sqr			-0.000026	-0.00013
			(0.000092)	(0.00012)
male			-0.026***	-0.022*
			(0.0090)	(0.012)
mother age * crisis				-0.015
				(0.011)
mother age sqr * crisis				0.00024
				(0.00019)
male * crisis				-0.0092
				(0.018)
crisis	-0.0013			
	(0.027)			
Constant	0.10***	0.11***	0.064	0.18*
	(0.026)	(0.017)	(0.075)	(0.11)
year of birth fixed effects	NO	YES	YES	YES
year of survey fixed effects	YES	YES	YES	YES
Observations	9,641	9,641	9,641	9,641
R-squared	0.021	0.021	0.024	0.024

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

Table 7:
Control experiment: Impact of Peruvian crisis on chronic illness (15 years afterwards)

CONTROL
Sample: children born in years 1991-1996
Dep. Variable: chronic illness

VARIABLES	(1)	(2)	(3)	(4)
mother educ.	-0.11***	-0.11***	-0.11***	-0.11***
	(0.015)	(0.015)	(0.015)	(0.015)
mother educ. * p-crisis	0.019	0.019	0.019	0.021
	(0.019)	(0.019)	(0.019)	(0.020)
mother age			0.0085	0.0077
			(0.0054)	(0.0084)
mother age sqr			-0.00011	-0.000084
			(0.000095)	(0.00015)
male			-0.032***	-0.044***
			(0.0094)	(0.015)
mother age * p-crisis				0.0018
				(0.011)
mother age sqr * p-crisis				-0.000049
				(0.00019)
male * p-crisis				0.021
				(0.019)
scrisis	0.010			
	(0.025)			
Constant	0.22***	0.18***	0.047	0.092
	(0.015)	(0.018)	(0.076)	(0.11)
year of birth fixed effects	NO	YES	YES	YES
year of survey fixed effects	YES	YES	YES	YES
Observations	10,292	10,292	10,292	10,292
R-squared	0.026	0.027	0.031	0.031

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

Table 8:

Impact of the Peruvian crisis on education and employment (15 years afterwards)

EXPERIMENT
Sample: children born in years 1988-1993
Dep. Variable: chronic illness

VARIABLES	School enrolment				Max educ.Primary school or less				Employed			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
mother educ.	-0.13***	-0.13***	-0.13***	-0.13***	0.21***	0.21***	0.21***	0.21***	0.34***	0.34***	0.34***	0.34***
	(0.014)	(0.013)	(0.013)	(0.013)	(0.011)	(0.011)	(0.011)	(0.011)	(0.016)	(0.016)	(0.016)	(0.016)
mother educ. * crisis	-0.037	-0.036	-0.036	-0.038*	0.031*	0.030*	0.030*	0.028	-0.0046	-0.0048	-0.0016	0.0019
	(0.023)	(0.023)	(0.023)	(0.023)	(0.017)	(0.017)	(0.017)	(0.017)	(0.025)	(0.025)	(0.025)	(0.026)
mother age			0.014**	0.0075			-0.0073	-0.0052			-0.016**	-0.030***
			(0.0066)	(0.0084)			(0.0053)	(0.0065)			(0.0074)	(0.0097)
mother age sqr			-0.00022*	-0.00011			0.00012	0.000079			0.00024*	0.00048***
			(0.00012)	(0.00015)			(0.000093)	(0.00011)			(0.00013)	(0.00017)
male			0.0061	0.0042			-0.0021	-0.0026			0.058***	0.055***
			(0.011)	(0.013)			(0.0087)	(0.011)			(0.012)	(0.016)
mother age * crisis				0.016				-0.0046				0.030**
				(0.014)				(0.011)				(0.015)
mother age sqr * crisis				-0.00025				0.000098				-0.00053**
				(0.00024)				(0.00019)				(0.00026)
male * crisis				0.0044				0.0013				0.0078
				(0.023)				(0.018)				(0.025)
crisis	-0.25***				-0.043**				0.0088			
	(0.029)				(0.022)				(0.032)			
Constant	1.04***	0.63***	0.41***	0.27*	0.082***	0.025*	0.13*	0.16	0.31***	0.32***	0.55***	0.32**
	(0.030)	(0.025)	(0.095)	(0.15)	(0.024)	(0.015)	(0.074)	(0.12)	(0.034)	(0.025)	(0.11)	(0.16)
year of birth FE	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES
year of survey FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	9,640	9,640	9,640	9,640	9,640	9,640	9,640	9,640	9,636	9,636	9,636	9,636
R-squared	0.049	0.110	0.111	0.112	0.096	0.099	0.099	0.099	0.111	0.111	0.116	0.116

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

Table 9:

Control experiment: Impact of the Peruvian crisis on education and employment (15 years afterwards)

CONTROL
Sample: children born in years 1991-1996
Dep. Variable: chronic illness

VARIABLES	School enrolment				Max educ.Primary school or less				Employed			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
mother educ.	-0.11***	-0.11***	-0.11***	-0.11***	0.19***	0.19***	0.19***	0.19***	0.37***	0.37***	0.37***	0.38***
	(0.014)	(0.013)	(0.013)	(0.013)	(0.011)	(0.011)	(0.011)	(0.011)	(0.017)	(0.017)	(0.017)	(0.017)
mother educ. * p-crisis	-0.018	-0.020	-0.020	-0.020	0.019	0.019	0.020	0.021	-0.036	-0.035	-0.035	-0.039
	(0.019)	(0.019)	(0.019)	(0.019)	(0.015)	(0.015)	(0.015)	(0.015)	(0.023)	(0.023)	(0.023)	(0.024)
mother age			0.0028	-0.0025			-0.0074	-0.0097			-0.024***	-0.019*
			(0.0057)	(0.0077)			(0.0048)	(0.0070)			(0.0070)	(0.010)
mother age sqr			-0.000037	0.000049			0.00012	0.00016			0.00037***	0.00027
			(0.000099)	(0.00013)			(0.000085)	(0.00013)			(0.00012)	(0.00017)
male			0.0037	0.0032			0.0057	0.015			0.046***	0.036**
			(0.0095)	(0.013)			(0.0076)	(0.011)			(0.012)	(0.017)
mother age * p-crisis				0.010				0.0044				-0.010
				(0.011)				(0.0095)				(0.014)
mother age sqr * p-crisi				-0.00016				-0.000085				0.00020
				(0.00020)				(0.00017)				(0.00024)
male * p-crisis				0.0010				-0.017				0.019
				(0.019)				(0.015)				(0.023)
scrisis	-0.16***				-0.040**				0.018			
	(0.023)				(0.016)				(0.031)			
Constant	0.85***	0.93***	0.88***	0.021	0.042***	0.067***	0.17**	-0.0030	0.32***	0.29***	0.64***	0.76***
	(0.013)	(0.015)	(0.081)	(0.13)	(0.0084)	(0.016)	(0.068)	(0.096)	(0.017)	(0.022)	(0.099)	(0.15)
year of birth FE	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES
year of survey FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	10,292	10,292	10,292	10,292	10,292	10,292	10,292	10,292	10,291	10,291	10,291	10,291
R-squared	0.034	0.088	0.088	0.088	0.088	0.090	0.090	0.091	0.125	0.126	0.131	0.131

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

Table 10:
Impact of early childhood health on selected variables: TSIV

	age 15		age 15 -18	
	coef.	s.e.	coef.	s.e.
chronic illness	2.19	1.45	2.36	1.06
school enrolment	-2.31	1.67	-0.12	0.81
primary school or less	1.93	1.30	2.04	0.88
employed	-0.31	1.62	-0.20	0.81

Table 11:

Impact of the Peruvian crisis on selected outcomes (15 years afterwards)

Sample: children born in years 1988-1993 at ages 15 to 18

VARIABLES	chronic illness				School enrolment				Max educ.Primary school or less				Employed			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
mother educ.	-0.10***	-0.10***	-0.10***	-0.10***	-0.094***	-0.095***	-0.096***	-0.096***	0.16***	0.16***	0.16***	0.16***	0.33***	0.33***	0.33***	0.33***
	(0.0069)	(0.0069)	(0.0070)	(0.0070)	(0.0085)	(0.0084)	(0.0085)	(0.0085)	(0.0049)	(0.0049)	(0.0050)	(0.0050)	(0.0087)	(0.0087)	(0.0087)	(0.0087)
mother educ. * crisis	0.037***	0.037***	0.036***	0.037***	-0.0014	-0.0034	-0.0038	-0.0050	0.032***	0.032***	0.032***	0.032***	-0.0035	-0.0035	-0.0016	-0.00014
	(0.0097)	(0.0097)	(0.0097)	(0.0099)	(0.013)	(0.013)	(0.013)	(0.013)	(0.0073)	(0.0073)	(0.0073)	(0.0074)	(0.013)	(0.013)	(0.013)	(0.013)
mother age			0.0011	0.0078*			0.0033	0.0027			-0.0017	-0.0031			-0.011***	-0.016***
			(0.0029)	(0.0042)			(0.0037)	(0.0050)			(0.0024)	(0.0033)			(0.0038)	(0.0052)
mother age sqr			6.4e-06	-0.000096			-0.000040	-0.000035			0.000035	0.000055			0.00014**	0.00022**
			(0.000050)	(0.000073)			(0.000064)	(0.000087)			(0.000042)	(0.000057)			(0.000065)	(0.000091)
male			-0.048***	-0.051***			-0.015**	-0.0074			0.0032	0.0028			0.073***	0.067***
			(0.0048)	(0.0068)			(0.0062)	(0.0084)			(0.0038)	(0.0050)			(0.0063)	(0.0086)
mother age * crisis				-0.014**				0.0013				0.0029				0.0096
				(0.0057)				(0.0074)				(0.0048)				(0.0075)
mother age sqr * crisis				0.00021**				-7.3e-06				-0.000042				-0.00017
				(0.000100)				(0.00013)				(0.000085)				(0.00013)
male * crisis				0.0054				-0.015				0.00097				0.012
				(0.0095)				(0.012)				(0.0076)				(0.013)
crisis	-0.013				-0.19***				-0.039***				0.016			
	(0.011)				(0.014)				(0.0071)				(0.014)			
Constant	0.080*	-0.031	-0.058	0.047	1.99***	-0.78***	-0.84***	-0.85***	0.28***	0.016	0.034	0.011	-0.11*	0.047	0.23*	0.16
	(0.045)	(0.081)	(0.091)	(0.098)	(0.061)	(0.11)	(0.12)	(0.13)	(0.036)	(0.065)	(0.073)	(0.081)	(0.060)	(0.11)	(0.12)	(0.13)
year of birth FE	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES
year of survey FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	37,671	37,671	37,671	37,671	37,668	37,668	37,668	37,668	37,668	37,668	37,668	37,668	37,653	37,653	37,653	37,653
R-squared	0.023	0.023	0.029	0.030	0.086	0.125	0.125	0.125	0.081	0.082	0.082	0.082	0.111	0.111	0.118	0.118

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

The Persistent Effects of in Utero Nutrition Shocks over the Life Cycle: Evidence from Ramadan Fasting in Indonesia

By Muhammad Farhan Majid

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ABSTRACT

Every year Muslims worldwide fast during the Islamic month of Ramadan. In 2010 alone, more than 1.2 billion Muslims globally, and 155 million Muslims in Indonesia, were potentially exposed to their mother's fasting. This paper uses longitudinal data (the Indonesian Family Life Survey, IFLS) to study the effects of in utero exposure to Ramadan on multiple outcomes, including adult labor supply, over the life cycle. The empirical analysis finds that: i) exposed adults aged 15-65 work 4.5% fewer hours and are 3.2% more likely to be self-employed; ii) exposed children aged 7-15 score 5.9% lower on Raven's Colored Progressive Matrices assessment and 7.8% lower on math test scores, have increased probability of engaging in child labor, and study fewer hours during elementary school; and iii) exposed children younger than 5 have lower birth weights, which may partially account for the former two effects. Estimates are robust to the inclusion of biological sibling fixed effects. Moreover, by exploiting novel religiosity data from the latest wave of the IFLS, these results are found to be the strongest in religious Muslim families, while insignificant for non-Muslims.

Keywords: Early Childhood Environment, Health and Economic Development, Religion, Ramadan, Pregnancy, Nutrition, Indonesia, Labor Supply and Labor Productivity

JEL Codes: I1, I12, I15, J1, J13, J22, J24, Z1, Z12

"The most valuable of all capital is that invested in human beings; and of that capital the most precious part is the result of the care and influence of the mother"- Marshall (1890, paragraph VI.IV.11)

1 Introduction and Background

A growing literature in economics documents that inequalities in health, education and income emerge early in life (Doyle et al., 2009). These studies are primarily motivated by the fetal origins hypothesis (Barker, 1995) which states that shocks during pregnancy may have long-term impacts on health and socioeconomic outcomes (Almond, 2006). The intrauterine environment, and nutrition in particular, may impact not only the metabolism of the fetus, which can lead to future adult health concerns such as obesity, type 2 diabetes and cardiovascular disease, but also the fetus's cognitive functioning (Almond and Currie, 2011). However, most studies provide evidence from either short-term outcomes (for e.g., birth weights) or long-term outcomes (for e.g., type 2 diabetes for the old) for certain segments of the life course and from different contexts. It is not clear if the contexts where long-term effects are found also register short-term effects. Moreover, not much is known about the effects on labor supply outcomes (Thomas, 2009). This is the first paper to utilize Ramadan, the Islamic month of diurnal fasting, as a natural experiment for identification of in utero nutrition shocks not only on adult labor supply outcomes, but on multiple outcomes over the life course using the same longitudinal dataset (Indonesian Family Life survey).

To identify the effects of in utero shocks on long-term health and productivity indicators, economists have recently utilized extreme events, such as the 1944 Dutch Famine and the 1918 Spanish Influenza, as natural experiments (Almond (2006); Chen and Zhou (2007))[1] Although these events provide natural experiments for the identification of causal effects, it is not clear whether the results from these studies can be generalized to other settings that are more susceptible to intervention through public policy (Almond, Mazumder and Ewijk, 2011) [2] In particular, little attention has been paid to the effects of (less severe) norms during pregnancy, some of which have been practiced for centuries and which may be expected to persist in the future.[3]

Moreover, scarce attention has been paid to behavioral adaptations from in utero health shocks. Current evidence, which is rather limited, suggests that short-term changes in productivity may have negligible immediate impact on the allocation of time, but that productivity changes over the long-term may lead to

[1] Recently the findings related to the 1918 Spanish Influenza have been challenged by Brown and Thomas (2011) who argue that those exposed to the Influenza had lower socio-economic status (SES) than families not exposed, leading to significant reduction in the size and statistical significance of the earlier effects.

[2] Referred to as AME (2011) from now on.

