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UNIVERSITY OF CALIFORNIA
RIVERSIDE

Adverse Shocks and Human Development

A Dissertation submitted in partial satisfaction
of the requirements for the degree of

Doctor of Philosophy

in

Economics

by

Muhammad Farhan Majid

June 2013

Dissertation Committee:

Professor Anil B. Deolalikar, Co-Chairperson
Professor Richard Arnott, Co-Chairperson
Professor Jorge Agüero
Professor Aman Ullah
Professor Steven Helfand

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To my parents.

ABSTRACT OF THE DISSERTATION

Adverse Shocks and Human Development

by

Muhammad Farhan Majid

Doctor of Philosophy, Graduate Program in Economics
University of California, Riverside, June 2013
Professor Anil B. Deolalikar, Co-Chairperson
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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. My first paper uses longitudinal data (the Indonesian Family Life Survey) to study the persistent effects of in utero exposure to Ramadan over the life cycle. The exposed children have lower birth weights, they study fewer hours during elementary school, do more child labor, score 7.8 percent lower on cognitive tests and 5.9 percent lower on math test scores. As adults, the exposed children work 4.5 percent fewer hours and are more likely to be self-employed.

In my second chapter, which is joint work with Jorge Agüero, we look at the role which conflict plays in perpetuating inequalities. The identification of the effect of wars on human capital tends to focus on the population of school age children at the time of the conflict. Our paper introduces a methodology to estimate the effect of war on the stock of human capital by examining the changes in the presence of educated people after the Rwanda genocide. We find that the genocide reduced the stock of human capital in Rwanda severely. The before-and-after results show that highly educated individuals (i.e., those with primary education or more) are "missing" at a rate that is 19.4% higher than the less educated. Moreover, Rwanda's average years of schooling is lower by 0.37 years.

When comparisons with Uganda are made, these estimates more than double suggesting that, if anything, the previous findings were biased downwards. Interestingly, when the cross-sectional variation within Rwanda variation in intensity of genocide is exploited there is no evidence of statistically significant differences. This suggests that the losses in the stock of human capital due to the Rwandan genocide were aggregate in nature.

In my third chapter, I review the most recent literature from developing countries on parental investment response to child endowment shocks. I find that parents tend to reinforce fetal shocks. Moreover, there seems to be a gender bias in parental investments. However, there is much heterogeneity over the life cycle, with little work done on prenatal investments and none on investment response for adult children. The most attention being drawn on those between 5-15, with a particular interest in schooling time and expenditure. These changes in turn can have significant impacts on the children as they grow up due to externalities, uncertainties and changes in parental investment behavior.

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Chapter 1

Introduction

Adverse shocks may play a critical role in increasing human inequality and underdevelopment in developing countries (Blattman and Miguel, 2010). In many ways, poor countries are poor because they get disproportionate exposure to adverse shocks than richer countries. In fact, developing countries may not only have more frequent insults, but less of a capacity to remediate (Currie and Vogl, 2012; Cerra and Saxena, 2008). Whereas some shocks may be rare and extreme such as genocide, others may be less severe and may be very much a result of human choices, such as the choice of fasting during pregnancy.

The fetal origin hypothesis states that (adverse) shocks during the fetal period can go a long way in explaining inequalities during adulthood. This suggests that the origins of human inequality lie in the womb. A growing literature in economics documents that inequalities in health, education and income emerge early in life (Doyle et al., 2009). 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).

The first chapter of my thesis uses in utero exposure to the Islamic holy month of fasting, Ramadan, as a natural experiment to test this hypothesis. 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 Ravens 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.

In my second chapter, which is joint work with Jorge Agüero, we look at the role which conflict plays in perpetuating inequalities paying particular attention to the aggregate versus sub-national nature of conflict. Instead of taking a fetal origins approach to inequality, we look at those who may have already benefited from childhood investments in schooling and look at how different is the rate at which the skilled population is likely to be missing compared to the less skilled. Most of the current literature has focused on the effects of war on schooling and health investments in children, partly motivated by the early childhood development literature. In contrast, we focus on the the rate at which the already educated cohorts are "missing" compared the less educated cohorts in the context of the 1994 Rwanda genocide. We also contribute to the literature by estimating the effect of the genocide using three datasets: cross-sectional variation using a post-war sample; data before and after the genocide and data from a neighboring country. We find that the genocide reduced the stock of human capital in Rwanda severely. The before-and-after results show that highly educated individuals (i.e., those with primary education or more) are missing at a rate that is 19.4% higher than the less educated. Moreover, Rwanda's average years of schooling is lower by 0.37 years. When comparisons with Uganda are

made, these estimates more than double suggesting that, if anything, the previous findings were biased downwards. Interestingly, when the cross-sectional variation within Rwanda variation in intensity of genocide is exploited there is no evidence of statistically significant differences. This suggests that destruction of the stock of human capital due to the Rwandan genocide was aggregate in nature.

In my third chapter, I review the literature from developing countries on parental investment response to early childhood shocks, to understand whether parents mitigate inequalities or reinforce, and what role do such responses play in child development. Integrating the most cutting edge literature on gender discrimination in parental investment with the most recent literature studying parental responses to fetal shocks, I find the following. Although a couple of papers in the gender bias literature have explored prenatal investment to the child's gender endowment, showing a female disadvantage, very little has been recently done on prenatal parental investment responses to prenatal shocks. Parental investment during age 0-5 have received more attention, whereby effects on breastfeeding duration and vaccines are most commonly explored. Investments tend to discriminate against girls and are reinforcing at this stage. However, the most attention has been received for schooling related monetary and time investments during for those aged 5-15. There is little causal evidence for gender discriminating investments at this stage of the life cycle. The evidence on skill biased parental responses tends to show no responses, compensatory responses but for the most part reinforcing responses. These responses seem to matter for children's development particularly in the context of uncertainty, externalities and when returns to parental investments are affected, such as through better parenting skills.

Whereas the three papers are separate essays, they do complement each other by exploring the relationship between adverse shocks and human inequality and development. Can less severe shocks to one's endowments- from norms/day-to-day activities- have severe effects? How persistent are such effects? I provide evidence that even a small shock at a critical group (of fetuses) can have large and persistent effects over the unborn child's life

cycle. Does war have significant effects on destruction of human capital? What is the nature of severe shocks such as civil wars? Macro or micro? The Rwanda study highlights that when it comes to war, those who have completed primary schooling may be particularly vulnerable, particularly at the aggregate level. Finally, what role do parents play in mitigating insults to their children's endowments? The review on parental investment highlights that parents in general reinforce adverse shocks suggesting that the channels through which insults shape human development, may be not just biological, but social and economic as well.

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Chapter 2

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

2.1 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 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

¹Recently 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.

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

³Currie 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.

⁴Long-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.

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,

⁵There 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.

i.e., pregnant mothers who fast during Ramadan, would be limited to Muslims only. Instead of estimating average treatment effects (ATE), the current literature 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,

⁶Ewijk (2011) also utilizes biological sibling fixed effects for analyzing the general health of children up to age 18.

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 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

⁷The sample is a sub-sample in terms of age cohort but not necessarily exactly the same individuals.

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 itself 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.

⁸The 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.

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.

2.2 Literature Review

2.2.1 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 reprogram-

ming 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.

2.2.2 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 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

⁹AME (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.

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.

2.3 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:

$$S = S(N, S_o, A, B_S, D, \mu, \epsilon_S), \tag{2.1}$$

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 :

$$U = U(C, L_j, S, A, B_U, \epsilon_U). \quad (2.2)$$

Choices are constrained by budget constraints, time constraints, labor supply and sectoral choice functions, in addition to the technology of skill formation (2.1). 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:

$$P_c C^* + P_n N^C = w_j L_j + V. \quad (2.3)$$

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 :

$$N^T + L + E = T, \quad (2.4)$$

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, μ , are all positive.

$$L_j = L(S, A, B_L, D, \mu, \epsilon_L), \quad (2.5)$$

$$j = j(S, A, B_j, D, \mu, \epsilon_j), \quad (2.6)$$

$$\text{Max}_{(j,L,N)} U(C, L_j, S, A, B_U, D), \quad (2.7)$$

subject to (2.1),(2.3) and (2.4) , (2.5) and (2.6) 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.

2.4 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

¹⁰The 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

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

2.4.1 Descriptive Statistics

Table 2.1 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.

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.

¹¹Ewijk (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."

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.

One of the unique features of the IFLS Wave 4 is that for the first time it asks detailed questions about religiosity.¹² Average religiosity among families is rather high in Indonesia with a mean of 2.796 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

¹²Religiosity is a self-reported measure on a scale of one to four, where four is the highest value possible

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.

2.5 Empirical Methodology

2.5.1 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 1-18.¹³

¹³It 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.

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.¹⁴ I demonstrate the usefulness of this approach by showing that households with higher religiosity have stronger effects. Religiosity is not even measured in 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.

2.5.2 Econometric Equations

The traditional OLS formulation is shown in (2.8) as follows:

$$Y_{if} = \alpha + \beta_1 exposure_{if} + \beta_2 X_{if} + FC_f + U_{if}, \quad (2.8)$$

¹⁴IFLS4 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.

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. X_{if} includes age measured in days, age squared, a gender dummy, a dummy for the three weeks post-Ramadan, and calendar month of birth fixed effects.¹⁵ FC denotes all family and community level covariates which are time invariant. 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’.

$$\Delta Y_{[t_1,t_2]} = \alpha + \beta_{1*} exposure_{[t_1,t_2]} + \beta_{2*} \Delta X_{[t_1,t_2]} + \Delta V_{[t_1,t_2]}. \quad (2.9)$$

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. In other words, in the absence of exposure to Ramadan, family members should not have different trends over time.¹⁶

2.6 Results

2.6.1 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.

¹⁵For some specifications, I explored an alternate set of controls for Ramadan month fixed effects. The estimates did not change much

¹⁶There is also a functional assumption that FC, family/community level covariates that may bias the estimates of exposure enter only in a linear and additive fashion

For an overall sample of individuals 15-65 years old, Figure 2.1 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 2.2 and Figure 2.3 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 ¹⁷. 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.

2.6.2 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 2.2 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

¹⁷More religious are those individuals who come from families which have above average self-reported religiosity

into the self-employment sector as adults.¹⁸ 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 health 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 2.3 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 2007.¹⁹ 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

¹⁸In the early version of this paper, I explored effects on wages and years of schooling for sub-sections of the data. In some cases, particularly for males, I found significant and large effects for wages, but in general the results were noisy and not robust. For years of schooling, I found generally negative but statistically insignificant results

¹⁹To 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.

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 2.4, 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 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 2.3 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 2.3, for the religious and less religious households. When estimates from Table 2.6 (religious households sample) are compared to Table 2.7 (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.²⁰

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

²⁰I also tested for robustness of the religiosity measure by using self-reports on number of times one prays. The results were robust

in the labor force. To deal with this concern, Table 2.8 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 2.8 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.²¹

2.6.3 Suggestive Pathways Over the Life Course

What are the possible channels through which labor supply and probability of self-employment are being affected? Table 2.9 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

²¹Although 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 2.8. 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 marital status and interaction of marital 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.

scores are in percentage terms. In contrast to Table 2.1 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 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 2.7 shows estimates for hours spent studying during elementary school and for child labor status.²²

Returning to Table 2.10, 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

²²In 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.

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 2.11 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.

2.7 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.²³ This conclusion is robust to not only household fixed effects, but also to biological sibling

²³In IFLS, I find that self-employed workers have lower years of schooling and worse general health.

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.²⁴

The fixed effect models give us confidence that the selective timing of pregnancies is unlikely to explain these effects.²⁵²⁶ 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.²⁷

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

²⁴It may be the case that mothers time their births differentially, or are more likely not to fast for some children than others. For example, if mothers learn about costs of fasting from first pregnancy, they may be more cautious in their next pregnancy. To address this, I include dummies for birth order for a sub-section of adults in Wave 4. The results are robust.

²⁵One possible concern with robustness exercises with fixed effects is that the fixed effects estimates may just be noise if there is not enough variation within families with respect to exposure or if Ramadan dates are imperfectly measured. If the fixed effect estimates were pure noise in the sense of classical measurement error, one may expect the estimates to be attenuated towards zero. However, the fact that the estimates are generally greater than OLS estimates for the main sample, and reveal no effects for the placebos, suggests that the estimates are not pure noise. One may further argue that the reason why the fixed effect estimates from the non-Muslim population are insignificant and small is because of such measurement error. However, the fact that the much larger less religious Muslim population also gives a similar story as that of non-Muslim population suggests that this may be unlikely

²⁶I also regress cohort size on exposure with controls for the adult sample in Wave 4 and find that there is no effect of Ramadan exposure, which gives me further confidence that there is no selective timing around Ramadan

²⁷As 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.

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 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 is 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.²⁸ That said, one cannot completely rule out other channels in addition to religious fasting. For example, religious mothers are more likely to perform extra rituals and suffer from

²⁸There may be lesser household income during Ramadan because of lesser capacity to work while fasting. But since the effects I find are strongest for the more religious population, higher religiosity has to be purely reflective of income differences. The fact that fixed effects estimates in general show similar effects as OLS estimates, casts doubts that income in general is explaining the results

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.²⁹

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

²⁹Future 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.

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 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.

2.7.1 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.

2.7.2 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. On a base of 15.2 hours worked for the control group, this corresponds to 1.8 more hrs/wk. 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.

2.8 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 coor-

dination 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.

2.9 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 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.³⁰ 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 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

³⁰Self-employment can be thought of as a less skill-intensive sector. In fact, as mentioned earlier, the self-employed have fewer years of schooling than wage workers in the IFLS.

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 take 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 to 1.6% 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.³¹

³¹This estimate is in fact larger than the estimate of birth weight effects from the US, found by Almond and Mazumder (2011), which is consistent with the idea that developing countries may receive more insults and have lesser capacity to remediate them. Although I found no effect on non-Muslims for birth weight outcome using OLS, fixed effects estimates for Muslims are more noisy and have large standard errors leading to statistically insignificant estimates. Since this is self-reported birthweight data it is quite possible that measurement error is attenuating the fixed effect estimates towards zero. Future work may want to use registry data with less measurement error to verify the birth weight effects more convincingly

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% (1.8 hrs/wk) more on average than those who were not, a decade later. In contrast, only a month of exposure to Ramadan in utero leads to 4.5% (1.6 hrs/wk) fewer hours worked. 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 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- choices which 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 different aspects such as gender, rural/urban status, mother's age, socio-economic status and wet and dry seasons, which can help policy makers to target the most relevant sub-group. It may also be important to consider effects of Ramadan on fetal deaths, and how differential gender mortality may bias any gender comparisons. Finally,

disentangling the role parents and society play in mitigating or reinforcing the in utero nutrition shocks remains an important topic of study.

