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Three Essays on Family and Labor Economics

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ABSTRACT

THREE ESSAYS ON FAMILY AND LABOR ECONOMICS

BY

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This dissertation consists of three distinct yet interrelated essays in family and labor Economics. In particular, I examine the impact of household demographic and socioeconomic characteristics on household decisions related to the investment in child human capital such as fertility, child health and child labor. The first chapter examines the impact of women's education on household fertility decision. I use the change in the length of primary schooling in Egypt in 1988 to extract an exogenous variation in female education using a nonparametric regression discontinuity design. My analysis shows that female education significantly reduces the number of children born per woman. The reduction in fertility seems to result from delaying maternal age rather than changing women's fertility preferences. I also provide evidence that female education in Egypt does not boost women's labor force participation or increase their usages of contraceptive methods. Female education, however, does increase women's age at marriage which might explain the delay of maternal age.

The second chapter uses the same identification strategy to examine the impact of parental education on child health outcomes. The results suggest that parental education does not have significant effects on child mortality or nutritional status. I provide evidence that among low-educated parents in Egypt, education has no significant impacts on parents' intermediate

outcomes that are essential to improve child health such as literacy skills, access to information, and health behavior.

The third chapter examines the effect of income on parents' decision to send their children to work. The empirical literature on child labor has found conflicting results regarding whether poverty does or does not lead parents to send their children to work. A majority of these studies treat child laborers as a single homogeneous group. Recent data from the International Labor Organization, however, reveals substantial differences among child workers in employer types, work patterns, and work intensity. This suggests that parental reasons for sending their children to work could vary with the type of work, which might explain discrepancies in previous studies. I use data from the 2010 Egypt National Child Labor Survey to estimate the effects of parental income on child labor for various subpopulations of working children. In addition to measures of household characteristics, this dataset provides rich information about the working conditions of child laborers. My analysis shows that the effect of parental income on child labor is minimal among children who work in family businesses and in jobs not highly physical, jobs nonhazardous, and jobs during school breaks. In contrast, higher parental income does decrease the likelihood of child labor in market work, jobs that are physical, hazardous jobs, and full-year jobs. In short, higher family incomes deter types of child labor most harmful to children, while having little effect on types of work least likely to be harmful.

THREE ESSAYS ON FAMILY AND LABOR ECONOMICS

BY

FATMA ROMEH MOHAMED ALI

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ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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DEDICATION

To my mother, Shadia Mohamed Elsayed, and my father, Romeh Mohamed Ali.

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Introduction

Household decisions have significant impacts on child cognitive and noncognitive skills, which affect children's educational attainment and earnings, plus other outcomes such as crime and health. Recent research has shown that a considerable portion of poverty and income inequality in society is due to factors determined by age 18 (Cunha & Heckman, 2007). Thus, understanding how poor households make investment decisions in their children and what incentives motivate them are crucial in designing effective interventions to reduce poverty. In this dissertation, using Egypt as a case study, I examine the impacts of household demographic and socioeconomic characteristics on household decisions related to fertility, child health, and child labor. I employ both econometric analysis and program evaluation methods, such as regression discontinuity, to extract exogenous variations and estimate causal effects.

The first chapter of this dissertation investigates the relationship between women's education and fertility outcomes. Gary Becker's child quantity-quality trade-off hypothesis suggests that smaller family size enhances children's outcomes since parents with fewer children have more resources available to spend on each child. Therefore, it is necessary to understand household decisions about how many children to have in order to design policies that effectively reduce family size and improve children's outcomes. In this chapter, I focus on the relationship between female education and fertility. Previous empirical studies of developing countries that examined this relationship have faced difficulties in handling the problem of omitted variables such as a woman's innate ability, preferences, and community background. For instance, some communities place a small value on female education and encourage big family sizes. In such context, both female education and family size are driven by other factors that are difficult to control for in empirical models.

In this chapter, I use the change in length of primary schooling in Egypt as the source of exogenous variation in education. Beginning in 1988, the Egyptian government cut the number of primary school years from six to five, moving from a 12-year system of pre-university education to an 11-year system. This policy change applied to all individuals born on or after October 1977. Using pooled cross-section data from 1992 to 2014 and a nonparametric regression discontinuity approach, I compare education and fertility of women born just before and right after October 1977. The results show that female education reduces the number of children born per woman and postpones maternal age. Women's education, however, does not appear to change women’s preferences toward the ideal number of children they want to have. I also provide evidence that female education in Egypt does not improve women’s job opportunities or increase their usages of contraceptive methods. Female education, however, does increase women’s age at marriage which explains the delay of maternal age.

The second chapter of my dissertation uses the same identification strategy of the first chapter to examine the impact of parental education on children’s health outcomes. The majority of the previous research has found that increasing parental education, especially maternal education, improves child health. For instance, a recent study that has been published in the British medical journal, the Lancet, found that half the reduction in child mortality over the past four decades can be attributed to the improvement in female education. The extent, however, to which these estimates amount to causality has remained a challenge, not least because of the potential endogeneity of parental education. In this chapter, I exploit the reduction in the length of primary schooling in Egypt in 1988 to create exogenous variations in parental education. Using data from the Birth Recode modules of the Egyptian Demographic and Health Survey (1995-2014) and applying a nonparametric regression discontinuity approach, I found that parental education improves child nutritional status and reduces mortality. These effects, however, are not statistically significant across a broad range of specifications and different restrictions on the sample. I also provide suggestive evidence that education has little effects on parents' intermediate outcomes that are expected to be essential to improve child health such as literacy skills, access to information, and health behavior.

The third chapter of my dissertation examines the effect of income on parents' decision to send their children to work. The empirical literature on child labor has found conflicting results related to the effect of parental income on child labor. A majority of these studies treat child laborers as a single homogeneous group. Recent data from the International Labor Organization, however, reveals substantial differences among child workers in employer types, work patterns, and work intensity (Diallo, Etienne, & Mehran, 2013). In particular, some children face unfavorable work conditions such as working in jobs that are highly physical and hazardous. On the other hand, other children work in favorable work conditions such as working in their families' businesses or working only during their school breaks.

The heterogeneity among working children suggests variations in parental perceptions on child's work, and hence, in parental reasons to send their children to work. To illustrate, some families, despite being nonpoor, might prefer to engage their children in work if they perceive nonpecuniary returns to child's work. Examples of these non-pecuniary returns include teaching children the importance of education in enhancing future outcomes and helping children develop self-reliance and independence. Parents who engage their children in work for non-pecuniary returns are more likely to ensure that their children gain these benefits under favorable working conditions. Therefore, they are less likely to allow their children to become exhausted from work or let work deter them from going to school. On the other hand, parents who view child's work as an additional source of income for the household (pecuniary return) are more likely to opt for types of child's work that have significant monetary returns but might provide unsafe and highly demanding working conditions for their children.

To support this argument, I use data from the 2010 Egypt National Child Labor Survey to estimate the effects of parental income on child labor for various subpopulations of working children. This dataset provides rich information about the working conditions of child laborers in addition to measures of household characteristics. My analysis shows that higher parental income does decrease the likelihood of child labor in unfavorable work conditions such as jobs that are physical, hazardous jobs, and full-year jobs. In contrast, higher family income does not significantly reduce child's work in jobs not highly physical, jobs nonhazardous, and jobs during school breaks. This finding gives rise to other nonpecuniary motivations that might drive parents' decisions in this case.

Chapter I: The Impact of Female Education on Fertility: A Natural Experiment from Egypt

1. Introduction

Educating girls has usually been advocated as an effective way to curtail rapid population growth in developing countries (UNFPA, 1994). The common justification, driven by Becker's writings and others, is that highly educated women tend to have higher income jobs and hence higher opportunity costs of childbearing. Additionally, highly educated women tend to have superior knowledge and practices regarding contraceptive methods (Becker, 1960, 1993; Becker & Lewis, 1973).

Earlier empirical studies of developing countries in the 1990s and early 2000s documented a negative correlation between female education and the number of children born per woman (Al-Qudsi, 1998; Bhargava, 2007; Cochrane, Khan, & Osheba, 1990; Handa, 2000; Lam & Duryea, 1999; Martín & Juάrez, 1995). These studies, however, did not adequately account for the endogeneity of female education. Unobserved factors such as a woman's ability, preferences, and family and community background are all expected to be correlated with schooling and fertility decisions. For instance, some communities in developing countries place a small value on female education and encourage big family sizes. In this cultural context, a negative correlation between female education and fertility can be observed even if female education per se does not have a causal impact on fertility.

To correct for the endogeneity of education, few recent studies of developing countries used government education reforms in Indonesia and Nigeria as sources of exogenous variations in education (Breierova & Duflo, 2004; Osili & Long, 2008). My chapter contributes to this growing literature by using a natural experiment from the Middle East to create an exogenous variation in education and examine its impact on fertility. In particular, I use the change in the length of primary schooling during 1988-89 to handle the endogeneity of education. Beginning in 1988, the Egyptian government cut the length of primary education from six to five years, moving from a 12-year system of pre-university education to an 11-year system (law No.233 of 1988)[1]. The five-year primary system was universal throughout the country. The first school cohort subjected to this system was born between October 1, 1977 and September 30, 1978.

Therefore, October 1, 1977, represents a cutoff date such that individuals born before that date had to attend one more year of primary schooling than individuals born on or after that date. Assuming that women born immediately after and just before October 1, 1977, are similar in baseline characteristics, the differences in their completed years of education in adulthood are exogenous. In fact, even if some parents can plan when their children are born, they are less likely to have full control over the exact date of birth. Moreover, the elimination of grade 6 occurred in 1988, and it applied immediately to children who were about 11 years old at that time. Hence, it is unlikely that parents would have anticipated that such a policy change would have happened 11 years in the future.

Therefore, the policy change can be regarded as good as a random local experiment around the cutoff date allowing one to compare fertility of women born right before and right after October 1, 1977, and relate this to the difference in their education. My identification strategy is quite close to the recent studies of developed countries that attempted to create

[1] Before this change, the high school diploma in Egypt was composed of six years primary school, three years preparatory school, and three years secondary school, for a total of 12 years. After the policy change, the high school diploma was composed of 11 years: five years in primary school, three years in preparatory school, and three years in secondary school. Following the elimination of grade 6, two cohorts of primary school graduated simultaneously and entered the middle school together in 1989.

exogenous variation in education to explore its impact on fertility (Amin & Behrman, 2014; Cygan-Rehm & Maeder, 2013; McCrary & Royer, 2011; Monstad, Propper, & Salvanes, 2008).

To examine the effect of education on fertility, I focus on three outcomes: the number of children ever born, ideal number of children, and age at first birth. I use a nonparametric regression discontinuity (RD) design. In particular, I estimate kernel-based local linear regression models for education and fertility. I also carry out a complementary analysis using local exponential mean regression models for count responses. The data for this study come from the recent seven waves of the Egyptian Demographic and Health Survey (DHS) (1992, 1995, 2000, 2003, 2005, 2008, and 2014). The total sample size for the six waves is 97,314 ever-married women of reproductive age. The DHS data provides rich information on fertility history and socioeconomic and demographics factors. Most importantly, for the purpose of this study, the data provides information about the year and month of birth of each woman so that one can identify which woman attended which primary school system.

This paper contributes to the growing literature that examines the causal effect of education on fertility. In particular, using a change in the length of primary schooling in Egypt, I estimate the causal effect of female education on fertility in the context of a developing country. To the best of my knowledge, this is the first study to use this natural experiment to create an exogenous variation in education in Egypt. The results show that women who faced the six-year primary system had completed in adulthood, on average, one more year of schooling in comparison to women who had faced the five-year primary system. Using this variation in education, my results show that each year of female education reduces the number of children born per woman by 0.104 children. That is, a woman with nine years of compulsory education has about one less child than a woman with no formal education. The results also show that the reduction in number of children seem to result from postponing maternal age rather than a change in women's attitudes and preferences. In particular, I found that each year of female education postpones maternal age by 1.8 to 2.8 months, with no significant impact on women's ideal number of children. I also provide evidence that the delay of maternal age results from delaying marriage rather than increasing women's labor force participation or increasing their usages of contraceptive methods. Second, contrary to almost all empirical studies of developing countries that used a single-year household survey data, access to seven waves of relatively richer pooled cross-sectional data facilitates the application of regression discontinuity design as well as carrying out the ensuing robustness checks.

The remainder of the chapter is organized as follows. Section 2 summarizes the existing literature on education and fertility and briefly discusses the change in the length of primary schooling in Egypt. Section 3 describes the EDHS data. Section 4 explains the regression discontinuity design. In section 5, I present the results of this chapter dividing them into baseline results ignoring the endogeneity and the main results of the RD analysis. Section 6 provides robustness checks. Section 7 investigates the channels through which female education affects fertility. Finally, section 8 provides the conclusion.

2. Background

I first summarize the theoretical background on the relationship between female education and fertility before turning to the review of the empirical literature. I then provide further details about the change in the length of primary education in Egypt.

Standard economic theory has ambiguous predictions about the relationship between female education and fertility. According to the substitution effect, a high level of education increases the opportunity cost of childbearing through enhancing women's labor market outcomes (Becker, 1960). Becker and Lewis (1973) also highlighted that more educated parents tend to invest more in children's human capital, resulting in increases in the cost of fertility per child. Moreover, education may reduce the cost of avoiding pregnancy because more educated women tend to be more efficient in using contraceptive methods (Rosenzweig & Schultz, 1989). On the other hand, higher education also has an income effect that may lead to the opposite conclusion. Highly educated women tend to have more earnings and hence can afford to have more children assuming children are normal goods. Another channel of the income effect, introduced by Behrman and Rosenzweig (2002), is that a highly educated woman tends to be matched with a highly educated partner, resulting in increased family income that allows having more children.

In addition to income and substitution effects, Jejeebhoy (1995) emphasizes the role of cultural context in the relationship between education and fertility. She argues that education improves a woman's autonomy through improving her social and economic self-reliance and hence enables her to make her own decisions. However, the power of education in enhancing a woman's autonomy is largely dependent on the contextual factors and woman's position in society. For instance, in societies with wide gender disparities, small amounts of education may have a negligible impact on a woman's autonomy and hence a weaker effect on fertility.

Whether the substitution effect dominates the income effect, and under which cultural context, remains largely an empirical question. A considerable number of empirical studies have been published in the past three decades to examine the impact of education on fertility. Empirical studies of developing countries have documented a negative association between education and fertility. These studies examined a broad range of countries such as several Arab countries (Al-Qudsi, 1998), Sierra Leone (Balley, 1989), Egypt (Cochrane et al., 1990), Jamaica

(Handa, 2000), Brazil (Lam & Duryea, 1999), and Latin American (Martin, 1995). Nearly all these studies did not adequately control for confounding variables, such as women ability, preferences, and family background that are expected to affect both education and fertility. Therefore, endogeneity remains a grave concern in the studies of developing countries, and so their results can be best regarded as partial correlations rather than causal impacts.

In an attempt to account for endogeneity, few recent studies have used governmental education reforms to create exogenous variations in education. In particular, Osili and Long (2008) used the Universal Primary Education (UPE) program in Nigeria as a natural experiment to estimate the impact of education on fertility. The UPE program provided tuition-free primary education and constructed new schools. The program had been heavily implemented in the states where educational facilities were initially low. The authors exploited the variation in funds distributed to each region to create an exogenous change in education. Using data from the 1999 Nigeria Demographic and Health Survey, they found that the UPE program had significantly increased female education and reduced fertility. Similar methodology and results were obtained by Breierova and Duflo (2004) using a massive school construction program in Indonesia during 1973-78 and data from the 1995 intercensul survey of Indonesia.

Recent studies of developed countries have exploited exogenous variations in education to establish a causal relationship between education and fertility. The most common exogenous variation used in this literature is the change in education policy. For instance, in 1959, Norway extended the number of compulsory years of schooling from seven to nine years. Monstad et al. (2008) exploited the time and regional variations in the program intensity as an instrumental variable. They found that extending compulsory education increased the years of schooling women attained, but there is no evidence that more education resulted in more women remaining childless or having fewer children. Using a similar policy change in West Germany between 1958 and 1969 which extended the compulsory schooling from 8 to 9 years, Cygan-Rehm and Maeder (2013) found that education reduced the number of children born per woman. McCrary and Royer (2011) used data from California and Texas to examine the effect of education on fertility for mothers born just before and after the school entry date. They found that women who enter school earlier tend to have more schooling years than women who begin later; however, there are no significant differences between them in fertility outcomes. Amin and Behrman (2014) found opposite results using twins data from Minnesota and applying twins fixed effects.

There is no consensus among the studies of developed countries on the impact of female education on fertility. Additionally, the socioeconomic and cultural contexts differ dramatically between developed and developing countries, which are expected to play important roles in the relationship between female education and fertility (Jejeebhoy, 1995). Therefore, one cannot generalize results from developed countries, if any, for the developing countries. The studies by Osili and Long (2008) and Breierova and Duflo (2004) attempted to handle the endogeneity of education in the case of developing countries using government reforms as sources of exogenous variations in education. There are some concerns, however, about using government programs as a source of exogenous variation as highlighted by Meyer (1995). Governments do not usually assign their programs randomly across regions. The characteristics of regions are taken into consideration. Additionally, political factors such as the election system and the level of corruption play crucial roles in developing countries regarding the amount of funds distributed to each region.

This chapter estimates the causal impact of female education on fertility in one of the Middle Eastern countries, Egypt, using the elimination of the sixth grade in 1989-88 as a source of exogenous variation in education (law No.233 of 1988).[2] Before the policy change, students attended a total of 12 years toward completing their high school diploma. After the policy change, students attended a total of 11 years toward completing their high school diploma[3].

The first school cohort subjected to the five-year primary system was born on or after October 1977. I use a regression discontinuity approach to compare education and fertility of adult women born close to October 1977 but attended different primary school systems. The change in the length of primary schooling in Egypt provides a good opportunity to explore the causal impact of female education on fertility in developing countries for several reasons. First, the five-year primary system was mandatory and universal all over the country, so households were not able to let their children attend different primary school systems. Another reason is that the switch to the five-year primary system was announced by the government in June 1988 and was immediately applicable to children who were about 11 years old at that time. Thus, it is unlikely that parents would have anticipated that change and adjusted their fertility behaviors accordingly[4].

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[2] There is no single explanation as to why the Egyptian government had implemented this change. Most of the evidences on that reform are drawn from the newspapers at that time. One of the reasons highlighted by some governmental officials is that the purpose of the policy change was to reduce the burden on the average Egyptian household by reducing their out-of-pocket spending on primary education. Another reason that was mentioned is that the policy change was a step to cope up with educational systems in developed countries that were claimed to implement an 11-year rather than 12-year pre-university system. The most common explanation, however, for the reduction in primary school years is that, by the mid-1980s, Egyptian elementary schools suffered from a shortage of classroom space because of large enrollments of children in the elementary schools. This problem led the government to shorten the length of the primary education from six to five years to save sixth-grade classrooms to absorb the large enrollment in the first grade.

[3] It is worth mentioning that starting in 2000 the number of years in primary school was changed back to six years. The first cohort who faced this change has not shown up yet in the DHS survey. The reverse change in 2000 was motivated by the government's desire to ensure that children spend enough time in primary school to get better education.

[4] Section 6 of this chapter tests for differences in baseline characteristics among women born right before and right after October 1977 and finds no evidence of significant differences.

3. Data

This chapter uses the seven recent waves of the Egypt Demographic and Health Survey (EDHS): 1992, 1995, 2000, 2003, 2005, 2008, and 2014. Each wave of the survey covers ever-married women of reproductive age (15-49)[5]. I focus my analysis on women age over 21 years to reduce the censoring in fertility variables, and because it is more likely that women would have completed their education by that age [6]. The total sample size for the seven waves is 97,314 women. The survey provides information about three fertility outcomes: the number of children ever born per woman, the preferred (ideal) number of children, and the age of each woman when she delivered her first child. The sample size differs across these variables. All the women in the sample (97,314 women) answered the question about the total number of children ever born to them. Of these women, 6,416 reported having no children at the time of the survey. The remaining 90,898 women in the sample who did report having children reported their ages at the time of their first birth. All the women in the sample were also asked about their preferred (ideal) number of children. There were 84,849 women able to provide a numerical response to this question.