[3] Currie and Vogl (2012) also express concern regarding mortality selection in all these papers. A particularly attractive feature of studying effects of (less severe) norms is that mortality selection may be less of a concern than in natural experiments exploiting extreme events such as famines.

reduced hours worked (Thomas, 2009). However, it is not clear whether health and productivity have a causal effect on hours worked in more general settings (Thomas, 2009). Studies that ignore labor supply effects provide an incomplete picture of the effects of health on labor market outcomes, leading to an understatement of the welfare losses associated with negative health shocks. These losses in turn may not be reflected in aggregate measures of economic growth, and may overstate the importance given to the association between health and wage income.[4]

This paper fills this gap in the literature by analyzing the effects of the norm of maternal fasting by Muslim pregnant women (during the Islamic holy month of Ramadan) on their children's labor market outcomes, as measured by hours worked as well as by the sector in which the children choose to work when they become adults. In addition, I pay particular attention to indicators of productivity (test scores), investment in schooling inputs (child labor status and hours of study), and birth outcomes (birth weight), as suggestive channels that determine the adult labor market outcomes. Current evidence from Muslim majority countries suggests that 70%-90% of pregnant women fasting during some part of their pregnancy. [5] And medical theory predicts that fasting can have an "acceleration starvation" type effect on the fetus, which may have long-term effects on health and cognition.

Within the economics literature, Almond and Mazumder (2011) are the first to systematically consider the effects of fasting during pregnancy by Muslim women on their children's outcomes. They find lower birth weights and lower sex ratios in the US, and evidence of learning disabilities in Uganda and Iraq, in addition to negative effects on certain wealth measures. Using Indonesian Family Life Survey data (Wave 3), Ewijk (2011) finds that those exposed to Ramadan have worse general health, lower sex ratios, and symptoms of coronary heart problems and type 2 diabetes in old age. And contemporaneously, in a working paper, AME (2011) use English registry data on Pakistani and Bangladeshi students to estimate lower math and reading test scores for children of age seven.

The common identifying assumption in the economics literature is that the timing of pregnancy is exogenous with respect to the timing of Ramadan. Compliance to treatment, i.e., the extent of fasting during pregnancy in Ramadan is unknown. However, since fasting during Ramadan is a Muslim ritual, it is reasonable to assume that compliers to treatment, i.e., pregnant mothers who fast during Ramadan, would be limited to Muslims only. Instead of estimating average treatment effects (ATE), the current literature

[4] Long-term changes in health may limit the capacity to work, which in turn would lead to lower total earnings. I am implicitly assuming that the welfare losses from the wealth effect will dominate any welfare gains from 'forced leisure', which seems to be a reasonable assumption in the case of Indonesia, a developing country where poverty is widespread. Thomas (2009) also implicitly assumes the same.

[5] There is empirical evidence that many pregnant Muslims fast for at least a few days during Ramadan. For example, a study in Singapore of 181 Muslim women found that more than 70% percent fasted at least a day during pregnancy (Joosoph et al., 2004)). In a study conducted in Sanaa City, Yemen, more than 90% percent fasted over 20 days. In general, estimates of compliance to fasting among pregnant women vary between 70%-90% (Makki, 2002). See Almond and Mazumder (2011) for a more detailed survey.

estimates intent to treatment (ITT) effects by comparing children of those mothers for whom at least a day of Ramadan coincided with at least a part of their pregnancy. In this sense, the current estimates can be understood to be lower bounds.[6]

This paper adds value to the current literature in several ways. This study analyzes the effect of Ramadan observance during pregnancy on children's adult labor market behavior (hours worked and sector of work). As mentioned earlier, this adds value to not just the economics of fasting literature, but to the larger literature on health and labor market outcomes. Second, the paper identifies indicators of cognitive ability (test scores) and behavioral changes related to investment in schooling inputs (study hours and child labor) as suggestive channels determining labor market outcomes. Even more fundamentally, it identifies changes in birth weight as a deeper channel through which these effects may be taking place. In my knowledge, this is the first paper which is able to track the effects an in utero shock at several stages of the life cycle using the same data set.

Third, in contrast to Almond and Mazumder (2011) and AME (2011), this paper uses data from Indonesia, a developing country with the largest Muslim population (and a significant non-Muslim minority of 12%). One may expect compliance to fasting to be lower in developed countries since individuals have better health facilities and are generally more educated than their counterparts in developing countries. This may imply that the bias in ITT estimates is somewhat less in developing countries than developed countries. The better SES in developed countries may even lead to higher compensatory investments by society so that the true fasting effect may be confounded. Different fertility trends may also exist. Ewijk (2011) also examines Indonesia but that study's focus is exclusively on health measures and is a cross-sectional study using IFLS Wave 3 (carried out in 2000). In contrast, this paper uses the latest Wave 4 (carried out in 2007-2008). Wave 4 is unique, in particular, because it contains new data on religiosity not available in previous rounds.[7] Moreover, this study uses Wave 1 (1993) to identify biological siblings in Wave 4 and to identify the effects on schooling inputs for a sub-sample of adults in Wave 4 when they were children.

Fourth, in contrast to Almond and Mazumder (2011) and AME (2011), this paper uses biological sibling fixed effects model to assess the effects on children's behavioral outcomes and test scores. This controls for not only any unobservables, which may be potentially driving any selective timing of pregnancy, but also controls for the bias associated with any lack of compliance to Ramadan, as long as compliance is time invariant within families. In this sense, this paper's estimates can be thought of as ATE rather than just ITT estimates. This insight alone has been ignored by the literature until now.

In the absence of panel data, it is usually not possible to carry out biological sibling fixed effects for adults

[6] Ewijk (2011) also utilizes biological sibling fixed effects for analyzing the general health of children up to age 18.
[7] The sub-sample is a sub-sample in terms of age cohort but not necessarily exactly the same sub-sample.

who may no longer live in the same households. By utilizing the panel feature of the IFLS, for a sub-sample of adults, this study is also able to carry out sibling fixed effects for adults aged 22 to 28 years to assess effects on their labor supply. This may be of interest not just to economics of fasting literature but to the broader literature of development studies. However, a clean biological sibling fixed-effects analysis cannot be carried out for the entire adult population of interest (15-65) because of the lack of longitudinal data over the entire life course. Household fixed effects are estimated for this purpose. A landmark paper (Almond, 2006) used the 1918 Spanish influenza pandemic in the United States as a source of exogenous variation to show that infections during pregnancy can worsen long-term outcomes of the fetus. However, Brown and Thomas (2011) have recently argued that those exposed to the Influenza had lower socio-economic status (SES) than families not exposed, leading to significant reduction in the size and statistical significance of the earlier effects documented in Almond (2006). This highlights the need for conducting household fixed effects.. If the main results of this study are robust to household fixed effects, this will provide further confidence in the estimates of this paper.

Fifth, in addition to using non-Muslims as a placebo for falsification, as had been used earlier, I am able to make use of unique questions on religiosity, found in Wave 4 of the IFLS. If the effects of Ramadan are indeed due to the act of religious fasting, one may expect that more religious Muslim families are more likely to have pregnant women who fast than less religious Muslim families. This may make one more confident that the effects are driven by religiosity rather than by other differences across Muslims and non-Muslims.

In addition, by using a continuous measure of exposure (proportion of days of overlap of pregnancy with Ramadan), I am able to carry out non-parametric estimates of exposure on labor market behavior. Moreover, this study uses exact date of birth information, along with information on one's religion, in all its estimates. This makes it potentially less prone to measurement error. Almond and Mazumder (2011) do not know the exact date of birth for their adult sample. For their estimates of birth weight effects, religion is unknown. AME (2011) also do not know the exact religion of the children. Ewijk (2011) is the only other study which uses exact date of birth information, along with data on religion, in all its estimates.

The results show that exposure to Ramadan fasting in utero has a wealth effect measured by 4.5% fewer hours worked, as well as a selection effect which involves a 3.2% increase in the probability of being self-employed. This conclusion is robust to not only household fixed effects, but also to biological sibling fixed effects for a sub-sample of adults. When falsification tests are done on non-Muslims, no such effects are found on the placebo. This gives further confidence that the estimates are not driven by any other behavioral and economic changes that may take place during Ramadan. For example, if changes in the general price level of a basket of goods, shared by Muslims and non-Muslims, were causing such effects, then non-Muslims should register similar effects. Hence, the effects are peculiar to Muslims during Ramadan. Moreover, if religious fasting is driving the Ramadan effect, then we may expect that individuals from more religious Muslim families should register stronger effects. Results support this prediction.

Suggestive channels through which these effects may be taking place are next explored. Mother's fasting lowers not just the Raven's CPM cognitive test scores by 5.9% but also lowers math scores by 7.8% for children aged 7-15. Moreover, these estimates are robust to biological sibling fixed effects.This suggests that mother's fasting lowers the stock of human capital of the children. Next, deeper channels are examined through which the changes in test scores may be taking place. Children are 3.3% more likely to be involved in child labor and study 3.4% fewer hours during elementary school.[8] Thus, behavioral changes related to schooling inputs may be one possible channel through which the tests score effects are taking place, apart from the direct effects on one's cognitive ability from fasting. In fact, as the theoretical framework in the paper clarifies, the behavioral response may it self be a response to the lower returns to schooling for the exposed children. Finally, if effects are being driven by the in utero nutrition shock and not due to some other post-natal shock per se, we may be interested in finding evidence on birth outcomes as well. Although the sample sizes are small and birth weights could be subject to possible measurement errors, results show that those exposed do register lower birth weights by as much as 270 grams.

When non-parametric analysis (without controls) is carried out, results yield qualitatively similar insights as the parametric estimates. The estimates also show that the major impact of fasting seems to occur between six and eighteen days of exposure to fasting. In the first six days, the marginal effects of fasting seem to be strongest, and after eighteen days of exposure, the marginal effects seem to flatten out. This is a useful finding and, if generally true, can help to identify the critical periods when Ramadan exposure during pregnancy is potentially most damaging to the fetus.

The rest of the paper is organized as follows. Section 2 performs a brief literature review from the epidemiology and economics literatures on maternal fasting and its effects. Section 3 presents a conceptual framework to interpret the empirical evidence presented in this paper. Section 4 discusses the data used to carry out the analyses. Section 5 presents the empirical methodology. Section 6 presents the results. Section 7 discusses the results. Section 8 discusses the policy implications. Section 9 concludes.

[8] The ordinary least squares (OLS) estimate for the overall sample is statistically insignificant but, the fixed effect estimate for a sub-sample is significant at the 10% level.

2 Literature Review

2.1 Epidemiological Theory and Evidence

Fasting during pregnancy is expected to have negative effects because excess demand for nutrition by the fetus, if unmet, impedes fetal growth, leading to permanent effects on the body. There are two main hypotheses concerning the effects of fetal health on long-term outcomes. These can be viewed under the umbrella of the fetal origins hypothesis (FOH). The first is described as fetal under-nutrition. According to this view, inadequate prenatal nutrition leads to developmental adaptations that are beneficial for short-term survival but affect the general growth of the fetus (e.g., lower birth weight). This effect takes place despite a short period of nutritional deficiency (Barker, 1997).

Often, such damage does not create problems immediately, but only later in life, as shocks sustained during the life course take their toll. This can lead to effects on the kidneys and, increased risk for type 2 diabetes. Type 2 diabetes, in turn, is a key risk factor in the development of coronary heart disease. In fact, low birth weight is itself understood to predict coronary heart disease in adult life. Almond and Mazumder (2011) provide evidence that Ramadan causes lower birth weights in the US. However, lower birth weight captures only part of the changes to the fetal body due to maternal undernutrition (Almond and Mazumder, 2011).

A second prominent hypothesis is that nutritional restrictions hamper the development of a placental enzyme that is required to convert cortisol into inactive cortisone, thereby exposing the fetus to excessive amounts of cortisol (Almond and Mazumder, 2011). It is believed that in utero exposure to glucocorticoids such as cortisol leads to a reprogramming of the hypothalamic pituitary adrenal axis (HPA), which is linked with not only type 2 diabetes, high blood pressure and also cognitive impairment (Seckl et al. (2007), Kapoor et al. (2006)).

Within epidemiology, Metzger et al. (1982) were one of the very first to document the high level of ketones, free fatty acids and low glucose levels in pregnant women compared to non-pregnant women after 12 hours of nighttime fasting. Two years later, Meis, Rose and Swain (1984) showed that daytime fasting for eight hours leads to symptoms that are as severe as those reported in Metzger et al. (1982). Both studies emphasized the necessity for pregnant women to eat during the daytime. Thereafter, several studies have shown that 'accelerated starvation' caused by fasting during pregnancy is correlated with the malfunctioning of certain cognitive functions (Rizzo et al., (1991)).

A sizable literature in epidemiology studies the impact of Ramadan fasting, in particular (see Almond and Mazumder (2011) for a more detailed summary of this literature). Recently, Dikensoy et al. (2009) reported that Ramadan fasting is associated with increases in cortisol levels during pregnancy. This finding is of interest because cortisol is a stress hormone understood to potentially 'program' health in adulthood (Kapoor et al., 2006). Many studies give evidence that pregnant women in Ramadan do indeed reach low levels of blood glucose and high levels of ketones. Arab (2004) found that 31% of pregnant women in Iran had ketonuria, whereas 61% had hypoglycemia before breaking their fast. In the UK and West Africa, Prentice et al. (1983) and Malhotra et al. (1989) measured unambiguous signs of accelerated starvation in Ramadan among pregnant women who were fasting.

Several studies of maternal fasting during Ramadan have found adverse effects on fetal health indicators. Mirghani et al. (2004) found evidence of reduced fetal breathing, where measures of fetal breathing were taken both before and after fasting on the same day. DiPietro et al. (2007) found a strong association between variation in fetal heart rate in utero and mental and psychomotor development and language ability during early childhood. The above are only few of the many studies. Most evidence points towards strong first-stage effects of exposure to Ramadan fasting among pregnant women and its effect on the health and nutrition of the mother (and the fetus).

Existing studies of the effects of fasting on birth outcomes have relied on comparisons between mothers who reported fasting with those who did not. One of most commonly cited study on the effects of Ramadan on birth weight, conducted a retrospective analysis of 13,351 babies born at full term from 1964-1984 in Birmingham, England (Cross et al.,1990). Cross et al. (1990) found a higher frequency of low birth weight among fasters during the second trimester of pregnancy, although there were no significant effects on mean birth weight. Malhotra et al. (1989) and Mirghani and Hamud (2006) found no effects on birth weight and APGAR (Appearance, Pulse, Grimace, Activity, and Respiration) scores, even though they detected substantial biochemical changes. In the same study, Mirghani and Hamud (2006) find that there is a higher incidence of gestational diabetes mellitus (GDM), induced labor, higher cesarean section rates as well as higher admission to the special care baby unit (SCBU) among the fasting group versus the control group.

Azizi et al. (2004) is the only well-known study in epidemiology that studies the long-term impact of fasting on human capital outcomes. They find no significant effect of maternal fasting behavior, during the third trimester of pregnancy, on the intelligence quotients (IQs) of school-age children.