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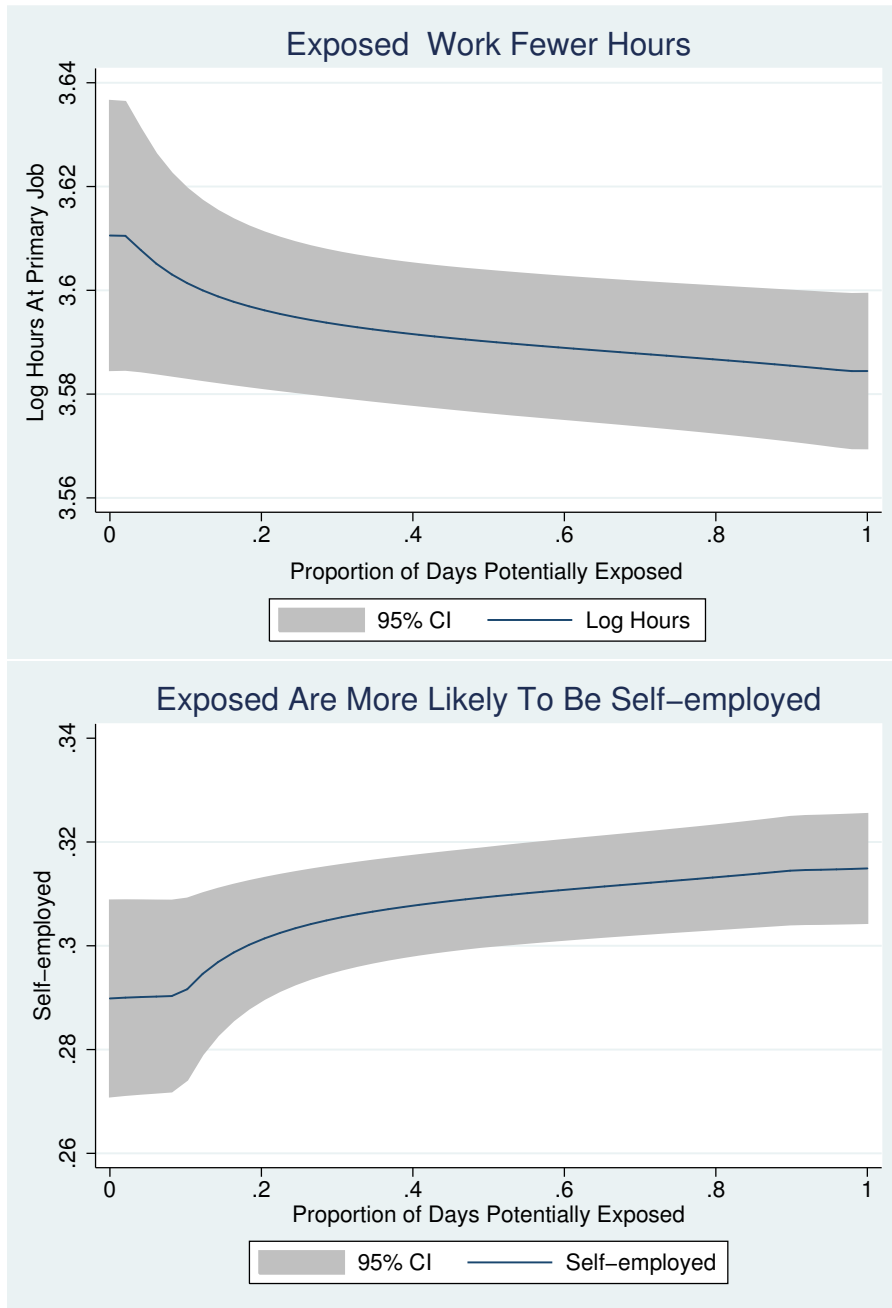
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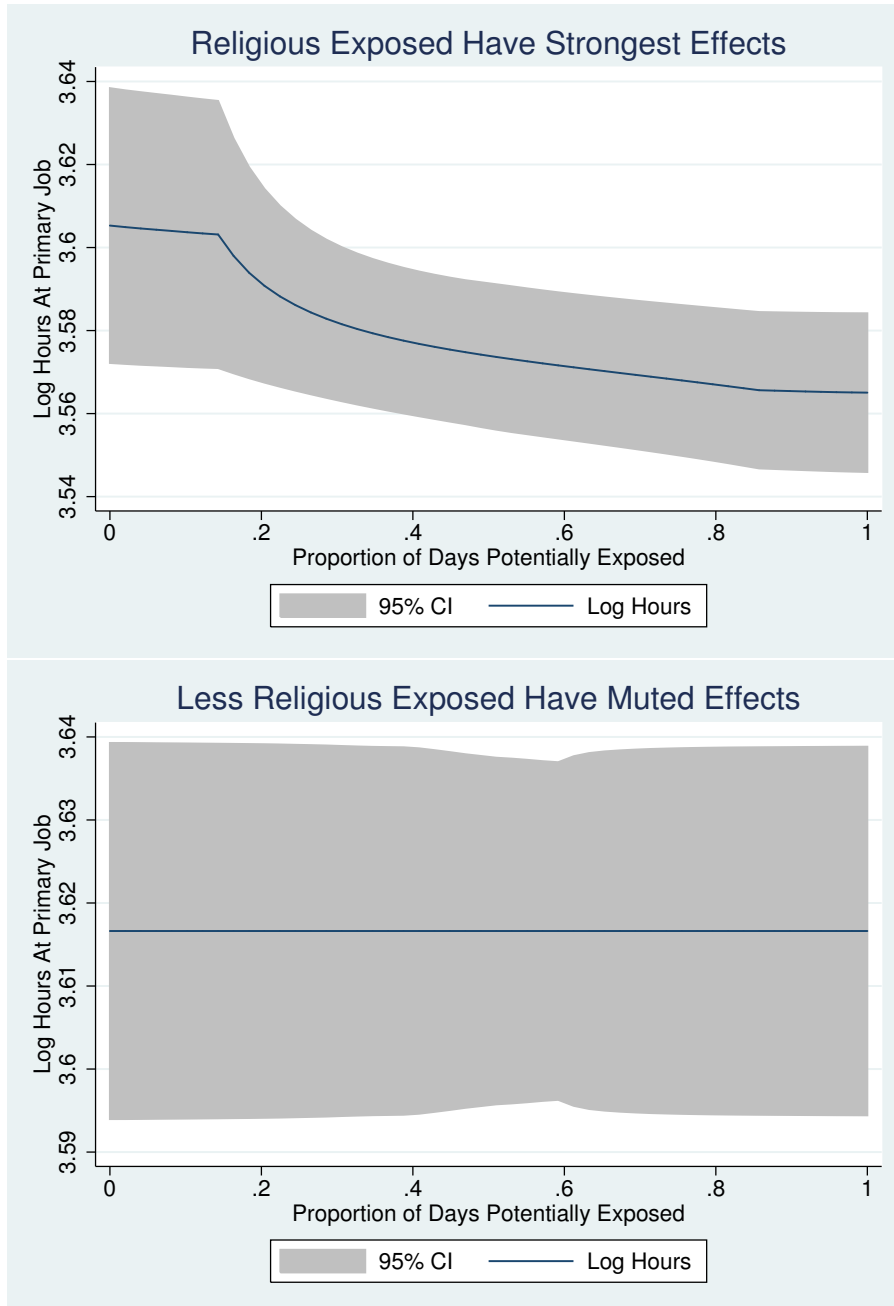
2.10 Appendix: Figures

Figure 2.1: Effect of Ramadan Exposure on Hours Worked and Self-employment Status



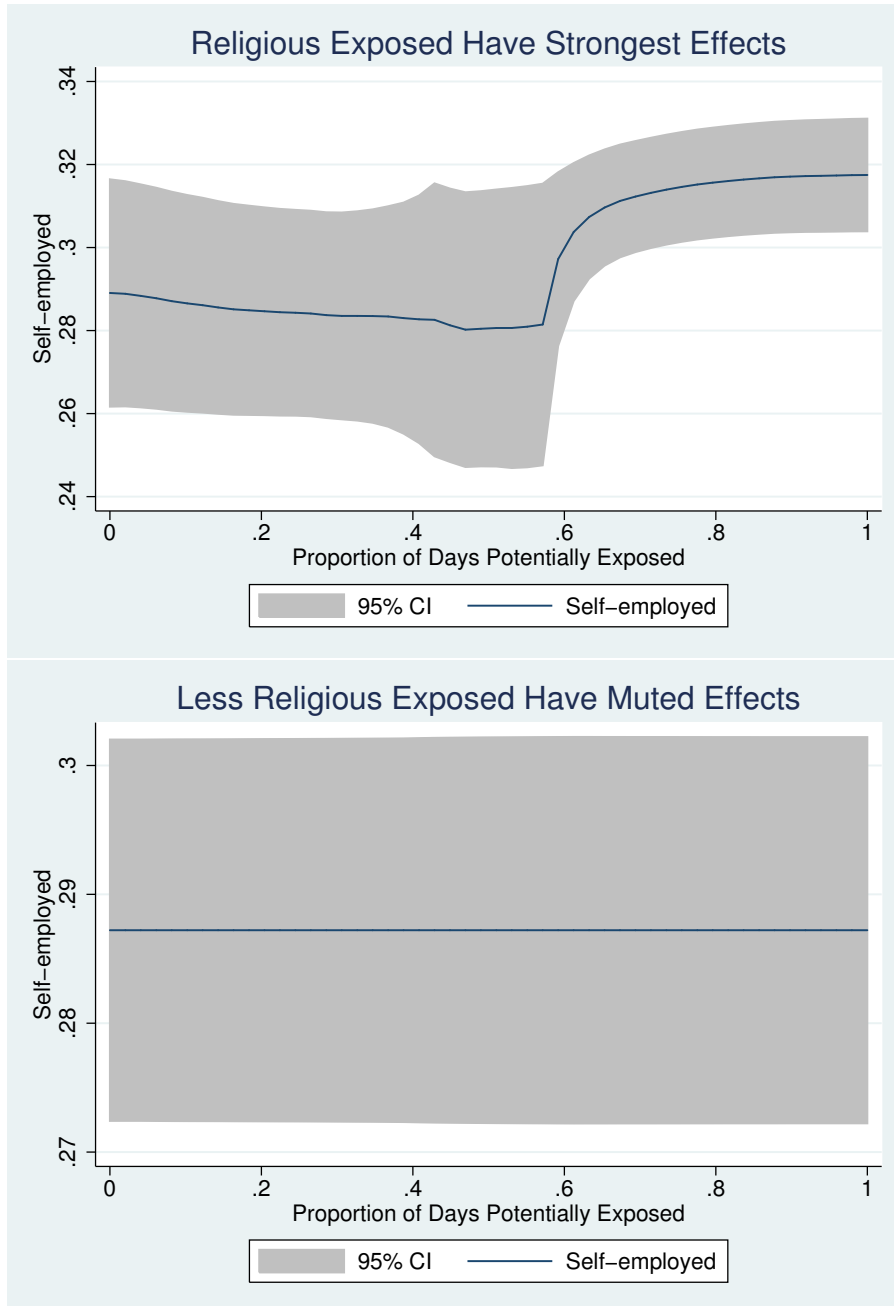
Note: The graphs are local polynomial smooth plots using the Epanechnikov kernel and a bandwidth of 0.405 for Self-employment and 0.436 for Log Hours. The bandwidths were determined using a cross validation technique. Shaded areas represent 95% confidence intervals. Days of potential exposure measures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

Figure 2.2: 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.

Figure 2.3: 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.

2.11 Appendix: Tables

Table 2.1: Summary Statistics by Exposure and Religion

	Muslims	Muslims	Non-Muslims	Non-Muslims	Total
	Exposed	Not Exposed	Exposed	Not Exposed	Total
Wave 4 Adults					
<i>Age: 15-65 years</i>					
Age	33.08 (12.51)	32.82 (12.25)	34.05 (13.43)	34.94 (13.85)	33.16 (12.59)
Male	0.495 (0.500)	0.518 (0.500)	0.513 (0.500)	0.560 (0.498)	0.500 (0.500)
Religiosity	2.781 (0.464)	2.770 (0.468)	2.940 (0.424)	2.903 (0.474)	2.796 (0.463)
Work	0.679 (0.467)	0.678 (0.467)	0.729 (0.445)	0.820 (0.385)	0.685 (0.464)
Log Hours	3.581 (0.666)	3.631 (0.611)	3.527 (0.696)	3.541 (0.734)	3.582 (0.664)
Self-employed	0.308 (0.462)	0.270 (0.444)	0.309 (0.462)	0.392 (0.490)	0.305 (0.460)
Observations	10207	1630	1223	191	13251
Wave 4 Children					
<i>7-15 years</i>					
Cognitive Scores	0.751 (0.226)	0.761 (0.216)	0.735 (0.247)	0.690 (0.267)	0.750 (0.227)
Math Scores	0.584 (0.263)	0.596 (0.270)	0.578 (0.261)	0.600 (0.217)	0.585 (0.263)
Total Scores	0.697 (0.209)	0.706 (0.201)	0.683 (0.223)	0.662 (0.230)	0.696 (0.210)
Observations	3615	543	390	67	4615
Wave 1 Infants					
<i>0-5 years</i>					
Continued ...					

Table 2.1: (continued)

Birth Weight	3.087 (0.550)	3.181 (0.554)	3.126 (0.597)	3.374 (0.584)	3.123 (0.573)
Observations	477	52	339	53	921
Wave 1 Children					
<i>6-14 years</i>					
Hours Studied-Elem.	1.441 (0.263)	1.448 (0.269)	1.497 (0.200)	1.532 (0.154)	1.451 (0.256)
Child Labor	0.0170	0.00673	0.0242	0.0200	0.0168
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.					
The End					

Table 2.2: Summary of Key Estimates

	Muslims	Non-Muslims
VARIABLES	Exposed	Exposed
Log Hours	-0.045** (0.020)	-0.027 (0.071)
Observations	8,051	1,035
Self-employed	0.032** (0.014)	-0.089** (0.041)
Observations	8,373	1,069
Cognitive Scores	-0.059*** (0.018)	0.083 (0.054)
Observations	3,514	379
Math Scores	-0.078*** (0.023)	0.036 (0.048)
Observations	3,521	380
Total Scores	-0.071*** (0.017)	0.062 (0.044)
Observations	3,521	380
Birth Weight	-0.271* (0.153)	0.449 (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 2.3: 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** (0.020)	-0.088*** (0.033)	-0.103*** (0.040)	0.032** (0.014)	0.079*** (0.023)	0.078** (0.036)
Observations	8,051	2,859	2,859	8,373	2,968	2,968
Exp- 1st Tri.	-0.055** (0.024)	-0.029 (0.041)	0.024 (0.083)	0.040** (0.017)	0.072** (0.028)	0.006 (0.071)
Observations	3,317	1,154	1,154	3,444	1,202	1,202
Exp- 2nd Tri.	-0.052** (0.025)	-0.092** (0.042)	-0.095 (0.069)	0.040** (0.017)	0.080*** (0.029)	0.034 (0.056)
Observations	3,211	1,149	1,149	3,320	1,185	1,185
Exp- 3rd Tri.	-0.038 (0.025)	-0.143*** (0.046)	-0.101 (0.135)	0.021 (0.017)	0.077*** (0.028)	0.101 (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 2.4: 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 (0.068)	0.087 (0.116)	-0.008 (0.154)	-0.089** (0.042)	-0.144** (0.067)	0.000 (0.101)
Observations	1,035	377	377	1,069	392	392
Exp- 1st Tri.	0.013 (0.078)	0.260** (0.125)	0.766* (0.446)	-0.073 (0.051)	-0.157* (0.086)	-0.161 (0.377)
Observations	428	162	162	442	170	170
Exp- 2nd Tri.	-0.097 (0.083)	0.008 (0.144)	-0.363 (0.806)	-0.032 (0.052)	-0.084 (0.099)	0.035 (0.392)
Observations	413	157	157	428	162	162
Exp- 3rd Tri.	-0.006 (0.081)	0.079 (0.154)	0.054 (0.613)	-0.125** (0.049)	-0.190** (0.080)	-0.169 (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 2.5: Estimates From Sibling Fixed Effects

VARIABLES	Muslims		Non-Muslims		Muslims		Non-Muslims	
	Log Hours	Log Hours	Log Hours	Log Hours	Self-employed	Self-employed	Fixed Effect	Self-employed
	OLS	OLS-Rest.	Fixed Effect	OLS	OLS-Rest.	Fixed Effect	OLS	Self-employed
Exposed	-0.159* (0.082)	-0.333** (0.137)	-0.434** (0.174)	-0.039 (0.186)	0.140 (0.113)	0.380** (0.179)	-0.108 (0.226)	
Observations	1,310	530	530	233	416	416	171	
Exposed-1st Tri.	-0.240 (0.186)	-0.560* (0.285)	0.220 (1.888)	-0.359 (0.490)	-0.082 (0.179)	-0.900 (1.753)	-0.058 (0.552)	
Observations	523	209	209	98	157	157	73	
Exposed-2nd Tri.	-0.286* (0.168)	-0.332* (0.183)	-0.879 (1.365)	0.155 (0.363)	-0.003 (0.060)	0.159 (2.285)	-0.684 (0.565)	
Observations	522	217	217	93	167	167	63	
Exposed-3rd Tri.	-0.168 (0.140)	-0.415* (0.248)	-0.907 (1.227)	-0.002 (0.343)	0.348* (0.200)	-0.579 (2.851)	-0.023 (0.085)	
Observations	532	215	215	105	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 2.6: 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** (0.026)	-0.131*** (0.043)	-0.133* (0.069)	0.040** (0.018)	0.097*** (0.033)	0.119*** (0.041)
Observations	5,031	1,412	1,412	5,232	1,465	1,465
Exp- 1st Tri.	-0.062** (0.031)	-0.044 (0.058)	-0.013 (0.143)	0.040* (0.022)	0.093** (0.041)	0.067 (0.084)
Observations	2,019	538	538	2,103	566	566
Exp- 2nd Tri.	-0.061* (0.032)	-0.142*** (0.054)	-0.258 (0.167)	0.061*** (0.022)	0.094** (0.043)	0.019 (0.140)
Observations	2,001	551	551	2,070	568	568
Exp- 3rd Tri.	-0.055* (0.031)	-0.245*** (0.063)	-0.304** (0.147)	0.026 (0.022)	0.121*** (0.041)	0.254** (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 2.7: 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 (0.034)	-0.049 (0.049)	-0.073 (0.068)	0.016 (0.023)	0.063** (0.032)	0.051 (0.041)
Observations	3,015	1,447	1,447	3,134	1,503	1,503
Exp- 1st Tri.	-0.033 (0.039)	-0.014 (0.056)	0.019 (0.124)	0.033 (0.027)	0.059 (0.039)	-0.014 (0.084)
Observations	1,294	616	616	1,335	636	636
Exp- 2nd Tri.	-0.028 (0.042)	-0.041 (0.063)	-0.101 (0.115)	0.004 (0.028)	0.072* (0.040)	0.045 (0.072)
Observations	1,207	598	598	1,246	617	617
Exp- 3rd Tri.	-0.016 (0.042)	-0.058 (0.065)	-0.041 (0.177)	0.008 (0.028)	0.033 (0.039)	0.018 (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 2.8: Estimates From IFLS 4 for Labor Force Participation: Muslims by Religiosity

VARIABLES	Muslims			Religious		
	OLS	OLS-Rest.	Fixed Effect	OLS	OLS-Rest.	Fixed Effect
	Work	Work	Work	Work	Work	Work
Exposed	0.004 (0.011)	0.017 (0.017)	0.035* (0.021)	-0.000 (0.014)	0.013 (0.022)	0.007 (0.029)
Observations	11,916	6,260	6,260	7,422	3,646	3,646
Exp- 1st Tri.	0.005 (0.013)	0.007 (0.021)	0.047 (0.034)	-0.014 (0.017)	-0.022 (0.027)	-0.004 (0.062)
Observations	4,853	2,511	2,511	2,972	1,429	1,429
Exp- 2nd Tri.	0.009 (0.013)	0.027 (0.021)	0.061 (0.040)	0.011 (0.017)	0.042 (0.028)	0.068 (0.051)
Observations	4,759	2,526	2,526	2,935	1,428	1,428
Exp- 3rd Tri.	0.000 (0.013)	0.020 (0.020)	0.025 (0.037)	0.000 (0.017)	0.030 (0.027)	-0.038 (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 2.9: Estimates For Test Scores for Children Aged 8-15