Information about women's education is also available in the survey. The survey asked women about their completed educational levels and grades. For instance, if a woman mentioned that she left school after the second grade of preparatory school level (middle school), then I compute her attended years of schooling as follows: six years primary if she was born before October 1977 (five years if she was born on or after October 1977) + two years in preparatory

[5] My analysis is focused on the sample of ever-married women since only for this sample does the Egypt DHS collect detailed information on fertility as well as the respondent's birth month and year, which is necessary for the RD strategy. In Section 6 below, I examine the potential sample selection bias that may arise due to the exclusion of never-married women.

[6] Women in Egypt are less likely to return school after leaving it, especially married women. The survey asked women whether they were attending school at the time of the survey. Less than 0.1 percent of the women have reported attending school at the time of the survey.

school = 8 years of schooling (7 years if she was born on or after October 1977). Additionally, the survey includes demographic and socioeconomic information. The questionnaires and other information about the survey are available in the recent EDHS report (Ministry of Health and Population [Egypt], El-Zanaty and Associates [Egypt], & ICF International, 2015).

Figure 1 depicts the trends in the fertility rate and the average years of schooling of women during 1992-2014. As can be seen from the graph, the average number of children born per woman has been declining over time from 4.2 in 1992 to 2.9 in 2014. During the same period, the average years of education of women has increased from 4.3 in 1992 to 8.3 in 2014. Thus, from a time series data perspective, there is a negative correlation between education and fertility over time. Figure 2 depicts the same relationship using cross-sectional data for women of completed fertility for each year separately. This chapter examines whether this negative correlation is a causal relationship.

Figure 1: Average numbers of children born per woman and average years of education

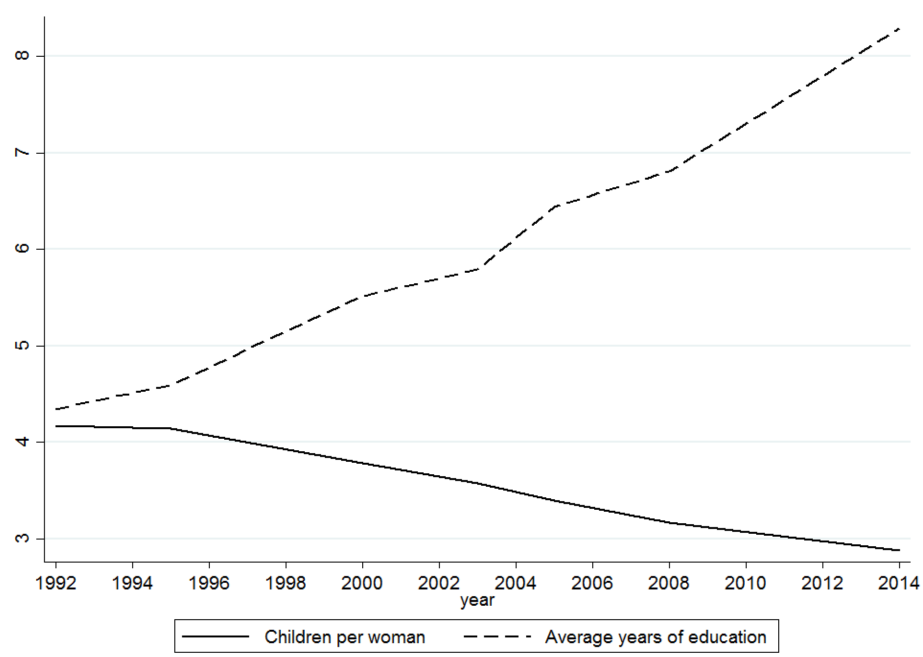

Figure 2: Average number of children born per woman and years of education (age 40-49)

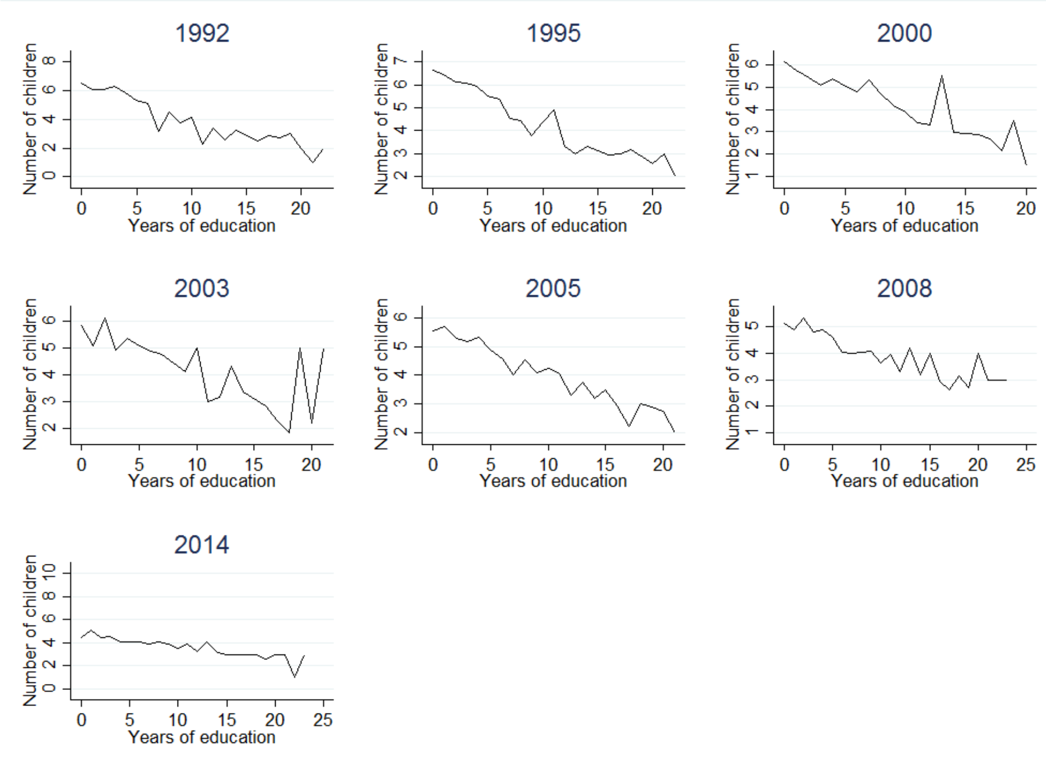

Table 1 provides means and standard deviations of fertility and education variables. Women in the full sample completed, on average, about 6.2 years of schooling and have 3.5 children. The sample of mothers in column 2 seems to be slightly less educated compared to the full sample in the first column. The average age of a mother at the birth of her first child is about 21.1 years. The last column provides descriptive statistics for the third fertility outcome: the ideal number of children. The sample in this column includes women who were able to provide numerical responses for their ideal numbers of children. These women seem to be more educated compared to the full sample. Their average ideal number of children is three kids (less than the actual number of children born per woman).

Table 1: Means and (Standard Deviations) for Fertility and Education Variables

Variables	**Number of children born per woman**	**Age at birth (Mothers' Sample)**	**Ideal number of children**
Fertility Outcome	3.500	21.108	3.027
	(2.333)	(4.120)	(1.441)
Education	6.245	6.132	6.689
	(5.862)	(5.837)	(5.865)
Observations	97,314	90,898	84,849

EDHS data (1992, 1995, 2000, 2003, 2005, 2008, 2014). The number of children born is based on the actual number of children born per woman at the time of the survey. Age at first birth is the age of a mother when she delivered her first child. The ideal number of children is a woman's preferred number of children.

4. Methodology

This section describes the RD design I use to estimate the impact of female education on fertility.[7] I employ a nonparametric RD approach exploiting the reduction in the length of primary schooling in Egypt in 1988-89 from six to five years as a source of exogenous variation in education.

The first school cohort who was subject to the five-year primary system were the children who were in their fifth grade during the academic year 1988-89 (September 1988 – May 1989). Given that a child has to turn six before October 1 to enter school in Egypt, this implies that children who were in their fifth grade in the school year 1988-89 were born between October 1, 1977 and September 30, 1978. Therefore, October 1977 represents a cutoff date such that individuals born before that date had to attend one more year of primary schooling.

A standard linear RD model for the fertility outcomes for woman *i* can be specified as:

$$fertility_i = \alpha_1 + \alpha_2 D_i + \alpha_3 (X_i - c) + \alpha_4 D_i * (X_i - c) + \alpha_5 Age_i + \varepsilon_{i,} \quad (1)$$

where $fertility_i$ refers to three outcomes: the number of children ever born to woman *i*, the ideal number of children of woman *i*, and the age of woman *i* at her first birth; X_i is the forcing

[7] See Imbens and Lemieux (2008) and Lee and Lemieux (2010) for a comprehensive review of the RD approach.

variable which is woman i's date of birth (expressed in year of birth and month of birth). Here c refers to the cutoff date at October 1977. Hence, $(X_i - C)$ denotes a woman's date of birth relative to October 1977. For instance, if woman i was born on October 1978, the value of her $(X_i - C)$ would be 12 months. In Equation (1), D_i is an indicator variable for being born at or after the cutoff date $(X_i \geq C)$, Age_i is the age of woman i at the time of the survey (I control for age for fertility outcomes other than age at first birth), and ε_i is an idiosyncratic error term.

If female education is a binary variable that is determined solely by the type of primary schooling (five-year versus six-year systems), then the parameter α_2 in Equation (1) would identify the causal impact of education on fertility. This applies to the case of a sharp RD design where the probability of treatment jumps from zero to one when the forcing variable crosses the cutoff. In this chapter, the total years of education of adult women is a continuous variable that may be affected by the type of primary education system as well as other factors. Hence, a fuzzy RD design is more appropriate in this case. The discontinuity in education (edu_i) is modeled similar to Equation (1) as follows, where ν_i is the equation error term.

$$edu_i = \beta_1 + \beta_2 D_i + \beta_3(X_i - c) + \beta_4 D_i * (X_i - c) + \beta_5 Age_i + \nu_i, \qquad (2)$$

In the fuzzy RD design, the impact of female education on fertility can be computed as the ratio of the reduced form estimate of discontinuity in fertility, α_2, to the reduced form estimate of discontinuity in education, β_2, provided that the same bandwidth (discussed below) is used to estimate Equations (1) and (2). Hahn, Todd, and Van der Klaauw (2001) showed that the ratio of the two RD gaps, α_2/β_2, is numerically identical to δ_2 in the following local two-stage-least- squares (2SLS) system using the binary variable D_i as an instrument for education[8]:

[8] The equality between the Local 2SLS estimate of δ_2 and the ratio of the two RD gaps, α_2/β_2, requires using the same bandwidth in estimating Equations (1)-(4). I follow the recommendation of Imbens and Lemieux (2008) to use the same bandwidth of the outcome equation.

$$fertility_i = \delta_1 + \delta_2\, edu_i + \delta_3(X_i - c) + \delta_4 D_i * (X_i - c) + \delta_5 Age_i + \varepsilon_{2i}, \quad (3)$$

$$edu_i = \gamma_1 + \gamma_2\, D_i + \gamma_3(X_i - c) + \gamma_4 D_i * (X_i - c) + \gamma_5\, Age_i + \nu_{2i}. \quad (4)$$

Following the literature on local RD approach by Hahn et al. (2001), I estimate these equations using kernel-based local linear regressions[9]. This nonparametric setting uses a weighted linear regression with weights computed using a kernel function, such that observations far from the cutoff date get smaller weights than observations near the cutoff date. The weighting function I use is based on the triangle kernel $K(.) = \max\{0, 1 - |\frac{(x-c)}{h}|\}$ similar to McCrary & Royer (2011), where h is the bandwidth.

I implemented both the plug-in rules method (Imbens & Kalyanaraman, 2012) and cross-validation procedure to compute the bandwidth, h. Each of these bandwidth selectors, however, chooses a bandwidth that is quite large (it ranges from h=80 to h=100). I chose a smaller bandwidth of 60 months throughout this study. A bandwidth of 60 months allows, to some extent, to control for cohort effects by focusing the analysis within a 10-year birth cohort. As discussed below, the estimates are, however, quite robust to the bandwidth sizes within a reasonable interval of the chosen bandwidth.

One concern about the previous setup is that the number of children outcomes (the number of children ever born and the ideal number of children) are count variables and hence the local linear model might not fit the data in this case. To account for the nonlinearity of these outcomes, I also estimate local Poisson, more precisely local (nonlinear) exponential, regression

[9] One possible concern is that the forcing variable, the normalized date of birth, is measured in months, which is a discrete variable. Lee and Card (2008) argued that the local linear regression is not suitable in this case as it is not possible to compare outcomes very close to the cutoff date as the bandwidth can never be shrunk to zero. According to them, one should use global polynomial models with discrete forcing variables. Lee and Lemieux (2010), however, pointed out that even in the case of continuous variables researchers usually use data far from the cutoff date. Therefore, in this essay I estimate local linear regression models following other studies (McCrary & Royer, 2011).

models for these outcomes. The expected outcome, conditional on observed and unobserved heterogeneity components, is specified as

$$E(fertility_i|.) = \exp[\lambda_1 + \lambda_2\, edu_i + \lambda_3(X_i - c) + \lambda_4 D_i * (X_i - c) + \lambda_5 Age_i]\xi_i, \quad (5)$$

where ξ_i is the unobserved heterogeneity component. The error function that is the basis of generalized method of moments estimation can be specified as[10]

$$\zeta_i = [fertility_i / \exp[-\lambda_1 - \lambda_2\, edu_i - \lambda_3(X_i - c) - \lambda_4 D_i * (X_i - c) - \lambda_5 Age_i]] - 1. \quad (6)$$

Analogous to the case of local linear RD approach, I employ weighted local exponential mean regression with weights estimated using a triangle kernel function. The variable edu_i is modeled similar to Equation (4) above.

5. Results

5.1. Baseline Results

This section replicates the results of the previous studies on developing countries when ignoring the endogeneity of education. In particular, I estimate Poisson models for the number of children outcomes and a linear model for the outcome of age at childbearing. In this section, I use six waves of the EDHS survey: 1995, 2000, 2003, 2005, 2008, and 2014. I exclude the 1992 survey round because the wealth index, one of the primary control variables, is not available in this round[11]. The total sample of the six rounds is 88,426 ever-married women age between 22 and 49 (the sample for the outcome of age at first birth is 82,571 women; while, the sample for the outcome of the ideal number of children is 77,669 women).

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[10] See Mullahy (1997) and Wooldridge (2010) (Section 18.5) for details on GMM estimation of the basic exponential model with endogeneity.

[11] The 1992 round will be included in the total sample of the RD analysis. However, as explained in subsection 5.2.2 below, because the RD approach uses only a local sample within a chosen bandwidth, the exclusion of the 1992 round does not affect the RD results. Therefore, the findings from the RD analysis can be compared to the findings in this section.

Table 2 below provides means of key socioeconomic and demographic variables of that sample. The average number of children born per woman and the average years of education in this sample is very close to the full sample of Table 1 (3.43 and 6.44 compared to 3.50 and 6.25, respectively in Table 1). Women with 7 or more children have as low as 1.38 years of education on average; whereas, women who have only 1 or 2 children have 8.91 years. More than 70 percent of women with 7 or more children have no formal education, and almost none of them has a college degree. On the other hand, 24 percent of women with only one or two children has a college degree. Table 2 also shows that women with more children tend to live in rural areas and poor households compared to women with fewer children.

Table 2: Means of Main Variables for all Women by Number of Children

Variables	Child -less	1-2	3-4	5-6	7 & above	Full sample	Standard deviation
Current age	30.26	29.91	35.26	38.70	41.87	34.45	7.85
Number of Children	0.00	1.65	3.41	5.39	8.27	3.43	2.28
Education years	7.98	8.91	6.83	3.16	1.38	6.44	5.88
Has no education=1	0.28	0.20	0.33	0.57	0.73	0.48	0.50
Primary education=1	0.12	0.11	0.16	0.23	0.22	0.07	0.25
Secondary education=1	0.36	0.45	0.37	0.17	0.04	0.33	0.47
College & above=1	0.24	0.24	0.14	0.03	0.01	0.12	0.33
Urban=1	0.48	0.52	0.47	0.33	0.24	0.44	0.50
Use contraception=1	0.76	0.94	0.96	0.94	0.88	0.93	0.26
1st wealth quintile=1	0.16	0.13	0.17	0.29	0.40	0.20	0.40
2nd wealth quintile=1	0.17	0.15	0.18	0.24	0.27	0.19	0.39
3rd wealth quintile =1	0.18	0.19	0.19	0.20	0.19	0.19	0.39
4th wealth quintile=1	0.22	0.23	0.21	0.16	0.11	0.20	0.40
5th wealth quintile =1	0.27	0.30	0.25	0.11	0.04	0.22	0.42
Observations	5,855	27,926	31,874	13,717	9,054	88,426	-

EDHS data (1995, 2000, 2003, 2005, 2008, 2014). This table excludes the 1992 survey round because the wealth index is not available in this survey wave.

Table 3 reports average marginal effects (AMEs) of female education on three fertility outcomes: number of children born per woman, age at childbearing, and the number of children preferred. The first specification (Spec 1) controls for religion (Muslim versus Christian) which shrinks the sample to 65,959 women because religion is missing in two survey rounds: 2000 and 2003. In the second specification (Spec 2) I exclude religion and run the regression on the same sample as Spec 1. In Specification 3 (Spec 3), I exclude religion and run the regression on the full sample (88,426 women). In all the specifications, I control for age at the time of the survey, age at the time of survey squared, marital duration, a set of dummy variables for years of birth, a dummy variable for contraception usage, a dummy for urban region, and a set of dummy variables for household wealth quintiles.

As can be seen from the results, excluding religion has a slight effect on the results. This can be explained by the fact that 95 percent of the Egyptian population are Muslims. This percentage is almost the same across all the fertility groups, indicating that religion may have little explanatory power in fertility behavior.

I focus my discussion on the results given by Spec 3 as the preferred set of estimates. Panel (a) shows the results of the effect of education on the number of children born per woman. Other factors held constant, each year of female education reduces the number of children born per woman by 0.08 children. This estimate is statistically significant at 99 percent confidence level. This result is consistent with previous studies of developing countries that ignored endogeneity. In particular, my estimate is comparable with Balley (1989), Al-Qudsi (1998), Handa (2000), and Bhargava (2007).[12]

[12] The unadjusted data before including regressors is modestly overdispersed with variance- mean ratio of about 1.6. I have tested the model for overdispersion after inclusion of explanatory variables. This overdispersion is eliminated upon inclusion of regressors. I found some evidence of moderate underdispersion and hence a negative binomial model is not appropriate in this case. I have also estimated a censored Poisson regression model allowing for right censoring

Panel (b) shows the impact of female education on age at childbearing. As can be seen from the table, each year of female education postpones the maternal age by 0.276 years (3.3 months), other factors held constant. The last panel shows that more educated women prefer to have fewer children compared to less educated women. In particular, each year of female education reduces the number of children preferred by 0.02 children. As can be seen, the effect of education on the actual number of children is four times bigger than the effect of education on women's fertility preferences. These effects are all statistically at 99 percent confidence level.

Table 3: AME of Female Education on Fertility Outcomes: Baseline Regressions

Variables	**Spec 1**	**Spec 2**	**Spec 3**
(a) Number of Children			
Estimate	-0.072***	-0.072***	-0.078***
Standard error	(0.001)	(0.001)	(0.001)
Control for religion	Yes	No	No
Observations	65,959	65,959	88,426
(b) Age at First Birth			
Estimate	0.269***	0.270***	0.276***
Standard error	(0.003)	(0.003)	(0.003)
Control for religion	Yes	No	No
Observations	61,499	61,499	82,571
(c) Ideal Number of Children			
Estimate	-0.018***	-0.018***	-0.019***
Standard error	(0.001)	(0.001)	(0.001)
Control for religion	Yes	No	No
Observations	60,150	60,150	77,669

This table is estimated using the EDHS data (six waves). Women age 22-49. In all the specifications, I control for age at the time of the survey (except for age at first birth outcome), age at the time of survey squared, a set of dummies for years of birth, marriage duration, a dummy variable for contraception usage, a dummy for the region, and a set of dummies for household wealth quintiles. Heteroskedasticty-robust standard errors are in parentheses. * refers to 90 percent confidence level, ** refers to 95 percent confidence level, and *** refers to 99 percent confidence level.

in the number of children due to the age of the mother. The results are qualitatively similar to simply including quadratic in age in the regular Poisson regression model.