There are a number of problems inherent in most of these empirical studies in epidemiology. These include small sample sizes, estimation of effects in a given trimester instead of a comprehensive study of the entire pregnancy period. More seriously, most of these studies have attempted to evaluate the average treatment effects of Ramadan by comparing outcomes for those who actually fasted and those who did not, under the assumption that the decision to fast is exogenous. Although some of these studies control for variables like mother's pre-pregnancy body mass index (BMI), the list is not exhaustive. For example, a number of these studies do not control for smoking behavior, father's education, diversity in ethnic backgrounds or varying levels of community health facilities available to different mothers, which may lead to different fasting behaviors on the part of fasting mothers. In fact, few of these studies are experimental/quasi-experimental, relying on simple OLS regressions with limited controls.

2.2 Evidence From Economics

Within economics, Almond and Mazumder (2011) are the first to systematically consider the effects of fasting during pregnancy by Muslim women on their children's long-term outcomes. Using data from Michigan, they first show that the health of newborns is negatively affected by in utero exposure to Ramadan. Using Ugandan data, they next look at long-term effects of exposure on the probabilities of having disabilities as an adult. They find that Muslims who were conceived during Ramadan had higher probabilities of having vision, hearing and mental or learning disabilities as adults. They also find an effect on the sex ratio (a lower share of males) which reflects adverse pre-birth environment.

Ewijk (2011) uses Indonesian Family Life Survey data (Wave 3) to study long-term effects of Ramadan on health measures. The paper shows that people who were exposed to Ramadan fasting during their mother's pregnancy have a poorer general health and are sick more often than people who were not exposed. This effect is especially pronounced among older people, who when exposed also report health problems more often that are indicative of coronary heart problems and type 2 diabetes. The exposed are smaller in body size and weigh less. In addition, the sex ratio is also lower, corroborating the findings of Almond and Mazumder (2011).

Contemporaneously, AME (2011) find that children of age seven exposed to Ramadan have lower math and reading test scores. English registry data is used for the study and, Pakistani and Bangladeshi ancestry is used as a proxy for the Muslim religion.

It is important to highlight the common methodology of these three papers. Instead of estimating ATE, they estimate ITT effects by comparing children of those mothers for whom at least some part of Ramadan coincided with some part of their pregnancy. The identification assumption is that the timing of the pregnancy is exogenous with respect to the timing of Ramadan. Who actually fasted is unknown. All we know is that non-Muslim mothers cannot be in the pool of the potential treatment group and that the actual group of mothers who fasted will be among the Muslim population. In this sense their estimates can be understood to be lower bounds.[9]

However, their identifying assumption is questionable. There are a host of social factors that not only determine whether a given Muslim pregnant women may fast, but also how her family (community) tries to

[9] AME (2011) utilize a differences-in-differences strategy between potentially Muslim children and non-Muslims to isolate any seasonal variations that may be biasing these estimates, which involve just ten cohorts. The differences-in-differences approach, however, yields similar results to the OLS approach, as no effects are found on non-Muslims.

remedy for any subsequent negative effects on the exposed child(ren) via intra-household (intra-communal) reallocations of resources. For example, mothers from educated families may attempt to selectively time their births to avoid any overlap between pregnancy and Ramadan. Or, more health clinics may be devoted to areas where there is a greater concentration of Muslim women fasting because in such areas there is greater incidence of low child birth. By including household fixed effects, such household level factors can be controlled for.

Moreover, it is not clear if the Ramadan effects are indeed driven solely by religious fasting (as medial theory predicts) or by some other factor not directly related to religiosity. For example, prices of basic food items may hike during Ramadan. Changes in eating behavior after sunset(*iftaar*), which involves eating greasy, oily and generally unhealthy foods, may be causing the real harm rather than calorie restriction during fasting. Sleeping patterns may also change. People may also work less during Ramadan due to fatigue. All these factors may confound the Ramadan effect from the fasting effect. However, if I compare religious and less religious Muslims and find effects mostly on religious groups, it its very likely that the Ramadan effects are due to some factor linked with religiosity amongst Muslims. This will make the assumption that the Ramadan effects are being driven by fasting much more tenable than what one can assume from earlier studies.

The next section presents a conceptual framework to understand the empirical evidence presented in this paper.

3 Economic Theory

This section presents a conceptual framework to understand the reduced form empirical estimates, that will be shown in the later sections of this paper. The framework incorporates aspects of the standard static health-over-life course approach, as summarized in Strauss and Thomas (2007), with static aspects of the technology of skill formation, as exemplified in Heckman (2007) in a Roy economy (as in Pitt et al. (forthcoming), Rosensweig and Zhang (2012) and Vogl (2012)). I show that an early health shock can lead to not just a wealth effect (from changes in the labor supply, for example) but that there is also a selection effect (as people with lower skills sort into less skill-intensive sectors). These changes are made possible because the early life shock affects production of skills. The production of human capital, in turn, is potentially affected by not just changes in endowments of cognitive ability because of the early life shock, but by behavioral responses to the early life shock during childhood.

Parents are assumed to make key health decisions for children, whereas an adult is assumed to make his or her own decisions. It is important to distinguish skill outcomes, such as general health and test scores, from health inputs such as birth weight, and health behaviors such as hours of schooling and incidence of child labor.

Assume there is a static skill production function for an individual:

$$S = S(N, S_o, A, B_S, D, \mu, \epsilon_S), \tag{1}$$

where S represents measured skill outcomes, such as test scores, in my case (and general health as in Ewijk (2011)). These depend on health behaviors, N, which are choices under the control of the individual making the choices. These include, for example, time allocated into production of schooling. The technology of skill formation may possibly evolve over the life cycle, varying by age and, with other social and demographic characteristics, A, such as sex. The technology is also likely to be a function of family background, which affects health, B_S, such as parental religiosity. The production technology may also depend upon environmental and communal factors, D, such as the disease environment, whether there are health clinics in the community and the average religiosity in the community. Finally, μ is assumed to be negative, and represents the in utero health insult , while ϵ_S represents unobserved factors (error term). It is assumed that the partial derivatives of S, with respect to inputs N, S_o, A, B_S, D, μ, are all positive.

Behavioral choices play a major role in my conceptual framework. Assume that an individual's welfare is increasing in the personal consumption of purchased commodities, C (or in the parent's consumption, if they are the decision makers) and decreasing in the labor supply, L_j in sector j. j is ordered such that the higher the j, the more skill-intensive the sector, so that, *ceteris paribus*, more skill-intensive sectors are preferred. Moreover, Utility, U, is assumed to be increasing in skill outputs, S, as well as in observed characteristics, A, family background, B_U, and unobserved characteristics, ϵ_U :

$$U = U(C, L_j, S, A, B_U, \epsilon_U). \tag{2}$$

Choices are constrained by budget constraints, time constraints, labor supply and sectoral choice functions, in addition to the technology of skill formation (1). Suppose that the individual earns wage, w, for each unit of labor supplied in sector j and that asset or non-labor income is V. The budget constraint is:

$$P_c C^* + P_n N^C = w_j L_j + V. \tag{3}$$

As in Strauss and Thomas (2007), consumption, C, is divided into two parts: consumption that is not related to the formation of skills, C*, with prices P_c, and purchased inputs for human capital production, N^C, with prices P_n. Time constraint is given by :

$$N^T + L + E = T, \tag{4}$$

where T, the total time endowment, can be used for the production of skills, N^T, leisure, E, and labor supply, L. The choices of labor supply and sector of work, will be affected by an in utero shock through the shock's effect on skill formation. But the in utero shock may also have effects through other unmeasured routes, which is captured by μ. All other unobservables are captured by the error term ϵ_L, in the case of labor supply, and ϵ_j, in the case of sector choice. Note that j is ordered such that higher skills are associated with a higher j, so that those with lower skills work in a lower sector (lower values of j) compared to those working in a higher sector (more skill-intensive sector). It is assumed that the partial derivatives of labor supply and sector choice, with respect to inputs $S, A, B_., D, \mu$, are all positive.

$$L_j = L(S, A, B_L, D, \mu, \epsilon_L), \tag{5}$$

$$j = j(S, A, B_j, D, \mu, \epsilon_j), \tag{6}$$

$$Max_{(j,L,N)} U(C, L_j, S, A, B_U, D), \tag{7}$$

subject to (1),(3) and (4) , (5) and (6) above.

The above static maximization problem without uncertainty is sufficient to generate some key theoretical predictions of this paper. The negative in utero health shock due to maternal fasting during Ramadan, will lead to a lower labor supply and a selection effect into a less skill-intensive sector. This will take place as mother's fasting lowers the child's stock of human capital (measured by lower test scores) and also possibly through other unmeasured ways. Human capital, in turn, is affected by not just changes in initial health stocks (as measured by birth weight) but also by changes in behavior (such as reduced schooling and more child labor). Reduced schooling, in turn, is a result of exposure to Ramadan, which lowers cognitive ability and, which in turn lowers productivity of schooling in all sectors - as is usually assumed in the literature on the returns to schooling (see Card (2001)). Other than causing reduced schooling time, lower cognitive ability may also lead exposed individuals to sort into sectors that have lower returns to cognitive ability.

At the same time, the framework suggests that the above predictions may be biased by parental characteristics. When parents make decisions for children, parental characteristics may be important. Those who invest more in unexposed children's schooling may be those who also encourage their children to be involved in skilled occupations and who encourage hard work, leading to more labor supply. The next sections will explore the data and, empirical strategy. to test some of the key predictions of the model.

4 Data

The data for this study comes from the Indonesian Family Life Survey (IFLS) consisting of four waves carried out during 1993, 1997, 2000 and 2007 (also known as IFLS1, IFLS2, IFLS3 and, IFLS4, respectively). IFLS collected a great amount of information at the individual, household and community level on a large collection of economic, health and social indicators. Sampling took place at the household level. Great care was taken to assure representativeness of the sample for the reference population. IFLS covers 13 of the (then) 26 provinces of Indonesia, which, in total, represent 83% of the Indonesian population. The analysis in this paper uses the IFLS4. But data from other waves such as IFLS1, carried out 15 years earlier, is also used.

One of the most appealing characteristics of the IFLS is its low attrition rates, comparing favorably even against longitudinal data sets in developed countries. In IFLS4, the re-contact rate was as high as 90.6% of the IFLS1 households. Another feature of the data set, which is conducive to my study, is that around 88% of the sample population is Muslim, which gives me a large enough sample size to compare siblings and household members in Muslim families. This also implies a significant minority (12%), which leaves sufficient room for any falsification tests on the non-Muslim population. One should find no Ramadan effect for non-Muslim pregnant women because they are not expected to be fasting during Ramadan. Although my study is primarily cross-sectional, focusing on the fourth wave, it also uses data from IFLS1 to link early childhood outcomes with adult outcomes in IFLS4. This is a particularly unique feature of this paper, made possible because of the unique longitudinal feature of the IFLS, which has followed people over a 15 year period. Very few developing countries, and almost none of the Muslim majority countries, have such a comprehensive data set.

I follow closely Almond and Mazumder (2011) and Ewijk (2011), in defining the exposure to Ramadan variable (see their papers for details on the construction of the exposure variable). However, my analysis differs from these earlier studies in two ways. First, I use the proportion of days that Ramadan overlaps with pregnancy to obtain a continuous measure of exposure. Although Almond and Mazumder (2011) use a similar measure, Ewijk (2011) does not. And unlike either papers, this study carries out a non-parametric estimation of exposure on the main variables of interest. This feature is particularly appealing because it allows one to explore the critical number of days of exposure it takes for the Ramadan effect to peak. Second, for regression analysis, I focus on those who are potentially exposed for a whole month rather than those who were exposed to Ramadan for a few days only.

To estimate exposure, using self-reported exact date of birth, this paper determines the number of days before an individual's date of birth the last Ramadan fell, restricting the sample to those Muslims born between 1942 and 1993 (15-65 years of age in 2007-2008).[10] Assuming that the average pregnancy lasted for 266 days, I calculate the conception date from the date of birth. If Ramadan starts and ends any time between an individual's date of birth and their estimated conception date, then they are potentially exposed to Ramadan fasting for a whole month. But, if Ramadan started and ended before the individual's conception date, they could not possibly be exposed to their mother's fasting during Ramadan. Days of exposure can be determined by calculating the number of days Ramadan overlapped with the period between conception and birth. Proportion of days of exposure is calculated by dividing days of exposure by 29 days (assumed average length of Ramadan).

One may be concerned that if pregnancy lasted longer than 9 months then those who were actually exposed may be declared not exposed, leading to an additional downward bias in the estimates. Since Kieler et al. (1995) document that very few pregnancies last more than three weeks beyond the average nine months, following Ewijk (2011), this study also controls for all those who were conceived within three weeks after the end of Ramadan.[11]

4.1 Descriptive Statistics

Table 1 reports selected summary statistics for Muslims and non-Muslims, by exposure. Exposure is a dummy for whether the individual was potentially exposed to a full month of Ramadan in utero. First, outcomes for adults (15-65) in Wave 4 are examined. Labor market outcomes include log of hours worked in a normal week at the primary job (Log Hours), self-employment status (Self-employed) and labor force participation (Work). The average of log hours for the sample of Muslims aged 15-65, is 3.61 (approx. 36 hours). Muslims who are exposed work fewer hours compared to those Muslims not exposed and this difference is larger among non-Muslims.

Overall, mean labor force participation is 69% with a standard deviation of 0.464, with the effect of exposure on participation being similar across exposure for Muslims. Interestingly, mean labor force participation is lower among Muslims compared to non-Muslims. On average, 30.5 % of the sample is self-employed, with a standard deviation of 0.460. Muslims who are exposed are more likely to be self-employed. This is in stark contrast to non-Muslims, who are more likely to be self-employed when not exposed to Ramadan.

[10] The start and end dates of Ramadan were taken from www.phys.uu.nl/ vgent/islam/ummalqura.htm and (before 14 March,1937) www.al-islam.com/eng. When other websites were explored, only very minor discrepancies in dates were found. It may also be noted, that, in many areas of the Muslim world, the start and end of Ramadan is determined by moon sightings which may cause small noise in the estimates of this paper.

[11] Ewijk (2011) explains: " If their mothers pregnancies lasted longer than average, their classification as not being exposed would be erroneous, which would create a relatively large amount of noise. Pregnancies lasting three weeks beyond term or more are rare (see for e.g. Kieler et al., 1995), so 21 days is a safe margin. Actually, this bandwidth is longer than necessary for just this purpose: taking it this long also ensures that almost all children are placed into this category who were conceived in the festive days following Ramadan, who may differ from children conceived at other time points."

One of the unique features of the IFLS Wave 4 is that for the first time it asks detailed questions about religiosity. Average religiosity among families is rather high in Indonesia with a mean of 2.796 (on a scale of one to four, where four is the highest value possible) and a standard deviation of 0.463. Interestingly, non-Muslims report even higher levels of religiosity than Muslims families, on average.