VARIABLES	OLS		OLS-Rest.		Fixed Effect		OLS		OLS-Rest.		Fixed Effect	
	Cog. Scores	Math Scores	Cog. Scores	Math Scores	Cog. Scores	Math Scores	Cog. Scores	Math Scores	Cog. Scores	Math Scores	Cog. Scores	Math Scores
Exposed	-0.059*** (0.018)	-0.085*** (0.024)	-0.100*** (0.035)	-0.078*** (0.023)	-0.143*** (0.052)	-0.099*** (0.033)	-0.071*** (0.017)	-0.143*** (0.052)	-0.094*** (0.023)	-0.071*** (0.017)	-0.120*** (0.036)	-0.094*** (0.023)
Observations	3,514	2,084	2,084	3,521	2,087	2,087	3,521	2,087	2,087	3,521	2,087	2,087
Exposed-1st Tri.	-0.027 (0.034)	-0.138** (0.055)	-0.068 (0.132)	-0.053 (0.042)	-0.248* (0.144)	-0.122 (0.076)	-0.045 (0.031)	-0.248* (0.144)	-0.141*** (0.052)	-0.045 (0.031)	-0.136 (0.129)	-0.141*** (0.052)
Observations	1,342	790	790	1,364	828	828	1,345	828	801	1,345	801	801
Exposed-2nd Tri.	-0.000 (0.030)	-0.056 (0.041)	-0.059 (0.083)	-0.032 (0.034)	0.132 (0.098)	-0.074* (0.042)	-0.014 (0.026)	0.132 (0.098)	-0.067* (0.035)	-0.014 (0.026)	-0.017 (0.082)	-0.067* (0.035)
Observations	1,344	790	790	1,366	828	828	1,348	828	803	1,348	803	803
Exposed-3rd Tri.	-0.070** (0.032)	-0.023 (0.042)	-0.163 (0.192)	-0.077* (0.040)	-0.076 (0.217)	-0.095* (0.053)	-0.070** (0.030)	-0.076 (0.217)	-0.045 (0.040)	-0.070** (0.030)	-0.128 (0.167)	-0.045 (0.040)
Observations	1,344	790	790	1,366	828	828	1,348	828	803	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 2.10: Estimates For Schooling Inputs for Children Aged 7-14 in IFLS1

VARIABLES	OLS		OLS-Res.		Fixed Effect		OLS		OLS-Res.		Fixed Effect	
	Log Hrs. School	Log Hrs. School	Log Hrs. School	Log Hrs. School	Log Hrs. School	Log Hrs. School	Child Labor	Child Labor	Child Labor	Child Labor	Child Labor	Child Labor
Exposed	-0.034 (0.024)	-0.046 (0.034)	-0.100* (0.055)	-0.100* (0.055)	0.033*** (0.012)	0.033*** (0.012)	0.039*** (0.014)	0.039*** (0.014)	0.016 (0.023)	0.016 (0.023)	0.016 (0.023)	0.016 (0.023)
Observations	1,815	941	941	941	2,164	2,164	1,117	1,117	1,117	1,117	1,117	1,117
Exposed-1st Tri.	0.046 (0.048)	0.004 (0.066)	-0.144 (0.262)	-0.144 (0.262)	0.037 (0.029)	0.037 (0.029)	0.035 (0.044)	0.035 (0.044)	0.130 (0.307)	0.130 (0.307)	0.130 (0.307)	0.130 (0.307)
Observations	746	382	382	382	887	887	446	446	446	446	446	446
Exposed-2nd Tri.	-0.043** (0.021)	-0.024 (0.016)	0.196 (0.230)	0.196 (0.230)	0.000 (0.001)	0.000 (0.001)	0.000 (0.003)	0.000 (0.003)	-0.014 (0.153)	-0.014 (0.153)	-0.014 (0.153)	-0.014 (0.153)
Observations	722	396	396	396	863	863	472	472	472	472	472	472
Exposed-3rd Tri.	-0.145*** (0.040)	-0.139** (0.059)	-0.255 (0.227)	-0.255 (0.227)	0.019 (0.022)	0.019 (0.022)	0.024 (0.018)	0.024 (0.018)	0.027 (0.083)	0.027 (0.083)	0.027 (0.083)	0.027 (0.083)
Observations	743	390	390	390	907	907	474	474	474	474	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 aged15-22 (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.

Table 2.11: Birth Weight Estimates From IFLS 1 for Those Aged 15-20 in IFLS4

VARIABLES	Birth Weight	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<0.1. 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.

Chapter 3

War and the Destruction of Human Capital

3.1 Introduction

Civil conflict has potentially serious consequences for economic development. A growing literature documents the effects of conflict on education (Shemyakina (2006), Blattman and Annan (2007), Akresh and De Walque (2008)) and health (Alderman et al. (2006), Akresh et al. (2009), Agüero and Deolalikar (2012)). However, not much is understood about the quantitative importance of different channels through which conflict affects human capital accumulation (Blattman and Miguel, 2009). This paper studies the medium to long-term effects of the 1994 Rwanda genocide, which is known to be the deadliest civil war of the 1990's, killing up to approximately 1 million people in a span of about 100 days (Murray et al., 2002). Our paper is unique in two major respects. First, in contrast to

most recent studies which focus on the effects of conflict on human capital investments in children of school going age (such as changes in child height and grade progression), we study the effects of the Rwandan genocide on the stock of human capital (education) as a potential source through which genocide may have long-term consequences on human capital accumulation and economic development in general. Second, we identify the effects of conflict by applying the differences-in-differences methodology on three census data sets at the sub-national as well as national level.

A key contribution of this paper is to focus on the stock of human capital (educated people) rather than educational attainment of people who were of school going age at the time of the genocide. This is important for at least four reasons. First, stock of human capital has been shown to be correlated with not only aggregate output across the globe and over time, but is also associated with social outcomes such as fertility and schooling of children (see for example Barro and Lee (1994, 2010), Breierova and Duflo (2004), Cutler et al., (2006), Lucas (1988) and Mankiw et al.(1992)). Second, the dropout age in low-income countries is low-around 11 in Rwanda-leaving a large proportion of the population out of these studies. This limitation is amplified when the war is relatively short as in the case of 100-day Rwanda genocide.

Third, in contrast to most of the current literature which has focused on children of school going age or younger, this paper focuses on older individuals who are at least as likely to be targeted during war than children. Figure 3.1 shows age and sex distribution of global war casualties in 2000. As is clear, although a large number of children died in war, the most deaths, for either males or females, are concentrated in those aged between

15-44. This suggests that older individuals are at least as potentially vulnerable, if not more, to war related mortality shocks than young children. Recently, Akresh et al. (2012) study the impact of the 1967-1970 Nigerian civil war on adult height. In contrast to earlier studies, the authors find that the children who were exposed to war during adolescence had the largest impact. Early childhood matters. But there exists certain types of shocks- such as wars- and certainly channels- violent deaths vs disease induced deaths- the most vulnerable population for which may not be children. Even if children were as likely to be killed from war than adolescents and adults, violent deaths may be far more concentrated among adolescents and adults rather than younger children.

Last, irrespective of the age group one studies, it is important to study effects of shocks- such as war- not only on investments but also destruction in the stock of human capital. If educated people are less likely to be killed or displaced in war, then it increases the returns to investing in education. Alternatively, if educated people are more likely to be killed or displaced then it raises questions about the value of education. Or at the very least, it is a call for providing complementary goods along side education which ensure that the investments made in schooling are not made redundant.¹ It may very well be the case that during critical periods in a country's history, such as the time just before a genocide or a revolution against educated elites, the returns to schooling may be negative for they may make one more likely to be attacked. Given that the Rwanda genocide is known to be

¹For example, Barro and Lee (2010) find that returns to secondary schooling is higher than primary schooling, which even has negative returns for many countries. Does this imply one should not invest in primary schooling? To the contrary, it suggests that investing in just primary schooling is not enough and should be complemented by secondary schooling

targeted at the more educated Tutsis as well as moderate Hutus, it is not clear how high the returns from schooling were at the time just before the genocide.

Other than asking an important yet ignored question, our identification strategy involves applying the differences-in-differences methodology on three census data sets at the sub-national as well as national level. The identification of the effects of conflict on economic development represents an empirical challenge for at least two reasons. First, it is difficult to isolate the effects associated to conflicts from other possible confounding factors including political and economic instability (Miguel and Blattman, 2009). Second, the use of household surveys relies on “survivors”. As the number of deaths increase during conflicts, it creates a change in the demographic composition of a country. Those who are interviewed after a conflict ends are unlikely to be a random sample of the population. If educated people are targeted during a genocide, then educated people may more likely to be missing. Estimating the effects of the conflict based on a sample of survivors could be biased if the probability of survival is correlated with the outcome of interest. If pre-conflict indicators such as education levels affect the probability of survival then since most development-related outcomes are likely to be correlated with these variables, the resulting estimates will be biased.

A possible solution is to obtain information about those who died from those still alive post-conflict. DeWalque (2010) and DeWalque (2005) use this method. The former uses the maternal mortality module of the Demographic and Health Survey of Rwanda in 2000 which asks reproductive-age women about the survival of her siblings as well as their gender and age. For those siblings who died, the DHS also collects information about

the year of death. Socio-economic variables are inferred by assuming that siblings shared similar levels of education. The authors use this information to identify the demographic characteristics of the dead.

However, two sources of non-random selection bias emerge. First, deaths are registered based on surviving women. If complete families were killed (or displaced out of Rwanda) or if a survivor is a male these families are not going to be included in the DHS. Thus, the death records are representative of families where a women member of the family survived and that is unlikely to be a random sample. Second, the DHS itself is not a random sample of all women. It is only for women of reproductive age. Those aged between 15 and 49 are subject to the maternal mortality module. Thus, the sample for the analysis is only for the subset of households where at least one survivor was a woman between the ages of 15 and 49 and this sample, almost certainly, is not representative of the population at large. For example, if there is an increase in the number of child headed households, it suggests that all adult members of the family were most likely killed. DHS maternal mortality schedule could not capture such cases.

An alternate method used by demographers is the reconstruction approach based on demographic accounting. Between any two dates, “excess mortality” can be inferred as a residual from changes in population unexplained by changes in number of births, deaths, immigrants and emigrants.² To get an estimate on the excess mortality, one needs to

²One of the most careful reconstruction exercise has been carried out, for Cambodia, by Heuveline (1998 (a), 1998 (b), 2001 (a), 2001 (b)). To compare demographic effects of civil war between 1970 and 1980, he projected data from the 1962 census to get an estimate of the 1970 population, which served as a baseline. An estimate of 1980 population is derived from by backward projection of 1993 electoral lists data. In addition, estimates of “natural” mortality and of “natural” migration are used to project backward the 1980 estimates to 1970 and project forward the 1970 estimates into 1980. Using the backward and forward projections,

know how different the actual trend is from the potential trends in population which would have existed in the absence of the shock. The solution of the reconstruction approach is to exclusively rely on temporal variation to determine the excess mortality. Moreover, it uses data on mortality, fertility and migration flows during the time of the genocide. As Heuveline (1998a) states, such data is not only hard to get but is highly prone to measurement error. Furthermore, the reconstruction approach is not only sensitive to over /underestimation of population size at the start relative to the end of the period, but also to migration, the other cause of changes in cohort size.

In this paper, we introduce a differences-in-differences methodology, which will identify the demographic changes generated by the 1994 genocide in Rwanda that avoids the sample selection problems described above. We estimate missing persons by comparing the size of cohorts in the post-genocide 2002 Rwandan census against their size in the pre-genocide 1991 census. Temporal variation - comparing before vs. after the genocide- is combined with the spatial variation of the conflict to further identify the characteristics of those missing. Rwanda is compared with Uganda, a neighboring country which despite experiencing civil war, like Rwanda, did not experience genocide in the same period.

To avoid any contamination due to fertility changes generated by the genocide and to not confound our estimates from naturally high infant mortality rates, we focus on those who were older than 12 years in 1991. Similarly, to disentangle our results from the naturally high mortality of the older cohorts, we focus on those aged 60 or less. To address concerns regarding immigration, we limit our attention to native born only. As an

residuals from the actual vs. projected estimates are calculated. The number of “excess mortality”, is estimated from the average of the backward and forward projection residuals

overwhelming majority of native born were present in their place of birth at the time of the census was carried out, this gives us some confidence that migration may not be driving our results. Lastly, we explore spatial heterogeneity in intensity of conflict within Rwanda to explore if the genocide was more of an aggregate nature.

The genocide reduced the stock of human capital in Rwanda severely. The before-and-after results show that highly educated individuals (i.e., those with primary education or more) are missing at a rate that is 19.4% higher than the less educated. Moreover, Rwanda's average years of schooling is lower by 0.37 years. When comparisons with Uganda are made, these estimates more than double suggesting that, if anything, the previous finding were biased downwards. Interestingly, when the subnational variation within Rwanda variation in the intensity of conflict is exploited for the same measures, there is no evidence of statistically significant differences. This suggests that the losses in the stock of human capital due to the Rwandan genocide were aggregate in nature.

3.2 Background

Rwanda is a landlocked country in East Africa with a small but highly dense population. It neighbors Uganda, Tanzania, Burundi and Congo (see Figure 3.2). For about 100 days from April 6 1994, when the then President of Rwanda, Habyarimana's plane was shot down, more than 500,000 people of largely Tutsi ethnicity were massacred by the Hutus. Estimates of the death toll have ranged widely from 500,000 to about 1 million people (Prunier, 1995). This genocide did not happen in a vacuum but was the

result of long standing rivalry between the majority Hutu peoples and a relatively more urbanized and educated Tutsi minority.

Although the two distinct ethnicities of Hutu and Tutsi did exist even before Rwanda was colonized, the Belgian colonizers sharpened the divide by discriminating against the Hutu majority, based on various factors including physical appearance. Things changed between 1950's to 1960's. The Hutu rallied for, and won, Rwandas independence, but not without a violent campaign against Tutsi's which led to numerous deaths and large-scale refugee movements into neighboring countries such as Uganda.

Rwanda continued to be ruled under different Hutu military dictatorships for the next three decades. During this period there was relatively less violence within Rwanda. And then came the year 1990. A rebel Tutsi group, The Rwandan Patriotic Front (RPF), invaded northern Rwanda from Uganda. The Rwandan civil war had begun as the Rwandan armed forces (Forces Armes Rwandaises, FAR) responded. More than three years of ethnic violence led to killings and emigration of numerous Hutus from northern Rwanda accompanied by similar but localized attacks against Tutsis in the south. Under international pressure, the Hutu-led government of President Habyarimana agreed to cease-fire in 1993 , sharing significant power with the Tutsi RPF group. This, however, proved to be unstable. In 1994 when Habyarimana's plane was shot down, the Hutu extremists unleashed what will be remembered as one of the most horrific genocides in human history. For in depth history of Rwanda, the reader may want to explore books written by G. Prunier (1995), C. Newbury (1988), D. De Lame (1996), F. Reyntjens (1994) and J.P. Chretien (2000), among others.

As a response to the genocide, many fled to neighboring countries such as Congo and Burundi. Because of the reality of migration, we focus on missing people rather than deaths. But having said that, between 1994 and 2002, many of these migrants returned back because of conflict in those countries. In 1996 and 1997, for example, violence in Burundi, Tanzania and Congo forced numerous Rwandans to repatriate (World Bank, 2003). A 2001 Rwandan nationally representative survey, *Enquête Intégrale sur les Conditions de Vie des Ménages*, shows that approximately 88-89% percent of individuals currently live in the province in which they were born. Thus although not all those we find as missing were killed, the majority are most likely to have been killed. This conclusion is further strengthened from data on net migration collected from World Development Indicators which show that net migration was about as little as 15,327 in 1990, fell to about -1.5 million by 1995 but rebounded to a positive 1.8 million by 2000, suggesting that if anything between 1990 and 2000, there has been a net positive migration of about 300,000 people.

3.3 Data

Main data are 10 percent random samples of the 1991 and 2002 Rwanda and Uganda population censuses (obtained from IPUMS international: <https://international.ipums.org/>).

The samples are restricted to native borns only.^{3 4}

³Comparisons with Uganda are made for two main reasons. First, its also an East African country so that trends which are common at the regional level for East Africa are not likely to explain the results. Second, Uganda is the only country African country in the IPUMS dataset for which census in available in 2002 and 1991, the period for which Rwandan data is available

⁴There is a census available for Rwanda pre-genocide from the 1970s. However, this data is not in IPUMS, and was rather hard to make use of. Future work may want to explore such data as additional source of variation for trends specific to Rwanda

3.3.1 Missing Rwandans

Figure 3.2 shows raw data for log population before (1991) and after the genocide (2002) in Rwanda. There are many more young than old, as one would expect in a developing country like Rwanda. If the conflict did lead to missing people, one would be able to observe this by comparing the pre-genocide vs post-genocide population size series. The 2002 population is strictly less than 1991. For all age cohorts. Figure 3.3 shows the log differences between 2002 and 1991 across birth year cohorts more clearly. There are fewer people across all birth cohorts. It is interesting to note that, those 12 or younger in 1991, are at least as likely to be missing than the older individuals. This confirms the basic insight of Figure 3.1 that young adults are particularly vulnerable to mortality and displacement than young children. Hence, the need to study the adolescent and adult sample.