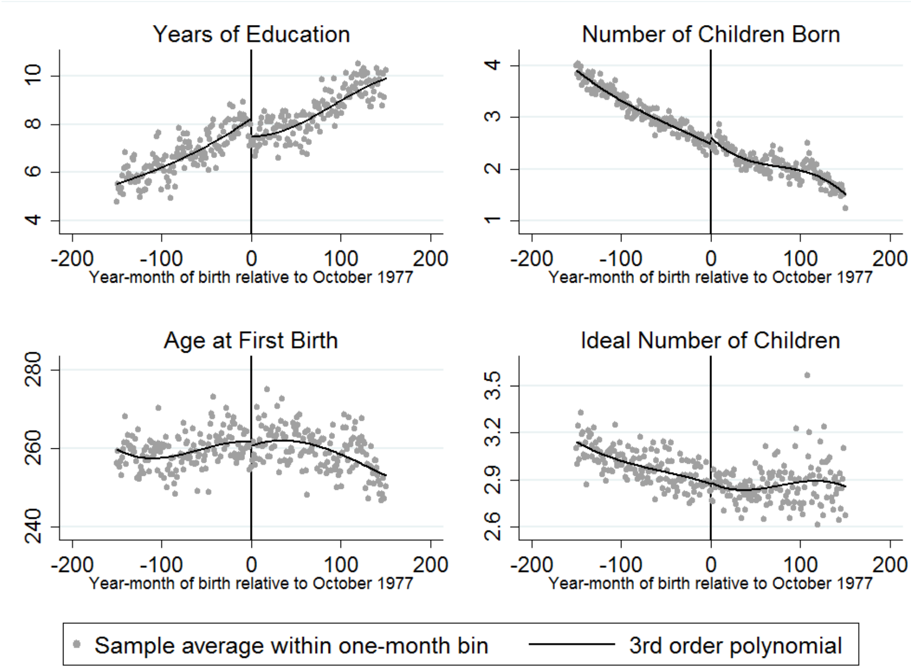
5.2. Regression Discontinuity Results

5.2.1 Graphical Representation

I start the RD analysis by a graphical representation for years of education and fertility outcomes over the support of the normalized forcing variable $(X_i - c)$ (date of birth relative to October 1977). These graphs are shown in Figure 3 below. The dots in these figures represent unconditional means outcome within a one-month bandwidth; whereas, the solid lines represent fitted regression lines from 3rd order global polynomial regressions.

Figure 3 shows that there is a discontinuity in years of education completed in adulthood at the cutoff date. Particularly, there is a decrease in years of education completed by women born after October 1977 who attended five years in primary school compared to women born before October 1977 who attended the six-year primary system. Figure 3 also shows some discontinuous increase in the number of children born per woman. Therefore, women who attended less time in primary school and have lower educational attainment in adulthood appear to have more children compared to women who attended more time in primary school. The figure does not, however, show, any discontinuity in the preferred number of children. Finally, Figure 3 shows that there is some discontinuous decrease at the cutoff date in the average age at childbearing among women born after October 1977 who attended less time in primary schooling.

Figure 3: Discontinuities in Years of Education and Fertility Outcomes



5.2.2. Main Results from the RD Analysis

This section discusses the estimates from the nonparametric regression discontinuity regressions. I estimate local linear regression as well as local exponential mean regression models using the triangular kernel weighting function within a bandwidth of 60 months. Table 4 below reports the average marginal effects of female education from the local linear regression model (column 1) and the local exponential mean regression model (local Poisson) (column 2).

Panel (a) of Table 4 shows that the estimate of discontinuity in education is negative and significant. In particular, women who attended five years in primary school have completed, on average, 0.89 less years of schooling than women who attended six years in primary school.

Panel (b) shows the causal impact of female education on fertility using this exogenous variation in education. The local linear model in panel (b.1) shows that the effect of female education on the number of children born per woman is - 0.06. That is, other factors held constant, each year of female education reduces the number of children born per woman by 0.06. The P-value of this coefficient is 0.12, which makes it statistically significant at 0.88 percent confidence level. As discussed before, the number of children outcome is a count variable, and hence, a local Poisson regression is more appropriate than a local linear regression. I focus the discussion on the results of the local Poisson models as my preferred set of estimates. As can be seen, using a local Poisson model increases the magnitude of the education coefficient from 0.06 to 0.10 and enhances its statistical significance to 99 percent confidence level.

Panel (b.2) shows the impact of female education on age at childbearing. An extra year of female education increases the age at childbearing by 0.15 years (1.8 months), other factors held constant. This effect is not however statistically significant at the conventional confidence levels (P-value equals 0.18). The result in panel (b.3) shows that the increase in female education does not change women' preference for their ideal number of children. This finding is true using both the local linear and the local Poisson regressions.

To make the RD results comparable to the results from the baseline regression in Table 3, I run the RD analysis after excluding the 1992 survey. Consistent with the discussion in footnote 11, excluding the 1992 survey does not affect the RD results. The reason is that my RD analysis uses a 60-month bandwidth, which restricts the regression sample (local sample) to women born within five years interval on both sides from October 1977. That is, the RD local sample includes women born between October 1972 and October 1982. Women surveyed in 1992 were born between 1943 and 1970 and hence do not appear in the RD local sample. Therefore, the findings from this section can be compared to the baseline results in section 5.1. As can be seen, the effect of female education on the number of children born using the RD analysis (kernel-based local

Poisson model) is higher than the estimated effect from the baseline Poisson in Table 3 (0.10 versus 0.08). The estimated effect using the RD analysis, however, is smaller than the effects in recent studies of developing countries that attempted to create exogenous variations in female education and examined its impact on fertility. In particular, Osili and Long (2008) found that increasing female education by one year reduces fertility by 0.26 births.

Table 4: The Effects of Female Education on Fertility: RD Results

Outcomes	Local Linear	Local Poisson (for count dependent variable)
(a) Discontinuity in education		
Estimate	-0.892	-
Standard error	(0.148)	-
P-value	[0.000]	-
Mean	{6.245}	-
Local Sample	26,673	-
(b) Effect of female education		
1) Number of children born		
Estimate	-0.060	-0.104
Standard error	(0.038)	(0.041)
P-value	[0.116]	[0.010]
Mean	{3.500}	{3.500}
Local sample	26,673	26,673
2) Age at birth (in years)		
Estimate	0.149	-
Standard error.	(0.11)	-
P-value	[0.175]	-
Mean	{21.108}	-
Local sample	24,480	-
3) Ideal number of children		
Estimate	0.025	0.028
Standard error	(0.038)	(0.049)
P-value	[0.507]	[0.561]
Mean	{3.027}	{3.027}
Local sample	24,744	24,744

This table is estimated using the EDHS data (seven waves), women age 22-49. I estimate local regression models with a triangular kernel and a bandwidth of 60 months. Standard errors are clustered by primary sampling unit. In all the regressions, I control for age at the time of the survey for fertility outcomes except age at first birth.

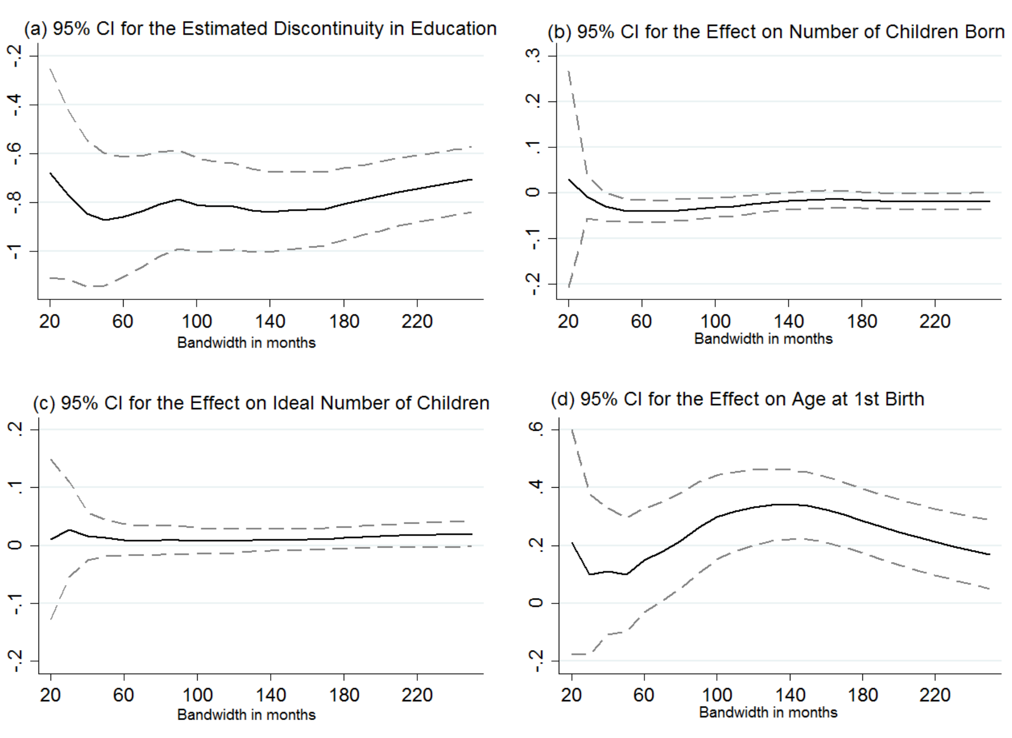
6. Robustness checks

This section reports some robustness checks to the main results in Table 4. Specifically, I examine whether the findings are sensitive to the choice of bandwidth, test for discontinuities in baseline characteristics, investigate whether husband education biases the RD results, explore the effects among actual treated and control groups, and finally explore potential threats of sample selection bias.

6.1. Sensitivity to Bandwidth

As indicated earlier, I use a bandwidth of 60 months, which is smaller than the optimum bandwidth suggested by the plug-in and the cross-validation methods. Although this restricts the analysis within a 10-year birth cohort (60 months on each side of the cutoff date), there are some concerns about the extent to which the results are sensitive to the choice of the bandwidth. Figure 4 displays the 95 percent confidence intervals for the estimates of Table 4 for a broad range of bandwidths. Panel (a) shows that the estimate of discontinuity in education is quite stable over the bandwidth. Except for bandwidths less than 20 months, the magnitude of this estimate ranges from 0.7 to 0.9. Increasing the bandwidth appears to enhance the efficiency of the estimate (through increasing the local sample size), but it does not seem to have a big effect on the magnitude. Panels (b) and (c) show the 95 percent confidence intervals for the estimated coefficient of female education on the number of children born and the ideal number of children, respectively, using the local Poisson models. Likewise, these graphs show that increasing the bandwidth has a minor impact on the magnitude of the coefficients. The last panel shows the confidence interval for the estimated impact of female education on age at childbearing. As can be seen from the graph, the estimated impact ranges from 0.1 to 0.3, and it is statistically significant at bandwidths bigger than 60 months.

Figure 4: 95% Confidence Interval for the Estimated Effects of Education on Fertility



6.2. Discontinuities in Baseline Characteristics

One of the central assumptions of the RD approach is that women born around the cutoff date share similar baseline characteristics. A violation of this assumption would imply that women near the cutoff did not randomly attend different primary schooling systems, and the results of the RD analysis would be biased. As was argued in the introduction, the first school cohort who was subject to the new five-year primary system was born 11 years before the government announced the change in the system. Thus, it is not possible that parents would have anticipated this change 11 years before it happened and adjusted their behaviors as a result.

I test for evidence of non-randomness around the cutoff date by estimating the discontinuities in baseline characteristics such as a woman's religion (Muslim or Christian), type of region (urban or rural), her mother's years of education, and her father's years of education. The data is not equally available for each of these variables. The region variable is available for all the women in the sample (97,314 women). It is based on the region of residence rather than the region where a woman obtained her primary schooling. However, the mobility in Egypt is quite small[13], and women are more likely to move within the same type of region. The religion variable is missing in two survey years: 2000 and 2003, which reduces the sample size to 74,847 women. Both mother's education and father's education contain many missing values, which shrinks the sample size to 5,813women and 3,493 women, respectively.

Table 26 in Appendix A shows the estimation results from a set of separate local linear regressions in the form of Equation (1) where each of the baseline covariate represents the dependent variable. If the assumption of the RD holds, these baseline covariates should evolve continuously around the cutoff date and hence the estimates of the discontinuity should not be statistically significant. The results show that there are no significant discontinuities in these baseline covariates.

[13] I used data from the Egypt Population, Housing, and Establishment Census 1996 and 2006 to compute the proportions of internal migrations from rural to urban areas and vice versa. The tabulations are shown in Table 25 in Appendix A. I restrict my computation to individuals of the same age group of 22-49 consistent with the analysis in this chapter. The data show that the percentage of rural-urban migration is very small, and ranges from 1.6 percent in 1996 to 2.8 percent in 2006. Similarly, the percentage of urban-rural migration ranges from 2.5 percent in 1996 to 1.2 percent in 2006. These percentages are quite small compared to developed countries such as the United States. In fact, in the United States, among those who live in a different state than their birth state, roughly 35 percent of the 18-34 years-old s have moved across states lines in the last five years (averaging across the 1980, 1990, and 2000 censuses) (Molloy, Smith, & Wozniak, 2011).

6.3. Controlling for Husband Education

It is not a priori clear whether one should control for husband's education in the regression of female education on fertility. Without the inclusion of husband's education, the effect of women's education represents both the direct impact of women's education and the indirect impact of husband's education, which is due to assortative mating effects (Behrman & Rosenzweig, 2002; Holmlund, Lindahl, & Plug, 2011). Thus, the inclusion of the husband's education in the regression will exclude assortative mating effects from the effect of female education on fertility.

The problem with my natural experiment setting, however, is that the change in the length of primary schooling had simultaneous effects on both women and men. Isolating an exogenous variation in female education may not, therefore, be possible. The cultural context in Egypt, however, provides a natural setting for controlling for husband's education and isolating the exogenous variation in women's education. In particular, most men in Egypt marry women younger than they are. This is confirmed by Table 5, which shows the joint distribution of lengths of primary schooling systems for both women and their husbands within a 60-month bandwidth. As can be seen, the majority of women in the sample are married to men who attended the old six-year primary system (older men), regardless of women's types of primary schooling. Therefore, in my analysis, I compare women on both sides of the cutoff (six-year primary vs. five-year primary) where the type of primary schooling attended by husbands is mostly the same on both sides.

In Table 27 in Appendix A, I reproduce the outputs of Table 4 in the text where I add husband's education to the set of the control variables. The purpose of this exercise is to explore the relative importance of husband's education in the relationship between woman's education and fertility. The results of this analysis show that controlling for husband's education does not change the main findings from Table 4. Specifically, female education reduces the number of children born per woman and increases the age at childbearing. The latter impact remains statistically insignificant within a 60-month bandwidth.

Table 5: The Joint Distribution of Primary Schooling Length for Women and their Husbands

	Husbands attended six years	Husbands attended five years	Total
Women attended six years	13,961	521	14,482
(%)	(96.4)	(3.6)	(100)
Women attended five years	10,074	2,587	12,661
(%)	(79.57)	(20.43)	(100)

This table is computed by the authors using seven waves (1992-2014) of the DHS survey. Row percentages are shown in parentheses.

6.4. Restricting the Sample to Actual Treated and Control Groups

So far, the RD analysis of this chapter has included all women regardless of their education levels. Women born before October 1977 are considered the control group (attended the six-year primary system); whereas, women born on or after October 1977 are considered the treated group (attended the new five-year primary system). However, a considerable portion of women in the sample has no formal education in addition to women who have dropped out before completing the primary school degree (37 percent and 10 percent, respectively). These women had neither faced the six-year primary system nor did they face the new five-primary system. They are however assigned to one of these systems based on their dates of birth. The reason for including them in the analysis is to account for the possibility that the decisions of women (or their parents) not to go to school or drop out of school may have been affected by the length of primary schooling. For instance, parents who dropped their children out of the school or decided not to enroll them in school under the six-year primary system may have decided differently if they knew the primary school would be five rather than six years. It is not clear however the significance of this portion in the sample.

The downside from adding these women in the analysis is that it underestimates the exogenous variation in women's education. To illustrate, women with no formal schooling are, on average, older and thus are more likely to be considered among the six-year primary cohort. Therefore, a considerable portion of women with no educational attainment is counted among women who attended one extra year of primary. This apparently underestimates the impact of that extra year of primary schooling on educational attainment. Smaller exogenous variations in female education may not provide enough variations to identify the effect of female education on fertility. It may also result in large standard errors rendering the estimated effects statistically insignificant.

To investigate this issue further, I run the RD analysis in this section on women who had completed at least a primary school degree. This restricts the total sample to 49,283 women. These are the women who had actually faced one of the primary schooling systems. I re-estimate the average marginal effects of Table 4 in Table 6 below using the restricted sample. I draw the estimated discontinuities in education and fertility outcomes in Figure 16 in Appendix A. I also draw the estimated coefficients of local regressions across a wide range of bandwidths in Figure 17 in Appendix A. As can be seen from the Table 6 below, restricting the sample to women with at least primary degree increases the exogenous variation in female education from 0.89 to 1.47. Figure 17 in Appendix A shows that the exogenous variation is much higher for cohorts born close to the cutoff (small bandwidths), and it decreases for cohorts born far from the cutoff (bigger bandwidths). The exogenous variation in female education, however, remains above one even at large bandwidths.

Using this exogenous variation, the estimated effect of female education on the number of children born is negative and statistically significant in both the local linear and the local Poisson regressions. The magnitude of the coefficient using the local Poisson regressions is 0.08, which is quite smaller than the estimated impact in Table 4 above using the full sample (0.10). Figure 17 shows that for bandwidths less than 120 months, the estimated coefficient of the local Poisson model remains statistically significant and quite stable. Consistent with the findings from the full sample, female education has no effect on women's preferences for the ideal number of children. Finally, the impact of female education on age at childbearing in the restricted sample is bigger and statistically significant compared to the estimated effect using the full sample (0.24 versus 0.15 versus). Figure 17 shows that the estimated effect ranges from 0.16 and 0.30 and remains statistically significant across a very broad range of bandwidths. Thus, increasing the extracted exogenous variation in female education has not substantially altered the main findings of the RD analysis in Table 4.

Table 6: The Effects of Female Education on Fertility: RD Results for the Restricted Sample

Outcomes	**Local Linear**	**Local Poisson (for count dependent variables)**
(a) Discontinuity in education		
Estimate	-1.472	-
Standard error	(0.097)	-
P-value	[0.000]	-
Mean	{11.497}	-
Local Sample	17,269	-
(b) Effect of female education		
1) Number of children born		
Estimate	-0.046	-.075
Standard error	(0.025)	(0.029)
P-value	[0.069]	[0.009]
Mean	{2.568}	{2.568}
Local sample	17,269	17,269
2) Age at birth (in years)		
Estimate	0.235	-
Standard error.	(0.078)	-
P-value	[0.002]	-
Mean	{22.504}	-
Local sample	15,716	-
3) Ideal number of children		
Estimate	-0.009	-0.003
Standard error	(0.024)	(0.009)
P-value	[0.716]	[0.692]
Mean	{2.803}	{2.803}
Local sample	16,379	16,379

This table is estimated using the EDHS data (six waves), women age 22-49. I restrict the sample to women who completed at least a primary degree. I estimate local linear regression models with a triangular kernel and a bandwidth of 60 months. Standard errors are clustered by primary sampling unit.