The overall sample is representative of males and females with a 1:1 sex ratio. And among those exposed, there are fewer men. Given that men are known to be more responsive to nutritional deficits in utero, this is consistent with the findings in Almond and Mazumder (2011) and Ewijk (2011), who find that males are more likely to die from Ramadan exposure.

The average age in the adult sample (15-65) is 33.16 years with a standard deviation of 12.59. Muslims who are exposed are slightly older (33.08 compared to 32.82 years), though this trend is the opposite in non-Muslims where exposed are younger. To account for age differences, controls for age and its quadratic term are added in the main regression estimates.

Next, data on test scores are used to estimate effects on cognition for children aged 7-15. Test scores include Raven's Colored Progressive Matrices (CPM) questions and a set of mathematics test questions. The Raven's CPM assessment is often used as a measure of general intelligence, and is recognized as the best available measure of Spearman's general intelligence factor "g" (Kaplan and Saccuzzo,1997). The test evaluates an individual's ability to recognize patterns through identification of the missing elements that best match the incomplete patterns.

The mean for the total scores is 69.6%, that of the CPM or cognitive test is 75% and for the math tests is 58.5%. The means for all the tests are lower for exposed Muslims, whereas for exposed non-Muslims the cognitive and total scores are actually higher. This gives one confidence in the identification strategy employed.

Next, summary statistics from Wave 1 are presented. IFLS asks questions about birth weights of infants (0-5 years old) in the pregnancy history module. A combination of certificates, birth records from physicians and, family records were primarily used as sources of birth information. Data from Wave 1 are used since those aged 0-5 in 1993 would be around 15-20 years old in 2007-2008. This allows me to estimate the birth weight effects for a sub-cohort of adults in Wave 4. The mean birth weight is 3123 grams with a standard deviation of 573 grams. On average, Indonesian Muslims and non-Muslims, are well above the 2500 gram low birth weight threshold. Non-Muslim children, in fact, have higher mean birth weights than Muslims infants. Moreover, exposed individuals have lower mean birth weight.

Lastly, a sample of children aged 6-14 in Wave 1 is examined. These children would be about 21-29 years old in Wave 4. This allows me to assess the effects on certain early childhood indicators for a sub-sample of adults in the labor market in Wave 4. In particular, the effects on investments in schooling inputs (hours studied during elementary school and child labor status) are explored. The mean hours studied for the sample under consideration is 4.23 hours during a normal day. Again, those exposed study fewer hours and non-Muslims have higher averages than Muslims. When child labor participation is examined, 1.7% of children report being involved in child labor. Those exposed are more likely to be involved in child labor. But despite studying more hours than Muslim children, non-Muslims are more than twice as likely to be involved in child labor. This is consistent with the summary statistics in Wave 4 where non-Muslim adults are more likely to participate in the labor force than Muslims.

5 Empirical Methodology

5.1 Identification of the Ramadan Effect

Ideally one would like to compare the outcomes for children whose mothers were randomly assigned to fast during Ramadan to the outcomes for children whose mothers were randomly assigned not to fast during Ramadan. This comparison would generate sound estimates of the average treatment effect of fasting during Ramadan. Unfortunately, no such randomized control trial exists.

In much of the epidemiology literature, the outcomes for children whose parents chose to fast are compared to the outcomes of children whose mothers chose not to fast, without sufficiently controlling for mothers' characteristics and other variables that might be correlated with both child outcomes and the decision to fast. Suppose, for example, that mother's level of education is negatively correlated with her choice to fast and positively correlated with the quality of nutrition her children receive, and that poor nutrition but not fasting per se adversely affects child outcomes. Then measured adverse effects of fasting during Ramadan on child outcomes would be at least partially due to the children of less educated mothers receiving poorer nutrition.

Almond and Mazumder (2011) employ an alternative approach that avoids this problem. The outcomes for children whose mother's pregnancy overlapped with Ramadan are compared to the outcomes for children whose mother's pregnancy did not overlap with Ramadan. This approach improves on the approach followed in the epidemiology literature, but falls short of the ideal. For one, it measures the effects of exposure to Ramadan rather than of fasting during Ramadan. It might be that childhood outcomes are affected by diet rather than daytime fasting per se, and that all mothers' diets change in the same way during Ramadan due to feasting. For another, mothers might selectively time their pregnancies to avoid having their children in utero during Ramadan, and variables that are not controlled for may affect both the selective timing of pregnancy and childhood outcomes. It may be that mothers with unwanted pregnancies are more likely to not time their birth away from Ramadan compared to the mothers who want children. It may also be the case that certain mothers are less likely to conceive after Ramadan, since one cannot have sexual intercourse while fasting in the day time. If less informed and less educated mothers are more likely to have unwanted pregnancies overlapping with Ramadan and are more likely to conceive during Ramadan, then a statistically significant Ramadan effect would be due to the exposed children having mothers who are less informed than the mothers of unexposed children.

A solution to the problem with the Almond and Mazumder (2011) approach may be to compare biological siblings who were potentially exposed to their mother's fasting during pregnancy versus those who were not. All time-invariant unobservables which may be driving any timing of pregnancy will be controlled for. Biological siblings fixed effects may also be useful since any time-invariant unobserved factors that may be driving the wedge between actual and potential exposure will be controlled for. Children from uneducated mothers witness more perverse effects not just because their mothers don't time their births away from Ramadan, but because uneducated mothers are more likely to actually fast as well. To the extent that all factors which determine whether the mother actually fasts are time invariant, unlike the Almond and Mazumder (2011) approach, estimates from biological siblings comparisons should not be biased downwards. Although Ewijk (2011) adopts this approach, that paper includes mother fixed effects only for a general health measure of children aged 1-18.[12]

However, most countries do not have longitudinal data that follow biological siblings from childhood well into their adult life and old age. Although sibling fixed effects are useful to identify variables, which identify the short-term to medium-term effect, given current data limitations, it may not be even feasible to apply this approach on adult populations. In this regard, a fourth approach involving household fixed effects may be particularly useful. All those time-invariant unobservables that are common between mothers and the households in which they live, will be controlled for, so that this may be a close approximation to the sibling fixed-effect approach. This is the first paper in the economics of fasting literature to do so.

A fifth approach may be to show differential effects for individuals whose mothers are more likely to have actually fasted, while at the same time using the Almond and Mazumder (2011) method. Religiosity can be thought of as a predictor of actual fasting behavior.[13] I demonstrate the usefulness of this approach by showing that households with higher religiosity have stronger effects. Religiosity is not even measured in

[12]It may be noteworthy that Ewijk (2011) does not carry out mother fixed effects for any other estimates. Not even for analyzing effects on the adult population for which the author finds the strongest effects. Moreover, that paper motivates fixed effects as a way to address selective timing of pregnancy, which, as this paper has pointed out, may not be a major concern in developing countries that lack basic family planning. Although Indonesia has made many strides in family planning, it is still not comparable to the US or UK in this regard. The author does not, for example, motivate the use of fixed effects as an approximation of the ATE compared to the downward biased ITT estimates.

[13]IFLS4 does not ask questions on fasting, but does ask questions on another major pillar of Islam: the five daily prayers. I find that the subjective religiosity measure I use is highly correlated with number of times one prays in a day, suggesting that this may be a good predictor of actual fasting behavior as well.

most data sets, and the data (IFLS4) I use are particularly unique in this regard.

Indeed one of the distinctive features of this paper is that, other than the epidemiology literature approach, which uses data on actual compliance to fasting, this study applies all the last four approaches mentioned above to achieve confidence in the robustness of the estimates. The following section will layout the OLS and fixed-effect regression equations.

5.2 Econometric Equations

The traditional OLS formulation is shown in (8) as follows:

$$Y_{if} = \alpha + \beta_1 exposure_{if} + \beta_2 age_{if} + \beta_3 {age^2}_{if} + \beta_4 male_{if} + \sum_{m=1}^{11} \gamma_m month_{mif} + FC_f + U_{if}, \qquad (8)$$

where Y_{if} is the set of human development outcomes of interest for individual i belonging to family f. $Exposure_{if}$ is a dummy for potential exposure to Ramadan for a full month in utero. age_{if} is age measured in days. γ_m denotes the coefficients for the calendar month of birth fixed effects. [14] In order to control for any communal and social factors that may bias the estimates of exposure to Ramadan, one can carry out a family fixed-effects study. I assume that family/community level covariates, remain constant over time, and so, can drop out the fixed effect FC by differencing across t_1 and t_2, the date of births of members of the family ‘f’.

$$\Delta Y_{[t_1,t_2]} = \alpha + \beta_{1*} exposure_{[t_1,t_2]} + \beta_{2*} \Delta age_{[t_1,t_2]} + \beta_{3*} \Delta {age^2}_{[t_1,t_2]} + \beta_{4*} \Delta male_{[t_1,t_2]} + \sum_{m=1}^{11} \gamma_{m*} \Delta month_{[mt_1,mt_2]} + \Delta V_{[t_1,t_2]}. \qquad (9)$$

This method compares family members who were exposed to Ramadan compared to those who were not, under the identifying assumption that timing of birth and timing of Ramadan is exogenous and fixed effects are time-invariant.

[14] For some specifications, I explored an alternate set of controls for Ramadan month fixed effects. The estimates did not change much

6 Results

6.1 Non-parametric Estimates

Potential fasting during pregnancy by Muslim women is inversely related to their children's adult labor market outcomes. These estimates are strongest for those from more religious families, suggesting that the actual act of fasting may be driving these results. Three sets of figures summarize this relationship. The sample is restricted to those not conceived in the three weeks after Ramadan ends. No other controls are added and a pure relationship between potential exposure to Ramadan and outcomes of interest is explored.

For an overall sample of individuals 15-65 years old, Figure 1 examines the non-linear relationship between proportion of days of potential in utero exposure to mother's fasting and children's hours worked at a primary job, as well as their self-employment status. Exposure to Ramadan reduces hours worked and increases the likelihood of being self-employed, which can be interpreted as a low skill sector. It is interesting to note that the gradient of these curves peaks in the interval of 0.2-0.6, which corresponds to roughly 6 to 18 days of exposure to Ramadan. The marginal negative effect of exposure is almost zero after one is exposed for about 18 days of Ramadan and is increasing most in the interval up to 6 days of Ramadan exposure in utero.

Figure 2 and Figure 3 explore effects on hours worked and self-employment status by average religiosity in the families where the individuals reside. Both figures show that the effects are strongest for those coming from more religious families, though the standard errors for the less religious are much larger. It is worth noting that those from more religious families have fewer average hours worked and are more likely to be self-employed, on average, than those from less religious families.

6.2 Estimates with Controls

Subsequent analyses will use a dummy for full potential exposure to Ramadan, as compared to the non-parametric analysis where a continuous variable was used. There are three reasons to use a dummy for exposure. First, the effects are rather linear, particularly for those exposed for more than 18 days of Ramadan. Second, most individuals have been exposed for a full Ramadan in utero as opposed to partial exposure. Last, the interpretation of results, in linear regression estimates, is cleaner for full exposure to Ramadan in utero.

Table 2 shows a summary of some key estimates of this paper from OLS regressions by religion. For Muslims, those exposed work fewer hours and are more likely to sort into the self-employment sector as adults. As children, their cognitive ability is hampered, which is reflected in lower math and Raven's CPM scores. Furthermore, when they are born they register lower birth weights. When falsification tests are done on non-Muslims, these effects vanish which gives strong evidence in support of the basic hypothesis. Fasting during pregnancy not only effects children's earliest heath indicators, but their cognition as well, which later is correlated with lower labor supply and sorting into self-employment rather than into the wage work sectors.

Table 3 shows estimates for hours worked in a normal week and self-employment status using both OLS and household fixed-effect approaches for Muslim adults aged 15-65 in 2007. [15] The first column shows OLS, the second, OLS restricted and the third, household fixed-effect estimates for each of the variables. In terms of rows, the first row shows point estimates from comparisons of those potentially exposed to a full month of Ramadan to those not potentially exposed to Ramadan at all, during any part of the pregnancy. This is followed by rows for each trimester, where 'Exp. 1st Tri.' stands for an exposure dummy for the overlap of the first trimester with Ramadan, and so on.

Those exposed work 4.5% fewer hours in a normal week at their primary jobs. When sample is restricted to households with three or more family members, the estimates surge to 8.8%, eventually more than doubling to 10% when fixed effects are applied. When these effects are explored by trimester, I find that although OLS estimates predict the first trimester to have the strongest effects, restricted OLS shows that the third trimester has the highest impact. When fixed effects are applied, the statistical significance of all of the trimester point estimates drops, though in terms of magnitude, the third trimester shows strongest effects.

Similarly, those exposed are 3.2% more likely to be self employed. When the sample is restricted to households with three or more family members, the estimates surge to 7.9%, eventually stabilizing to 7.8% when fixed effects are applied. When these effects are explored by trimester, I find that although OLS estimates predict the first and second trimesters to be equally harmful, restricted OLS estimates show the second trimester as marginally more harmful, followed by the third trimester. When fixed effects are applied, the statistical significance of all of the trimester point estimates drops, though in terms of magnitude third trimester remains the most affected.

Next, falsification tests are carried out in Table 4, on non-Muslims in Indonesia. Although sample sizes are much smaller, I do not find similar negative effects for non-Muslims. If anything, some of the estimates show the opposite. Could it be that there are some spill-over effects so that non-Muslims exposed to Ramadan benefit from the more skilled wage-paying jobs where exposed Muslims no longer have the comparative advantage? In any case, the results are reassuring. For example, an argument could be made that it is the high food prices of basic commodities during Ramadan that may be driving these results. But if that is

[15] To explore concerns about model misspecification for the self-employment results, I also tried using logit and probit models for the overall sample associated self-employment in the first column. The three models present a consistent story suggesting that model misspecification is not a serious concern. Moreover, I use robust standard errors and more than 95% of the predicted probabilities also fall within the 0-1 interval. This gives me confidence that the standard concerns regarding bias and inconsistency of linear probability models do not seem to be applicable in my case.

the case, it may be expected to impact Muslims and non-Muslims alike. The fact that non-Muslims do not register negative effects makes such an alternate hypothesis less appealing in favor of the fasting hypothesis.

One of the appealing features of the IFLS is that it is a panel study that has followed individuals for 15 years, between 1993 and 2007. I take advantage of this longitudinal feature of the IFLS, by identifying biological siblings of adults aged 19-29 (19-26 in the case of self-employment results) in 2007 from their Wave 1 files when they were about 5-14, living most likely together with their parents. Although sample sizes are small, if one does find qualitatively similar results, it will be reassuring. Table 3 shows that is indeed the case. Exposed Muslims work fewer hours than their biological siblings and these effects are concentrated in the second and third trimesters, according to the fixed-effect analysis. Non-Muslims - though of an even smaller sample size-register no such effects. Similarly, the estimates for self-employment are robust, with the second trimester effects standing out.