To get a better idea of how big the changes in Rwandan population are, we recompute Figure 2.3 in Figure 2.4 whereby Rwanda is compared to Uganda. Rwanda's series is particularly distinct. It has a much larger magnitude of missing than Uganda. In the main analyses of this paper, we will restrict the sample to those 13 or older so as to limit our sample to those old enough to have completed primary schooling. It is worth noting, that although Rwandans are missing at higher rates than Ugandans, the Rwandan series is very flat for the bulk of individuals aged between 13-45 in 1991. Since our sample is focused mainly on this sub-sample, it suggests that there may not be much heterogeneity by age in our adolescent-adult sample.⁵

⁵For the rest of the paper age dimension is not exploited further. We did run regressions individuals heterogeneity by age and they confirmed the basic insight of these figures i.e. there is not much heterogeneity by age within old enough adolescents and adults

3.3.2 Cohort Analysis and Education Variable

Each census contains region and year of birth of all individuals as well as gender and education level. Those defined as having no schooling or less than 6 years (primary) level of education are labelled as “low” educated. Those undocumented are assigned missing values. We limit our main sample to those born before 1979 so that we do not have natural infant mortality biasing our results and so that individuals are old enough to have completed their primary schooling. This makes it less likely that someone identified as low educated in 1991 (13 or more years old at the time) will be still in the process of completing their primary schooling, and thus likely to become high educated by 2002. We also exclude those born before 1930 so that naturally high mortality rate for the old does not confound our estimates of excess mortality. All empty cohorts and cohorts with no observed high educated people were omitted along with corresponding cohort from opposite census year.

A cohort for Rwanda is defined by birth year, gender, whether completed primary schooling or not, province of birth and census year. For Uganda, instead of province of birth, we have district of birth. As a result, we exclude all districts for whom we did not have before and after data. Information about spatial intensity of genocide within Rwanda comes from www.genodynamics.com and has been used by other scholars like Akresh et al.(2008). Measure A is a measure of genocide intensity. It is proportion of days during genocide when killings occurred. Measure A is a continuous variable. However, we define a discrete version of A for some of our analysis which assumes value 1 when a given regression has higher than average exposure to days with attacks, and 0 otherwise. This

measure has also used by Akresh et al. (2008). See data appendix (3.8) for details on other measures used.

By aggregating data at cohort level we address the problem of clustered standard errors. The assumption is that although randomization may not happen at individual level, the unit of randomization is at the cohort level. This is one of the solutions to the problem of clustering proposed by Bertrand et al. (2004). The cohort being defined by year of birth, region of birth, gender, education and census year. Sample weights are defined by inverse of cohort size and are used in all weighted regressions. All regressions use robust standard errors which accounts for heteroscedasticity in distribution of error terms.

3.3.3 Summary Statistics

Row 1 of Table 3.1 shows cohort size in 1991 versus 2002 for Rwanda and Uganda. Both Rwanda and Uganda witness a decline in cohort size. Uganda has been going through a low-intensity civil war during the same period as well. Row 2 explores the cohort size of educated individuals (those completed primary schooling). In 1991, although Rwanda and Uganda had a similar cohort size of educated people of around 1000, twice as many educated Rwandans are missing than educated Ugandans between 1991 and 2002. It may be the case that a similar trend exists for less educated individuals, so that the educated are not particularly more likely to be missing. Row 3 explores ratios of educated to less educated individuals. In contrast to Rwanda, where in 1991 for every 100 individuals who did not complete primary schooling there were 40 who did, in Uganda the corresponding number was 78. But by 2002, in contrast to Uganda where we see that the ratio of educated

people has increased to almost parity, in Rwanda it falls even further to around 31. A clear divergence. A similar story emerges when average years of schooling is explored. In the 1991 sample, it was about 3 years for Rwanda versus 3.8 for Uganda. But by 2002, although Uganda witnesses an increase in years of schooling to 4.2, Rwanda witnesses a decline to 2.7.

3.4 Empirical strategy

Estimating the effect of a conflict is complicated, among other factors, by the identification of the control group. The main missing data problem comes from the inability to observe what would have happen to the treatment group—those exposed to the conflict—in the absence of the war. The literature has used several approaches based on different assumptions about the nature of the control group. Several papers including (Mansour and Rees (2012), Akresh, Lucchetti, Thirumurthy (2012), Akresh et al. (2011)) use a post-conflict cross-sectional survey and exploit (within-country) spatial variation in the intensity of the conflict (the intensity could include total absence of conflict). In this case the data consists of observations varying by cohort i and space j . The following equation represents this methodology

$$y_{ij} = \beta_1 G_{ij} + \alpha_i + \alpha_j + \epsilon_{ij} \tag{3.1}$$

where y_{ij} is the outcome of interest and α_i and α_j are fixed effects at the cohort and space-level, respectively. Thus, β_1 is the parameter of interest as it captures the difference

in y_{ij} for the treatment group comparing its actual value against the predicted value based on the observed y_{ij} of the control group.

This model assumes that all differences in the treatment and control groups are captured by α_j and that there are not time-varying unobserved characteristics. Note also the impact evaluated here by β_1 is comparing areas with high against low intensity of conflict. In most cases (references needed) the levels of violence in the low-intensity areas is not zero, as in the case of Rwanda, thus the effect measured by β_1 could be underestimating the true effect because the control group has been “contaminated.” This possible bias is amplified when the country-wide effect of the conflict lead to small variation across spaces or regions within the country. If the only data available is post-conflict cross-section then we cannot test for the possible bias in β_1 .

Suppose that there is another cross-sectional survey that took place prior to the conflict period or when the war was just starting. The observations are now given by cohorts (i), space (j) but also time, denoted by (t). Thus, equation 3.1 can be rewritten as

$$y_{ijt} = \beta_2 G_{ijt} + \alpha_i + \alpha_j + \alpha_t + \gamma_{ijt} X_{ijt} + \epsilon_{ijt} \quad (3.2)$$

Equation 3.2 incorporates two new sets of parameters. First at is the survey-year fixed effect and second, the γ_{ijt} captures the effect of the cross-products (by pairs included in X_{ijt}) of cohorts, space and time. In this equation G_{ijt} is now the triple interaction and β_2 is the parameter of interest. Unlike β_1 , β_2 might exhibit less bias because the effect is capturing differences with respect a period of “peace” or less conflict. This is the approach followed, for example, by Akresh et al. (2008). An alternative to model 3.2 is to observe

not a pre-conflict dataset but to use a different country as an alternative control group. In this case the model is given by

$$y_{ijc} = \beta_3 G_{ijc} + \alpha_i + \alpha_j + \alpha_c + \gamma_{ijc} X_{ijc} + \epsilon_{ijc} \quad (3.3)$$

In equation 3.3 we substituted the index t (year of survey) for c that indexes the countries. Like 3.2, equation 3.3 has an effect that accounts for nation-wide effects. Whether equation 3.2 or 3.3 are better at capturing these nation-wide or aggregate effects depend on the validity of the external country as a comparison group and the presence of violence in the pre-conflict data. The choice between equation 3.2 and 3.3 depends on what datasets are available to the researchers. Consider now the case where there are two surveys per country

$$y_{ijct} = \beta_4 G_{ijct} + \alpha_i + \alpha_j + \alpha_c + \alpha_t + \gamma_{ijct} X_{ijct} + \epsilon_{ijct} \quad (3.4)$$

in this case the effect we can account for country and time unobserved factors. A key advantage of this paper is the access to all four data sources. This will allow us to compare the sensitivity of the estimates depending on the assumption about the aggregate nature of the genocide. In the next section we present the empirical framework for the case of Rwanda.

3.5 Results

Subnational Estimates

If we just had a post-genocide survey and only exploited spatial intensity of the genocide within Rwanda, we would be estimating the equivalent of equation 3.1 for Rwanda. For our analysis, each observation is a cohort defined by birth year, province of birth, gender and census year, and is weighted by the cohort size. Our analysis controls for gender and year of birth fixed effects. We treat Byumba and Umutara provinces as one since between the two census the Rwanda government merged these two provinces. To explore the spatial intensity of genocide, we explore the number of days, per province, that the genocidal killings occurred in Rwanda (Measure A). Using Measure A, the effect of the genocide on average years of schooling is estimated in Column (1) of Table 3.2. The results show that the higher the genocide intensity, the higher the average years of schooling. At first this may sound surprising, as one does not expect genocide to have a positive effect on schooling. However, once we realize that the genocide may be targeted in areas which had higher schooling levels (Tutsis), these results are suggestive of selection effects.

To control for time-invariant province level unobservables which may be confounding the estimates from Column (1), we take advantage of our unique pre-genocide census in 1991. Many conflict studies, do not have such data available, and are left with exploiting spatial and cohort variation only, instead of time variation. When we estimate the equivalent of equation 3.2 for Rwanda in Column (2), we find that there is no evidence of any significant effects of the genocide intensity within Rwanda on years of schooling. This is an intriguing result, as previous research (for e.g. DeWalque 2010) suggests that genocide is

correlated with excess mortality of educated people. It may, however, be the case that the Rwandan genocide is of an aggregate nature so that subnational comparisons are biased downwards and do not reveal the true effects on education.

Aggregate Estimates

Column (3) shows estimates of the effect of the genocide at the national level rather than the sub-national level as was done in the previous section. Instead of exploiting variation within Rwanda, only variation over time is exploited to analyze the aggregate effect of the genocide. The estimates reveal that genocide lowers the stock of average years of schooling by as much as 0.37 years of schooling! This estimate is significant more for at least two reasons. First, the magnitude is large- more than one third of an year of schooling is lost. And second, because estimates of effect of conflict on investment in child schooling by Akresh et al. (2008) also show a remarkably similar estimate. This suggests that destruction of human capital is not only large, it is broadly comparable to changes in investment in child schooling.

An alternative to estimating equation 3.2 is to observe not a pre-conflict dataset as a control but to use a different country as an alternative control group. Uganda, which is Rwanda's neighbor and which did not suffer any genocide during the time period, is the only other African country we could identify which has census data for 1991 and 2002 as well as relevant data on years of schooling. Column (5) shows estimates for equation 3.3 in which post-genocide comparisons between Rwanda and Uganda are carried out. Consistent with the summary statistics in Table 3.1, we find that there is 1.42 fewer years of schooling

in Rwanda compared to Uganda. This is a massive effect. However, it may also be reflecting unobservable differences between Rwanda and Uganda which would have existed even in the absence of genocide.

To explore how much of this difference may be driven by time varying factors which would have changed in Rwanda even in the absence of genocide, I estimate the counterpart of the Column (3) for Uganda in (4). In stark contrast to Rwanda, Uganda witnessed a 0.49 increase in years of schooling during this time.

One may be skeptical about the magnitudes in cross-sectional comparisons in 2002 between Rwanda and Uganda, and about the time varying changes within Rwanda for they may be reflecting preexisting changes or because we do not understand the counterfactual trend in the absence of genocide very well. We now carry out a differences- in-differences method, as in equation 3.4, exploiting both temporal variation and across country variation to address such a concern. Column (6) shows that after controlling for temporal and country differences, 0.83 years of schooling is lost in Rwanda compared to Uganda between 2002 and 1991. This significant estimate is more than double than the loss of 0.37 years of schooling found by using a pre-genocide control within Rwanda, suggesting that if anything the massive effect within Rwanda may be biased downward.

3.5.1 Alternate Measure of Human Capital: Ratio of Highly Educated to Less Educated

Table 3.3 carries out a similar exercise as in Table 3.2, but with log ratio of educated to less educated people. Column (1) shows that there are more highly educated people compared to less educated ones in areas which witnessed more days of conflict. At

first this may sound puzzling, just like the estimate from Column (1) in Table 3.2. However, once one realizes that the genocide is known to be targeted at Tutsis who were generally more educated than Hutus, it should not be surprising that despite the genocide, the regions where conflict was most intense still has a higher ratio of educated people. This suggests that just utilizing within country spatial variation in genocide is not enough, if one does not have a good control.

To control for time-invariant province level unobservables which may be confounding the estimates from Column (1), we take advantage of our unique pre-genocide census in 1991. When we estimate the within difference model for Rwanda in Column (2), we find that similar to the estimates for years of schooling, there is no evidence of any significant effects between cohort size of educated and less educated. This provides us further confidence that estimates for war's effects on stocks of human capital may be biased downwards if they only consider subnational comparisons. Nonetheless, it is important to verify if indeed aggregate effects are also found for the ratio of highly educated to less educated measure.

Column (3) shows estimates of the effect of the genocide at the national level rather than the sub-national level as was discussed above. Instead of exploiting variation within Rwanda, only variation over time is exploited to analyze the aggregate effect of the genocide. The estimates reveal that genocide lowers the stock of educated people versus less educated people by as much as 19.4% !

An alternative to estimating equation 3.2 is to observe not a pre-conflict dataset as a control but to use a different country as an alternative control group. Column (5) shows estimates for equation 3.3 in which post-genocide comparisons between Rwanda and

Uganda are carried out. Consistent with the summary statistics in Table 3.1, we find that the ratio of educated to less educated is 79.1% lower in Rwanda compared to Uganda. This is a massive effect, consistent with the large Table 3.2 estimates. However, it may also be reflecting unobservable differences between Rwanda and Uganda which would have existed even in the absence of genocide.

To explore how much of this difference may be driven by time varying factors which would have changed in Rwanda even in the absence of genocide, I estimate the counterpart of the Column (3) for Uganda in (4). In stark contrast to Rwanda, Uganda witnessed a 27.8% increase in size of educated to less educated people.

As mentioned earlier in discussion of Table 3.2 results, one may be skeptical about the magnitudes in cross-sectional comparisons in 2002 between Rwanda and Uganda, and about the time varying changes within Rwanda for they may be reflecting preexisting changes or because we do not understand the counterfactual trend in the absence of genocide very well. We now carry out a differences-in-differences method, as in equation 3.4, exploiting both temporal variation and across country variation to address such a concern. Column (6) shows that after controlling for temporal and country differences, there are 44.8% fewer educated people compared to less educated ones in Rwanda versus Uganda, before versus after the genocide. This significant estimate is more than double than the loss of 19.4% in stock of educated versus less educated people found by using a pre-genocide control within Rwanda, suggesting that if anything the massive effect within Rwanda may be biased downward.

The results from Table 3.3 are very encouraging as they corroborate the findings in Table 3.2. However, the fact that there is no subnational effects on years of schooling and ratio of educated cohorts raises some interesting questions: Was the genocide so homogenous that there is no effect on just the educated cohorts (without comparing with less educated cohort)? Or is that that there is subnational evidence as well, but for educated as well as uneducated so that the differential effect is zero? Similarly is the years of schooling effect (say based on temporal variation within Rwanda) driven by excess losses in educated cohorts compared to less educated cohorts at the aggregate level or is that the rate at which educated cohorts were increasing is less than that of less educated cohorts? To explore these questions, we now look at the pure effect at the subnational and aggregate level on size of educated cohorts.

3.5.2 Educated Cohort Size Variation in Rwanda

Table 3.4 carries out a similar exercise as in Tables 3.2 and 3.3, but with log ratio of educated to less educated people. In contrast to Tables 3.2 and 3.3, Column (1) shows that there are less highly educated people compared to less educated ones in areas which witnessed more days of conflict. This suggests that although there were more educated to less educated people in high vs less conflict intense regions of Rwanda (Tables 3.2 and 3.3), educated people are also missing but they are missing at a lower rate than less educated in these areas. The difference-in- difference model in Column (2) further supports this hypothesis, as even at the subnational level, there is evidence of fewer educated people. Results from Tables 3.2 and 3.3 showed that aggregate estimates are often very large. To

verify if the large aggregate effect also holds of the effect on educated cohort size, temporal variation within Rwanda is explored in Column (3). The estimates reveal that the Rwandan genocide indeed lowered the stock of educated people as as much as a staggering 47.6%, in contrast to 19.4% (Table 3.3) effect when effects on less educated are taken into account.

An alternative to using a pre-conflict dataset as a control is to use a different country as an alternative control group. Column (5) shows estimates for equation 3.3 in which post-genocide comparisons between Rwanda and Uganda are carried out. We find that post-genocide, the size of educated cohort was 26.8% lower in Rwanda compared to Uganda. This is in contrast to the massive 79.1% effect found in Table 3.2. This suggests that he size of uneducated cohort was much higher in Rwanda versus Uganda post-genocide.

To explore how much of this difference may be driven by time varying factors which would have changed in Rwanda even in the absence of genocide, I estimate the counterpart of the Column (3) result for Uganda in (4). Between 2002 and 1991, the cohort size of educated fell by 24.1% even in Uganda. Given how big and negative this impact is, it highlights the general need to take into account time variation for placeboes, to get more accurate estimates of the impact of conflict . Nonetheless, Column (6) which shows the estimates for the differences- in-differences model shows even that after controlling for temporal and country differences, there are 19.8% fewer educated people compared in Rwanda versus Uganda, before versus after the genocide. In contrast to Tables 3.2 and 3.3, this signifiant estimate is less than half of the loss of 47.6% in stock of educated people found by using a pre-genocide control within Rwanda.

Overall, results from Table 3.3 tell us the following. First, there are both subnational and aggregate effects of the genocide on size of educated cohorts, which shows that there are indeed missing educated people. Second, there are missing educated people in Rwanda even postgenocide, which to a large extent is explained by the excess missing educated Rwandans versus Ugandans between 2002 and 1991. Third, there are interesting subnational versus aggregate difference in these effects. At the subnational level Tables 3.2, 3.3 and 3.4 together suggest that there were indeed fewer educated cohorts, but the rate of which they were missing is similar to less educated cohorts, leading to no effects for years of schooling and ratio of educated to less educated measures. However, since the aggregate effects for years of schooling and ratio of educated to less educated measures are massive, this shows that one needs to look at the aggregate level and not the subnational level to really see the stark differences in educated versus less educated cohorts.