6.5. Sample Selection Issue

I finally address potential bias that may arise by focusing the study on ever-married women. Unfortunately, the data for never-married women do not allow me to observe detailed information on fertility as well as respondent's birth month and year, which is necessary to implement the RD strategy. Even if I assume that never-married women have no children, I cannot include them in the analysis due to the lack of data on date of birth. Hence, the analysis is focused on the sample of ever-married women since the Egyptian DHS collect detailed information on fertility as well as the respondent's birth month and year for only these women. Restricting the analysis to ever-married women is expected to bias the results if education affects marriage and marriage affects fertility. The direction of the bias, however, is expected to be downward, as explained below.

The bias equals the product of two terms, which are the effect of being never-married on fertility and the correlation between education and the probability of being never-married. The first term is expected to be negative because out-of-wedlock births are rare in Egypt due to cultural and religious reasons. Like most Arab and Muslim countries, sexual relations outside of marriage in Egypt are socially prohibited and penalized by the law. There are no official statistics on the out of wedlock births. Questions on fertility and children outcomes in the Egyptian DHS survey and other surveys such as Population Census, Egypt Labor Market Panel Survey (LMPS), and the Egyptian Household Income, Expenditure and Consumption Survey (HIES) target married women only. It is considered very offensive in Egypt to ask an unmarried woman whether she has any children (Fisher, 2015).

The second term in the bias equation is expected to be positive as women with higher levels of education are at a higher risk of being never married compared to less educated women (Mensch, Singh, & Casterline, 2005). In fact, I computed the average years of education for never-married women age 22-49 using both the Egypt DHS household member survey (six rounds) and the Egypt Population, Housing, and Establishment Census 2006. The results are shown, respectively, in Table 28 and Table 29 in Appendix A. Both datasets show that a young never-married woman has, on average, more education than an average young ever-married woman. Figure 18 in Appendix A shows that the majority of never-married women in the DHS data are young with an average age of 26. Therefore, the bias term is expected to be negative, and hence, the exclusion of never-married women from the analysis is expected to bias my results downward. Consequently, my findings using data on ever-married women provide lower bound estimates of the effects of education on fertility.

7. Explaining the Effect of Female Education on Fertility

The findings of this chapter show that the increase in female education reduces the actual number of children born per woman with no significant impacts on women's fertility preferences. This finding gives rise to the possibility that female education reduces the number of children born per woman through postponing maternal age, which is confirmed by the results of the previous sections. In fact, each year of female education delays maternal age by 1.8 to 2.8 months. This section further explores the reasons for postponing childbearing.

The delay of maternal age does not appear to result from enhancing women's job opportunities or increasing their usages of contraceptive methods, as suggested in the literature (Becker, 1960, 1993; Becker & Lewis, 1973). In fact, female labor force participation in Egypt has been historically low, as can be seen from Figure 5 below, despite the remarkable increase in female educational attainment overtime (Figure 1)[14]. On the other hand, the usage of contraceptive methods in Egypt has been historically high since the government has expanded family planning programs and publicity campaigns to curtail population growth in the early 1990s. Figure 5 below shows that the percentage of women using or intending to use contraceptive methods in the DHS data remained above 80 percent during the period (1992-2014). To support this argument, Table 7 below show the RD results of the impact of female education on the probability of work and the probability of using contraceptive methods[15]. As can be seen from the first two columns, the increase in female education has no significant impacts on the probability of work and the probability of using contraceptive methods, respectively. This finding raises the question as to what might have caused the delay in maternal age if both the probability of work and the probability of using contraceptive methods have not affected.

The delay of maternal age appears to result from an additional channel, which has not been quite emphasized in the previous literature. In particular, the increase in female education has led to a delay of marriage which resulted in a delay in maternal age[16]. As can be seen from the third column of Table 7, each year of female education delays marriage by 0.279 years (3.35

[14] Explaining the low levels of female labor force participation in Egypt is out of the scope of this chapter. Assaad and Krafft (2013) and Hendy (2015) provide a discussion of this issue. In particular, the authors highlight factors related to the supply of female labor such as family circumstances, women's preferences, and reservation wages, as well as factors related to labor demand such as shrinking public sector and discrimination in the private sector.

[15] I conduct the fuzzy RD analysis for these two outcomes using local Probit models and using the change in the length of primary schooling to instrument female education.

[16]Other studies have also documented that more-educated women generally marry later than their less-educated counterparts in Arab countries (Rashad, Osman, & Roudi-Fahimi, 2005). While this could be partially explained by the fact that more educated women stay longer in the school, a considerable portion of educated women remain unmarried after leaving school, and some of them do not marry at all. Some studies indicated that returns in the marriage market, rather than labor market, provide a strong incentive for girls' schooling in Egypt (Lloyd et al., 2003; Mensch, Ibrahim, Lee, & El-Gibaly, 2003), in that more educated women are expected to marry educated and wealthy men. This increase in women's expectations about their future husbands combined with higher poverty levels in society and increasing the cost of marriage, which is born mostly by grooms,   have all contributed to increasing age at marriage among more educated women.

months). This result is statistically significant at 99 confidence level. Given the nature of the Egyptian society, where out-of-wedlock births are socially prohibited and penalized by the law, only married women are allowed to have children. Thus, postponing marriage results in postponing women's age at first birth (maternal age). In fact, the correlation coefficient between age at first marriage and age at first birth in the DHS data is 0.9.

Figure 5: Female Labor Force Participation and Contraceptive Usage Overtime

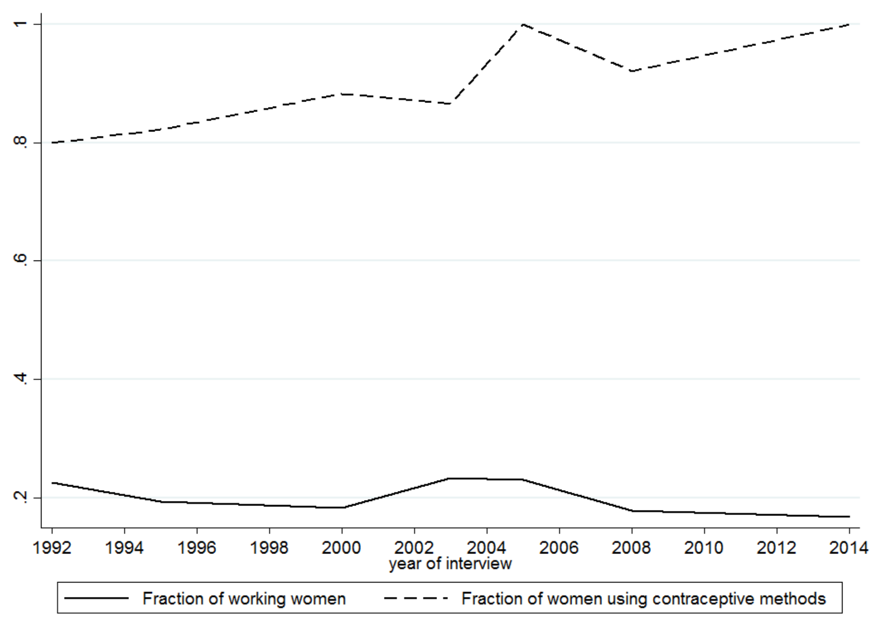


Table 7: The Effects of Female Education on Intermediate Outcomes: RD Results

	Pr(work=1)	Pr(use contraceptive=1)	Age at first marriage
Woman Education	0.009	0.003	0.279***
Standard error	(0.011)	(0.006)	(0.105)
P-value	(0.432)	(0.645)	(0.008)
Observations	26,640	26,640	26,640

This table is estimated using the EDHS data (six waves), women age 22-49. I estimate local Probit models for the first two outcomes and a local linear model for the third outcome. All the regressions use a triangular kernel weighting function within a bandwidth of 60 months. Standard errors are clustered by primary sampling unit.

8. Conclusion

Does educating young girls reduce fertility in developing countries? Several empirical studies have examined this question but have mostly faced difficulties in addressing the endogeneity of female education. My paper provides causal evidence on the impact of female education on fertility from a Middle Eastern country. In particular, I use data from the Egyptian Demographic and Health Survey (1992-2014) to examine the effect of female education on three fertility outcomes: the actual number of children born per woman, the preferred number of children per woman, and the age of women at first birth. I use the change in the length of primary schooling in Egypt in 1988, which reduced the years of primary education from 6 to 5 years, to create an exogenous variation in female education. The first cohort who was subject to this policy change was individuals who were born on or after October 9177. I implement a nonparametric regression discontinuity design to compare adulthood education and fertility outcomes of women born just before and right after October 1977.

The results show that women who attended the five-year primary system have completed, on average, one less year of schooling in adulthood as compared to women who attended the six-year primary system. Using this exogenous variation in education, the RD results show that each year of female education reduces the number of children born per woman by 0.104 children. That is, a woman with nine years of compulsory education has about one less child than a woman with no formal education. This estimated effect is statistically significant at the 99 percent confidence level and is much larger than the estimate (0.08) from the baseline Poisson regression, which ignores the endogeneity of female education.

I explore whether the estimated effect of education on fertility reflects a change in women's preferences towards the optimum number of children. The results of this analysis show that the increase in female education did not significantly change women's fertility preferences. I find, however, that the increase in female education has increased women's ages at their first birth. In particular, the results show that each year of female education postponed maternal age by 1.8 to 2.8 months. Thus, my estimates indicate that the reduction in the number of children born per women as education increases results from postponing maternal age rather than changing women's attitudes and preferences towards the optimum number of children.

I also provide evidence that the delay of maternal age results from delaying marriage rather than increasing women's labor force participation or increasing their usages of contraceptive methods. The results of this chapter are quite robust to several robustness checks and sample restrictions, including varying the bandwidth, including husband education in the regression equation, and restricting the analysis to only women who were directly influenced by the policy change.

Chapter II: Parents' Education and Child Health: A Regression Discontinuity Approach

1. Introduction

In the last few decades a substantial improvement in child health and nutritional status has been experienced. Since 1990, the number of under-five deaths worldwide has declined from 12.7 million in 1990 to 5.9 million in 2015 (UNICEF, 2015). While that reduction translates into around 18,000 fewer children dying every day in 2015 than in 1990, it still implies the deaths of more than 16,000 children under age five every day in 2015. Furthermore, the recent reports by the UNICEF, WHO, and the World Bank Group estimate that, in 2014, there were 667 million children under 5 in the world. Of these children, 159 million were stunted, 41 million were overweight, and 50 million were wasted (UNICEF & WHO & World Bank Group, 2015). The magnitudes of these estimates are much higher in developing countries than in developed countries. For instance, children in sub-Saharan Africa are more than 14 times more likely to die before the age of 5 than children in developed regions (UNICEF, 2015).

These persistent challenges to child health represent a growing concern for both policymakers and academics. In particular, many developing countries, with the technical and financial assistance of international organizations, have adopted a wide range of policies to improve child health and reduce mortality such as providing easy and affordable access to improved water and sanitation. Most of these policies have focused on improving family care practices through promoting parental education. The focus on family has been motivated by the recognition that decisions made by parents have substantial impacts on child health. Parents' decisions determine, among other things, the amount and quality of health care their children receive, the type of food they eat, and the amount of their physical activities.

Therefore, improving the quality of parental decisions through ensuring that parents have adequate education has been promised to improve children's health outcomes. One of the possible mechanisms is that education may increase household income and thus allow parents to invest more resources to improve their children's health. Additionally, education may provide parents with general cognitive skills that enhance their capabilities of processing information and obtaining the health knowledge. Education is also expected to change parental attitudes toward traditional methods of treating their children's health problems (Glewwe, 1999).

The focus on parental education as an important tool to improve child health not only reflects the conventional wisdom among policymakers; it has also been supported by an extensive body of literature in the past three decades. The majority of this research has found that increasing parental education, especially maternal education, improves child health (Currie, 2009; Grossman, 2006; Strauss & Thomas, 1995). For example, a recent study published in the British medical journal, the Lancet, and quoted by the Washington Post magazine has shown that half the reduction in child mortality over the past 40 years can be attributed to increasing female education (Gakidou, Cowling, Lozano, & Murray, 2010). More specifically, Gakidou et al. have found that every one-year increase in the average education of women is associated with a 9.5 percent decrease in the child deaths.

The extent, however, to which these estimates reflect causality has remained a challenge, not least because of the potential endogeneity of parental education. To illustrate, a positive association between parental education and child health does not necessarily indicate that parental education causes improvements in child health. Other unobservable factors such as noncognitive skills, community backgrounds, and time preferences might be responsible for this observed correlation. For example, Fuchs (1982) argues that individuals who have a high degree of time preference for the future invest more in their education and make also larger investment in their own health and their children health. Thus, ignoring time preference could bias the effects of schooling on child health.

This chapter attempts to estimate the causal impact of parental education on children's health outcomes in Egypt using the same identification strategy of the first chapter. In particular, I use the change in the length of primary schooling from six to five years in 1988 as the source of exogenous variation in parental education. Beginning in 1988, the Egyptian government cut the number of primary school years from six to five years, moving from a 12-year pre-university system to an 11-year system. This policy change was mandatory throughout the country. The first school cohort who was subject to this change includes individuals who were born between October, 1977, and September 1978. Therefore, October 1, 1977 represents a cutoff date such that individuals born before that date had to attend one more year in primary school compared to individuals born on or after that date.

I use a nonparametric regression discontinuity analysis to compare parents who were born close to cutoff date but attended different primary schooling systems. In particular, I compare adult educational attainments of the five-year and six-year primary school cohorts, within a reasonable bandwidth of the cutoff date, and relate the difference in their educational attainment to the difference in their children's health outcomes.

The data for this study comes from the six recent waves of the Birth Recodes modules of the Egyptian Demographic and Health Survey (EDHS) from 1995 until 2014. This data provides rich information about child health and nutritional status along with other parental socioeconomic characteristics. For the purpose of this analysis, this data also provides information about year and month of birth for mothers and fathers so that the length of primary schooling attended by each of them can be determined. The results suggest that parental education has no significant effects on child health. These findings are consistent with Lindeboom, Llena-Nozal, & van der Klaauw (2009) and McCrary and Royer (2011).

I provide several explanations for the insignificant effects of parental education on child health. In particular, I argue that the low levels of parental education in Egypt (the average years of education is 4.6 among mothers and 6 among fathers in the DHS Survey) accompanied with the poor quality of schooling, especially at the primary level[17], result in little effect of education on parents' intermediate outcomes that are expected to improve child health such as parents' literacy skills, access to information, and health behavior. Specifically, I provide evidence that education has no significant impact on health practices, cognitive skills, or information processing capabilities of low-educated parents.

This chapter contributes to the literature in two main ways. First, this chapter is one of few studies that examine the causal impact of parental education on child health in the Middle Eastern and North African (MENA) region. The lack of evidence for the MENA region is surprising given the impressive improvements in education in the last few decades (World Bank, 2015; UNICEF, 2015). For example, between 1990 and 2000, literacy rates for the adult population have increased by 19 percent, from 59 to 78 percent; net enrollment ratio[18] in primary school rose from 62 to 92 percent between 2000 and 2010; and gross enrollment rates in secondary and higher education increased by threefold and fivefold, respectively, during the period 1973-2003. During the same period, the region witnessed significant improvements in

[17] the quality of primary education in Egypt ranked very low according to the recent Global Competitiveness Report (World Economic Forum, 20013).

[18] Net enrollment ratio is measured as the number of students enrolled in primary school who are of the official age group for primary school divided by the total population of the same age group.

child health (World Bank, 2008). Because of these two major developments (rise of education and improvements in child health), the MENA countries offer a natural setting to test the extent to which the relationship between parental education and child health is causal.

Second, most of the relevant literature on the effects of parental education on child health in developing countries focuses on policy interventions that target individuals with relatively high levels of education to create exogenous variations in parental education. Examples of these policy interventions include the 1968 expansion of compulsory education in Taiwan from 6 to 9 years (Chou et al., 2010) and the 1980 expansion of secondary education in Zimbabwe (Grépin and Bharadwaj, 2015). The extent to which interventions targeting individuals with low levels of education, such as the policy change in Egypt, produce similar effects is unclear. The evidence in this chapter shows that such interventions seem to have small effects on parental health knowledge and practices, and therefore, have insignificant effects on child health outcomes.

The chapter proceeds as follows. Section 2 provides a literature review for the impact of parents' education on child health. Section 3 describes the data used in this study. Section 4 discusses the identification strategy. Section 5 presents the main results. In section 6, I test the sensitivity of the main results to alternative specifications and sample restrictions. Section 7 explains the insignificant effects of parental education. Section 8 concludes.

2. Literature Review

An extensive body of research has investigated the relationship between parental education and child health (see Strauss and Thomas (1995); Grossman (2006); Currie (2009)). Desai and Alva (1998), for example, use data from the Demographic and Health Survey (DHS) from 22 developing countries to examine the relationship between maternal education and three measures of child health: infant mortality, height-for-age, and immunization status. They find that the effect of maternal education on child mortality and height-for-age declined substantially after controlling for region of residence and a small set of socioeconomic characteristics, such as father/stepfather education and access to piped water.

The main limitation of Desai and Alva (1998), and many of the earlier research on parental education and child health in general, is that these studies are based on a correlation between parental education and children's health outcomes. In particular, the earlier literature does not adequately control for the endogeneity of parental education. More educated parents may differ systematically from less-educated parents in ways that affect child health. For instance, the observed correlation between parental education and child health may reflect omitted factors related to family background or parental noncognitive skills. Therefore, ignoring the endogeneity of education may bias the effects of parental education on child health.

To address this selection problem, a small and recent number of studies have attempted to identify the causal impact of parental education on child health. Most of this research, however, has focused on developed countries and has reached mixed conclusions. Currie and Moretti (2003) use college openings in the U.S. in the 1960s and 1970s as an instrument for maternal education. Drawing on national birth records for years 1970 to 1999, they find that maternal education has a large positive impact on birth weight and gestational age. Their results suggest that an additional year of education reduces the probability of low birth weight by 10 percent. Currie and Moretti (2003) examine several mechanisms through which maternal education may affect child health. Their findings indicate that maternal education reduces the probability of smoking and increases use of prenatal care.

McCrary and Royer (2011) compare fertility and child health for women born just before and just after school entry dates, using a regression discontinuity design. Unlike Currie and Moretti (2003), McCrary and Royer (2011) find no effects of mother's education on either fertility or birth weight. M. Lindeboom, A. Llena-Nozal, and B. van der Klaauw (2009) use the 1947 school reform in the United Kingdom to examine the effect of parental education on a broad range of children's health outcomes. They exploit the fact that the 1947 reform raised the minimum school leaving age from 14 to 15 year as the source of exogenous variation in parental education. Consistent with McCrary & Royer (McCrary & Royer, 2011), their results suggest that parental education has a small effect on child health.

One possible explanation for the mixed evidence of this literature is that their results may represent different local average treatment effects. In particular, the populations of individuals affected by the policy interventions used to extract exogenous variations in parental education are not similar across these studies. Studies that have found a positive effect of parental education on child health have relied on exogenous variations in education that are caused by policy interventions at higher education levels (Currie & Moretti, 2003). These interventions mainly target individuals at the upper end of education distribution. On the other hand, studies that have found no impact have focused on changes in compulsory schooling laws or age-at-entry policies that mainly affect low-educated individuals (M. Lindeboom et al., 2009; McCrary & Royer, 2011). These interventions are apparently less likely to have an impact on individuals' health behavior or attitudes and, as a result, do not affect child health.

The evidence from developing countries is still limited but growing. For instance, Breierova and Duflo (2004) use a large-scale school construction program that was implemented in Indonesia in the 1970s as the source of exogenous variation in parental education. They exploit the difference in the exposure to the program, resulting from an individual year of birth and region of birth, as an instrument for parental education. Their findings suggest that parental education substantially reduces infant mortality, with no significant difference between the effects of paternal and maternal education. Chou, Liu, Grossman, and Joyce (2010) used the 1968 expansion of compulsory schooling in Taiwan from 6 to 9 years to create an exogenous variation in parental education. Using birth and death records for the years 1978-1999, Chou et al. found that parental education reduces the probability of both low birth weight and child mortality.

Grépin and Bharadwaj (2015) exploit the 1980 expansion of secondary education in Zimbabwe to extract an exogenous variation in maternal education. They examine health outcomes of children born to three groups of mothers: mother fully exposed to the policy reform, mothers partially exposed, and mothers in the control group. Their results suggest that children born to mothers most likely benefited from the reform were about 21 percent less likely to die than children born to slightly older mothers. They also find that maternal education improves women economic opportunity and increases the age at childbearing.