The analysis, thus far, uses alternate identification strategies, to argue that in utero Ramadan exposure negatively affects Muslims and not non-Muslims, in terms of their labor supply and sector of work (self-employment). However, the estimates are based on potential in utero exposure to Ramadan and, there is no concrete measure of actual fasting behavior. Given the lack of questions about fasting behavior for most people in the IFLS, I take advantage of a unique feature of Wave 4 of the IFLS, which asks questions about religiosity. A self-reported measure of religiosity, which asks people to rate their religiosity from none to very religious, is used. If individuals from families who are the most religious have the largest effects, then this will provide further confidence that indeed it is fasting behavior during Ramadan, rather than some other factor not related to religiosity, which is driving the results.

I redo my analysis in Table 3, for the religious and less religious households. When estimates from Table 6 (religious households sample) are compared to Table 7 (less religious households sample), one finds that the largest and most statistically significant effects are found in the religious sample. In terms of trimester analysis, the first and the second trimester seem to be most impacted when OLS estimates are used, but the second and the third trimester appear to be the most impacted (yet again) when OLS restricted and fixed-effect estimates are used.

Selection into Labor Force

A potential concern with the aforementioned labor supply estimates is differential participation into the labor force. For example, estimates could be further biased downward if Ramadan exposure led individuals with very low work capacity and skills to not participate in the labor force. To deal with this concern, Table 9 explores differential participation into the labor force due to Ramadan exposure. Column (1) shows that there is no differential selection into labor force participation in the full sample of adults. This suggests that the key estimates for the adult sample of the 4.5% reduction in labor hours and 3.2% increase in self-employment documented in Table 3 are not biased by selection into or out of labor force. This is reassuring.

Another concern may be that children from more religious or less educated mothers may be more likely to be exposed to Ramadan and if these children are also less likely to be participating in the work force then the estimates of effect of exposure on labor force participation may be biased. The fact that the signs of the estimates in Table 9 are mostly positive, suggest that if anything the more exposed are more likely to be working. None the less, column (2) restricts the sample to households with 3 or more members. The estimates are still insignificant. Column (3), which estimates of fixed effects estimates by trimester and for overall sample. The by-trimester estimates are all insignificant. However, the estimate for overall exposure is positive and significant at 10%. This may partly explain why the fixed effect estimates in Table 3 were marginally higher than OLS-restricted estimates. However, when I explore the highly religious sample, in columns (4)-(6), in contrast to Table 3 where strong and significant effects of exposure on labor hours worked and self-employment probabilities were found, no effects are found on labor force participation. [16]

6.3 Suggestive Pathways Over the Life Course

What are the possible channels through which labor supply and probability of self-employment are being affected? Table 8 shows estimates for test scores for children aged 7-14 in IFLS4. Test scores include both Raven's cognitive test scores and math scores. The scores are in percentage terms. In contrast to Table 1 where household fixed effects were shown, biological sibling fixed effects are estimated in this sample. Those children who were potentially exposed score 5.9% lower in their cognitive scores, 7.8% lower in math scores, so that total scores are 7.1% lower. Restricted OLS estimates for the biological siblings sample and fixed effect estimates are even larger, though broadly similar. All these estimates are statistically significant at 1% level, after bootstrapping the standard errors.

In terms of the trimester effects, this paper finds that both fixed effects and OLS estimates are strongest in the third trimester for cognitive scores, but for math scores, the first and third trimesters are the strongest for the fixed effect analysis. A similar conclusion is reached when analyzing total scores, where the strongest effects seem to be in the first followed by the third trimester, although most of the trimester estimates continue to be statistically insignificant when fixed effects are used.

As strong as these test score estimates are, it is not clear if the adults, who were 15 years and older in

[16] Although the regression model is the linear probability model, I also considered the logit and probit models for the sample associated with column (1) in Table 9. The estimates from all the three models were broadly similar and give a consistent picture. In addition, I carry out a simple two-step Heckman selection model using martial status and interaction of martial status with gender in the selection equation for the overall sample of Muslims and for the non-Muslims. I find that the estimates for the overall sample are robust. This gives me confidence that neither results for the treatment (Muslims) and the placebo (non-Muslim) samples are driven by selection of exposed individuals into or out of the labor force.

2007, did indeed have lower schooling outcomes as children. AME (2011) point out that that one of the gaps in the literature is the inability to link early childhood insults with long-term measures for the same cohort. To fill this gap in the literature, I exploit the panel feature of the IFLS and examine different measures of schooling inputs. Those who were aged 7-14 in 1993 would be 22-29 in 2007. We have already seen that this age group did have lower labor supply and are more likely to be self employed. I now provide some evidence that children of a similar age group, but not necessarily exactly the same individuals, do indeed have worse schooling outcomes. Table 7 shows estimates for hours spent studying during elementary school and for child labor status.[17]

Returning to Table 9, those exposed in the second and third trimesters spent 4.3% and 14.3% fewer hours studying during elementary school, respectively. In the restricted OLS model, third trimester effects continue to exist at 14%. In sibling fixed-effect estimates, those exposed study 10% fewer hours during elementary schooling, with the strongest effects in the third trimester followed by the first trimester.

Next, effects of exposure on child labor status are explored. Child labor is often perceived to be a negative outcome, and those with lower human capital may be more likely to engage in it. I find evidence that indeed this is the case, as those exposed are about 2.3% to 3.9% more likely to be involved in child labor, with the effects strongest in the first trimester followed by the third trimester. Although, the sibling fixed-effects estimates are not statistically significant for child labor, they do have the right (positive) sign, similar to the OLS estimates.

I have so far presented evidence that schooling and cognitive outcomes are being affected by Ramadan exposure and these may indeed be possible channels through which the labor supply and self-employment status are being affected. Now this paper will explore whether those who were age 15-20 in 2007 (age 0-5 in 1993) have lower reported birth weights. If I do find evidence of this, it will give me all the more confidence about the deeper channels through which the labor effects may be taking place, as predicted by the fetal origins hypothesis. Table 10 shows OLS estimates for birth weight effects. Those exposed weigh about 270 grams lower, with those in the second and third trimester having the strongest effects.

7 Discussion

The results show that fasting during pregnancy by Muslim mothers has a wealth effect, measured by fewer hours worked, as well as a selection effect, with those exposed choosing the self-employment sector rather than the more skill-intensive wage work sector.[18] This conclusion is robust to not only household fixed

[17] In addition, I explored effects on the age of starting schooling, age of quitting schooling, whether attended school last year, grade progression and whether child failed grade. Although the OLS effects for most of the categories had the right signs, and were also statistically significant in many cases, the fixed effect estimates were not statistically significant.

[18] In IFLS, I find that self-employed workers have lower years of schooling.

effects, but also to biological sibling fixed effects for a sub-sample of adults. Any other explanation would have to be not only specific to different household members, but for a sub-sample, sibling specific. The fixed effect models give us confidence that the selective timing of pregnancies is unlikely to explain these effects. Selective timing of pregnancy is also an unlikely explanation since, in many developing countries, parents may not plan pregnancies. Moreover, Ewijk (2011) and Almond and Mazumder (2011) provide evidence against any selective timing based on observables. What can then explain the slight downward bias of OLS relative to fixed effects estimates? An obvious candidate is compliance to fasting. To the extent that compliance is time invariant within households, then fixed effects estimates can control for the downward bias associated with lack of compliance in OLS estimates. In this sense the fixed effects estimates may be thought of as ATE. [19]

Although this paper identifies the Ramadan effect, it is not clear whether religious fasting is driving these results. For example, prices of basic food items may hike during Ramadan. Or, a change in eating behavior after sunset(*iftaar*), which involves eating greasy, oily and generally unhealthy foods may be causing the real harm rather than calorie restriction during fasting. Changes in sleep patterns may also occur. People may also work less during Ramadan due to fatigue. All these factors may confound the Ramadan effect from the fasting effect. Falsification tests on non-Muslims are carried out to check the viability of some of these alternate hypotheses. If the Ramadan effect is driven primarily by changes in prices, the price changes should also affect non-Muslims. The fact that this study does not find any similar effects on non-Muslims is comforting.

It may be that Muslims consume a different basket of goods during Ramadan feast times than non-Muslims. In this case, comparisons between Muslims and non-Muslims may hide the fact that the price increase is only in the greasy, oily and unhealthy products consumed by Muslims during Ramadan. If this change in the basket of commodities (for which prices have also risen) is causing the Ramadan effect, then *a priori* one may not expect more religious families to necessarily eat more of these goods than less religious families, in the absence of fasting. If I do not find similar effects on less religious families, then following the logic of the alternate hypothesis, it must be the case that religious Muslims are more likely to eat expensive unhealthy food than less religious Muslims. This will be a weaker assumption than one assumed by the earlier literature. In fact, if anything, we may expect less religious Muslims to also eat the same food items even if they are not fasting, because fasting may have spillover effects on non-fasting Muslims too. In this case, one may find effects on less religious Muslims as well– if it is this non-fasting-related eating behavior that is explaining the effect. My results show that effects are strongest on Muslims from religious households

[19] As mentioned earlier, estimates of compliance to fasting among pregnant women in Muslim majority countries vary between 70%-90%. See Almond and Mazumder (2011) for a more detailed survey.

compared to less religious ones, which casts doubts on this alternate hypothesis.

In general, comparisons between religious and less religious Muslims find effects mostly on religious groups. It its very likely that the Ramadan effects are due to some factor linked with religiosity amongst Muslims. This will make the assumption that the Ramadan effects are being driven by fasting much more tenable compared to earlier studies. That said, one cannot completely rule out other channels in addition to religious fasting. For example, religious mothers are be more likely to perform extra rituals and suffer from more sleep deprivation. But the same rituals may also relieve mental stress. Any positive effects associated with habits of religious Muslims may imply that the effects of fasting itself are biased downwards, whereas sleep deprivation may imply that the estimates are biased upwards.[20]

When suggestive channels through which these effects may be taking place are examined, evidence shows that mother's fasting lowers not just the Raven's CPM cognitive test scores but also the math scores, for children aged 7-15. Moreover, these estimates are robust to biological sibling fixed effects. This suggests that mother's fasting lowers the stock of human capital of the children. Although AME (2011) study test scores, their sample is restricted to those of age 7 only and they use school registry data rather than Raven's CPM, which is considered the gold standard of measuring Spearman's general intelligence factor "g" (Kaplan and Saccuzzo, 1997). Moreover, this paper is able to apply biological sibling fixed effects, which AME (2011) do not.

When the deeper channels through which the changes in test scores may be taking place are analyzed, results show that exposed children are more likely to be involved in child labor and study fewer hours during elementary school. Thus behavioral changes related to schooling inputs may be one possible channel through which the tests score effects are taking place, apart from the direct effects on one's cognitive ability from fasting (as predicted by medical theory). In fact, as the theoretical framework in the paper clarifies, the behavioral response may itself be a response to the lower returns to schooling for the exposed children. The finding of an effect of maternal fasting on child labor is unique, and provides new evidence that lower health and cognition has causal effects on the incidence of child labor. Moreover, child labor itself may accentuate the initial health shock leading to further cognitive and health effects, as measured by lower test scores and general health (Ewijk, 2011). Similarly, not much is understood about how in utero health shocks can affect schooling behavior. The fact that this paper is able to carry out biological sibling fixed effects for these estimates and find qualitatively similar results gives further confidence in the estimates.

Finally, if the effects are really due to the in utero nutrition shock and not due to some other post-natal shock per se, one may be interested in finding evidence on birth outcomes as well. Although the sample

[20] Future work may want to investigate this concern in greater detail, by disentangling the effect of religious fasting from any other factor, associated with religiosity but not related to undernutrition, from fasting per se.

sizes are small and birth weights could be fraught with possible measurement errors, I do find evidence that indeed those exposed do register lower birth weights. The fact that estimates of this study are qualitatively similar to the results found in Almond and Mazumder (2011) is reassuring.

When non-parametric analysis (without controls) is carried out, the results are qualitatively similar to my parametric estimates. However, the estimates show that the major long-term harm to the fetus occurs between 6 and 18 days of exposure to maternal fasting. In the first six days, the marginal effects of fasting are strongest and after 18 days of exposure the marginal effects flatten out. This is an interesting find by itself, for it helps to identify an estimated interval within which additional fasting is most damaging to the fetus. Any policy intervention aimed at creating awareness about the effects of fasting will find such an estimate helpful, as it suggests that even if mothers do not fast for the full month but only for about 18 days, their children can experience effect sizes similar to those fully exposed.

7.1 Effects By Trimester For Salient Outcomes

Almond and Mazumder (2011) summarize select human and animal studies on the effects of nutritional disruption (including fasting) by gestational stage. There is significant heterogeneity of effects for any given outcome and different periods of gestation matter differently for different outcomes. In the case of birth weight, effects vary depending upon the channel and sample. For example, when fasting leads to low blood glucose levels it leads to low birth weight in the third trimester, or low birth weight may occur towards the end of the second trimester due to factors associated with a shorter gestation. Almond and Mazumder (2011) found the strongest effects on birth weights in the first trimester, which is consistent with studies emphasizing changes in HPA axis and exposure to ketones as channels. This paper's finding, that fasting leads to lower birth weight in all trimesters but is strongest (and statistically significant) in the second and third trimesters, is broadly consistent with the literature. Scholl et al. (2001) find that nutritional disruptions early in the third trimester leads to lower birth weights possibly through lowered blood glucose levels.

Most common long-term effects on cognitive function occur in the first trimester, though effects in the third trimester are also found. AME (2011) find effects on test scores primarily in the first trimester in the UK. This paper finds that the strongest (and statistically significant) effects on math test scores are in the first trimester, when siblings fixed effects are used. This is also consistent with the findings of Rizzo et al.(1991), who ascribe low blood glucose levels as a mechanism through which cognitive functioning is impaired in the first trimester. The effects for Raven's CPM measurement, though statistically insignificant for all trimesters, are strongest for the third, followed by the first, trimester. Mirghani et al. (2005) argues that Ramadan fasting affects cognitive function, through changes in fetal heart rate, in the third trimester. This may suggest that changes in Raven's CPM scores may be taking place through changes in fetal heart rates.

For labor supply, the OLS estimates for the full sample show that the first two trimesters are the most critical. However, when the sample is restricted (OLS restricted) to those families with three or more family members, the OLS estimates suggest that the second and third trimesters are more important. When effects are analyzed for the religious sub-sample, the same conclusion holds. Similar to labor hours worked, self-employment full-sample effects are strongest in the first and second trimesters as well and are robust to the religious sub-sample. However, when the sample (OLS restricted) is restricted to families with three or more members, the effects across trimesters become more homogenous, though still marginally highest in the second and third trimesters. The labor supply and self-employment estimates suggest that the first two trimesters are particularly sensitive periods, though there is heterogeneity by sample so that when large enough families are analyzed the last two trimesters become more sensitive.