3.6 Further Robustness Tests

3.6.1 Cohort Size Variation Within Rwanda

It may be the case the first set of results (Tables 3.2 and 3.3) we showed on subnational comparisons within Rwanda were showing inconsistent results because our measures are not really capturing genocide intensity well enough. After all, it's possible that the genocide was homogenous enough there is not much differential conflict intensity within Rwanda. However, this argument is not convicting, Akresh et al. (2008) for example exploit the same spatial intensity of the Rwandan genocide to identify the effects of the genocide on completion of schooling for children. Moreover, Table 3.3 clearly shows that there are fewer

educated people at the subnational level. We now verify that there are fewer people in general in more conflict hit areas in Table 3.5. Apart from Measure A, we also use three other genocide intensity measures. Measure B, a more sub-aggregated measure, is dummy for all those provinces which were most hit. Measure C uses satellite data to identify mass grave sites and memorials per province, and Measure D represents the proportion of Tutsis per province in 1991. All of our measures of genocide intensity are negative and statistically significant. With Measure B showing that those provinces with high intensity have as much as 15% fewer people compared to less intensity areas in 2002 vs 1991.

3.6.2 Alternate Measures of Genocide Intensity Within Rwanda

Table 3.6 explores for the robustness of subnational estimates in Tables 3.2, 3.3 and 3.4 with the alternate measure of genocide intensity. Column (1) further verifies that there among the fewer people in conflict hit areas, there were educated people as well. But since both educated and less educated were targeted at the subnational level, consistent with Tables 3.2 and 3.3, there are no statistically significant differential effects for the educated compared to the less educated and for the average years of schooling (Columns (2) and (3)) when the difference-in-difference model is estimated.

3.7 Discussion and Conclusion

Since the 1960s, one out of very three nations has been affected by a civil war, with as many a 20% of nations witnessing at least 10 years of civil war (Blattman and Miguel, 2010). The economic impact of war in general has not been understood well enough. Rodrik

(1999) argues that conflict is the main factor in explaining lack of persistence in economic growth rates for many nations and in explaining why several countries have experienced a negative growth shock ever since the mid-1970s. Justino and Verwimp (2006) find that in Rwanda alone, 20% of the population slipped into poverty after the 1994 genocide. This paper has explored the effects of the Rwandan genocide of 1994 on the stock of educated people in Rwanda, whereby approximately 1 million people are reported to have been killed in just 100 days and which is known to have been targeted at the more educated (Tutsis). We have carried out our analysis using novel census data which brackets the genocide in Rwanda, to explore not only the effects at the sub-national level but also at the aggregate level by exploiting various control groups. The stock of human capital is measured in three alternate ways: cohort size of educated individuals, cohort size of educated relative to the less educated individuals, and average years of schooling. Four different measures of genocide intensity within Rwanda have been used for robustness of sub-national estimates. And neighboring Uganda used to test for robustness of aggregate effects in Rwanda.

The neoclassical growth framework provides a useful starting point to think about channels. One of the ways through which war can effect the society is through human capital, an important factor of production. But the framework also tells us that the stock of human capital at any given point is determined not only by investments made in the last period, but by the depreciation in (destruction of) the stock of accumulated human capital. Most of the recent literature which has focused on the effects of war on human capital, has focused on war's effects on investment in children of schooling going age, analyzing

children's health and schooling outcomes. But there is an alternate channel through which human capital is affected- the destruction of existing stock of human capital.

To measure the destruction of human capital, this paper adopts a rather unconventional approach in the economics of conflict literature. Since we cannot distinguish between those who were killed or those displaced, we estimate the rate at which individuals are missing. That said, we have argued that the majority of the missing are likely to have been killed. First, much of the displacement happens from one town/village to another within the same province. And not everyone is able to leave Rwanda. A 2001 Rwandan nationally representative survey, *Enquête Intégrale sur les Conditions de Vie des Ménages*, shows that approximately 88-89% percent of individuals currently live in the province in which they were born. Second, existing evidence suggest that many of the migrants who fled Rwanda to neighboring countries returned back by 2002. In 1996 and 1997, for example, violence in Burundi , Tanzania and Congo forced numerous Rwandans to repatriate (World Bank, 2003). This conclusion is further strengthened from data on net migration collected from World Development Indicators which show that net migration was about as little as 15,327 in 1990, fell to about -1.5 million by 1995 but rebounded to a positive 1.8 million by 2000, suggesting that if anything between 1990 and 2000, there has been a net positive migration of about 300,000 people. From this paper's perceptive, both displacement and mortality present a negative shock to the stock of human capital which we put under the common umbrella term of missing. ⁶

⁶There may be different reasons to believe that migration will have different effects on accumulation of human capital than mortality. For one, the educated can return in a later period. Although, to the extent that they don't return in the period under consideration, it may still be treated as a loss. Even if the migrated do not return, the mental health effects on survivors who know that they have relatives and friends in other countries may be very different than those whose close ones died. Moreover, the migrated may send

Many studies choose to only carry out subnational or aggregate analysis. We do both. And find that the two present a very different picture. When sub-national comparisons are done, although we do find that there are fewer educated people compared to less educated ones, we find no significant differences in high vs low conflict areas, in either years of schooling in areas or ratio of educated to less educated cohorts. We do however document that there are fewer educated people both at the subnational and aggregate level. Even although educated people are more likely to be missing in high conflict areas at the sub-national level, the rate at which they are missing is no different than the rate at which less educated are missing.

In contrast to sub-national analysis, aggregate analysis suggests a rather stark effect of the Rwandan genocide. In terms of the ratio of educated to less educated cohorts, the before-and-after results within Rwanda show that highly educated individuals are missing at a rate that is 19.4% more than less educated individuals. When a differences-in-differences over time in Rwanda versus Uganda is carried out, we find that 44.8% more are missing in Rwanda than Uganda. This is driven by the fact that instead of having fewer educated people, Uganda had fewer less educated individuals by as much as 26.8%.

When average years of schooling is used as a measure, we get a very similar story. The before-and-after results within Rwanda show that the average years of schooling is 0.37 years less. An estimate strikingly similar to what Akresh et al. (2008) find in terms of investment effects on child schooling in Rwanda. But at the aggregate level when a

back remunerations to their close ones at home and that may serve as a buffer in mitigating some of the negative shocks on human capital accumulation. Mortality may have different effects on investments in the next generation's human capital as well. Although if entire families migrate or are killed, such remunerations may not be as relevant.

difference-in-difference over time in Rwanda versus Uganda is carried out, we find that average years of schooling is less in Rwanda by as large as 0.83 years of schooling. This is driven by the fact that instead of having lesser years of schooling, Uganda had 0.49 more years of schooling over the same period. Rwanda not only had fewer educated people, the loss of the educated cohort was in stark contrast even compared to its own less educated citizens and to its neighbors.

Together these results reveal that there is much heterogeneity in the effects of the Rwandan genocide on destruction of human capital. The largest effects are found at the aggregate level, compared to the subnational level. In terms of average years of schooling lost, the smallest effect size is 0.37 years of schooling, remarkably similar to what Akresh et al. (2008) find in terms of effects on children's schooling.

The Rwandan genocide was destructive not only because it had large effects on schooling and health investment of the future generations (children). The genocide was not only destructive because of the more than 800,000 who have been known to be killed in a matter of just 100 days. It was destructive also because it led to strikingly large loss of educated cohorts over and above the rest of the population leading to substantial destruction in Rwanda's stock of human capital. Moreover, these effects are manifest at the aggregate level rather than at the subnational level, suggesting that the Rwandan genocide was not a subnational phenomena, but a nation wide catastrophe.

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3.8 Data Appendix

GENOCIDE INTENSITY MEASURES

Measure A: Number of days, per province, that the genocidal killings occurred. The data is obtained from an online database : www.genodynamics.com. The authors of the database attempted to collect all available information from local human rights organizations, Rwandan government ministries, and international organizations on the timing and geographic extent of all killings that took place during the one hundred days of genocide.

Measure B: Dummy for the four provinces - Kigali Ngali , Butare, Kibuye and Kibungo - with most reported killings as reported in www.genodynamics.com.

Measure C: Number of mass graves sites and memorials per province, with data taken from the Rwandan Genocide Project at Yale University (Rwandan Genocide Project, 2007).

Measure D: Proportion of Tutsis per province found in 1991 Census in Rwanda. This a a measure of potential exposure to genocide, since Tutsis are known to have been systematically targeted.

DEMOGRAPHIC VARIABLES

Lowed : Those defined as having no schooling or less than 6 years (primary) level of education are labelled as “low” educated.

Highed : Those not missing and not “low” educated are labelled highed or “highly” educated. The undocumented are assigned missing values.

3.9 Tables

Table 3.1: Summary Statistics

VARIABLES	Rwanda		Uganda	
	1991	2002	1991	2002
Cohort Size	3530	2472	2738	1882
Number of High Educated	1070	630.7	1010	813.1
Ratio HighEd/LowEd	.4087	.3183	.7848	.9266
Years of Education	2.967	2.711	3.839	4.211
Cohorts	1,010	1,010	3114	3114

Cohorts are defined by year of birth, gender, and province of birth (Rwanda) or district of birth (Uganda). Sample includes only natives who are born between 1930 and 1978. All empty cohorts and cohorts with no observed high educated people were omitted along with corresponding cohort from opposite census year.

Table 3.2: Effects of the Rwandan Genocide on Years of Schooling

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
<i>Subnational Estimates</i>						
MeasureA	0.0174*** (0.00163) 1,010					
C2002 X MeasureA		-0.001 (0.003) 2,020				
<i>Aggregate Estimates</i>						
C2002			-0.371*** (0.029) 2,020	0.490*** (0.040) 6,230		
Rwanda					-1.419*** (0.041) 4,124	
C2002 X Rwanda						-0.829*** (0.063) 8,250
<i>Type of variation</i>						
Within Rwanda Post-Genocide	YES	YES				
Within Rwanda Diff-n-diff		YES				
Within Rwanda Temporal		YES	YES			YES
Within Uganda Temporal				YES		YES
Across Country Post-Genocide					YES	YES
Across Country Diff-n-diff						YES
<p>Note: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1 Dependent variable is the average years of schooling within the cohort. Individuals with undefined years of education are not included and years of education are censored at 12 years. Measure A represents number of days, per province, that the genocidal killings occurred. Each observation is a cohort defined by birth year, province(district) of birth for Rwanda(Uganda), gender and census year, and is weighted by the cohort size. For Rwanda, we treat Byumba and Umutara provinces as one since between the two census the Rwanda government merged these two provinces. Ugandan observations are from districts included in both 1991 and 2002 censuses. All regressions include controls for gender and year of birth fixed effects. In addition (3), (4) include controls for province(district) of birth fixed effects and interaction between province and year of birth fixed effects. The Rwandan (Ugandan) samples includes only those native Rwandans (Ugandans) born between 1930 and 1978. Source: IPUMS International</p>						

Table 3.3: Effects of the Rwandan Genocide on Ratio of Educated People

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
<i>Subnational Estimates</i>						
MeasureA	0.009*** (0.001) 1,010					
C2002 X MeasureA		-0.002 (0.002) 2,020				
<i>Aggregate Estimates</i>						
C2002			-0.194*** (0.023) 2,020	0.278*** (0.024) 6,230		
Rwanda					-0.791*** (0.025) 4,124	
C2002 X Rwanda						-0.448*** (0.042) 8,250
<i>Type of variation</i>						
Within Rwanda Post-Genocide	YES	YES				
Within Rwanda Diff-n-diff		YES				
Within Rwanda Temporal		YES	YES			YES
Within Uganda Temporal				YES		YES
Across Country Post-Genocide					YES	YES
Across Country Diff-n-diff						YES
<p>Note: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1 Dependent variable is log ratio of educated cohort size to less educated cohort size. Measure A represents number of days, per province, that the genocidal killings occurred. Each observation is a cohort defined by birth year, province(district) of birth for Rwanda(Uganda), gender and census year, and is weighted by the cohort size. For Rwanda, we treat Byumba and Umutara provinces as one since between the two census the Rwanda government merged these two provinces. Ugandan observations are from districts included in both 1991 and 2002 censuses. All regressions include controls for gender and year of birth fixed effects. In addition (3), (4) include controls for province(district) of birth fixed effects and interaction between province and year of birth fixed effects. The Rwandan (Ugandan) samples includes only those native Rwandans (Ugandans) born between 1930 and 1978. Source: IPUMS International</p>						

Table 3.4: Effects of the Rwandan Genocide on Educated Cohort Size

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
<i>Subnational Estimates</i>						
MeasureA	-0.003** (0.002) 1,010					
C2002 X MeasureA		-0.009*** (0.002) 2,020				
<i>Aggregate Estimates</i>						
C2002			-0.476*** (0.022) 2,020	-0.241*** (0.023) 6,230		
Rwanda					-0.268*** (0.024) 4,124	
C2002 X Rwanda						-0.198*** (0.038) 8,250
<i>Type of variation</i>						
Within Rwanda Post-Genocide	YES	YES				
Within Rwanda Diff-n-diff		YES				
Within Rwanda Temporal		YES	YES			YES
Within Uganda Temporal				YES		YES
Across Country Post-Genocide					YES	YES
Across Country Diff-n-diff						YES
<p>Note: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1 Dependent variable is log of educated cohort size. Measure A represents number of days, per province, that the genocidal killings occurred. Each observation is a cohort defined by birth year, province(district) of birth for Rwanda(Uganda), gender and census year, and is weighted by the cohort size. For Rwanda, we treat Byumba and Umutara provinces as one since between the two census the Rwanda government merged these two provinces. Ugandan observations are from districts included in both 1991 and 2002 censuses. All regressions include controls for gender and year of birth fixed effects. In addition (3), (4) include controls for province(district) of birth fixed effects and interaction between province and year of birth fixed effects. The Rwandan (Ugandan) samples includes only those native Rwandans (Ugandans) born between 1930 and 1978. <i>Source:</i> IPUMS International</p>						

Table 3.5: More People Missing in High Conflict Areas

VARIABLES	(1)	(2)	(3)	(4)
C2002 X MeasureA	-0.008*** (0.001)			
C2002 X MeasureB		-0.147*** (0.031)		
C2002 X MeasureC			-0.012*** (0.002)	
C2002 X MeasureD				-0.011*** (0.003)
Observations	2,020	2,020	2,020	2,020

Note: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1
 Dependent variable: Log cohort size. Sample includes only those native Rwandans born between 1930 and 1978. Each observation is a cohort defined by birth year, province of birth, gender and census year, and is weighted by the cohort size. Regressions include controls for gender and year of birth fixed effects. We treat Byumba and Umutara provinces as one since between the two census the Rwanda government merged these two provinces. For (3) individuals with undefined years of education are not included and years of education are censored at 12 years.
 Source: IPUMS International

Table 3.6: Subnational Effects of Genocide Are Robust to Alternate Measures

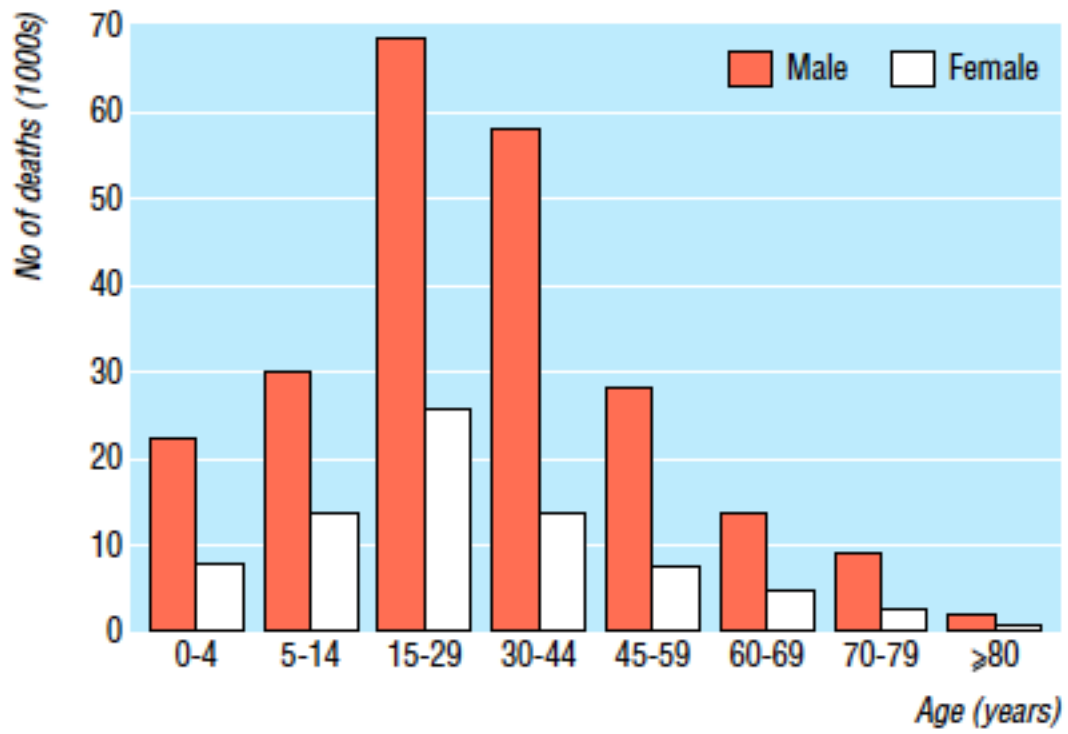
VARIABLES	(1) Highed	(2) Highed Ratio	(3) Years of Schooling
<i>Within Rwanda Post-Genocide Variation</i>			
MeasureB	-0.120*** (0.033)	0.053** (0.026)	0.0585 (0.0363)
MeasureC	-0.010*** (0.002)	0.014*** (0.002)	0.0241*** (0.00228)
MeasureD	-0.008*** (0.003)	0.019*** (0.002)	0.0324*** (0.00298)
Observations	1,010	1,010	1,010
<i>Within Rwanda Diff-n-diff</i>			
C2002 X MeasureB	-0.195*** (0.048)	-0.071 (0.048)	-0.084 (0.062)
C2002 X MeasureC	-0.014*** (0.003)	-0.003 (0.003)	-0.001 (0.004)
C2002 X MeasureD	-0.011*** (0.004)	0.000 (0.004)	0.003 (0.005)
Observations	2,020	2,020	2,020

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

Dependent variables: (1) log cohort educated size (completed primary school), (2) the log ratio of high to low educated cohorts, (3) the average years of school within the cohort. Sample includes only those native Rwandans born between 1930 and 1978. Each observation is a cohort defined by birth year, province of birth, gender and census year, and is weighted by the cohort size. Regressions include controls for gender and year of birth fixed effects. We treat Byumba and Umutara provinces as one since between the two census the Rwanda government merged these two provinces. For (3) individuals with undefined years of education are not included and years of education are censored at 12 years. See Appendix for details of genocide measures.