This chapter contributes to the growing literature of developing countries by examining the causal impact of parents' education on children's health outcomes in one of the Middle Eastern and North African countries, Egypt. Most of the existing literature on developing countries has focused on interventions targeting individuals with relatively high levels of education. The findings of this literature show that parental education improves children health outcomes. This chapter contributes to the growing literature by shedding light on interventions targeting individuals with relatively low levels of education and exploring whether they produce similar effects on children health outcomes.

3. Data

The data for this study come from the Birth Recode module of the Egyptian Demographic and Health Survey (EDHS). I use data from the six recent waves: 1995, 2000, 2003, 2005, 2008, and 2014. The total sample includes 51,776 living children who were under age five at the time of the survey and 24,535 children who died before their fifth birthdays. Thus, the total sample includes 76,311 children born to 47,463 mothers and fathers[19].

The key variables in the analysis of this chapter are parental education and child health outcomes. I measure maternal education and paternal education, separately, as completed years of schooling attained in adulthood. The DHS survey provides information on educational levels and the grades attended at each level, which are used to compute years of schooling as explained in chapter I. I examine the effects of parental education on two outcomes: child mortality and child nutritional status. Measures of child mortality are calculated from retrospective information that was collected by the EDHS survey about children who have died. Information is collected about sex, month and year of birth, and age at death. I use this information to create three different measures of mortality: neonatal, infant, and under-five mortality. Neonatal mortality is defined as the probability of dying within the first month of life. Infant mortality is defined as the probability of dying during the first year of life. Whereas, under-five mortality is defined as the probability of dying before the fifth birthday.

Child nutritional status is measured using child height-for-age and weight-for-height. The EDHS data provides information on height-for-age and weight-for-height for all living children under age five. Measurements of height and weight were administered and collected by EDHS

[19] Since the analysis in this essay focuses on the effects of mothers' and fathers' education, I exclude single-parent households. They represent about three percent of the total sample.

interviewers at the time of the survey. The collected measures were then standardized by the EDHS team using data from the World Health Organization. Using these standardized measures, I create three binary variables that indicate whether a child was stunted, thin, or overweight at the time of the survey.

Children whose height-for-age measures are below minus two standard deviations from the median of the reference population are considered short for their age, i.e., stunted. Children whose weight-for-height measures are below minus two standard deviations from the median of the reference population are too thin for their height, i.e., wasted. Adverse health consequences are also associated with overweight among young children. Therefore, I create a binary variable for children whose weight-for-height is more than two standard deviations above the median of the reference population.

Table 8 provides descriptive statistics for parents and children in the sample. The total sample is composed of 47,463 mothers and fathers. As can be seen, the average years of education for mothers in the sample is 4.6 years. Fathers are more educated than mothers. In particular, the average father in the sample has six years of education. Also, fathers in the sample are about eight years older than mothers (39.7 years versus 32). Almost all the fathers in the sample (98 percent) had jobs at the time of the survey; whereas, only 13 percent of the mothers were participating in the labor market. The majority of households in the sample (68 percent) live in rural areas, and slightly more than half (52 percent) of the families are in the lowest two wealth quintiles.

Table 8 also provides descriptive statistics for living children under age five and children who died before their fifth birthdays. The 47,463 parents in the sample have a total of 51,776 living children and 24,535 dead children. As can be seen from the table, the average living child is about two years old at the time of the survey. The sample is equally distributed between male and female children. Twenty-three percent of the living children in the sample are considered stunted, that is, they are too short for their ages. Additionally, there is six percent of children whose weights are considered too small for their heights (thin); whereas eleven percent of children have weights that are considered too large for their heights (overweight). The bottom part of Table 8 describes the characteristics of children who died before their fifth birthdays. Of these children, 83 percent had died before their first birthdays (infant mortality) while 43 percent had died before their first month (neonatal mortality).

Table 8: Descriptive Statistics of Key Variables

Characteristics	**Mean**	**Standard Deviation**
Characteristics of mothers		
Mother education	4.62	5.18
Mother age	32.03	8.10
Mother age at first birth	20.08	3.87
Mother work=1	0.13	0.33
Number of mothers	47,463	-
Characteristics of fathers		
Father education	6.00	5.06
Father age	39.70	10.26
Father work=1	0.98	0.13
Number of fathers	47,463	-
Characteristics of households		
Urban=1	0.32	0.47
First wealth quintile=1	0.29	0.46
Second wealth quintile=1	0.23	0.42
Third wealth quintile =1	0.20	0.40
Fourth wealth quintile=1	0.17	0.37
Fifth wealth quintile =1	0.11	0.31
Characteristics of living children under five		
child male=1	0.51	0.50
child age	1.99	1.40
child is stunted=1	0.23	0.42
child is thin=1	0.06	0.23
child is overweight=1	0.11	0.31
Number of living children under five	51,776	-
Characteristics of dead children under five		
Child male=1	0.51	-
Neonatal mortality	0.43	-
Infant mortality	0.83	-
Under-five mortality	0.100	-
Number of dead children under five	24,535	-

This table is calculated using the Birth Recodes questionnaire of the EDHS survey, six waves (1995-2014). Mortality measures in the last panel are calculated as a ratio of total children died under five (24,535 children).

Table 9 and Table 10 below show the means of children's health outcomes by mothers' and fathers' education, respectively. The conclusions of these tables are quite similar. Altogether, the percentage of stunted children is smaller among highly educated parents. Highly educated parents, however, appear to have higher percentages of overweight children compared to less educated parents. The percentages of wasted (thin) children do not seem to follow a particular pattern with any of the parents' education. Tables 9 and Table 10 also show the means of the mortality outcomes. The data show that more educated parents are less likely to have children die before their fifth birthdays (under-five mortality). The same is also true regarding infant mortality and neonatal mortality.

Table 9: Means of Children Health Outcomes by Years of Mothers' Education

Children Health Outcomes	No formal edu.	Edu. years 1-6	Edu. years 7-9	Edu. years 10-12	Edu. years >12	All
Malnutrition outcomes						
child is stunted=1	0.26	0.24	0.22	0.19	0.19	0.23
child is thin=1	0.05	0.05	0.06	0.06	0.05	0.06
child is overweight=1	0.09	0.10	0.11	0.12	0.12	0.11
Mortality outcomes						
Under-five mortality	0.45	0.40	0.17	0.10	0.08	0.32
Infant mortality	0.40	0.36	0.16	0.09	0.07	0.28
Neonatal mortality	0.25	0.22	0.10	0.06	0.05	0.17

This table is computed using the six waves of the EDHS (1995-2014).

Table 10: Means of Children Health Outcomes by Years of Mothers' Education

Children Health Outcomes	No formal edu.	Edu. years 1-6	Edu. years 7-9	Edu. years 10-12	Edu. years >12	Overall
Malnutrition outcomes						
child is stunted=1	0.27	0.24	0.21	0.21	0.19	0.23
child is thin=1	0.06	0.05	0.06	0.06	0.05	0.06
child is overweight=1	0.09	0.10	0.12	0.12	0.11	0.11
Mortality outcomes						
Under-five mortality	0.49	0.38	0.23	0.15	0.16	0.32
Infant mortality	0.44	0.33	0.21	0.13	0.15	0.28
Neonatal mortality	0.28	0.20	0.12	0.08	0.10	0.17

This table is computed using the six waves of the EDHS (1995-2014).

4. Methodology

A common approach to examine the effect of parental education on child health is to use a standard regression analysis. The regression equation is identified as follows:

$$Y_{ij} = \alpha + \beta\ parental\_education_j + u_{ij} \quad (1)$$

where Y_{ij} is a measure of child health for child i of parent j, such as child mortality or nutritional status; $parental\_education_j$ is a measure of mother's/father's years of schooling; and u_{ij} is an idiosyncratic error term. If parental education is uncorrelated with the error term, then β represents the true causal effect of parental education on the health outcomes of their children. However, parents with different levels of education tend to be systematically different in their abilities, motivations, and backgrounds. Therefore, the estimate of β is expected to be biased.

One way to minimize this bias is to control for observable characteristics of both parents and their children, as follows:

$$Y_{ij} = \alpha + \beta\ parental\ education_j + \delta\ Z_i + \gamma\ X_j +\ u_{ij} \quad (2)$$

where Z_i is a vector of child characteristics such as age and gender and X_j is a vector of parents' characteristics such as region and income. For this approach to produce unbiased estimates, it requires that all factors that contaminate the relationship between parental education and child health should be controlled for. In most cases, only a small set of parents' characteristics can be observed. Other important characteristics such as parents' genetic cognitive ability, which is expected to have substantial impacts on both parental education and child health, are inherently difficult to measure and, therefore, are missing from the regression equation. Failure to control for unobservable factors would result in a biased estimate of β.

An alternative approach is to use a natural experiment that creates an exogenous variation in parental education, independent of the characteristics of parents. I follow this approach in this chapter, using the identification strategy of the first chapter. In particular, I use the change in the length of primary schooling in Egypt, introduced in the late 1980s, to extract an exogenous variation in parental education. As explained in Chapter I, in 1988, the Egyptian government reduced the length of primary schooling from six to five years, moving from a system of 12 years to a total of 11 years toward completing a high school degree. The first school cohort who was affected by the Egyptian reform comprises individuals who were born between October 1977 and September 1978. Therefore, October 1977 represents a cutoff date. Individuals born before that date had to attend one more year in primary school compared to individuals born on or after that date.

I use a nonparametric regression discontinuity (RD) design to estimate the effect of parental education on child health. In particular, I compare parental education around the cutoff date and relate the difference in their children's health outcomes to the difference in their educational attainment. I estimate two separates RD equations for mothers and fathers. A standard linear RD model for a parent (mothers and fathers separately) is specified as follow:

$$Y_{ij} = \gamma_1 + \gamma_2 D_j + \gamma_3 (X_j - c) + \gamma_4 D_j * (X_j - c) + \gamma_5 Z + \varepsilon_{ij} \qquad (3)$$

where Y_{ij} is the outcome of interest (child mortality and child nutritional measures) for child i of parent j; D_j is an indicator variable equal one if parent j was born on or after October 1977; X_j is the forcing variable, which is parent j's birthdate; whereas, c is the cutoff date; $(X_j - c)$ represents the deviation in months of parent j's birthdate from the cutoff date; Z is a vector of control variables that includes child's age (or child year of birth for mortality outcomes), child gender, a binary variable for urban status, a set of binary variables for region of residence (upper rural, upper urban, lower rural, lower urban, urban governorates, and frontier governorates), and survey-fixed effects. I also estimate a polynomial (quadratic) model to account for any nonlinearity in the relationship between the forcing variable and child health. The quadratic regression is specified as follows.

$$Y_{ij} = \delta_1 + \delta_2 D_j + \delta_3 (X_j - c) + \delta_4 D_j * (X_j - c) + \delta_5 (X_j - c)^2 + \delta_6 D_j * (X_j - c)^2 + \delta_5 Z + \varepsilon_{ij.} \qquad (4)$$

I estimate Eq.3 and Eq.4 using kernel-based local regressions within a 60-month bandwidth. The weighting function I use is based on the triangle kernel $K(.) = \max \{0, 1 - |\frac{(x-c)}{h}|\}$ similar to McCrary & Royer (2011), where h is the bandwidth. I also test the sensitivity of results to the chosen bandwidth in the robustness checks section of this chapter

5. Results

5.1. Graphical Representation

As the first step in my analysis, I examine the discontinuities in parents' education and children health outcomes graphically. Figures 6 through 9 show the results of this analysis. The first two figures show the discontinuities in mother education and child health outcomes; while the last two figures show the discontinuities in fathers' education and child health outcomes. For mother's sample and father's sample, child nutritional status and child mortality are analyzed separately. In all the figures, the horizontal axes measure the standardized forcing variable, which is the deviations of parental dates of birth from October 1977.

These figures show noticeable discontinuities in both mother's and father's education. As can be seen, parents who attended less time in primary schooling ended up with lower educational attainments in adulthood. The discontinuity in father's education appears to be larger than the discontinuity in mother's education. The comparison of children health outcomes around the cutoff for both mothers' and fathers' samples show, however, small discontinuities for all children's health outcomes.

Figure 6: Discontinuity in Mother Education and Child Malnutrition

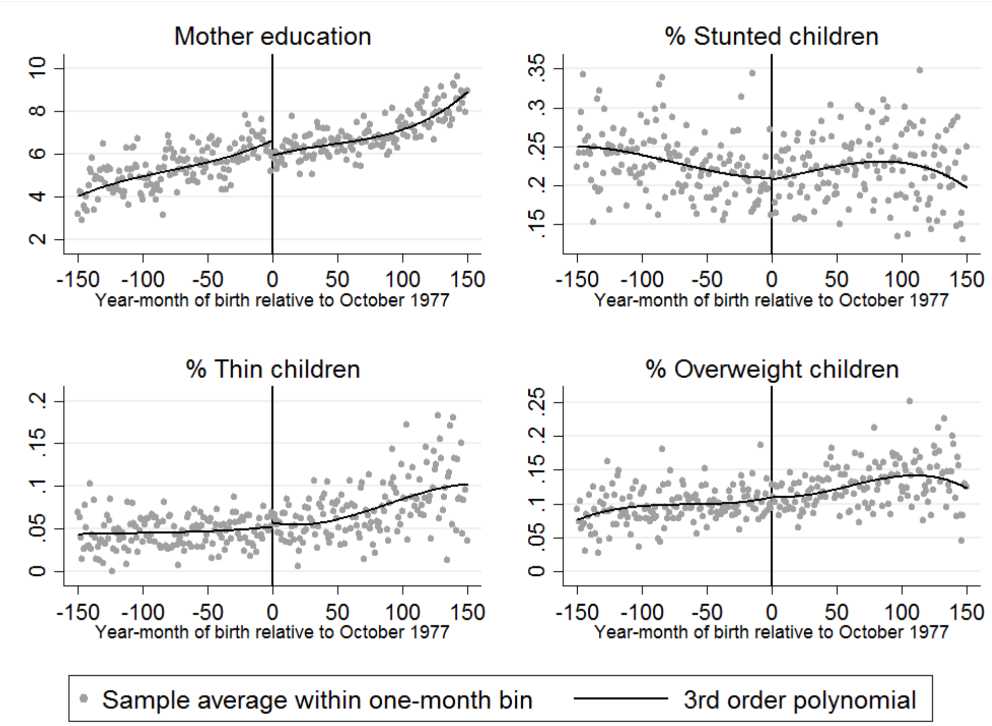


Figure 7: Discontinuity in Mother Education and Child Mortality

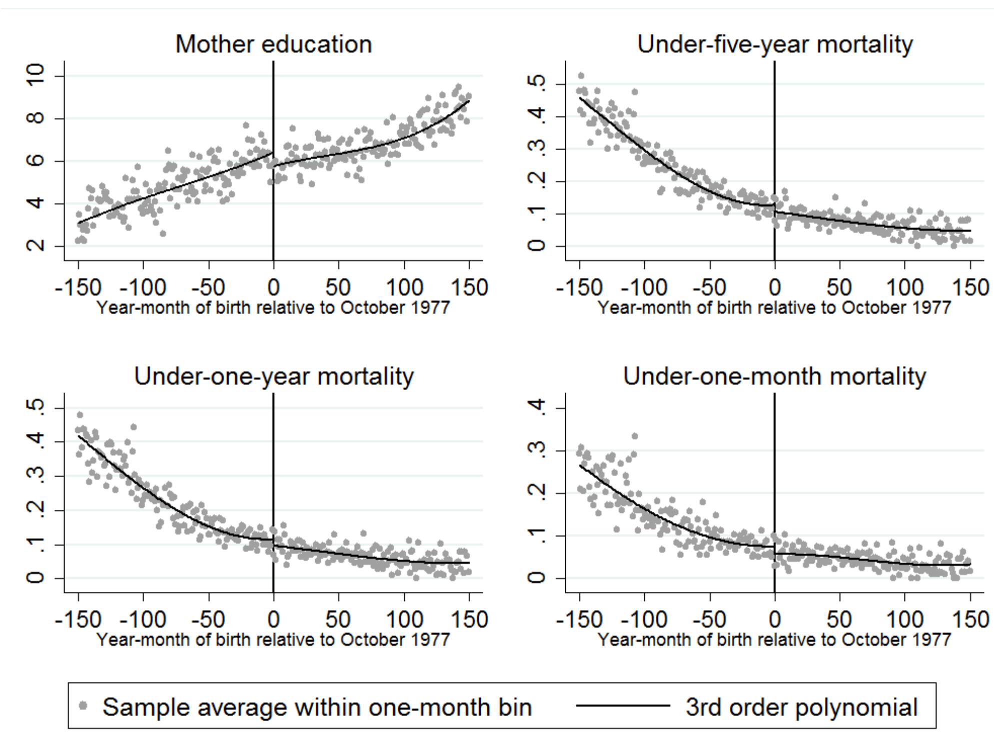


Figure 8: Discontinuity in Father Education and Child Malnutrition

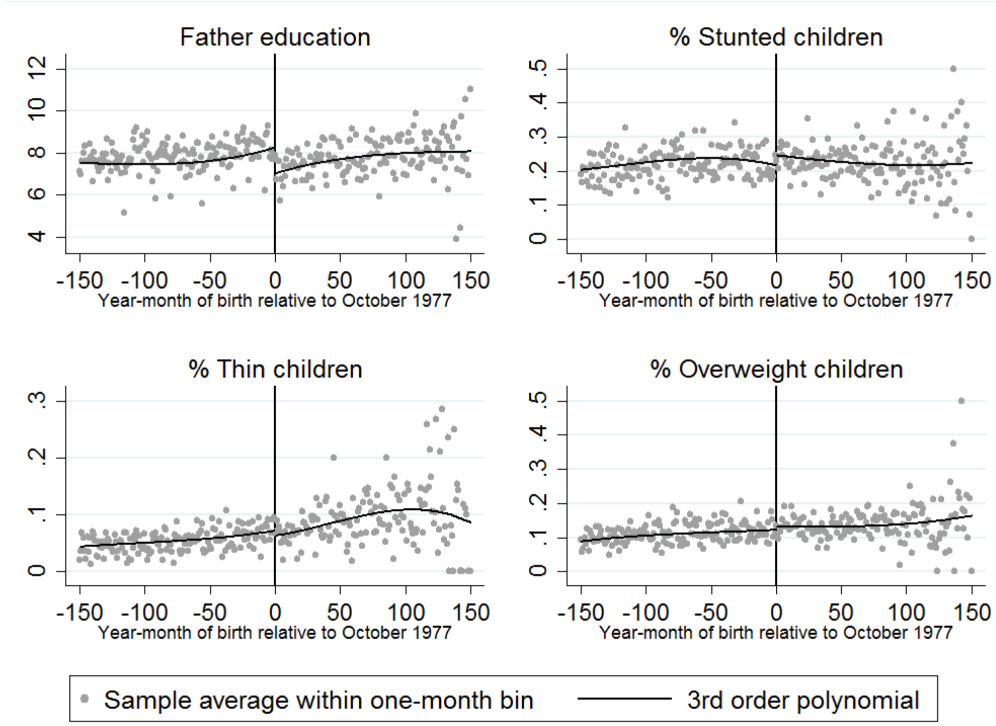
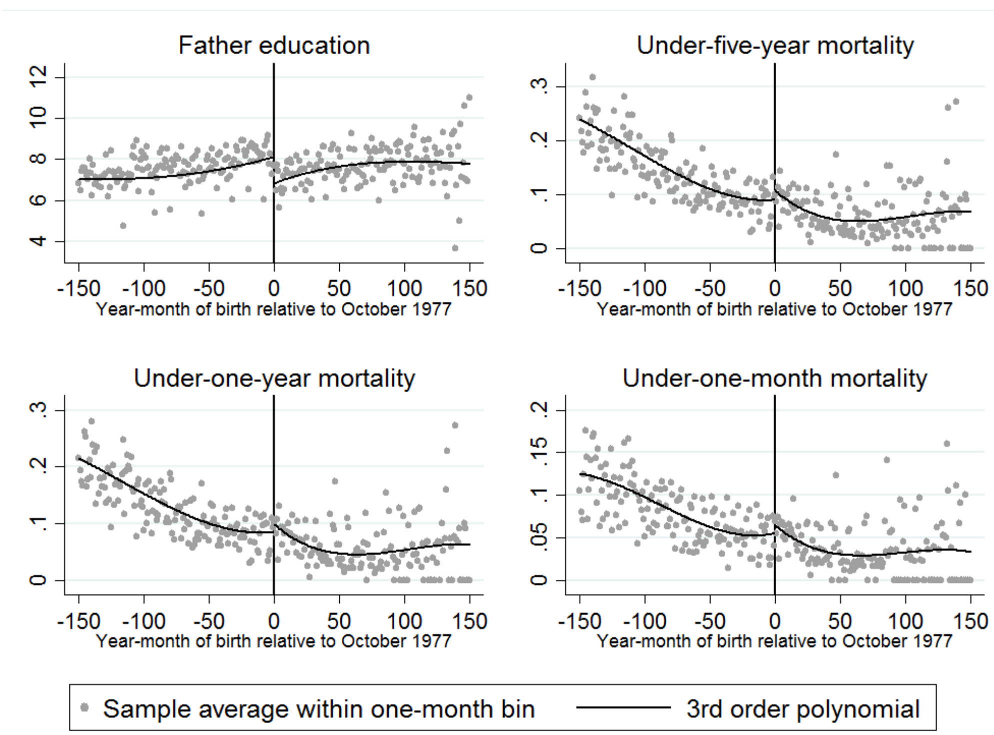


Figure 9: Discontinuity in Father Education and Child Mortality



5.2. Main Results

This section presents the main results. I estimate separate models for mothers and fathers. In the robustness checks section, I include both mother's and father's education in the same regression to explore the interaction between the two variables. Table 11 shows the effects on child nutritional status. The sample in these models includes living children aged 0-4 at the time of the survey (51,776 children). I use three measures of nutritional status: a binary variable that indicates whether a child was stunted at the time of the survey, a binary variable for whether a child was underweight (thin or wasted) and a binary variable for whether a child was overweight. I report results from both linear and polynomial kernel regressions.