Together these results suggest that fasting during pregnancy, through perhaps lowering blood glucose levels, effects not just birth weight but cognitive outcomes, leading to effects on labor market behavior.

7.2 Importance of the Magnitude of Some Key Estimates

I have estimated that, on average, an adult who was exposed to Ramadan in utero worked 4.5% fewer hours than an adult who was not exposed. Since the mean number of hours worked per week in the sample is about 36 hrs, this corresponds to a reduction of about 1.6 hrs/wk. How does this result compare to the results of other studies that estimate the effects on adult labor supply of various childhood and other experiences? Baird et al. (2011) estimated that in rural Kenya children aged 9-16 who were given two or three years of deworming treatment worked 12% more hours on average than those who were not a decade later. Meng and Qian (2009) find that early childhood exposure (age 1 to 2) to China's Great Famine between 1959-61, which decreased average cohort size (of those exposed to the famine) by 1% reduced hours worked by 13.9% or 12.64 hrs/wk after 30 years. No statistically significant effects were found for in utero exposure to the famine. Thomas et al. (2006) find that among those adults aged 30 to 70 who were treated with 120 mg of iron per week for a year, there was no change in hours worked. Adhvaryu and Nysahadham (2011) found that, in Tanzania, providing better quality health care to sick adults had only a small (and not statistically significant) effect on their hours worked. Taken together, these results suggest that my finding about reduced hours worked as an adult due to an in utero shock is noteworthy. More generally, the results tentatively suggest that early childhood experiences may be more critical for labor supply than adult experiences.

Arguably, the strongest evidence is for a change in an individual's work sector, away from wage work to self-employment. Thomas et al. (2002) document the economic impact of the East Asian financial crises in Indonesia on adults. They find very modest changes in total employment rates, but that male employment declined by 3.7% in the wage sector, with a 1.74% increase in the self-employment sector as a result. The estimates of the effects of mother's fasting during pregnancy on children's self-employment probabilities (of about 3.2%) is broadly similar, and if anything larger, compared to the labor supply response during the financial crises.

The estimates of the Ramadan effect on test scores are comparable to those found by Cas (2012), who uses the IFLS to identify the effects of the Safe Motherhood program. The author's estimates of cognitive test scores (of 5.12% to 5.49%) are remarkably comparable to this paper's estimates, which are 5.9% to 7.8%. Together, these estimates, which are equivalent to around 0.25 standard deviations of change in (standardized) cognitive test scores, are comparable to the effects of nutritional intervention, as found in the famous Institute of Nutrition of Central America and Panama (INCAP) experimental study in Guatemala. AME (2011) find effects of about 0.6 standard deviations in the UK. However, they do not use the same measures of test scores; their estimates are only for those aged 7, and their estimates may be prone to measurement error as they do not know the exact religion of the child. Moreover, they study a developed country where the society may invest in the less able child to close the inequality gap created across exposure levels, compared to a developing country like Indonesia, where similar investments are not made. If anything, the inequality gap may be reinforced by investments in the more able children for efficiency concerns.

8 Policy Implications

Knowledge regarding the effects of fasting during pregnancy is important not only because of the size of Muslim population affected by it, but also because it highlights potential concerns over any practice that disrupts the timing of nutrition in utero in any society. Around 75% of all pregnancies overlap with Ramadan in any given year, suggesting that in 2010 alone, more than 1.2 billion Muslims globally and 155 million Muslims in Indonesia were potentially exposed to their mother's fasting in utero (Grim and Karim, 2011). This number is more than twice the roughly 500 million directly affected by the 1918 Spanish Influenza and 240 times the roughly 5 million directly affected by the 1944 Dutch Famine, two extreme events that have received much attention.

From a biological perspective, since fasting during pregnancy affects the intrauterine environment similar to other disruptions in the timing of prenatal nutrition, the result of this study may also generalize to non-Muslims (Almond and Mazumder, 2011). Muslims are also not the only religious group to fast. One of the most intriguing aspects about fasting is its almost universal practice since ancient times. Fasting appears to have emerged independently in different societies. Both eastern and western cultures have practiced it (Arbesmann, 1951). The norms of fasting may vary, but the practice of fasting (and/or skipping meals) does persist to this day across most religions and societies. For example, one in every five pregnant women in the US skip their breakfast (Almond and Mazumder, 2011). Many Baha'i may fast during Ala, Christians during Lent, Hindus during festivals such as Durga Puja Navaratri and Karva Chauth, Jains during Paryushan and Jews may fast during Yom Kippur, to name a few. Given how deeply fasting is linked with material consumption, that it has had such a rich historical legacy, and how universally it seems to have been practiced, it is surprising to see the little attention economists have given to this area of study. This research takes an exception to this trend.

Knowledge of the harmful effects of fasting during pregnancy may be useful to policy makers who may want to create appropriate awareness programs and solve any coordination failures between religious, health and economic sectors of the society. Campaigns, for example, may be aimed at creating awareness of the health and economic impacts of fasting not only to families, through the print, electronic and social media, but they may also be targeted at local midwives and doctors as well as imams so that they offer contextualized solutions. It is indeed helpful to know that Islam exempts women from fasting during pregnancy if their health is adversely affected. The local imams can be encouraged to give talks on this topic, such as during Friday *khutbahs*, to create awareness. This may involve, for example, encouraging husbands to take their pregnant wives to the local doctors for regular health checks in general, and during Ramadan in particular. And when negative health effects are clear, imams can encourage delayed fasting, as allowed by Islamic law.

9 Conclusion

This paper examines the effects of fasting during pregnancy by Muslim mothers on their children's outcomes over the life cycle. Non-parametric analysis for adult labor market outcomes reveals that partial exposure in utero to Ramadan, for even 18 days, generates effects similar to those from from full exposure. Moreover, the marginal damage to the fetus, from mother's fasting during Ramadan, peaks during the 6-18 days window.

Parametric estimations with limited controls show that fasting during pregnancy by Muslim mothers has a wealth effect measured by 4.5% fewer hours worked in a normal week (in their primary jobs) as well as a selection effect, which involves a 3.2% increase in those choosing self-employment sector.[21] This conclusion is robust to not only household fixed effects, but also to biological sibling fixed effects for a sub-sample of adults. If anything, the estimate sizes marginally increase with fixed effects. When falsification tests are done

[21] Self-employment can be thought of as a less skill-intensive sector. In fact, the self-employed have fewer years of schooling than wage workers in the IFLS.

on non-Muslims, no effects are found. Thus, the effects this paper details are peculiar to Muslims during Ramadan. Moreover, if the Ramadan effect is indeed driven by religious fasting, then the religious families should register the strongest effects since they may be the most likely to participate in fasting. Evidence supports this prediction. The effects are strongest in the more religious families than in the less religious families.

To explore suggestive channels through which these labor market effects are taking place, this study examines the effects on test scores. Evidence shows that mothers' fasting lowers not just the Raven's CPM cognitive test scores by 5.9% but also the math scores for children aged 7-15, by 7.8%. Moreover, these estimates are robust to biological sibling fixed effects (in fact they increase). This suggests that mothers' fasting lowers the stock of human capital of the children which in turn may be determining the labor market behavior. Furthermore, this paper explores the deeper channels through which the changes in test scores may be taking place. Evidence shows that children are 3.3% more likely to be involved in child labor (though the estimates decrease to1.6% and become insignificant with fixed effects) and study 3.4% fewer hours during elementary school (estimates grow significant and larger with fixed effects). Thus behavioral changes related to investments in schooling inputs may be one possible channel through which the tests score effects are taking place, apart from the direct effects on one's cognitive ability from fasting (as predicted by epidemiological theory). In fact, as the theoretical framework in this paper clarifies, the behavioral response itself may be a response to the lower returns to schooling for the exposed children. Finally, if the effects we are observing are indeed due to the in utero health shock and not due to some other post-natal shock per se, we may be interested in finding evidence on birth outcomes as well. Although the sample sizes are small and birth weights could be subject to possible measurement errors, I do find evidence that those exposed register lower birth weights of about 270 grams. [22]

In terms of magnitudes, the estimates of this paper on hours worked are of particular interest (to the broader audience), as few studies exist on this topic. Baird et al. (2011) estimated that in rural Kenya children aged 9-16 who were given two or three years of deworming treatment worked 12% more hours on average than those who were not a decade later. Meng and Qian (2009) find no statistically significant effects for in utero exposure to the famine. Thomas et al. (2006) find that among those adults aged 30 to 70 who were treated with 120 mg of iron per week for a year, there was no change in hours worked. Adhvaryu and Nysahadham (2011) found that, in Tanzania, providing better quality health care to sick adults had only a small (and not statistically significant) effect on their hours worked . The effect sizes for self-employment effects are broadly comparable to those from the East Asian financial crises (Thomas et al., 2002). And

[22] This estimate is in fact larger than the estimate of birth weight effects from the US, found by Almond and Mazumder (2011)

the estimates of test scores are broadly comparable to those found by Cas (2012), who studies the Safe Motherhood program in Indonesia, as well as to the famous INCAP experimental study in Guatemala.

There is significant heterogeneity of effects by trimester for any given outcome and by outcome. In general, birth weight, Raven's CPM, hours studied in elementary school and labor market behavior are most strongly affected in the second/third trimester, though math test scores are strongest in the first trimester. This is broadly consistent with the medical literature, which suggests that lower blood glucose levels during pregnancy may play a critical role in shaping these effects.

There are important policy implications of this paper. In contrast to the most recent studies on in utero shocks which study the effects of pollution, war, weather and famine, this study identifies the long-term effects of mild behavioral choices made during pregnancy on children, who are more under the control of decision makers such as fathers and mothers. This makes this study unique and of interest to not only policy makers, health practitioners, and imams, but also to fathers and pregnant mothers themselves. Also, in contrast to studies such as Maccini and Yang (2009), which identify effects of rainfall shocks on rural populations, my sample is not restricted to rural or urban regions but rather includes both, making this study of broader interest. In fact, as Muslims reside in developing and developed countries across the globe, the results have much wider significance. The findings on long-term effects on labor market behavior are also of particular interest since they imply that current studies understate the welfare losses associated with negative health shocks. These losses may not be reflected in aggregate measures of economic growth and overstate the importance given to the association between health and wage income (Thomas, 2009).

These findings also imply that interventions such as the Safe Motherhood program in Indonesia, which seek to improve upon quality and/or quantity of midwives in developing countries, may have higher returns than earlier thought. Access to midwives, for example, may lead to more informed health choices, which may contribute to optimal fasting and minimize losses for children from maternal fasting during pregnancy. Furthermore, this also creates room for new and creative interventions which create awareness about the effects of fasting during pregnancy. Media may be used so that health effects of maternal fasting during pregnancy are highlighted. The local imams can be encouraged to give talks on this topic, such as during Friday *khutbahs*, to create awareness. This may involve, for example, encouraging husbands to take their pregnant wives to the local doctors for regular health checks in general ,and during Ramadan, in particular. And when negative health effects are clear, imams can encourage delayed fasting, as allowed by Islamic law.

Future extensions of this paper can exploit panel feature of the IFLS to determine not only effects on the levels of outcomes, but growth rates as well. One can also explore heterogeneity by mother's age, education and income. Finally, it will be very interesting to disentangle the role parents and society play in mitigating or reinforcing the in utero nutrition shocks.