Source: IPUMS International

Figure 3.1: Estimated global age and sex distribution of war casualties in year 2000.



Source: Murray et al. (2002)

Figure 3.2: Map of Rwanda



Figure 3.3: Population by cohort and census in Rwanda

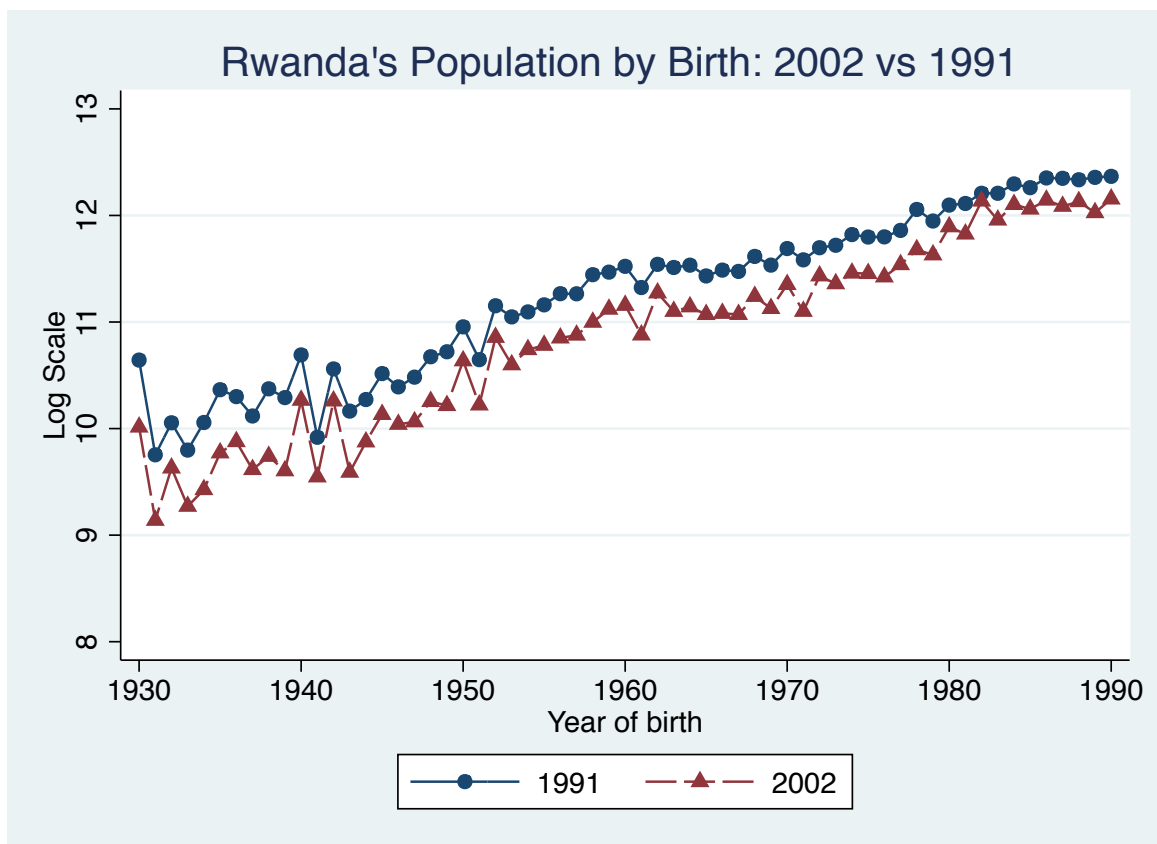


Figure 3.4: Differences in population by cohort in Rwanda

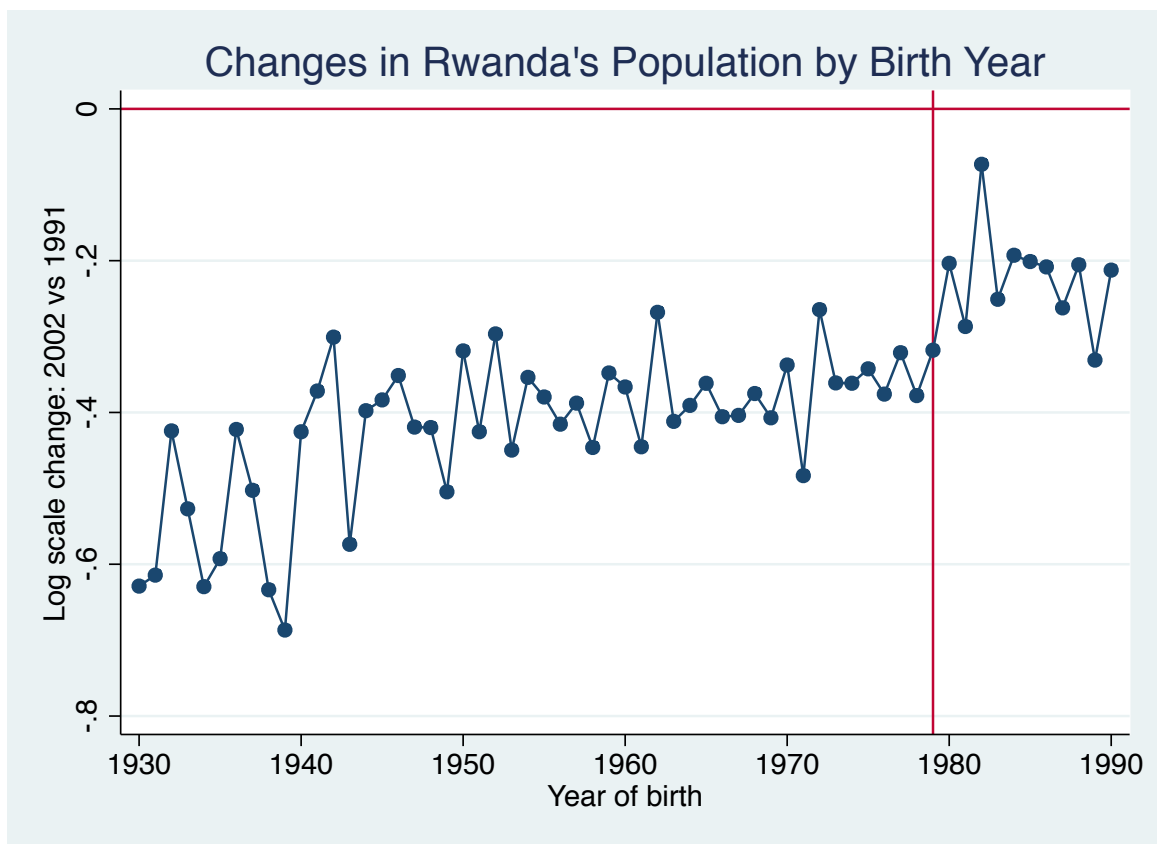
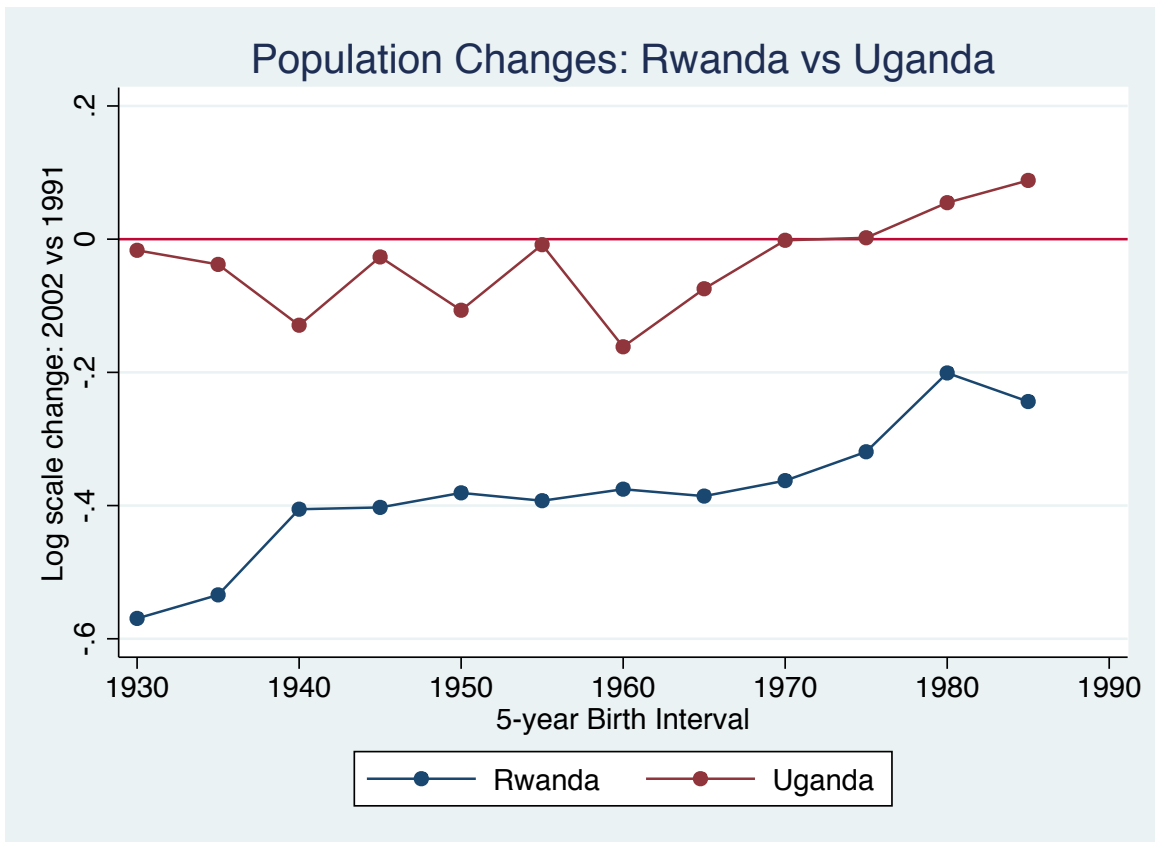


Figure 3.5: Mortality by Year of Birth Interval: Before and After Genocide in Rwanda vs Uganda



Chapter 4

The Role of Parental Investments in Skill Formation Over the Life Cycle: Evidence from Developing Countries

4.1 Introduction

Parents play a fundamental role in the upbringing of their children, from the fetal period to the grave. A large literature in economics has been interested in understanding the role of families in general, and the role of mothers in particular, in determining not only child development (Case, Fertig and Paxson 2005; Grantham-McGregor et al. 2007), but adolescent and adult outcomes as well (Almond and Currie 2011). In each stage of the life

cycle parents determine the outcomes of their children through multiple channels, which include determining genetic endowments as well as by shaping the broader environments which their child face.

Parental investment may play a crucial role in shaping the broader environment for children. These investments may be classified into two types- those which are child-specific and those which occur at the family level, affecting all children. For example, the location choices of parents may affect all children by giving them access to a cleaner environment, better schools, better health facilities in the vicinity. One can even think of parental decisions to start certain jobs, to get more schooling, etc to be decisions which are partly made keeping the interests of all the children. In a broader sense, associative mating, based on say socio-economic status (SES) and human capital, can be thought of as reflecting couples decisions to mate with those who are more or less likely to invest in their future offspring (Trivers 1972)

Given the vastness of the literature on parental investment, particularly if one considers contributions from disciplines like evolutionary biology and evolutionary psychology, I choose to focus on the following question focusing on the recent (post 2008) economics literature: What is the evidence on causal effects of shocks to child endowments on parental investment? To a limited extent, I will also discuss what role which investment play in childhood development.

A recent paper by Almond and Mazumder (2013) carries out a review of parental responses to fetal shocks covering select papers from developed and developing countries. In contrast, this review will cover a greater range of papers focusing on developing countries,

with a particular focus on gender biased parental investments which Almond and Mazumder (2013) do not review. Moreover, I also document some recent causal evidence related to parental investments and its effects on children's outcomes in developing countries. This review is also different because of its focus on how parents respond to shocks over different stages of the life cycle, covering prenatal as well as post-natal investments to fetal shocks. In particular, I highlight the need to study parental investments over the life cycle. Moreover, I make recommendations for future work, based on my review.

When faced with shocks to children's skill endowments, parents tend to reinforce the inequalities among their children aged 0-5, consistent with the domination of the efficiency motive over any inequality aversion. When children grow up to between 5-15, we witness more heterogeneity in parental responses, with evidence for no response and compensatory response also found. Overall, however, most studies find reinforcing responses during this stage as well. The fact that most of the heterogeneous parental response tends to be focused on the 5-15 aged group, calls for future studies to explore if other age groups also tend to have diverse effects or if such heterogeneity is peculiar to this age group. Interestingly, I could not find any recent good quality study which studies prenatal investment response to an exogenous prenatal skill shock, or any study which looked at effects on inter vivos transfers, dowries and bequests at later stages of the life cycle. Much work can be done in these areas.

However, the sex imbalance literature has explored the effects of gender on prenatal investments as well as postnatal investments (for those aged 0-5). It is often hard to differentiate between the pure investment effects vs effects taking place through sex selective

abortion, recall bias, etc. Arguably, the cleanest study studies may be those which not only have rich multidimensional measures of parental investments but which can exploit exogenous variation in knowledge about gender in utero (such as exploiting spread of ultrasound technology) and which can rule out any biases from sex selective, son biased stopping rules, and other unobservables which may be different in families which invest more in boys vs girls. Although many of the studies address some of these concerns, but very few studies, if any, have addressed all of these concerns. Nonetheless, the current evidence points to persistent female disadvantage in parental investment both in prenatal period and during neonatal and infancy period. It may be interesting to see how much of such early disadvantage is reinforced or mitigated later on at the time of marriage when dowries are given.

These changes in parental investment response, may play an important role in childhood development specially in the presence of uncertainty regarding returns to investments, and externalities. I briefly review evidence from epidemiology /medicine on breastfeeding and investment in parenting skills, both of which show that parenting can (indirectly or directly) can have positive effects on childhood development. However, arguably, one of the most promising channel through which childhood endowment shocks may play a significant role is by changes in the returns to investments i.e. through dynamic complementariness, as emphasized by Heckman (2007) as well. Unfortunately, most of the evidence to date has only descriptive evidence on dynamic complementarity (see Almond and Mazumder (2013) for more details). Future work should seek to exploit double shocks to not only endowments but to returns to parenting as well, in order to explore not only the

response of parents to endowment shock but also evaluate the role they play in childhood development over the life cycle for girls and boys.

The next section will explore the most recent causal evidence on parental responses, followed by a section on effects of parental investments. Last, I summarize the literature and conclude.

4.2 Evidence on parental responses to fetal shocks

There is a small, but growing empirical literature which studies causal effects of fetal shocks on parental investment. One can think of two sub-questions within this literature. What are the effects of a pure endowment shock which affects all children (say all children were exposed to exposure to maternal fasting in utero)? What are the effects of differences in child endowments on parental investment? The review below will be address the second question.¹

¹The first question, dealing with wealth effects, to the best of my knowledge, has not been dealt in the recent empirical literature motivated by the fetal origin hypothesis. There may be various reasons why this question has perhaps not been empirically dealt with. First, much of the theoretical literature starting from Becker and Tomes (1976) has focused on endowment differences and the role which parents play in mitigating or reinforcing such inequalities. If there is no inequality within child endowments, it may not be relevant to understanding inequality within the family and the role it plays in society. Second, it is difficult to identify shocks across families, which effect different families endogenously. Since there is no variation in child endowments within families, sibling and twin analysis within families cannot be used in this case. Identifying a shock which exogenously effects all children of one family (without affecting other household members), and none of the members of another family, may be an empirical challenge.

This should not mean that the question itself is not important. Its important to understanding how parents respond not just to inequalities in children endowments (the gradients between their children's endowment), but to parallel shifts in the endowments of their children, because public programs may want to target all children of a family for say improving their health (say because all of them need to be vaccinated against a disease). In fact, answering this question may well be one way in which one can differentiate between the role played by parental aversion to inequality vs parental concerns for efficiency. If children in families which, say all are exposed to mothers fasting, are compared to children in families where none are exposed, the differences in the average parental investment in the children of the exposed family vs those not exposed, cannot be attributed to parental aversion to inequality. This may allow us to identify the true role played by the technology of skill formation in determining the parental investment.