As shown in the first row, the exogenous variation in mother education is -0.816, which is significant at the 99 percent confidence level. This indicates that mothers who attended the five-year primary school system completed, on average, 0.82 fewer years of education in adulthood than mothers who attended the six-year system. Using a local Polynomial regression slightly reduces the estimate of discontinuity in mother's education from 0.816 to 0.764. Both the local linear and polynomial models produce very similar results with slight changes in magnitudes. I focus the discussion on the findings from the local polynomial models as the preferred set of estimates to account for the possibility of nonlinearities in the relationship between the forcing variable and the outcomes. By looking at the effects of mother's education on child malnutrition in panels a, b, and c, mother's education seems to improve child health. For example, each year of maternal education reduces the probability of stunting and overweight by 1.6 and 0.9 percentage points, respectively. Surprisingly, the probability of underweight increases by 0.1 percentage points with each additional year of mother's education. None of these effects are, however, statistically significant at the conventional confidence levels.

Columns 3-4 of Table 11 also show the effects of father's education on the same set of children's health outcomes. As can be seen from the last column, the exogenous variation in father's education is statistically significant with a magnitude that is higher than mother's education. In particular, fathers who attended the five-year primary system ended up completing 0.91 fewer years of education in adulthood compared to fathers who attended the six-year system. Similar to mothers' results, father's education has a positive impact on child nutritional status. Each year of a father's education reduces the probability of stunting and underweight by 2.5 and 2.1 percentage points, respectively. These effects are not, however, distinguishable from zero.

Table 11: The Effects of Parent Education (Separately) on Child Malnutrition: RD Models

Variables	Mothers' Sample		Fathers' Sample	
	Local Linear	Local Polynomial	Local Linear	Local Polynomial
Exogenous variation in education	-0.816***	-0.764***	-0.979***	-0.912***
	(0.183)	(0.266)	(0.185)	(0.190)
Effects of education on:				
(a) Stunting	-0.003	-0.016	-0.022	-0.025
	(0.015)	(0.024)	(0.016)	(0.017)
(b) Underweight	0.001	0.001	0.012	0.014
	(0.009)	(0.014)	(0.009)	(0.011)
(c) Overweight	0.004	-0.009	-0.017	-0.021
	(0.012)	(0.019)	(0.012)	(0.014)
Local sample	21,947	21,947	16,823	16,823
Total sample	51,776	51,776	51,776	51,776

In all regressions, I use a 60-month bandwidth and a triangle kernel weighting function. I also control for child's age, child gender, a binary variable for urban, a set of binary variables for region of residence (upper rural, upper urban, lower rural, lower urban, urban governorates, and frontier governorates), and survey-fixed effects. Standard errors are shown in parentheses. *** refers to the 99 confidence level. ** refers to the 95 confidence level, and * refers to the 90 confidence level.

Table 12 presents the results for the child mortality outcomes. I use three binary measures of child mortality: under-five mortality, infant mortality, and neonatal mortality. Under-five mortality is coded as 1 for children who died before their fifth birthday; infant mortality is coded as 1 for children who died before their first birthday; and neonatal mortality is coded as 1 for children who died in their first month. The reference group in all the three measures comprises children who were alive at the time of the survey. The effects of mother's education are shown in the first two columns while the effects of father's education are shown in the last two columns. As can be seen, the exogenous variation in mother's education amounts to about 0.8 years; whereas, the exogenous variation in father's education is about one year of schooling. These estimates are very close to the estimates in Table 11 above.

Focusing the discussion on the results of the local polynomial regressions, the results show that mother's education seems to reduce child mortality. In particular, each year of mother's education reduces the probability that a child dies before her fifth birthday by 0.1 percentage points. The effect is even larger for infant mortality (about 0.7 percentage points). Similar to the results in Table 11, these effects are not distinguishable from zero. Estimates from the fathers' sample also show that the increase in father's education reduces child mortality rates. Unlike mother's education, the effect of father's education is larger for neonatal mortality. In particular, each additional year of father's education reduces the probability that a child dies before her first month by 0.7 percentage points. Father's education also reduces the probability of under-five mortality and infant mortality by 0.3 and 0.4 percentage points, respectively. Similar to mother's education, the effects of father's education on child mortality are not statistically significant at the conventional confidence levels[20].

[20] I also found similar insignificant results for both mother's education and father's education using local Probit regressions.

Table 12: The Effects of Parent Education (Separately) on Child Mortality: RD Models

Effects of education on:	**Mothers' Sample**		**Fathers' Sample**	
	Local Linear	**Local Polynomial**	**Local Linear**	**Local Polynomial**
(a) Under-five mortality				
Exogenous variation in education	-0.810***	-0.784***	-1.047***	-0.973***
	(0.179)	(0.262)	(0.187)	(0.192)
Estimated effect (2nd stage)	0.001	-0.001	-0.003	-0.004
	(0.009)	(0.013)	(0.008)	(0.009)
Local sample	24,925	24,925	18,520	18,520
Total sample	76,311	76,311	76,311	76,311
(b) Infant mortality				
Exogenous variation in education	-0.829***	-0.808***	-1.052***	-0.980***
	(0.180)	(0.263)	(0.187)	(0.192)
Estimated effect (2nd stage)	0.000	-0.007	-0.003	-0.003
	(0.008)	(0.013)	(0.008)	(0.008)
Local sample	24,620	24,620	18,367	18,367
Total sample	72,129	72,129	72,129	72,129
(c) Neonatal mortality				
Exogenous variation in education	-0.868***	-0.849***	-1.052***	-0.987***
	(0.182)	(0.268)	(0.187)	(0.192)
Estimated effect (2nd stage)	0.005	-0.000	-0.006	-0.007
	(0.007)	(0.010)	(0.006)	(0.007)
Local sample	23,602	23,602	17,768	17,768
Total sample	62,231	62,231	62,231	62,231

In all regressions, I use a 60-month bandwidth and a triangle kernel weighting function. I also control for child's year of birth fixed effects, child gender, a binary variable for urban, a set of binary variables for region of residence (upper rural, upper urban, lower rural, lower urban, urban governorates, and frontier governorates), and survey-fixed effects. Standard errors are shown in parentheses. *** refers to the 99 confidence level. ** refers to the 95 confidence level, and * refers to the 90 confidence level.

6. Robustness Checks

In this section, I present multiple robustness checks to the main results in Tables 11 and 12. First, I examine the sensitivity of the main results to the chosen bandwidth. Second, I explore the contribution of mother's and father's education by including both variables in the same regression. Finally, I restrict the sample to parents who actually attended one of the primary school systems and explore the effects of parents' education on child health among these parents.

6.1. Sensitivity to Bandwidths

The main results of this chapter show that parental education has no significant impacts on child health. To examine to what extent these findings are sensitive to the chosen 60-month bandwidth, I re-estimate the models using a broad range of bandwidths. The estimates from these models are shown in Figures 10-13, with 90-percent confidence intervals. Figures 10 and 11 provide the estimated effects of mother's education on child nutritional status and child mortality; whereas, Figure 12 and Figure 13 present the effects of father's education on the same set of outcomes. Altogether, these graphs show that the effects of parental education are not statistically significant regardless of the bandwidths used in the analysis. Increasing the bandwidth slightly changes the magnitude of the estimated effects, but all the effects remain statistically insignificant.

Figure 10: 90% CI for the Estimated Effect of Mother's Education on Child Malnutrition

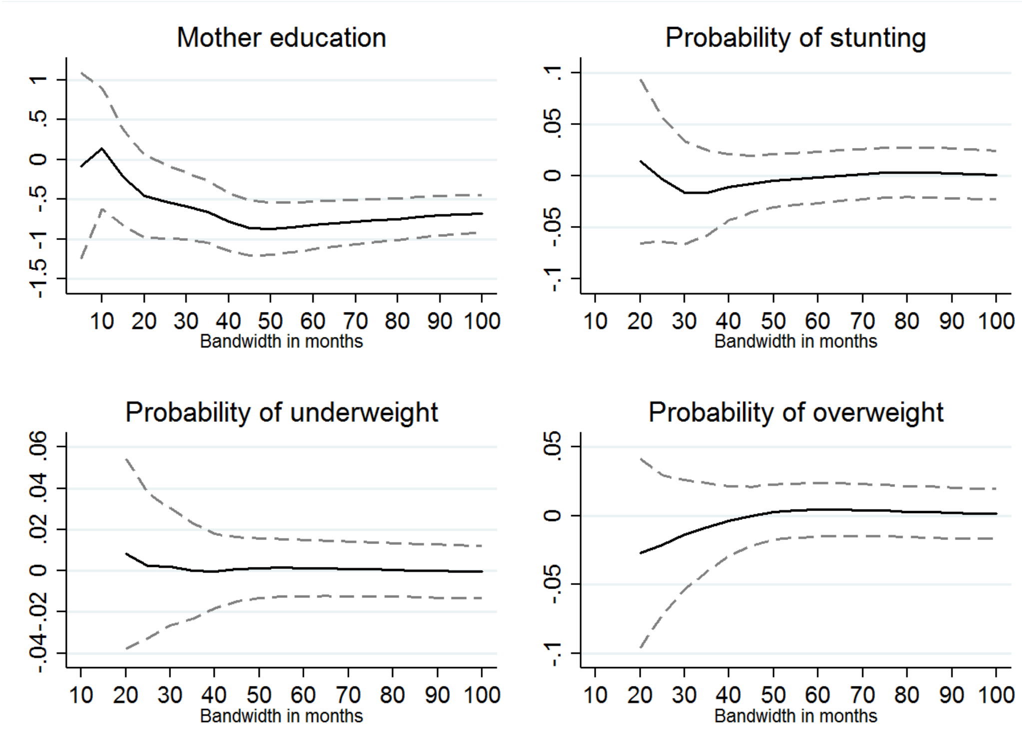


Figure 11: 90% CI for the Estimated Effect of Mother's Education on Child Mortality

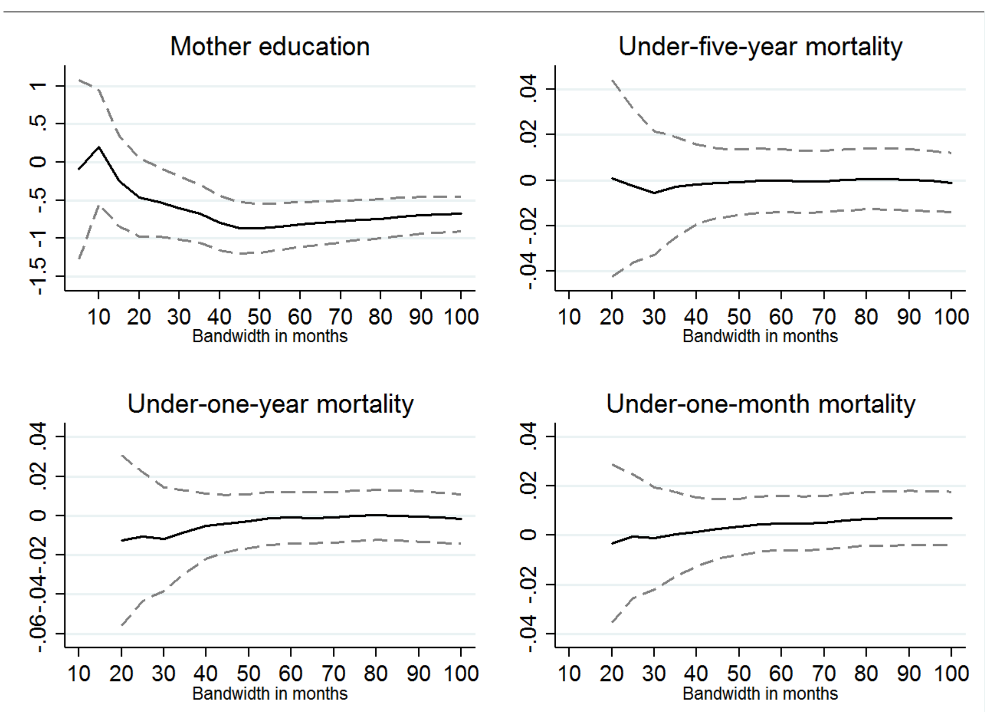


Figure 12: 90% CI for the Estimated Effect of Father's Education on Child Malnutrition

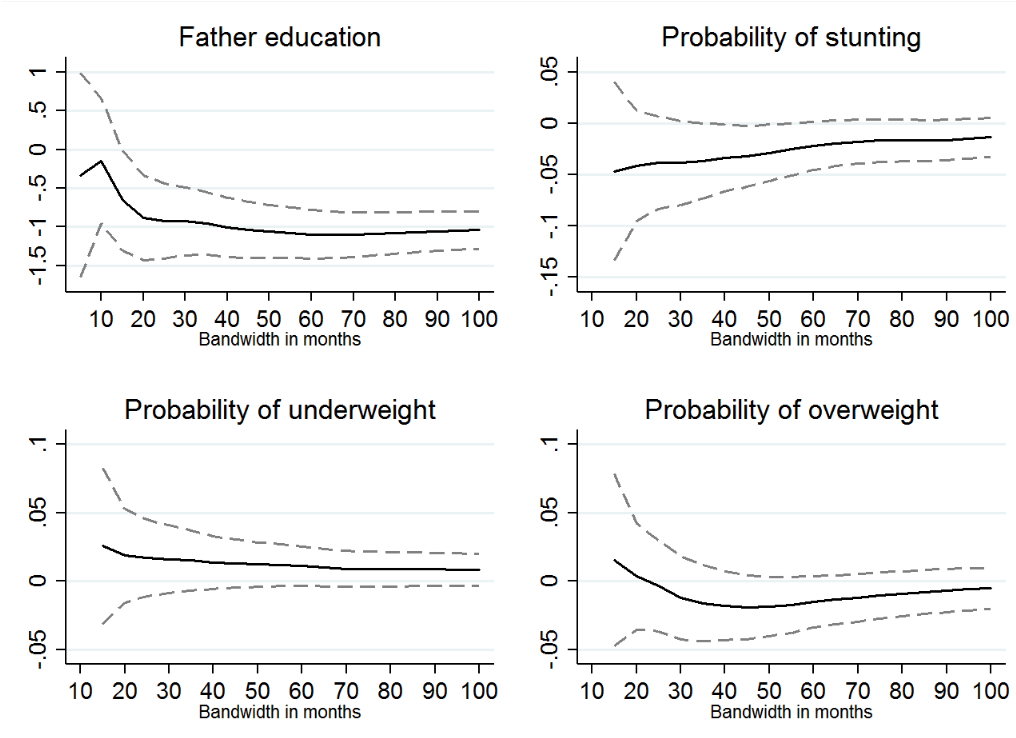
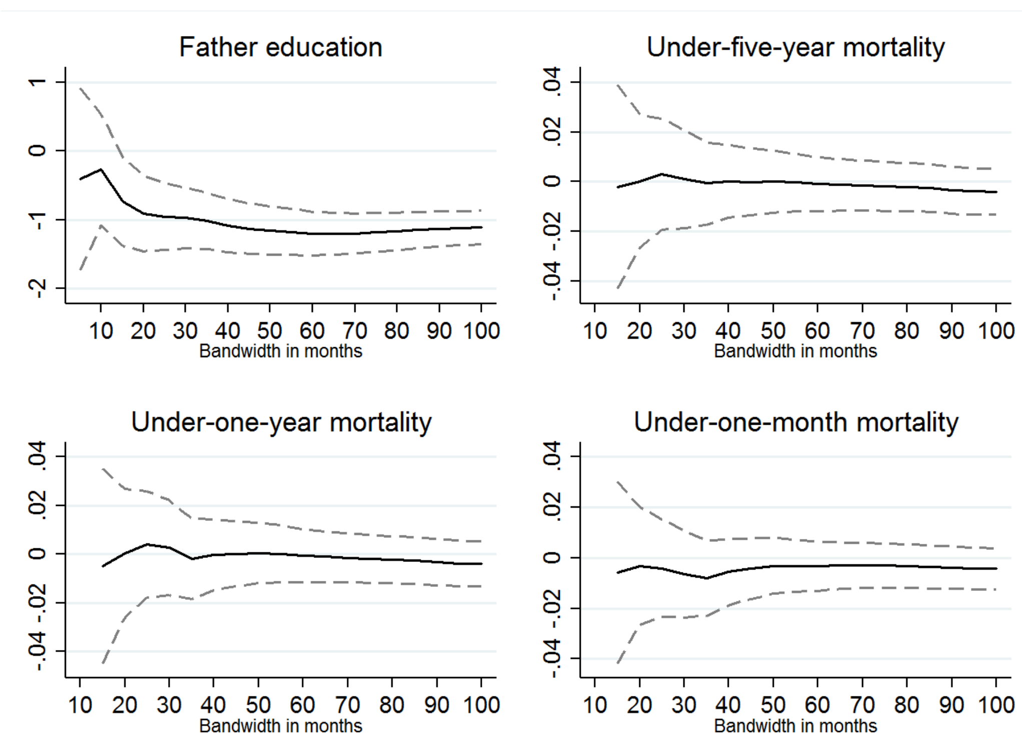


Figure 13: 90% CI for the Estimated Effect of Father's Education on Child Mortality



6.2. Interaction between Mother and Father Education

In the main analysis, I estimate the effects of mother's education and father's education, separately, without controlling for spouse education in each model. This is done in order not to block one of the mechanisms through which each parent' education may affect child health. To illustrate, the association between partners' education is well documented in the literature. Behrman and Rosenzweig (2002) show that under positive assortative mating, high-educated women are more likely to marry high-educated men. Thus, part of the effect of each parent's education on children's outcomes is augmented through the education of the other partner.

I explore further the contribution of mother's education and father's education, separately, I estimate one regression model where I include both mother's education and father's education. I use the types of primary schooling systems attended by each of them to instrument their education. The results of the local Polynomial regressions for child malnutrition outcomes are shown in Table 13, whereas, the results of the local Polynomial regressions for child mortality outcomes are presented in Table 14 below. The local linear estimates are provided in Tables 30 and 31 in Appendix B. The first column of each table reproduces the results from Tables 11 and 12, respectively. As can be seen, the magnitudes of the effects of mother's education and father's education have quite changed, but all estimates remain statistically insignificant.