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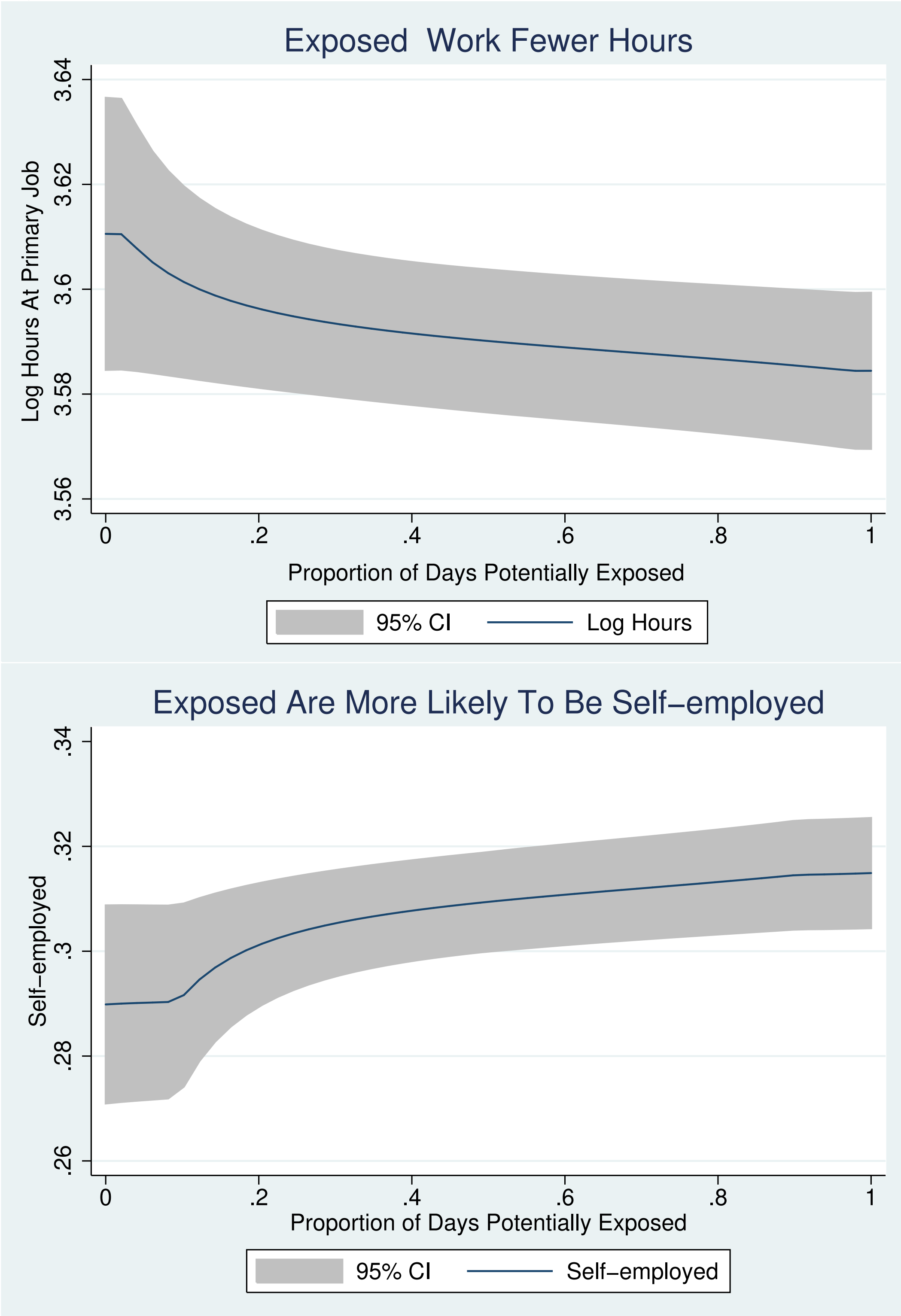
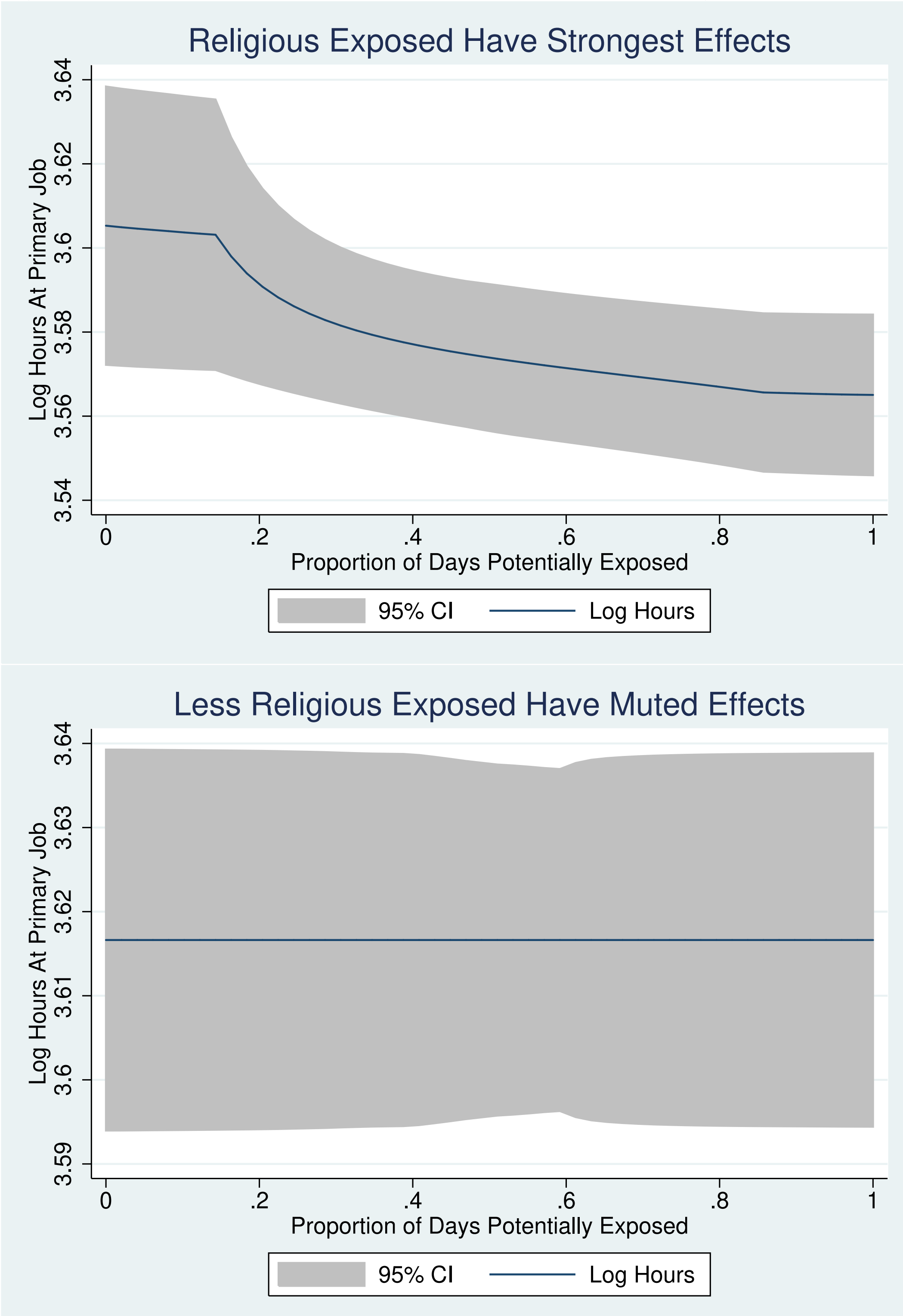
Fig 1: Effect of Ramadan Exposure on Hours Worked and Self-employment Status



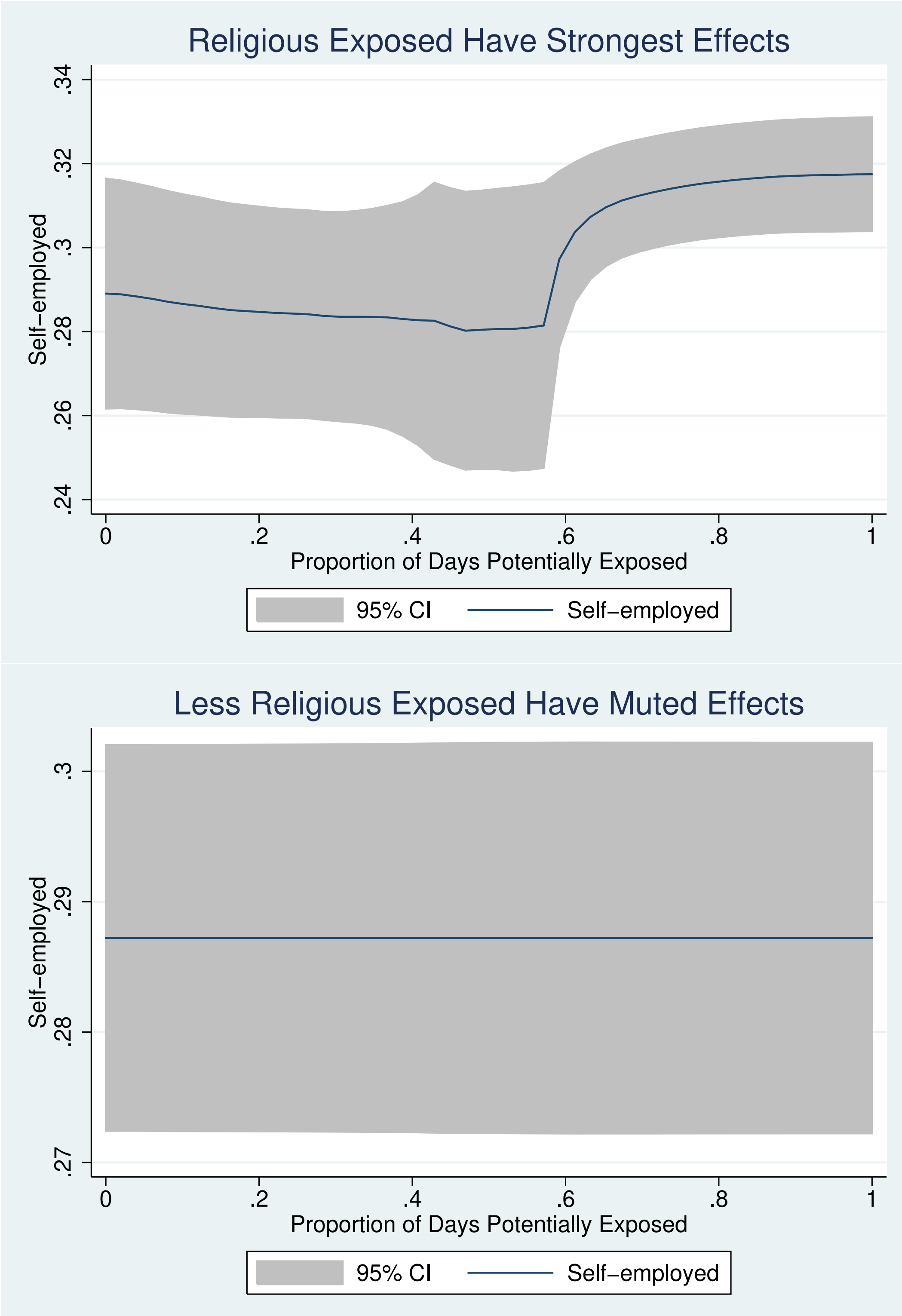
Note: The graphs are local polynomial smooth plots using the Epanechnikov kernel and a bandwidth of 0.405 for Self-employment and 0.436 for Log Hours. The bandwidths were determined using a cross validation technique. Shaded areas represent 95% confidence intervals. Days of potential exposure measures the proportion of days Ramadan overlapped with in utero.

Fig 2: Effect of Ramadan Exposure on Hours Worked By Family Religiosity



Note: The graphs are local polynomial smooth plots using the Epanechnikov kernel and a bandwidth of 0.3829468 for those religious, and 326568.9 for those not so religious. The bandwidths were determined using the cross validation technique. Shaded areas represent 95% confidence intervals. Days of potential exposure measures the proportion of days Ramadan overlapped with in utero.

Fig 3: Effect of Ramadan Exposure on Self-employment By Family Religiosity



Note: The graphs are local polynomial smooth plots using the Epanechnikov kernel and a bandwidth of 0.1917202 for those religious, and 40434.37 for those not so religious. The bandwidths were determined using the cross validation technique. Shaded areas represent 95% confidence intervals. Days of potential exposure measures the proportion of days Ramadan overlapped with in utero.

Table 1: Summary Statistics by Exposure and Religion

	Muslims Exposed	Muslims Not Exposed	Non-Muslims Exposed	Non-Muslims Not Exposed	Total Total
Wave 4 Adults: 15-65 years					
Age	33.08	32.82	34.05	34.94	33.16
	(12.51)	(12.25)	(13.43)	(13.85)	(12.59)
Male	0.495	0.518	0.513	0.560	0.500
	(0.500)	(0.500)	(0.500)	(0.498)	(0.500)
Religiosity	2.781	2.770	2.940	2.903	2.796
	(0.464)	(0.468)	(0.424)	(0.474)	(0.463)
Work	0.679	0.678	0.729	0.820	0.685
	(0.467)	(0.467)	(0.445)	(0.385)	(0.464)
Log Hours	3.581	3.631	3.527	3.541	3.582
	(0.666)	(0.611)	(0.696)	(0.734)	(0.664)
Self-employed	0.308	0.270	0.309	0.392	0.305
	(0.462)	(0.444)	(0.462)	(0.490)	(0.460)
Observations	10207	1630	1223	191	13251
Wave 4 Children: 7-15 years					
Cognitive Scores	0.751	0.761	0.735	0.690	0.750
	(0.226)	(0.216)	(0.247)	(0.267)	(0.227)
Math Scores	0.584	0.596	0.578	0.600	0.585
	(0.263)	(0.270)	(0.261)	(0.217)	(0.263)
Total Scores	0.697	0.706	0.683	0.662	0.696
	(0.209)	(0.201)	(0.223)	(0.230)	(0.210)
Observations	3615	543	390	67	4615
Wave 1 Infants: 0-5 years					
Birth Weight	3.087	3.181	3.126	3.374	3.123
	(0.550)	(0.554)	(0.597)	(0.584)	(0.573)
Observations	477	52	339	53	921
Wave 1 Children: 6-14 years					
Hours Studied-Elem.	1.441	1.448	1.497	1.532	1.451
	(0.263)	(0.269)	(0.200)	(0.154)	(0.256)
Child Labor	0.0170	0.00673	0.0242	0.0200	0.0168
	(0.129)	(0.0819)	(0.154)	(0.141)	(0.128)
Observations	2235	372	432	72	3111

Note: Mean of each variable with standard deviation in parentheses. Sample does not include those conceived less than 21 days after the end of Ramadan.

Table 2: Summary of Key Estimates

	Muslims	Non-Muslims
VARIABLES	Exposed	Exposed
Log Hours	-0.045**	-0.027
	(0.020)	(0.071)
Observations	8,051	1,035
Self-employed	0.032**	-0.089**
	(0.014)	(0.041)
Observations	8,373	1,069
Cognitive Scores	-0.059***	0.083
	(0.018)	(0.054)
Observations	3,514	379
Math Scores	-0.078***	0.036
	(0.023)	(0.048)
Observations	3,521	380
Total Scores	-0.071***	0.062
	(0.017)	(0.044)
Observations	3,521	380
Birth Weight	-0.271*	0.449
	(0.153)	(0.369)
Observations	828	144

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Robust standard errors are clustered at current household level.
For labor market outcomes, sample is restricted to adults in Wave 4 who were 15-65 years old in 2007. For test scores, sample includes children aged 7-15 in 2007 from Wave 4. 'Cognitive Scores' are the Raven's CPM intelligence test scores. All scores are in percentages. For birth weight, sample is restricted to those 0-5 years old in 1993 (Wave1). All regressions control for gender, month of birth fixed effects, age and age squared, where age is defined in days. In addition, I control for all those estimated to be conceived less than 21 days after the end of Ramadan. Exposed is a dummy which assumes value 1 if child was potentially exposed to a full month of Ramadan and 0 otherwise.

Table 3: Estimates From IFLS 4 for Muslims

	OLS	OLS-Rest.	Fixed Effect	OLS	OLS-Rest.	Fixed Effect
VARIABLES	Log Hours	Log Hours	Log Hours	Self-employed	Self-employed	Self-employed
Exposed	-0.045**	-0.088***	-0.103***	0.032**	0.079***	0.078**
	(0.020)	(0.033)	(0.040)	(0.014)	(0.023)	(0.036)
Observations	8,051	2,859	2,859	8,373	2,968	2,968
Exp- 1st Tri.	-0.055**	-0.029	0.024	0.040**	0.072**	0.006
	(0.024)	(0.041)	(0.083)	(0.017)	(0.028)	(0.071)
Observations	3,317	1,154	1,154	3,444	1,202	1,202
Exp- 2nd Tri.	-0.052**	-0.092**	-0.095	0.040**	0.080***	0.034
	(0.025)	(0.042)	(0.069)	(0.017)	(0.029)	(0.056)
Observations	3,211	1,149	1,149	3,320	1,185	1,185
Exp- 3rd Tri.	-0.038	-0.143***	-0.101	0.021	0.077***	0.101
	(0.025)	(0.046)	(0.135)	(0.017)	(0.028)	(0.068)
Observations	3,222	1,076	1,076	3,354	1,118	1,118

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
The OLS-restricted and fixed effect estimates are clustered at household level.
'Fixed effects' are household fixed effects which include household head, their spouse, children and their siblings and siblings-in-law. Sample is restricted to Muslim adults who were 15-65 years old in 2007. The OLS-restricted limits sample further to those households with three or more household members. All regressions control for gender, month of birth fixed effects, age and age squared, where age is defined in days.
In addition, I control for all those estimated to be conceived less than 21 days after the end of Ramadan. Exposed is a dummy which assumes value 1 if child was potentially exposed to a full month of Ramadan and 0 otherwise.