A rich literature in economics has been interested in how families allocate their resources for inequalities in their children's endowments. The theoretical framework of Becker and Tomes (1976) predicts that parents may reinforce differences in their children's endowments driven by the efficiency motive, where as Behrman, Pollak and Taubman (1982) argue that if parental preferences for equity are strong enough, parents may compensate.

Heckman (2007) briefly summarize recent work on a multi-staged dynamic technology of skill formation over the life cycle, which highlights the complex relationships between parental characteristics, child capabilities and parental investment. Using a CES production function, their framework highlights that different features of the technology of skill formation (ease of substitution between inputs, elasticity of substitution, capabilities and interest rates) determine parental investment in different ways. For example, if the technology is one of perfect substitutes in inputs, then parents may find it optimal to compensate for negative fetal shocks in the later period. Where as if there is perfect complementarity between inputs, parents may find it optimal to reinforce inequalities in their own children.

One of the key differences between Becker and Tomes (1976) and Heckman (2007) is that childhood lasts only one period in Becker and Tomes (1976), who implicitly assume perfect substitution in investment within different periods of childhood. Where as in Heckman (2007), childhood lasts multiple periods, and investments during different periods of childhood can be complements. These differences lead to differing notions of complementarity in the technology of skill formation between Becker and Tomes (1976) and Heckman (2007). One can claim that the concept of complementarity in Becker and Tomes (1976) to be that of "static complementarity," which implies a positive relationship between child

endowment and parental investment. Whereas, Heckman (2007)'s “dynamic complementarity” implies a positive relationship between child endowments and returns to parental investments.

Most of the empirical literature I will review below is effectively testing for static complementarity, where as there is mostly descriptive evidence for dynamic complementarity.² I now review the empirical literature focusing on parental investments during different stages of the life cycle, with a particular interest in skill vs gender biased responses.

4.2.1 Prenatal investments

The literature on prenatal parental investment response to prenatal shocks is far less than the literature which identifies postnatal investment effects. In this section, I will selectively discuss results and methodology used in two recent papers which have attempted to find causal effects of fetal shocks on prenatal parental investment.

Almond, Li, and Meng (2010) analyses the effects of shocks to the knowledge of child gender in utero on prenatal and postnatal investments in boys versus girls in China. Differential timing in the introduction of ultrasound technology across China is used to carry out a difference-in-difference analysis involving child gender. The paper finds that although there is no evidence of differential post-natal investments, they do document higher neonatal mortality for females following the introduction of ultra sound and attribute this to differences in prenatal investment.

²There is also not much evidence which can disentangle role of preferences vs technology in reinforcing or mitigating fetal shocks.

Although similar work involving usage of ultrasound, had been done in the US (see for example, Lhila and Simon (2008)), Almond, Li, and Meng (2010) is the first paper in the recent literature I know of, which looking at a developing country, attempts to document the causal effect of an in utero shock on prenatal investment.

However, it is not clear to what extent neonatal mortality is reflective of biases due to sex selective abortions, prenatal investments or contemporaneous neonatal investments. Although they have information about vaccines, breastfeeding and whether mother took care of the child, this data is for postnatal investments and no direct measures are available for pre-natal investment. Even within the neonatal period (within 7-28 days), there is no information about parental investments during that time so that one cannot rule out the role neonatal investments played.

Moreover, it may be the case that after ultrasound was introduced, parents carried out more sex selective abortions, which lead to only those females surviving which were more likely to be cared for. However, this will bias the estimates downwards.

The paper by Bharadwaj and Nelson (2013) addresses some of the concerns in their study involving sex-selective prenatal investments with a focus on India but with robustness tests on Bangladesh, China and Pakistan as well. The basic argument of the authors is that in the absence of knowledge regarding fetal gender, prenatal investments should not differ by gender. They find that Indian women, particularly from North India -where son preferences in known to be strong- are twice as likely to attend prenatal care, get greater number of tetanus shots and are more likely to have a non-home delivery when pregnant with a male child. Supporting evidence is found in other Asian countries as well.

Bharadwaj and Nelson (2013) addresses three other alternate stories rather than one based on prenatal investment per se. First, is son preference-based fertility stopping rules. To overcome the bias associated with certain families having more kids after finding out that they have a daughter, following Barcellos, Carvalho, and Lleras- Muney (2010)'s solution, Bharadwaj and Nelson (2013) limit the sample to those where the youngest child's age is less than two years. The results are robust. A second alternate story is with regards to biases from sex-selective abortions. To tackle this, the authors explore gender gap only for those who have had prenatal care at least once. This is done to avoid reverse causality arising from mothers learning about the sex of their child in their first prenatal care visit, leading to a gender gap emerging from sex-selective abortions rather than sex selective prenatal care. Furthermore, no gender gaps are found in prenatal investments if the fetus was less than four months old. This is reassuring as there is no way the gender should be known in this period. Moreover, using ultrasound data, for a sub-sample, Bharadwaj and Nelson (2013) show that there is no evidence of sex-selective prenatal investments before ultrasound technology was available. They also use mother-fixed effects to rule out that mothers time-invariant characteristics (say those with stronger son preference) are more likely to carry out sex-selective abortions and that it is these unobservables that are driving the results.

The biggest strength of Bharadwaj and Nelson (2013)'s approach is that they use explicit measures of prenatal investment such as tetanus vaccinations, iron supplements consumed during pregnancy, and propensity of non-home deliveries and carefully rule out many alternate hypothesis which Almond, Li, and Meng (2010) do not explicitly address.

However, one of the biggest problems with their study, which is the strength of Almond, Li, and Meng (2010), is that the data on ultrasounds is rather weak. Future work in this direction should see to pay not only more attention to contexts with better data on ultrasound expansion, such as China, but also those that explicitly measure prenatal investments and rule out alternate stories such as those related to sex-selective abortion (or a ban on such abortions) in exploring the prenatal parental investment response to in utero shocks on knowledge regarding child gender.³

Overall, there is very little causal evidence on prenatal parental investment response to prenatal shocks. I now look into gender and skill biased postnatal parental investments.

4.2.2 Gender biased postnatal investments: Age 0-5

Oster(2009) is an interesting study for it not only studies the differential vaccination during infancy by gender, but also the importance of such investments in determination of overall sex imbalance at later stages of the life cycle. The study is based in India. To differentiate between natural versus non-natural sources of differences in sex imbalance, Oster carries out a difference-in-difference analysis of India with Sub-Saharan Africa. To estimate

³One way to interpret the lower investment in females during prenatal period is to think of it as due to taste based discrimination. However, none of the studies are able to differentiate between taste based versus statistical discrimination. It may be the case that parents perceive that historically the expected net benefits of having a female child outweighs the net costs, so that the decision to invest less is not based on endowment differences between the gender, but because they expect this historical trends to continue. In this sense, in contrast to the channels empathized by Becker and Tomes (1976) or Heckman (2007), it may be the case that despite no differences in skill endowments between children, parents invest less in some children due to perceived net benefits from those children. Such expectations in fact may be later on in life, when girls need to be payed high amounts of dowries and get lower wages from the labor market, leading to a self-fulfilling prophecy type equilibrium. In fact, one reason why women may be given less in utero, may be that later on in life, high amounts of dowries will have to be given with little pay back from the in laws for the girl's parents.

the effect of health investment during infancy, the author compares differences in estimates of sex imbalance in regressions which control for health investment versus those which do not. The authors find that not only do females get less investments, but that vaccinations during infancy can explain between 20 and 30% of excess female mortality!

Although this paper does better than other studies by carrying out a difference-in-differences with Sub-Saharan Africa in terms of effects of gender on healths investment, and that it explores the role which differences in investments play in determination of sex-imbalance at age 5 and later, such evidence cannot be said to be causal. One needs an exogenous shock in the knowledge of gender, control for other factors such as prenatal investment, female feticide (or other omitted variables which may cause such behavior), to look at the pure role of post-natal investments. Moreover, a separate class of instruments in needed for parental postnatal investment to study their role on sex imbalance at age 5 or later on. It may be the case that parental vaccinations are correlated with other health and non-health inputs which also effect sex imbalance.

A particularly interesting recent paper on this topic is by Barcellos, Carvalho, and Lleras-Muney (2010). The authors study differential investments in response to gender of child in India. The authors look at investments such as childcare time, breastfeeding duration, vaccinations and vitamin supplements. Much of previous work assumes that girls and boys live in families with similar unobservables. Yet, in the presence of documented son-biased stopping rules in India (Jensen 2005), this assumption may be invalid. Girls may end up in larger families, as these families try to have more boys, and it may be the lower standard of living associated with larger family size that causes the apparently

less investments in girls, rather than a pure gender effect. Barcellos, Carvalho, and Lleras-Muney (2010) propose a simple solution: restrict sample to those families which have young enough children, whose mothers are yet to have more kids.

Results show that there is indeed differential investments across genders. Families with a child under age 1 spent 15% more time on childcare than on an infant girl, with this differences rising to 30% for families with only one boy under age 6. There is also evidence of greater investment in the form of be vaccines, breastfeeding duration and vitamin supplements. However, an interesting finding is that gender differentiated investments tend to be large yet disappear with age for those investments which occur over the life cycle, such as childcare time. In contrast, the gender bias in investments is smaller but more persistent across age, for one-off investments occurring early in life (such as vaccinations). In general, Barcellos, Carvalho, and Lleras-Muney (2010) find investments in girls tends to be at least 10% lower for girls, with the effects prominent mainly for rural areas, and less so in urban areas.

Barcellos, Carvalho, and Lleras-Muney (2010) is particularly interesting for not only exploring the gender bias in investments associated with son-biased stoping rules, but also because it uses measures such as time invested in child care, along with other standards measures such as breastfeeding duration and vaccines. Not many studies do that. The distinction between investments over the life cycle (such as time) versus one-off investments in infancy, is something other studies may also look into. One needs to verify if indeed gender biased time investment tends to dissipate with age and those such as vaccines tend to be similar for older and younger children. One way to interpret such differences

is to think of one-off investments to be more substitutable over the infancy period, but that the early childhood (first year after birth or so) is a more critical period for life time investments.

Nonetheless, there are many limitations of this study. The sample is restricted to those under age of 2 for most of this analysis, so its not clear how much can be learn for older children. It may be the case that girls tend to be in families which anticipate more children, leading parents to invest less because of expected future investment. This argument seems to have some merit particularly for breastfeeding effects as argued by Jayachandran and Kuziemko (2010) as well. Another concern, not directly addressed by the authors, is that of prenatal investment. The key identifying assumption that in the absence of sex-selective abortion, the gender of the child at birth is random may not be really true for India (and other Asian countries) as the paper by Bharadwaj and Nelson (2013) argues.

Since I could not find recent causal evidence for postnatal gender biased investments after age 5, I now look into the literature on skill biased postnatal investments.

4.2.3 Skill biased postnatal investments

Studies which analyze the effects of fetal skill shocks on post-natal investment have to not only identify an exogenous fetal shock, but also control for prenatal investments, differential mortality, and any other unobservable which determines not only differential prenatal but postnatal investments. I will look into the literature for those aged 0-5 and 5-15 separately as investments may be very different in early childhood period compared to adolescence.

Age 0-5

Adhvaryu and Nyshadham (2012) is an interesting study focusing on parental investments during early childhood. Using the 1999 round of the Tanzania Demographic and Health Surveys (DHS), the authors study the response of parental postnatal investments to an iodine supplementation program targeted at pregnant women in Tanzania. Field et al. (2009) had earlier showed that the same program lead to an improvement in educational attainment among the children. One reason to look at parental investment response is to understand the role parents play in the long-term effects of shocks to the fetal environment. The authors find that parental investment reinforce the positive cognitive endowment shock in their children. Moreover, they argue that this effect is not driven by any health awareness due to the program. In support of their argument, they provide evidence that there are no effects found on birth weights and neonatal health investments.

A particular strength of this study is that it helps us understand the parental investment response to a specific type of treatment- iodine supplements, rather than a black box of health, nutrition, cognitive and non cognitive shocks. The study also finds, that although parental investments are reinforcing, there are also important spillover effects (of almost half the magnitude as own effects). This suggest that just because there is parental reinforcement in utero shocks, does not mean that parents don't partially compensate other children. Similarly, one may expect that in the presence of a negative in utero shock, other siblings may also get negatively effected. More research needs to study spillover effects for they tell us a more complete picture of the extent to which parents reinforce own shocks.

This also suggests that current parental investments may be sub-optimal, making a stronger case for public policy interventions.

However, they do not study gender differentials in parental investments, nor do they study schooling related expenditures or parental time investments particularly for those of school going age. Also, although parental investments are shown to be on the net reinforcing, its not clear how important such reinforcements are in the determination of the longer term schooling attainment effect found in Field et al. (2009).

Age 5-15

The most amount of interest has been on analyzing the effects of fetal shocks on parental investment outcomes- in particular, schooling expenditure and time investments- for those aged about 5-15. However, in contrast to the literature on sex biased investments, most of this literature has not been interested in gender biased postnatal investments, but in the question of compensating or reinforcing investments in response to skill related fetal shocks. Since effects of different types of shocks has been studied, I will divide the literature by the type of shock.

Twins Twin studies provide, perhaps one of the most convincing ways to rule out the role of prenatal investment response when evaluating the effect of fetal endowments, in

particular birth weight, on postnatal investment. This is because it is practically impossible to differentially invest in twins in utero.^{4 5}

Bharadwaj, Eberhard and Neilson (2011) consider differential parental investments in Chilean twins, using birthweight differences as a measure of endowment differences. Using repeated tests scores on the same children from grades 1 through 8, they find that the twin estimates are remarkably stable over time. Moreover, using measures of parental investment such as books given to each child, time spent helping with homework, making child read short texts, etc they find that one reason for the persistence in test scores over multiple grades is due to homogeneity in parental investment across twins. The authors also carry out a similar analysis among non-twin siblings, and find that the effect of birth weight declines from grades 1 to 8. They also find that the larger the birth spacing between siblings, the larger the decline in the birth weights. Interestingly, they find that when it comes to siblings, parents tend to compensate for lower endowments, which may be explaining the declining differences in test scores.

However, they do not verify if controlling for parental investment really wipes out the effects. More seriously, to identify the role which parental investment played in determining test scores, one needs an alternate source of randomization/ instruments which can help us understand the extent to which parental investment determines child outcomes.

Given that twins on average have lower birth weights than non-twins and that there is literally no birth spacing between twins, these results also provide evidence that lack of

⁴However, for the same reason, comparison between average prenatal investments between twins and non-twins may provide a clean way to assess the effects of a homogenous shock to children.

⁵A by product of studying parental resource allocation behavior in families with twins is that it can play an important role in over or under estimating the effects of reducing quantity on quality of children (Q-Q tradeoff) and even in determining whether the Q-Q model holds (Rosenzweig and Zhang 2009).

birth spacing (and the financial constraints it places on families) between twins may be one reason explaining persistence in twins estimate (and the no-differential parental investment response effect). Another possibility, not systemically explored by the authors, is that there may be non-linearity in birth weights. If the differences in birth weights between non-twin siblings are generally larger than twins, it may be that if similar quantiles of birth weight differences are explored across twins and siblings, the estimates of twin versus siblings may be more comparable.

Rosenzweig and Zhang (2009) test the Q-Q model, whereby birth of twins (along with family fixed effects) is used as an instrument for a quantity shock and effects on quality on twins and non-twin sibling are analyzed. The authors use data on parental investments such as schooling expenditure on uniforms, books, etc to find that parental investment amongst siblings and twins are reinforcing. In fact, they argue that because twins both have no birth spacing and have lower average birth weights, resources are more likely to be diverted away from the average twin to the non-twin sibling, a finding which helps them find evidence in favor of the Q-Q tradeoff (in contrast to some recent studies which were finding that it does not hold).

It is interesting to note that although both Bharadwaj, Eberhard and Neilson (2011) and Rosenzweig and Zhang (2009) use differences within twins to identify parental response to fetal endowments, and they study those in the 7-14 age group, they find seemingly contradictory finding. This is a call for future work on this topic. Future work which may want to pay more attention to channels such as the levels of differences in birth weights (and non-linearities associated with it) and comparability (and multidimension-

ality) of parental investment measures. For example, it may be the case that parents in Chile are reinforcing in schooling expenditure and those in China are not responding in time allocations (such as helping with home work). Rosenzweig and Zhang (2009) do use time investments, but they treat them as a public good and look at effects of twinning (quantity shock) on time spent, net of birth weight differences. It would have been interesting if they explore the effects of birth weight differences, like they did for schooling expenditure, on time spent as well to allow for comparability with the Chilean results. ⁶

Neonatal health care: Bharadwaj, Loken and Neilson (2012) use administrative data from Chile and Norway to implement a regression discontinuity (R-D) design. Infants who weigh under 1500 grams are classified as very low birth weight (VLBW), and are often provided access to special medical treatments (e.g. surfactant) after birth. One strength of the paper is its ability to check for non-random heaping around the discontinuity. In Chile, all children below 32 weeks of gestation (irrespective of being VLBW) are eligible for treatment. If heaping associated with SES outcomes was an important driver of the results, one may expect those just below 1500 to benefit irrespective of whether the child had less than 32 weeks of gestation. A more plausible story is that since everyone between 32 weeks got treatment, the cutoff for VLBW did not matter. This suggests that its more likely that its the differential treatment for those just below 1500 grams (and above 32 weeks of gestation) that is driving the results.

⁶It may be noted that although Conti, Heckman, Yi and Zhang (2011) look at time investments across twins in China, they look at effects of postnatal health shocks, rather than birth weights so that their results, mentioned later in this review, are not comparable to the Chilean results mentioned above.