Table 13: The Effects of Parent's Education on Child Malnutrition

Variables	**Local Polynomial (separate regressions, Table 11)**	**Local Polynomial (combined regressions)**
Exogenous variation in mother's education	-0.764***	-0.807***
	(0.266)	(0.271)
Exogenous variation in father's education	-0.912***	-1.268***
	(0.190)	(0.213)
(a) Probability of Stunting		
Effect of mother's education	-0.016	-0.032
	(0.024)	(0.043)
Effect of father's education	-0.025	0.024
	(0.017)	(0.036)
(b) Probability of Underweight		
Effect of mother's education	0.001	-0.009
	(0.014)	(0.025)
Effect of father's education	0.014	0.015
	(0.011)	(0.021)
(c) Probability of Overweight		
Effect of mother's education	-0.009	-0.020
	(0.019)	(0.033)
Effect of father's education	-0.021	0.008
	(0.014)	(0.028)
Local sample	21,947	21,947
Total Sample	51,776	51,776

The first column re-produces the outputs of Table 11 above (separate regressions). The second column combines both mother's education and father's education in one regression. In all regressions, I use a 60-month bandwidth and a triangle weighting function. I also control for child's age, child gender, a binary variable for urban, a set of binary variables for region of residence (upper rural, upper urban, lower rural, lower urban, urban governorates, and frontier governorates), and survey-fixed effects. Standard errors are shown in parentheses. *** refers to the 99 confidence level. ** refers to the 95 confidence level, and * refers to the 90 confidence level.

Table 14: The Effects of Parent Education on Child Mortality

Variables	**Local Polynomial (separate regressions, Table 12)**	**Local Polynomial (combined regressions)**
(a) Under-five-year mortality		
Exogenous variation in mother's education	-0.784***	-0.877***
	(0.262)	(0.259)
Exogenous variation in father's education	-0.973***	-1.238***
	(0.192)	(0.207)
Effect of mother's education	-0.001	0.004
	(0.013)	(0.020)
Effect of father's education	-0.004	-0.011
	(0.009)	(0.017)
Local sample	26,916	26,916
Total Sample	76,311	76,311
(b) Under-one-year mortality		
Exogenous variation in mother education	-0.808***	-0.903***
	(0.263)	(0.260)
Exogenous variation in father education	-0.980***	-1.239***
	(0.192)	(0.207)
Effect of mother's education	-0.007	-0.004
	(0.013)	(0.019)
Effect of father's education	-0.003	-0.005
	(0.008)	(0.016)
Local Sample	26,611	26,611
Total Sample	72,129	72,129
(c) Under-one-month mortality		
Exogenous variation in mother education	-0.849***	-0.963***
	(0.268)	(0.264)
Exogenous variation in father education	-0.987***	-1.230***
	(0.192)	(0.207)
Effect of mother's education	-0.000	0.005
	(0.010)	(0.015)
Effect of father's education	-0.007	-0.009
	(0.007)	(0.014)
Local sample	25,593	25,593
Total Sample	62,231	62,231

The first column re-produces the outputs of Table 12 above (separate regressions). The second column combines both mother's education and father's education in one regression. In all regressions, I use a 60-month bandwidth and a triangle weighting function. I also control for child's year of birth, child gender, a binary variable for urban, a set of binary variables for region of residence (upper rural, upper urban, lower rural, lower urban, urban governorates, and frontier governorates), and survey-fixed effects. Standard errors are shown in parentheses. *** refers to the 99 confidence level. ** refers to the 95 confidence level, and * refers to the 90 confidence level.

6.3. Restricting the Sample to Parents with at Least a Primary School Degree

The analysis of this chapter assigns parents to one of the primary schooling systems based on their dates of births. In particular, parents born before October 1977 are considered among the six-year primary cohort, whereas, parents born on or after October 1977 are considered among the five-year primary cohort. However, about 47 percent of the mothers in the sample has no formal education, and another 10 percent did not complete their primary education. Likewise, 32 percent of the fathers has no formal education whereas 10 percent did not complete their primary education. These parents neither attended the six-year primary system nor did they attend the five-year system. They are however classified into one of these systems based on their dates of births.

The idea behind including these parents in the analysis is that the decisions of individuals not to go to school or drop out of the primary school might be influenced by the type of primary schooling system. For example, households who decided to drop their children out of the primary school after the fourth grade under the six-year primary system might had chosen differently if they knew that their children would only need to stay one more year toward completing a primary school degree. Having said that, the length of primary schooling, however, might hardly explain household decisions not to enroll their children in school.

Given the considerable portion of parents with no formal education in the sample, there are some concerns about the extent to which they might drive the main results of section 5. In particular, a considerable portion of these parents was born before 1977, and hence, they are classified among the six-year primary cohort[21]. Therefore, including these parents in the analysis

[21]Within the chosen 60-month bandwidth, 57 percent of these mothers are classified among the six-year primary cohort whereas, 66 percent of these fathers are classified among the six-year primary cohort.

underestimates the exogenous variation in education[22] which might explain the insignificant results of section 5. To explore this issue further, I focus the analysis in this section on parents who completed, at least, a primary school degree. These are the parents who actually faced either the six-year primary system or the five-year primary system. The results of this analysis are shown in Tables 15 and 16 for children malnutrition outcomes and children mortality outcomes, respectively. As can be seen from these tables, the exogenous variations in parents' education are larger than before, but all the effects of mother's education and father's education remain statistically insignificant.

Table 15: The Effects of Parent's Education on Child Malnutrition: Restricted Sample

Variables	Mothers' Sample		Fathers' Sample	
	Local Linear	Local Polynomial	Local Linear	Local Polynomial
Exogenous variation in education	-1.444***	-1.737***	-1.454***	-1.430***
	(0.127)	(0.186)	(0.121)	(0.122)
Effects of education on:				
(a) Stunting	0.009	0.002	-0.015	-0.017
	(0.011)	(0.014)	(0.012)	(0.012)
(b) Underweight	0.006	0.005	0.009	0.010
	(0.006)	(0.008)	(0.007)	(0.008)
(c) Overweight	0.005	-0.009	-0.009	-0.011
	(0.009)	(0.012)	(0.009)	(0.010)
Observations (local sample)	12,227	12,227	11,636	11,636

The restricted sample consists of parents who have at least a primary degree (Primary degree and higher). In all these regressions, I control for child's age, child gender, a binary variable for urban, set of binary variables for region of residence (upper rural, upper urban, lower rural, lower urban, urban governorates, and frontier governorates), and survey-fixed effects. Standard errors are shown in parentheses. *** refers to the 99 confidence level. ** refers to the 95 confidence level, and * refers to the 90 confidence level.

[22] This is because the majority of these parents are classified among the six-year primary cohort along with the fact that their educational attainment is almost zero or close to zero. This might create a downward bias of the true effect of one additional year in primary schooling.

Table 16: The Effects of Parent's Education on Child Mortality: Restricted Sample

Effects of education on:	Mothers' Sample		Fathers' Sample	
	Local Linear	**Local Polynomial**	**Local Linear**	**Local Polynomial**
(a) Under-five-year mortality				
Exogenous variation in education	-1.448***	-1.743***	-1.497***	-1.485***
	(0.127)	(0.189)	(0.120)	(0.121)
Estimated effect (2nd stage)	0.003	0.004	0.002	0.001
	(0.006)	(0.008)	(0.006)	(0.006)
Local sample	13,393	13,393	12,571	12,571
(b) Under-one-year mortality				
Exogenous variation in education	-1.450***	-1.753***	-1.492***	-1.481***
	(0.127)	(0.189)	(0.120)	(0.121)
Estimated effect (2nd stage)	0.003	0.001	0.003	0.002
	(0.006)	(0.008)	(0.006)	(0.006)
Local sample	13,281	13,281	12,489	12,489
(c) Under-one-month mortality				
Exogenous variation in education	-1.456***	-1.766***	-1.483***	-1.469***
	(0.127)	(0.188)	(0.120)	(0.121)
Estimated effect (2nd stage)	0.008	0.005	-0.001	-0.001
	(0.005)	(0.007)	(0.005)	(0.005)
Local sample	12,958	12,958	12,189	12,189

The restricted sample consists of parents who have at least a primary degree. In all these regressions, I control for child's year of birth, child gender, a binary variable for urban, set of binary variables for region of residence (upper rural, upper urban, lower rural, lower urban, urban governorates, and frontier governorates), and survey-fixed effects. Standard errors are shown in parentheses. *** refers to the 99 confidence level. ** refers to the 95 confidence level, and * refers to the 90 confidence level.

7. Explaining the Insignificant Effects of Parental Education

The main results of this chapter suggest that the effect of parental education on child health is not distinguishable from zero. In this section, I provide a possible explanation for the insignificant effects of parental education. Specifically, I argue that the lack of effects on child health can be explained by two factors that are closely related: the low levels of parental education in Egypt and the poor quality of schooling, particularly at the primary school level. In particular, the average years of schooling for mothers in the sample is 4.6 years and 6.0 years for fathers. Education at this low level is not expected to produce significant effects on child health, especially given the fact that not only parents in the sample are low educated, but also the quality of education they receive is poor[23]. Among this group of low-educated individuals, education is expected to have little effect on intermediate outcomes that are expected to improve child health such as health knowledge and practices.

To test this explanation, I examine the effect of education on three key intermediate outcomes (channels) through which parental education is thought to improve child health (Glewwe, 1999). More specifically, I examine the effect of parental education on three mechanisms: literacy skills, access to information, and health behavior. I measure literacy skills using a binary variable that is coded 1 for mothers who can read easily and zero otherwise. Parental access to information is measured using a binary variable that takes 1 if a mother reads a newspaper at least once a week and zero otherwise. Health behavior is measured using two measures that capture health practices during pregnancy: the probability of visiting a doctor and the number of doctor visits. It is worth noting that these outcomes are only available for mothers in the sample, and therefore, I cannot generalize the results to fathers.

Figure 14 shows no discontinuity in any of these three intermediate outcomes. That is, mothers around the cutoff are very similar in literacy skills, access to information, and the likelihood and the number of doctor visits during pregnancy. The conclusion from the graphical analysis is supported by the regression estimates in Table 17. As can be seen from the table, education has no significant impacts on literacy skills and other intermediate outcomes[24].

[23] According to estimates from the UNESCO (2015), Egypt is one of ten countries that account for three quarters of the world illiterate adults. Further, a recent report by the World Economic Forum ranked Egypt last in the quality of primary education worldwide (2013).

[24] The findings of this chapter are quite consistent with the findings of the first chapter. In particular, in the first chapter I found that the increase in female education, resulting from the change in the length of primary schooling, did not change women's fertility preferences, enhance women's job opportunities or increase their usages of contraceptive methods.

Figure 14: Discontinuity in Parents' Intermediate Outcomes

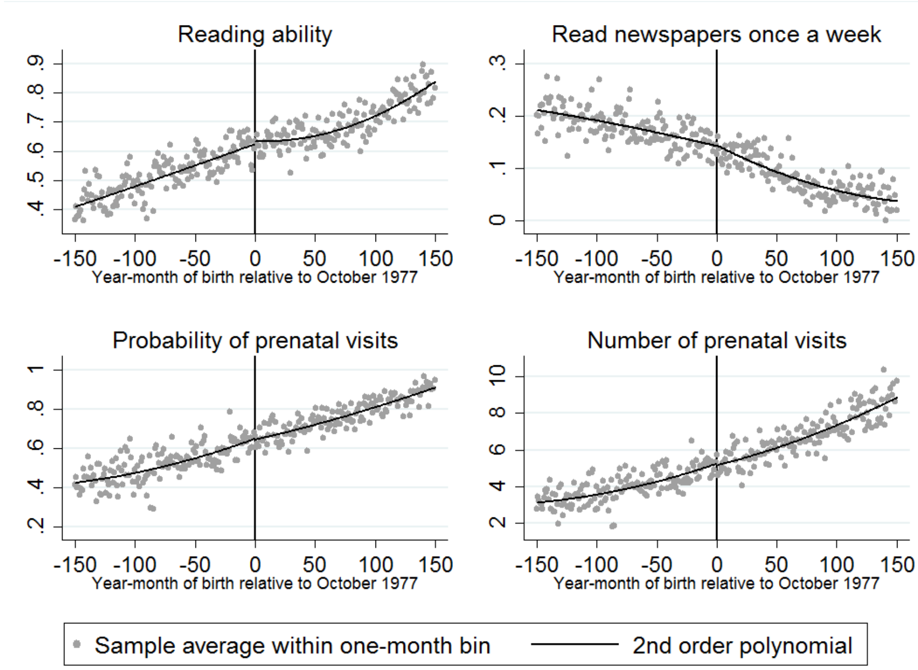


Table 17: The Effects of Mother's Education on Intermediate Outcomes

	Mothers' Sample	
Variables	Local Linear	Local Polynomial
Exogenous variation in education	-0.783***	-0.731***
	(0.185)	(0.271)
Effects of education on:		
(a) Mother can read easily	0.009	0.010
	(0.013)	(0.017)
(b) Mother read newspaper	0.014	0.021
	(0.010)	(0.013)
(c) Number of doctor visits	0.284	0.405
	(0.224)	(0.348)
(d) Probability of visiting a doctor	0.017	0.022
	(0.020)	(0.031)
Local sample	22,570	22,570

This table is estimated using the DHS date (six waves).

8. Conclusion

This chapter examines the causal impact of parental education on child mortality and nutritional status. I exploit the reduction in the length of primary schooling in Egypt in 1988 from six to five years to create exogenous variations in parental education. The results suggest that individuals who spent six years in primary school have completed, on average, one more year of schooling compared to individuals who spent five years. Using this exogenous variation, I find that parental education has no significant effects on children's health outcomes. These results are quite robust to several robustness checks and sample restrictions. My findings are also consistent with some of the current evidence on the effects of parental education on child health in developed countries (Maarten Lindeboom, Ana Llena-Nozal, & Bas van der Klaauw, 2009; McCrary & Royer, 2011).

The results of this chapter, however, differ from the findings of the existing literature on developing countries. This chapter argues that the difference in the findings could be explained by the difference in the interventions used to create the exogenous variations in parental education. In particular, the existing literature has focused on interventions that influenced individuals with relatively higher levels of education compared to individuals influenced by the educational policy change in Egypt in 1988. Lower educational levels are usually combined with the poor quality of education in developing countries, and therefore, education at these levels is expected to have small effects on health knowledge and practices. To support this argument, I provide suggestive evidence that education at that level has little effects on parents' intermediate outcomes that are expected to be essential to improve child health such as literacy skills, access to information, and health behavior.

The findings of this chapter have important implications for policies that aim to reduce child mortality and improve child health in developing countries. In particular, the evidence presented in this chapter suggests that policy interventions that target education at low levels are expected to have small effects on children's health outcomes. Therefore, policymakers in developing countries should not rely only on education to improve child health, especially among low-educated parents. Instead, policymakers should either improve the quality of education or adopt supplementary policies that focus on improving health awareness and practices of parents. For example, health education can be augmented in compulsory education through adjusting curricula to ensure that individuals obtain the essential health knowledge that positively affect their health behaviors and enable them to raise healthy children in the future.

Chapter III: In the Same Boat, but Not Equals: The Heterogeneous Effects of Parental Income on Child Labor

1. Introduction

Despite vigorous efforts by international organizations such as the International Labor Organization (ILO) and the United Nations Children's Fund (UNICEF), child labor has remained a serious issue all over the world. The recent report by the ILO has shown that by the end of 2012 there were roughly 168 million children around the world in the child labor force (Diallo et al., 2013). More than half of those children (115 million) work in hazardous conditions—including working in mines, working with chemicals in agriculture or with dangerous machinery (UNICEF, a) .

In the past three decades, a considerable number of studies have been published on the determinants of child labor. The seminal theoretical work by Basu and Van (1998) emphasized the role of poverty as the main reason for child labor. They explained that parents send their children to work if and only if households cannot meet its subsistence needs without the child's income. The empirical literature of child labor however has found conflicting results related to the effect of parental income on child labor. While some studies have found that poverty is the primary reason for children's work and that improving economic conditions of poor households reduce child labor (Beegle, Dehejia, & Gatti, 2006; Edmonds, 2005; Wahba, 2006), other studies have found that high incidences of child labor are associated with high levels of income (Bhalotra & Heady, 2003; Dumas, 2007; Kambhampati & Rajan, 2006; Kruger, 2007; Ray, 2000). Additionally, Bourguignon, Ferreira, and Leite (2003) and Schultz (2004) found that giving money to the poor does not have an impact on child labor.

Both the theoretical and empirical literature on child labor provide little distinction between the types of child work. In particular, economic theory models child labor from a labor supply prospective where a representative household allocates child's time across different activities (e.g., school, work, leisure) such that the rates of return to child's time are equal across different activities. The economic theory implicitly assumes pecuniary returns to child's work represented by either child's wage or the increase in household production, and hence there is little reason in theoretical studies of child labor to distinguish between types of work (Edmonds, 2008). Likewise, the empirical literature has treated child labor as one homogenous group. Almost all the empirical studies of child labor have relied on Living Standards or Labor Force Surveys, which usually target adult members in households and lack detailed information about work characteristics of children. Studies relying on these surveys measured child labor as a single indicator variable equal to one if a child was economically active during the week before the survey. Edmonds (2008) provides a comprehensive review of 34 theoretical and 90 empirical studies and concludes that the child labor literature lacks a clear description of the nature and the characteristics of child's work.

Despite the treatment of child labor in the literature, data from the International Labor Organization (ILO) reveal substantial differences among working children regarding the type of employer, work intensity, hazard exposure, and work pattern (Diallo et al., 2013). Some children work in unfavorable work conditions such as working full-year job, working in the formal labor market, working with a heavy workload and facing hazardous conditions. On the other hand, other children work in relatively favorable conditions including working for their families, working a light workload in nonhazardous jobs, and working only during school break.

The heterogeneity among working children suggests variations in parental perceptions on child's work and hence, in parental reasons to send their children to work. To illustrate, some families, despite being nonpoor, might prefer to engage their children in work if they perceive nonpecuniary returns to child's work. For example, Rosenzweig and Wolpin (1985) argued that land-rich households engage their children in agriculture work because they believe their children will be better off learning farm-specific knowledge about the land that will be transferred to them in the future. Other non-pecuniary returns to child's work might include teaching children the importance of education in enhancing future outcomes, helping children build strong personalities, gaining general life experience, and developing self-reliance and independence.

Parents who engage their children in work for non-pecuniary returns are more likely to ensure that their children gain these benefits under favorable working conditions. Therefore, they are less likely to allow their children to become exhausted from work or let work deter them from going to school. Instead, they will opt for types of child's work that may have small monetary returns but provide a safe and relatively non-demanding working environment for their children. On the other hand, parents who view child's work as an additional source of income for the household (pecuniary return) are less likely to be cautious about their child work choices. Their children might work in the formal labor market with heavy workload under hazardous work conditions.

Altogether, this chapter argues that the mixed evidence in the empirical literature related to the effect of parental income on child labor- might be explained by the failure of these studies to account for the heterogeneity of child's work. On the one hand, poverty might be a key reason for children working in unfavorable conditions such as job hazards and heavy workloads. On the other hand, poverty may not be a key reason for child's work in nonhazardous jobs and light workloads. Depending on the composition of working children in the data, empirical studies might reach different results. If the data, for instance, is comprised mainly of children working for their family, the effect of parental income, in this case, may not be crucial since other non-pecuniary factors may drive parental decisions to engage their children in the family business. Likewise, if the survey interviews were conducted in the summer, the majority of working children in the data might be full-time students who work only during their school break, as opposed to the case if the interviews were conducted in other seasons. Therefore, the results of the previous empirical studies might not be contradictory if they estimate income effects for different subgroups of working children. The lack of detailed information about child's work conditions in these studies makes it difficult to attribute any of their findings to specific groups of working children.