Table 4: Estimates From IFLS 4 for Non-Muslims Only

	OLS	OLS-Rest.	Fixed Effect	OLS	OLS-Rest.	Fixed Effect
VARIABLES	Log Hours	Log Hours	Log Hours	Self-employed	Self-employed	Self-employed
Exposed	-0.027	0.087	-0.008	-0.089**	-0.144**	0.000
	(0.068)	(0.116)	(0.154)	(0.042)	(0.067)	(0.101)
Observations	1,035	377	377	1,069	392	392
Exp- 1st Tri.	0.013	0.260**	0.766*	-0.073	-0.157*	-0.161
	(0.078)	(0.125)	(0.446)	(0.051)	(0.086)	(0.377)
Observations	428	162	162	442	170	170
Exp- 2nd Tri.	-0.097	0.008	-0.363	-0.032	-0.084	0.035
	(0.083)	(0.144)	(0.806)	(0.052)	(0.099)	(0.392)
Observations	413	157	157	428	162	162
Exp- 3rd Tri.	-0.006	0.079	0.054	-0.125**	-0.190**	-0.169
	(0.081)	(0.154)	(0.613)	(0.049)	(0.080)	(0.281)
Observations	442	162	162	454	169	169

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
The OLS-restricted and fixed effect estimates are clustered at household level.
'Fixed effects' are household fixed effects which include household head, their spouse, children and their siblings and siblings-in-law. Sample is restricted to Non-Muslim adults who were 15-65 years old in 2007. The OLS-restricted limits sample further to those households with three or more household members. All regressions control for gender, month of birth fixed effects, age and age squared, where age is defined in days.
In addition, I control for all those estimated to be conceived less than 21 days after the end of Ramadan. Exposed is a dummy which assumes value 1 if child was potentially exposed to a full month of Ramadan and 0 otherwise.

Table 5: Estimates From Sibling Fixed Effects

	Muslims			Non- Muslims	Muslims			Non-Muslims
	OLS	OLS-Rest.	Fixed Effect	OLS	OLS	OLS-Rest.	Fixed Effect	OLS
VARIABLES	Log Hours	Log Hours	Log Hours	Log Hours	Self-employed	Self-employed	Self-employed	Self-employed
Exposed	-0.159*	-0.333**	-0.434**	-0.039	0.149**	0.140	0.380**	-0.108
	(0.082)	(0.137)	(0.174)	(0.186)	(0.069)	(0.113)	(0.179)	(0.226)
Observations	1,310	530	530	233	1,010	416	416	171
Exposed-1st Tri.	-0.240	-0.560*	0.220	-0.359	0.005	-0.082	-0.900	-0.058
	(0.186)	(0.285)	(1.888)	(0.490)	(0.128)	(0.179)	(1.753)	(0.552)
Observations	523	209	209	98	390	157	157	73
Exposed-2nd Tri.	-0.286*	-0.332*	-0.879	0.155	0.027	-0.003	0.159	-0.684
	(0.168)	(0.183)	(1.365)	(0.363)	(0.037)	(0.060)	(2.285)	(0.565)
Observations	522	217	217	93	405	167	167	63
Exposed-3rd Tri.	-0.168	-0.415*	-0.907	-0.002	0.240**	0.348*	-0.579	-0.023
	(0.140)	(0.248)	(1.227)	(0.343)	(0.117)	(0.200)	(2.851)	(0.085)
Observations	532	215	215	105	403	161	161	73

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The OLS-restricted and fixed effect estimates are clustered at the mother level. ‘Fixed effects’ are biological sibling fixed effects. Sample for Log Hours is restricted to Muslim adults who were 19-29 years old in 2007, and for Self-employed, 19-26 years old. The OLS-restricted limits sample further to those households with two or more household members. All regressions control for gender, month of birth fixed effects, age and age squared, where age is defined in days. In addition, I control for all those estimated to be conceived less than 21 days after the end of Ramadan. Exposed is a dummy which assumes value 1 if child was potentially exposed to a full month of Ramadan and 0 otherwise.

Table 6: Estimates From IFLS 4 for Highly Religious Muslims Only

	OLS	OLS-Rest.	Fixed Effect	OLS	OLS-Rest.	Fixed Effect
VARIABLES	Log Hours	Log Hours	Log Hours	Self-employed	Self-employed	Self-employed
Exposed	-0.054**	-0.131***	-0.133*	0.040**	0.097***	0.119***
	(0.026)	(0.043)	(0.069)	(0.018)	(0.033)	(0.041)
Observations	5,031	1,412	1,412	5,232	1,465	1,465
Exp- 1st Tri.	-0.062**	-0.044	-0.013	0.040*	0.093**	0.067
	(0.031)	(0.058)	(0.143)	(0.022)	(0.041)	(0.084)
Observations	2,019	538	538	2,103	566	566
Exp- 2nd Tri.	-0.061*	-0.142***	-0.258	0.061***	0.094**	0.019
	(0.032)	(0.054)	(0.167)	(0.022)	(0.043)	(0.140)
Observations	2,001	551	551	2,070	568	568
Exp- 3rd Tri.	-0.055*	-0.245***	-0.304**	0.026	0.121***	0.254**
	(0.031)	(0.063)	(0.147)	(0.022)	(0.041)	(0.114)
Observations	2,024	530	530	2,109	551	551

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
The OLS-restricted and fixed effect estimates are clustered at household level.
'Fixed effects' are household fixed effects which include household head, their spouse children and their siblings and siblings-in-law. Sample is restricted to religious Muslim adults who were 15-65 years old in 2007. Religious individuals come from families whose mean corresponds to "Very Religious" or "Religious". The OLS-restricted limits sample further to those households with three or more household members. All regressions control for gender, month of birth fixed effects, age and age squared, where age is defined in days. In addition, I control for all those estimated to be conceived less than 21 days after the end of Ramadan. Exposed is a dummy which assumes value 1 if child was potentially exposed to a full month of Ramadan and 0 otherwise.

Table 7: Estimates From IFLS 4 for Less Religious Muslims Only

	OLS	OLS-Rest.	Fixed Effect	OLS	OLS-Rest.	Fixed Effect
VARIABLES	Log Hours	Log Hours	Log Hours	Self-employed	Self-employed	Self-employed
Exposed	-0.029	-0.049	-0.073	0.016	0.063**	0.051
	(0.034)	(0.049)	(0.068)	(0.023)	(0.032)	(0.041)
Observations	3,015	1,447	1,447	3,134	1,503	1,503
Exp- 1st Tri.	-0.033	-0.014	0.019	0.033	0.059	-0.014
	(0.039)	(0.056)	(0.124)	(0.027)	(0.039)	(0.084)
Observations	1,294	616	616	1,335	636	636
Exp- 2nd Tri.	-0.028	-0.041	-0.101	0.004	0.072*	0.045
	(0.042)	(0.063)	(0.115)	(0.028)	(0.040)	(0.072)
Observations	1,207	598	598	1,246	617	617
Exp- 3rd Tri.	-0.016	-0.058	-0.041	0.008	0.033	0.018
	(0.042)	(0.065)	(0.177)	(0.028)	(0.039)	(0.093)
Observations	1,194	546	546	1,240	567	567

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
The OLS-restricted and fixed effect estimates are clustered at household level.
'Fixed effects' are household fixed effects which include household head, their spouse children and their siblings and siblings-in-law. Sample is restricted to less religious Muslim adults who were 15-65 years old in 2007. Less religious individuals come from families who self-reported "Somewhat Religious" or "Not Religious" on average. The OLS-restricted limits sample further to those households with three or more household members.
All regressions control for gender, month of birth fixed effects, age and age squared, where age is defined in days. In addition, I control for all those estimated to be conceived less than 21 days after the end of Ramadan. Exposed is a dummy which assumes value 1 if child was potentially exposed to a full month of Ramadan and 0 otherwise.

Table 8: Estimates For Test Scores for Children Aged 8-15

	OLS	OLS-Rest.	Fixed Effect	OLS	OLS-Rest.	Fixed Effect	OLS	OLS-Rest.	Fixed Effect
VARIABLES	Cog. Scores	Cog. Scores	Cog. Scores	Math Scores	Math Scores	Math Scores	Total Scores	Total Scores	Total Scores
Exposed	-0.059***	-0.085***	-0.100***	-0.078***	-0.099***	-0.143***	-0.071***	-0.094***	-0.120***
	(0.018)	(0.024)	(0.035)	(0.023)	(0.033)	(0.052)	(0.017)	(0.023)	(0.036)
Observations	3,514	2,084	2,084	3,521	2,087	2,087	3,521	2,087	2,087
Exposed-1st Tri.	-0.027	-0.138**	-0.068	-0.053	-0.122	-0.248*	-0.045	-0.141***	-0.136
	(0.034)	(0.055)	(0.132)	(0.042)	(0.076)	(0.144)	(0.031)	(0.052)	(0.129)
Observations	1,342	790	790	1,364	828	828	1,345	801	801
Exposed-2nd Tri.	-0.000	-0.056	-0.059	-0.032	-0.074*	0.132	-0.014	-0.067*	-0.017
	(0.030)	(0.041)	(0.083)	(0.034)	(0.042)	(0.098)	(0.026)	(0.035)	(0.082)
Observations	1,344	790	790	1,366	828	828	1,348	803	803
Exposed-3rd Tri.	-0.070**	-0.023	-0.163	-0.077*	-0.095*	-0.076	-0.070**	-0.045	-0.128
	(0.032)	(0.042)	(0.192)	(0.040)	(0.053)	(0.217)	(0.030)	(0.040)	(0.167)
Observations	1,344	790	790	1,366	828	828	1,348	803	803

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The OLS-restricted and fixed effect estimates are clustered at the mother level. Standard errors are bootstrapped for fixed effect estimates.'Cog. Scores' are cognitive section of the Raven's test scores. Scores are in percentages. 'Fixed effects' are biological siblings fixed effects. Sample is restricted to Muslim children who were 8-15 year old in 2007. The OLS-restricted limits sample further to those households with two or more household members. All regressions control for gender, month of birth fixed effects, age and age squared, where age is defined in days. In addition, I control for all those estimated to be conceived less than 21 days after the end of Ramadan. Exposed is a dummy which assumes value 1 if child was potentially, exposed to a full month of Ramadan and 0 otherwise.

Table 9: Estimates From IFLS 4 for Labor Force Participation: Muslims and Religious Muslims

	Muslims			Religious		
	OLS	OLS-Rest.	Fixed Effect	OLS	OLS-Rest.	Fixed Effect
VARIABLES	Work	Work	Work	Work	Work	Work
Exposed	0.004	0.017	0.035*	-0.000	0.013	0.007
	(0.011)	(0.017)	(0.021)	(0.014)	(0.022)	(0.029)
Observations	11,916	6,260	6,260	7,422	3,646	3,646
Exp- 1st Tri.	0.005	0.007	0.047	-0.014	-0.022	-0.004
	(0.013)	(0.021)	(0.034)	(0.017)	(0.027)	(0.062)
Observations	4,853	2,511	2,511	2,972	1,429	1,429
Exp- 2nd Tri.	0.009	0.027	0.061	0.011	0.042	0.068
	(0.013)	(0.021)	(0.040)	(0.017)	(0.028)	(0.051)
Observations	4,759	2,526	2,526	2,935	1,428	1,428
Exp- 3rd Tri.	0.000	0.020	0.025	0.000	0.030	-0.038
	(0.013)	(0.020)	(0.037)	(0.017)	(0.027)	(0.061)
Observations	4,805	2,456	2,456	3,006	1,439	1,439

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
'Work' is a dummy for those participating in the labor force- whether self-employed or wage workers. The OLS-restricted and fixed effect estimates are clustered at household level. 'Fixed effects' are household fixed effects which include household head, their spouse children and their siblings and siblings-in-law. Sample is restricted to adults who were 15-65 years old in 2007. Religious Muslims come from families whose mean corresponds to "Very Religious" or "Religious". The OLS-restricted limits sample further to those households with three or more household members. All regressions control for gender, month of birth fixed effects, age and age squared, where age is defined in days. In addition, I control for all those estimated to be conceived less than 21 days after the end of Ramadan. Exposed is a dummy which assumes value 1 if child was potentially exposed to a full month of Ramadan and 0 otherwise.

Table 11: Birth Weight Estimates From IFLS 1 for Those Aged 15-20 in IFLS4

VARIABLES	Birth Weight	Birth Weight	Birth Weight	Birth Weight
Exposed	-0.271*			
	(0.153)			
Exposed 1st Tri.		-0.183		
		(0.300)		
Exposed 2nd Tri.			-0.671**	
			(0.335)	
Exposed 3rd Tri.				-0.461***
				(0.174)
Observations	828	290	316	312
R-squared	0.037	0.047	0.049	0.070

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0. The table shows OLS estimates for reported birth-weights from Wave 1 of the IFLS. Errors are clustered at household level. Sample is restricted to Muslims who were 0-5 years old in 1993. All regressions control for gender, month of birth fixed effects, age and age squared, where age is defined in days. In addition, I control for all those estimated to be conceived less than 21 days after the end of Ramadan. Exposed is a dummy which assumes value 1 if child was potentially exposed to a full month of Ramadan and 0 otherwise.

Table 10: Estimates For Schooling Inputs for Children Aged 7-14 in IFLS1

VARIABLES	OLS Log Hrs. School	OLS-Res. Log Hrs. School	Fixed Effect Log Hrs. School	OLS Child Labor	OLS-Res. Child Labor	Fixed Effect Child Labor
Exposed	-0.034	-0.046	-0.100*	0.033***	0.039***	0.016
	(0.024)	(0.034)	(0.055)	(0.012)	(0.014)	(0.023)
Observations	1,815	941	941	2,164	1,117	1,117
Exposed-1st Tri.	0.046	0.004	-0.144	0.037	0.035	0.130
	(0.048)	(0.066)	(0.262)	(0.029)	(0.044)	(0.307)
Observations	746	382	382	887	446	446
Exposed-2nd Tri.	-0.043**	-0.024	0.196	0.000	0.000	-0.014
	(0.021)	(0.016)	(0.230)	(0.001)	(0.003)	(0.153)
Observations	722	396	396	863	472	472
Exposed-3rd Tri.	-0.145***	-0.139**	-0.255	0.019	0.024	0.027
	(0.040)	(0.059)	(0.227)	(0.022)	(0.018)	(0.083)
Observations	743	390	390	907	474	474

Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The OLS-restricted and fixed effect estimates at the mother level. Standard errors are bootstrapped for fixed effect regressions. 'Log. Hrs School' are hours spent studying while at elementary school. 'Fixed effects' are biological siblings fixed effects. Sample is restricted to Muslim children aged 15-22 (7-14 in 1993) in 2007. The OLS-restricted limits sample further to those households with two or more members. All regressions control for gender, month of birth fixed effects, age and age squared, where age is defined in days. In addition, I control for all those estimated to be conceived less than 21 days after the end of Ramadan. Exposed is a dummy which assumes value 1 if child was potentially exposed to a full month of Ramadan and 0 otherwise.