I will focus here on Chile's results, to focus on a developing country. Children exposed to the treatment have lower infant mortality (death within 1 year of birth). A particularly nice feature of the paper is that they are able to follow the same children between grades 1 and 8. They find that the treated children score about 0.15 SD better in math scores. Although its not the main focus of their paper, but Bharadwaj, Loken and Neilson (2012) explore parental investment response to neonatal health care treatment. The authors explore effects on quality of schooling which the children attend and, for a sample of fourth grades only, time spent by parents with their children on activities such as reading. Interestingly, there is no evidence of differential parental responses along any of the parental dimension (even in Norway where they have different measures such as whether mother returns to labor force after giving birth).

However , its not clear how much weight to give to these results. The time investment results are only applicable to those in 4th grade, as mentioned. Moreover, quality of schooling attend may be a noisy measure of parental investment, since it may be partly a function of child's characteristics (including willingness to attend). The possibility of measurement error attenuating estimates towards zero, can also not be discarded. It may be interesting to see non-linearities, by running an interaction term with average quality of the school. Gender differences may also reveal some interesting insights, as has been seen in the literature on gender biased parental investments.

Malaria eradication program: Venkataramani (2012) study the long-term effects of a Mexican malaria eradication program during the year of birth on various outcomes including

cognitive test scores. As part of their analysis, the authors look at the age of entry and exit for schooling. The argument is that in presence of a positive endowment shock, the returns to schooling relative to costs may rise, leading to an earlier start. However, as the end of schooling approaches the returns to other options (say wage work) may rise enough so that it may not be as advantageous to stay in school. This, the author claims, may explain why children leave school early. The results show that children who were treated indeed do better on cognitive scores. Moreover, they enter and leave school early.

However, it is not clear how much of this is related to parental investment. For example, as children finish schooling, it may also be possible that parents may have less agency, making it hard for one to interpret such a result in light of parental response per se. However, the decision to send school early may be thought of as a noisy measure of parents reinforcing the positive shock. Unfortunately, the authors do not study time investment in schooling, schooling expenditures or other more direct measures of parental investment.

Early childhood disease: Conti, Heckman, Yi and Zhang (2011) is one of the most interesting papers which analyses parental response to early childhood shocks by analyzing Chinese twins in the context of the one-child policy. In contrast to Rosenzweig and Zhang (2009), who explored effects of twinning and birth weights in twins, on parental responses in the context of testing the quantity-quality (Q-Q) model, Conti, Heckman, Yi and Zhang (2011) develop and test a model which integrates the dynamic model of skill formation in Heckman (2007) with a standard intra-household allocation model (Becker and Tomes

1976; Behrman, Pollak and Taubman 1982). Early childhood health shocks are measured by early childhood disease (such as serious diarrhea and calcium deficiency) for those 0-3. Rich parental investment measures are available for those between 6-18, which include not only educational and medical expenditures but also time investment.

A key feature of their framework is the multidimensionality of child endowments, which also allows parents to compensate and reinforce along different dimensions. The theoretical model they develop predicts that while parents may adopt a reinforcing investment strategy in cognitive skills from a negative health shock, due to complementarity between health and cognitive skill, due to substitutability between health (cognition) and investment in health (cognition), parents may actually compensate in health.

The empirical evidence verifies the predictions from the theoretical framework. There is evidence of compensatory investment in health but parental investment reinforces early shock shocks through educational spending. Interestingly, the authors find no differential time investments among the twins, perhaps due to the public good nature of time spent by parents across twins (as reported in Rosenzweig and Zhang (2009) as well).

The results from this paper suggest that one reason why there is no consensus on whether parents reinforce in utero shocks may be because different studies are exploring shocks to different types of endowments (health versus cognitive versus a hybrid of skills), or because of the differential dimension along which parents investments are being measured, or the type of investment (public vs private good) or a combination of these factors.

One problem which the paper does not address satisfactorily is that the early childhood health shocks may not be exogenous. Twin-specific unobservables which determined

differences in endowments , may also be playing a role in neonatal and early childhood period. Moreover, there are at least three other issues which this paper does not deal with. First, gender biased intra-household allocation problem is not directly dealt with. This suggests a need for future work to carry out a similar exercise as in Conti et al. (2011) but that which integrates models of gender biased allocation in households with the literature on dynamic technology of skill formation as in Heckman (2007). Second, Conti et al. (2011) do not show how important such differential investment are in the determination of human capital outcomes for children. In other words, why is studying multidimensional nature of investment response so important? In fact, what role did any of the health or schooling investments play in determination of human capital outcomes? Third, Conti et al. (2011) did not study the role of parental investments during infancy (before 5): how did breastfeeding duration respond to the negative early life health shock? Did mothers take more vaccines but still spend similar time with both twins?

4.2.4 Summary of evidence on parental response

The literature on parental responses to skill endowments paints a complex picture. In some cases parents do not seem to be playing an important role (Bharadwaj, Loken and Neilson 2012), in others there is evidence of compensatory investments (Bharadwaj, Eberhard and Neilson 2011), while in most other cases there is evidence of reinforcement (Adhvaryu and Nyshadham 2012; Rosenzweig and Zhang 2009). A possible explanation for such diversity of responses is that skills are multidimensional and parents may be responding in different ways (time versus health care versus schooling expenditures) to shocks to

different types of endowment effects (health versus cognition versus non-cognitive skills), as argued by Conti et al. (2011).

The evidence of reinforcing parental investments is consistent with static complementarity as in Becker and Tomes (1976) i.e. endowment shocks are positively related to investments. However, such evidence does not test for dynamic complementarity i.e. whether fetal shocks and positively correlated with returns to parental investments. This requires not only exogenous variation to the fetal endowment but also to returns to parental investment, which most studies do not carry out (Almond and Mazumder 2013).

In contrast, most of the literature which has studied parental investment response to gender endowment of the child, finds evidence that parents tend to invest less in girls, in the prenatal period (Almond, Li, and Meng 2010; Bharadwaj and Nelson 2013) and post-natal period (Oster 2009). The gender results seem to be inconsistent with any explanation which focuses on parental aversion to inequality, and more consistent with theories of taste based/statistical discrimination and those which emphasizes the efficiency motive (returns to investment in girls are less than boys).

The next section of the paper will briefly discuss the role of parental investments on childhood outcomes.

4.3 Do parental investments matter?

The longterm effects of a fetal shock can be decomposed into those working through direct effects the fetal shock on child outcomes, and those which operate through investments. Bleakley (2010) suggests that at the optimal levels of parental investments, the marginal

returns to investments should be zero (based on the envelope theorem). So that even if parents compensate or reinforce in utero shocks, the effects which such shocks may have on children's outcomes should be zero. However, fetal shocks may affect children's outcomes if they alter the returns to parental investment. In other words, if there is evidence of dynamic complementarity in parental investments and fetal shocks.

However, it is not clear why actual parental investments may be optimal. The gender biased investment literature tells us that current evidence is consistent with gender based discrimination, which implies that the net private benefit from parental investments may not be equal to net social benefits, leading to negative externalities. This may be one reason for why parental interventions matter, which may actually increase child capacity irrespective of the effects on returns to investments. There is also evidence that education and health may have positive externalities, which parents do not internalize. Recent work on deworming suggest that such externalities may be large (Miguel and Kremer 2004). Uncertainty in returns to childhood investments may also imply sub-optimal investments.

There is a large literature, spanning multiple fields, which studies the association between different forms of parental investments (breastfeeding, vaccines, vitamin supplements) and child outcomes. It is beyond the scope of this paper to review the entire literature, particularly if one considers the medical literature which has extensively studied the effects of health investments such as breastfeeding.

Horta et al. (2013) review the vast medical literature for the World Health Organization (WHO), which includes several Randomized Controlled Trials (RCTs), on the longterm effects of breastfeeding (or promotion campaigns for breastfeeding) on child out-

comes. The main outcomes considered include obesity, blood pressure, total cholesterol, type-2 diabetes and performance in intelligence tests. The meta-analysis shows that there are significant effects of breastfeeding on these outcomes. For example, the authors found that pooling all studies showed that breastfeeding was associated with an improvement in 3.5 points in test scores, with effects still as much as 2.2 points higher when low quality studies are excluded. Although these estimates are rather modest, the fact that high quality studies includes two randomized trials, suggests that evidence from most of the observed effects may well be causal.

Horta et al. (2013) also note that the effects of breastfeeding in general are largest for children and adolescents, consistent a gradual depreciation of the effects over time. However, as the authors note, most of the studies even in developed world, are based on observational studies which may suffer from self-selection and omitted variable biases. Despite the wealth of studies, there seems to be a greater need for more rigorous evaluations, particularly RCTs, which can analyze effects of breastfeeding (say via breastfeeding promotion programs) on children's longterm outcomes.

Future studies in economics should explore the causal effects of breastfeeding further, and how it changes with age. Even if breastfeeding duration changes as a response to early childhood shocks, this may not be an important channel explaining the longterm effects on fetal shocks. In that case, the low returns to breastfeeding should be consistent with the argument made by Bleakley (2010) that investments per se do not matter if the returns to investment are not affected. Future work should also explore spillover effects and other forms of externalities which may give us other reasons to investment in breastfeeding.

Engle et al. (2011) is perhaps more relevant, as it exclusively assesses effects of parenting skills on children in low-income and middle income countries. The authors, which include economists, review the literature on the effects of early childhood investment programs on children's outcomes, with a focus on parenting components of the programs. Their review includes 15 assessments, 11 of which were effectiveness trials and 4 were scale-ups. Interventions included group sessions, home visits, community activities, primary health care and nutritional services targeted at improving parent-child interactions. Out of these studies, 7 exclusively targeted parents or caregivers, whereas 8 targeted parents/caregivers in the presence of their children.

The studies showed significant benefits of parenting interventions on cognitive, social-emotional development, and learning activities with children. The programs which included both parents and children had greater effects than those which targeted parents only. Effects were also strongest on younger children than on older children, and stronger for poorer rather than richer children. Those programs which were primarily information based registered modest effects. The best programs were those with superior training methods for staff, a structured curriculum, and opportunities for parenting practice with child feedback.

One way to interpret the large body of experimental evidence cited in Engle et al (2011) is that the shocks to parenting skills improve the returns to parenting, and it is these changes in returns to parenting, rather than parental investment per se, which is showing strong effects.

There is a need for future work shock can not only identify shock to fetal endowments, but shocks to returns to parental investment (as in Engle et al 2011) as well as explore the role of spillover effects of such shocks in making parental investment worthwhile.

4.4 Summary of Evidence

Table 4.1 summarizes the recent literature on parental response to childhood endowments in developing countries. I roughly categorize the literature in two main dimensions. First is along stages of the life cycle, as the first column of Table 1 shows. Second, is along what I call skill versus gender biased responses. In my knowledge, this is the first time the gender biased investment literature has been discussed in the context of the more recent literature on parental investment responses to in utero shocks. The life cycle approach to investments has also not been seriously taken, as is evidenced by the fact that most studies only look at parental investment at one stage of the life cycle.

Although the methods of the gender bias studies suffer from potential biases, due to selection concerns and lack of a good natural experiment, studies which explore parental investment response to gender find that girls have a disadvantage both in the prenatal period and early childhood period. These effects are found not only in certain South Asian countries, but in China as well. It remains to be seen how other developing countries behave in this regard. Also, not much is done on gender biased investments for girls older than 5. An interesting possibility is to explore not only differential investments in schooling, but also dowries versus bequests to sons. Similarly, exploring the substitutability of dowries

(son bequests) versus early childhood investment may also be a fruitful area of research, in the context of an endowment shock (either gender or skill based shock).

The table clearly illustrates that most of the work on parental investment response to differences in skill endowments tends to focus on those aged 5-15. Overall, for both early childhood and adolescence, evidence is in favor of reinforcing responses. Various methods have been used for identification of the fetal shock, including birth weight differences in twins, regression discontinuity design, as well as public programs on iodine supplements and malaria eradication. Clearly, there is much more more for research on parental responses on other identifications such as deworming in early childhood, and exposure to Ramadan, to civil war and to pollution in utero. In general, however, the best identifications will be those which can identify shocks to specific endowments as iodine or vitamin supplements, because investment responses may well be different to different types of endowments shocks as Conti et al. (2011) document in the case of China.

A particular concern with most studies is the noisy measures of parental investment which are used. For example, Venkataramani (2012) just look at entry and exit for schooling, which may well be endogenous to childhood characteristics (personality, cognition,etc) rather than a pure parental response effect. Similarly, Bharadwaj, Loken and Neilson (2012) look at quality of school attended which again may be catching up the direct effects of smarter children nudging their parents end them to better schools (with other similar minded smarter kids) rather than a pure parental investment effect. There is a need for cleaner measures and more multidimensional measures (Conti et al. 2011).

Last, all the studies cited have provided evidence in favor of static complementary i.e. child endowments are positively correlated with parental investments. But what is not clear is why and how such parental responses can affect childhood outcomes (Bleakley 2010). Current evidence from epidemiology/medicine on breastfeeding and investment in parenting skills show that parenting can (indirectly or directly) can have positive effects on childhood development. However, arguably, one of the most promising channel through which childhood endowment shocks may play a significant role is by changes in the returns to investments i.e. through dynamic complementarity. (Heckman 2007). One possibility is to look at differential returns to programs with parental investment components by exposure to fetal shocks, an approach somewhat similar- though still distinct- from that of Bhalotra and Venkataramani (2012) who consider gender differences in brain- versus brawn-intensive occupations in Mexico, for those who benefited from a gender neutral early childhood sanitation program.

4.5 Conclusion

A growing literature has been documenting the longterm effects of fetal shocks (Almond and Currie 2010). However, the channels through which fetal shocks can affect longterm outcomes are not clear. To what extent are the effects driven by biology versus driven by parental investment behavior? A review of the most cutting edge research in this area suggests that parents tend to reinforce fetal skill shocks and that there is a female disadvantage when it comes to such responses. Parental responses are not restricted to one stage of the life cycle, but occur both during prenatal and postnatal periods in multidimen-

sional ways. However, much work needs to be done not only on integrating the literatures on gender biased investments to skill based shocks, but to develop a life cycle approach to parental investment identifying substitutability of investments in one period with another, as well as identifying the sensitive and critical periods when parental investments are the most productive in shaping life time outcomes. An exciting time of research waits ahead of us!

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Table 4.1: Recent Studies: Parental Responses to Fetal Shocks in Developing Countries

Study	Methods	Outcomes	Results
<i>Prenatal Investments</i>			
Almond et al. (2010)	Diff-n-diff	Neonatal mortality	Female bias
Bharadwaj and Nelson (2013)	Diff-n-diff, mother fixed effects	Vaccines, iron supplements, home delivery	Female bias
<i>Postnatal Investments: Age 0-5</i>			
Oster(2009)	Diff-n-diff	Vaccines	female bias
Barcellos et al. (2010)	Sample restriction: Son-biased stopping rules	Vaccines, breastfeeding, time spent, vitamins	Female bias
Adhvaryu and Nyshadham (2012)	Iodine supplementation	Vaccines, breastfeeding supplements, home delivery	Reinforcing spillovers
<i>Postnatal Investments: Age 5-15</i>			
Bharadwaj et al. (2011)	Twins/siblings	Books given, time spent	No effects (twins) compensating (siblings)
Rosenzweig and Zhang (2009)	Twins/family fixed effects	Schooling expenditures	Reinforcing
Bharadwaj et al. (2012)	Regression discontinuity	Schooling quality, time spent	No effects
Venkataramani (2012)	Malaria eradication	Age of entry, exit of school	Reinforcing
Conti et al. (2011)	Twins, multidimensional	Schooling, medical expenditures, time spent	Reinforcing and compensating

Chapter 5

Conclusion

This thesis has explored the complex interaction between adverse shocks and human development through multiple dimensions. My first essay found strong evidence that norms and informal institutions during pregnancy can have persistent effects on skill accumulation and labor market outcomes at the various stages of the life cycle of the fetus. The case in point being exposure to the Islamic month of fasting Ramadan, when every year millions of Muslims globally fast, including pregnant women. I argue that maternal fasting during Ramadan may be a likely channel, as it has been understood to have an accelerated starvation type effect on the fetus in the medical literature. This result is significant not only from a historical perspective, since Muslims have been fasting for over 1400 years, but also from a particular public policy perspective as more than 1.2 billions Muslim in 2012 alone were potentially exposed to Ramadan. Many more are expected to be exposed every year in the foreseeable future. Clearly, there is need for extensive research on this topic as

it has immediate policy implications on the lives of potentially billions of people around the world.

My second essay finds that the Rwanda genocide of 1994 led to significant destruction in stock of human capital for those who had completed primary schooling. Moreover, the effects were found to be at the aggregate level and not at sub-national level suggesting the macro nature of the shock. This suggests that severe shocks can increase inequalities at the aggregate level without doing so at the sub-national level, a caution for many recent studies which just looking at sub-national in particular. Stock of human capital has been documented to affect aggregate output, fertility and human capital of the next generation, suggesting the significance of this result. Since educated cohorts seem to be particularly vulnerable, this paper suggests a greater need for investment in complementary goods such as security and skill-based insurance.

My third essay explores the role which society, in particular parents, play in the link between adverse shocks and human development. I find that parents in general reinforce adverse shocks and tend to discriminate against females at various stages of the life cycle in multidimensional ways, suggesting that the channels through which insults shape human development, may be not just biological, but social and economic as well.

Adverse shocks, even if they are small, can have large effects on human development of society at the individual and aggregate level, and society itself may play in critical role in reinforcing such shocks.