This chapter takes an advantage of the Egyptian 2010 National child Labor Survey (ENCLS), which provides rich information about the characteristics and the conditions of child work. I divide the population of working children based on several dimensions to differentiate between favorable versus unfavorable work conditions. I then investigate whether parental income is a significant factor in household decision to engage their children in work for each group of working children. Namely, I disaggregate the population of working children based on the following five dimensions: type of employer (unpaid family work vs. market work), hazard exposure (hazardous work vs. nonhazardous work), work intensity (light workload vs. heavy workload), age at first job (starting work as a young kid vs. starting work as an adolescent), and work pattern (working in full-year job vs. working only during summer break).

The ENCLS survey is the first national survey of working children in Egypt, which aimed to provide a description of the patterns of child employment, the conditions of such employment, and the household and community backgrounds. The survey was administered to a nationally representative sample of 30,143 households containing 66,122 children 5 to 17 years of age, representing 17.1 million children in Egypt. To the best of my knowledge, this is the first study to exploit the availability of such detailed data about child labor in Egypt. The results of this chapter show that as income increases, other factors held constant, parents are less likely to send their children to work. Most of the reduction in child's work however comes from the reduction in types of child labor most harmful to children. In particular, the reduction in the likelihood of employment is higher for market work versus family work, for child labor versus light economic activity, for hazardous work versus nonhazardous work, and for full-year work versus working only during school break. Using an instrumental variable approach to account for the potential endogeneity of household income, the results show that hazard exposure and work intensity are the most significant criteria where parental income has different effects.

The rest of the chapter is organized as follows. Section 2 reviews the literature on the impact of income on child labor. Section 3 describes the legal framework of the child labor in Egypt. Section 4 summarizes the data used in this study. Section 5 explains the methodology. Section 6 presents the results. Section 7 concludes.

2. Literature Review

Over the past two decades, a considerable number of studies have been published on the determinants of child labor. Comprehensive reviews of this literature can be found in Basu (1999), U.S. Department of Labor (2000), Basu and Tzannatos (2003), Edmonds and Pavcnik (2005), Edmonds (2007), and Fors (2012).

The seminal theoretical work by Basu and Van (1998) suggests that parents send their children to work if and only if income brought by adults in the household is insufficient to the meet household subsistence needs. Once the adult income rises sufficiently to cover household needs, parents will withdraw their children from the labor market because parents consider their children's non-work a luxury good (the luxury axiom). Along similar lines, Baland and Robinson (2000) emphasize the role of liquidity constraints in creating child labor. They show that inefficient child labor could arise despite parental altruism because of the inability of poor parents to borrow against the future income of their children. Similarly, Ranjan (2001) shows that credit constraints lead to inefficiently high levels of child labor.

Despite the unambiguous theoretical prediction about the effect of household income on child labor, empirical studies have found quite conflicting results. Some studies have found that income reduces child labor. Edmonds (2005), for example, examined the relationship between improvements in economic status and child labor using data from the Vietnam Living Standards Survey. He measured child labor as an indicator variable that is equal to one if a child is engaged in any work in the previous week and zero otherwise. His results suggest that improvements in per capita expenditure explain 80 percent of the decline in child labor in Vietnam between 1993 and 1997. Wahba (2006) used data from the Egypt 1988 Labor Force Survey to examine the effect of adult market wages on child labor. She found that higher adult wages reduces the probability of child labor in market work. Beegle et al. (2006) studied the relationship between household income shocks and child labor. They used data from the Tanzania household panel survey and defined child labor as the total hours spent on work in the previous week. Their findings indicate that transitory income shocks are negatively correlated with child labor. In particular, they show that a household crop loss leads to a significant increase in child labor.

Several studies have questioned the explanatory power of income as the primary determinant of child labor. Ray (2000), for example, used data from two household surveys from Peru and Pakistan to test the poverty hypothesis. The author used a single indicator variable to define child labor and a binary variable to define poverty threshold. Ray (2000) found evidence of the poverty hypothesis in Pakistan, but failed to find such evidence in Peru. Seid and Gurmu (2015) have also fail to find evidence supporting the poverty hypothesis using data from an Ethiopia longitudinal household survey and defining child labor with a single indicator variable.

One important critique of the poverty hypothesis comes from Bhalotra and Heady (2003). They used data from the rural samples of the 1991 Ghana Living Standards Survey and the 1991 Pakistan Integrated Household Survey. They measured child labor as total hours of child work on the family farm. They found that households with large land sizes are more likely to engage their children in work. Given that land is the main source of wealth in rural areas, the authors concluded that their results were inconsistent with the poverty hypothesis. Similar analysis and conclusion were drawn by Dumas (2007) using the Burkina Faso farm household survey.

Kambhampati and Rajan (2006) examined the relationship between regional economic growth and child labor in India and found results opposite to Edmonds (2005). In particular, they used a rural sample from the household socioeconomic survey and measured child labor with a single binary variable. They found that economic growth increases rather than decreases child labor. Likewise, Kruger (2007) examined the relationship between the county-level value of coffee production and child labor using a sample of children from the Brazilian national household survey. He found that increases in the county-level value of coffee production were associated with a higher incidence of child labor.

Despite the extensive literature on child labor, the heterogeneity of working children has received little attention. Edmonds (2008) provides a comprehensive review of 34 theoretical papers and 90 empirical studies published in peer-reviewed academic journals. He has documented that there is a little distinction between types of child work in both the theoretical and the empirical literature of child labor. The theoretical literature of child labor has discussed child labor from a labor supply perspective. According to that literature, a family maximizes its joint utility by allocating children time across various activities such that marginal rates of returns to child's time are equal. The theoretical literature implicitly assumes pecuniary returns to child work, represented by market wage or increases in household production. Thus, all types of child's work have been treated similarly and combined into one homogenous group.

Likewise, the empirical literature does not clearly identify which types of child work are relevant to the analysis. Almost, all empirical studies have relied on Living Standards Survey or Labor Force Survey, which are not designed specifically to collect information about working children. Therefore, information about the characteristics of child's work is limited in these surveys. Most empirical studies represented child labor with a single indicator variable, treating child labor as one coherent group without careful consideration of the heterogeneity of child labor. The lack of information on the composition of working children in these studies makes it difficult to attribute any of their findings to specific populations of working children.

To address these limitations, this chapter takes advantage of detailed data available about working children in Egypt to explore heterogeneous effects of parental income on child labor. In particular, this chapter disaggregates the population of working children and investigates the effect of household income on child labor across various subpopulations of child workers.

3. Legal Framework of Child Labor in Egypt

The Egyptian government has undertaken several steps to reduce child labor. In particular, the government has ratified both ILO Convention No. 138 and the ILO Convention No. 182, concerning setting a minimum age for working children and eliminating the worst forms of child labor, respectively. According to these laws, children younger than 15 years old are not allowed to work. Children 15 to 17 years may work on the condition they do not perform hazardous tasks. Additionally, the work should not be more than 6 hours per day; allows at least a 1-hour break per day; does not involve working overtime, on holidays, more than four consecutive hours, and between 7 p.m. and 7 a.m. The Egyptian law specifies 44 hazardous industries, where children under 18 are not allowed to work. Examples of these industries include working with explosives, agricultural activities involving the use of pesticides, cotton compressing, and leather tanning (United States Department of Labor, 2009).

The problem, however, is that most of the child labor in Egypt is performed in the informal sector, which is highly unregulated and generally disregards labor laws. There is no enforcement mechanism to protect children working in agriculture, unregistered businesses, or domestic service, which are expected to host the majority of child workers in Egypt. The informality of child labor makes it difficult to obtain precise information about the intensity and the characteristics of work done by children. Additionally, the government does not publish statistics on the enforcement of child labor laws (United States Department of Labor's Bureau of International Labor Affairs, 2015).

In 2010, the government made efforts, in collaboration with the ILO, to collect comprehensive data on child labor in Egypt. In particular, the government's Central Agency for Public Mobilization and Statistics (CAPMAS) and the ILO conducted the National Child Labor

Survey in 2010 (ENCLS) and publicly released the complete report in 2012. The ENCLS survey is considered the first internationally acknowledged assessment of the child labor, which aimed to provide an accurate picture of the nature of child labor in Egypt.

4. Data

The data in this study come from the ENCLS survey, which was carried out in April/May 2010 by the Egyptian CAPMAS with the financial and technical assistance of the ILO's International Program on the Elimination of Child Labor (IPEC). The ENCLS survey interviewed 30,143 households containing 163,628 individuals of whom 66,122 were children between the ages of 5 and 17. The non-missing sample that I focus on in this chapter is composed of 54,994[25] children between age 6 and 17.

The ENCL survey includes three questions about child's work in the previous week of the survey. The first question asked children[26] whether they were engaged in any work, at least, one hour during the past week. This is the common question asked in household and labor force surveys used by previous studies. 5,583 children in the sample answered yes to this question. The second question asked children who answered no to the first question whether they were engaged in any of the following activities during the past week, even for one hour: run any kind of business, did any work for payment, worked as a domestic worker for payment, helped in unpaid household business, did any agriculture work in household farm, did any construction work in

[25] I restrict the sample to children living in households where at least one parent is present. This limits the sample to 66,075 children between the ages of 5 and 17. Since this chapter focuses on child labor and schooling decisions, I exclude children who are too young to attend school (based on households' answers to the question of why a child does not go to school) because attending school, in this case, is not a choice that families can make. This restricts the sample to 60,336 children of age 6-17. I have also dropped the observations where household monthly income is missing or below 100 EGP. This restricts the sample to 59,916 children. After dropping missing observations for all other variables used in the analysis, the final sample is composed of 54,994 children between.

[26] About 50 percent of the working children was interviewed in the company of an adult or an older child.

home, caught any fish for sale, fetched water or collected firewood for household use, etc. Household chores were excluded from these examples[27]. The purpose of the second question was to ensure that respondents did not misunderstand the first question. Some family members, for instance, may not count unpaid household work as a type of child's work. Thus, the survey used examples of child's work to clarify that for them. 548 children answered yes to the second question. The third question asked children who answered no to the second question if they had a job that he/she will definitely return to. 419 children answered yes to this question.

Altogether, 6,550 children in the sample worked at least one hour during the week prior to the survey or they had a job to return to. Therefore, 11.92 percent of children in the sample are economically active. This is a broad definition for child labor, similar to that used in almost all the empirical studies of child labor. The ILO, however, defines child labor as a subgroup of economically active children who are under age 12 and are employed for at least one hour per week in any type of work (excluding household chores), children age 12-14 employed for 14 or more hours per week, and children under 18 engaged in hazardous work[28]. The ENCLS survey asked working children about hazardous conditions they face during work. The description of hazardous work is determined by the ILO and is shown in Table 1-1 in the ENCLS report (ILO & CAPMAS, 2012). Table 32 in Appendix C shows the percentage distribution of working children in the sample by type of hazards they face at work. The most common types of hazards in the data include exposure to dust and fumes, exposure to extreme cold or heat at work, unavailability of bathrooms at work, exhaustion, and bending for a long time

[27] The survey also asked children several questions about household chores. However, household chores seem to be underreported in the data (0.4 percent) and hence will be excluded from the analysis of this essay.
[28] The definition of child labor of the ILO is guided by the principles enshrined in the ILO's Minimum Age Convention No. 138 and the Worst Forms of Child Labor Convention No. 182.

Based on the ILO's definition of the child labor, 5,307 children in the sample (9.56 percent) are child laborers. This indicates that 1,243 children in the sample are only economically active (performing light work) without being child laborers. For the purpose of comparing the results of this chapter with the previous studies, I focus the discussion in this chapter on the broad definition of child labor of all economically active children. In particular, I use the broad definition of child labor to investigate the heterogeneous effects of household income for four work dimensions: type of employer, hazard exposure, age at the first job, and work pattern. I also explore the heterogeneous effect of household income by work intensity, where I divide the population of working children into two subgroups: children who are economically active but not child laborers (performing light or safe work) and child laborers as defined by the ILO. I use the word "child labor" to refer to the broad definition of child labor; whereas, I use the word "child labor-ILO" to refer to the narrow definition of the ILO.

Table 18 provides marginal and joint distributions of all children by their school attendance and child labor situations. As can be seen from the table, the majority of children in the sample (about 83 percent) attend school and do not work. About 7.3 percent of children combine school with work, while 4.6 percent of children participate in the labor market and do not attend school. The rest of the children (about 5 percent) neither work nor do they go to school.

Table 18: Percentage Distributions for School Attendance and Child Labor (N=54,994)

	School Attendance		
Child Labor	**No (%)**	**Yes (%)**	**Total (%)**
No	5.41	82.68	88.09
Yes	4.58	7.34	11.92
Total	9.99	90.02	100.00

This table is computed using the NCLS Survey.

The percentage of child labor increases with age. In particular, 24.4 percent of children aged 15-17 are child laborers, compared to 14 percent and 4.7 percent among children aged 12-14 and 6-11, respectively. Additionally, the percentage of child laborers is higher among boys, with 17.7 percent of boys are child laborers compared to 5.8 percent female child laborers. The incidence of child labor in Egypt is concentrated in rural areas, with 17.3 percent of rural children are child laborers compared to 6.7 percent child labor rate in urban areas. Governorates such as Mounofia, Fayoom, Minya, Banisuif, Behira, Souhag, and Quena, are among the governorates with the highest incidence of child labor. Urban governorates such as Port Said, Giza, and Cairo are among the governorates with the lowest child labor rates.

There are differences in household socioeconomic status between working and nonworking children. Table 19 provides descriptive statistics for working and nonworking children. As can be seen from this table, parents of nonworking children are more educated compared to parents of working children. Additionally, parents of nonworking children had joined the labor market later than parents of working children who were themselves child workers. The comparison of income and wealth between working and nonworking children shows that the average household monthly income[29] for nonworking children is higher than that of working children (1,305 EGP vs. 1,077 EGP). Households of working children are, however, more than twice as likely to own land with larger sizes in comparison to households of nonworking children.

[29] Throughout this study, I measure household income as the sum of income brought by all household members excluding income brought by children under the age of 18.

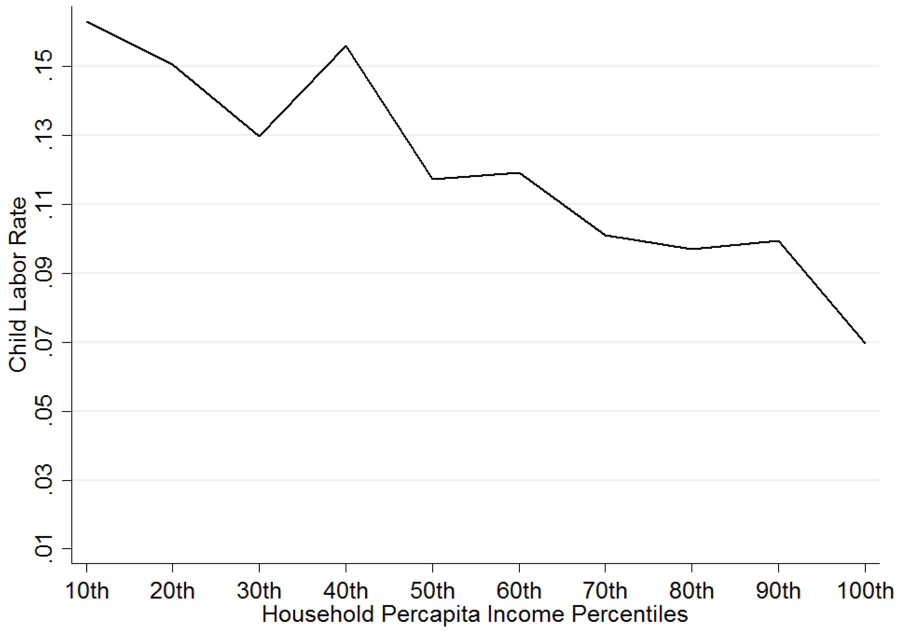
Table 19: Means and Standard Deviations of Key Variables (N=54,994)

Variables	Nonworking children		Working children	
	Mean	**S.D.**	**Mean**	**S.D.**
Child Characteristics				
Child is male=1	0.48	0.50	0.76	0.42
Child age	11.27	3.22	13.84	2.64
Child goes to school=1	0.94	0.24	0.62	0.49
Child years of schooling	5.19	3.20	6.14	3.23
Household Characteristics				
Urban=1	0.54	0.50	0.28	0.45
HH size	5.90	1.60	6.34	1.83
Age of the head of household	44.28	7.77	46.80	8.66
Education of the head of household	7.60	5.57	3.94	4.72
Head was a child laborer=1	0.20	0.40	0.41	0.49
Mother currently work=1	0.33	0.47	0.65	0.48
Single parent HH=1	0.06	0.23	0.09	0.28
HH monthly income (in EGP)*	1,305.39	8,060.87	1,076.55	5,962.18
HH monthly income per capita*	231.70	1,331.29	1,76.40	9,93.90
Ln(HH monthly income)	6.67	0.68	6.57	0.65
Own land=1	0.18	0.38	0.45	0.50
Size of land holdings (in hectares)ǂ	0.77	3.65	1.96	4.87
Own livestock=1	0.34	0.47	0.71	0.45
Observations	48,444		6,550	

*The range of household total monthly income in the data is 100 EGP to 401,996 EGP, while range of income per capita is 8.3 EGP to 66,666 EGP. ǂ The range of the size of land holdings is zero to 99.9 hectares.

The conventional wisdom is that child labor is caused by poverty, and this is broadly accurate. Table 19 shows that average household monthly income is higher for nonworking children compared to child laborers. Figure 15 below depicts the relationship between child labor and the percentiles of household income per capita. The figure shows that although child labor falls as income rises, a substantial number of households in the higher income percentiles engage their children in work. Moreover, almost 85 percent of children in households in the lowest income percentile are not child laborers. This figure suggests that income is only part of the story and that other factors also contribute to child labor.

Figure 15: Relationship between Child Labor and Income Percentiles



This chapter explores the types of child's work chosen by higher-income households and lower-income households and whether there are systematic differences in the nature of work chosen by each of them. The existence of this difference would suggest a heterogeneity in parents' motivations to send their children to work, which is reflected in parental choices of the types of child's work. In particular, higher income households may perceive nonpecuniary returns to child's work and therefore opt for types of child's work that may have small monetary returns but provide a safe and relatively non-demanding working environment for their children. To investigate this hypothesis, I disaggregate the population of working children using several criteria in a way that is believed to differentiate the severity of the work. I then examine the relationship between household income and child labor for each group of working children.

In particular, Table 20 disaggregates the population of working children based on five dimensions: work intensity (light economic activity vs. child labor-ILO), hazard exposure (hazardous work vs. nonhazardous work), age at first job (started work as a teen 12-17 vs. started work as a kid under 12), work pattern (work in school break only vs. full-year work), and type of employer (family work vs. market work).

Panel (1) in Table 20 shows the disaggregation of working children based on work intensity. As mentioned earlier, the ILO definition of child labor includes children under age 12 and employed for at least one hour per week in any type of work (excluding household chores), those ages 12-14 employed for 14 or more hours per week, and children under 18 engaged in hazardous work. Employed children age 12-14 who work for less than 14 hours per week and children age 15-17 not involved in hazardous work are not considered child laborers under the ILO definition. Panel (1) shows that 81 percent of working children in the sample are child laborers based on the ILO definition, whereas 19 percent of working children are considered economically active but not child laborers. The average household monthly income per capita is 3.1 percent higher for households where children are engaged in light economic activities compared to households where children are defined by the ILO as child laborers.

Panel (2) shows the disaggregation of working children based on hazard exposure. As indicated earlier, the ENCLS survey asked working children about several hazardous conditions they may face at work and children answered yes or no to each of them. The most common types of hazards in the data include exposure to dust and fumes, exposure to extreme cold or heat at work, unavailability of bathrooms at work, exhaustion, and bending for a long time. The disaggregation based on hazard exposure in panel (2) shows that the majority of working children (about 67 percent) in Egypt are exposed to some hazardous conditions during work.

Rich households are less likely to engage their children in a hazardous work compared to poor households. In particular, the household monthly income per capita for children working in hazardous work is 15 percent less than the household monthly income per capita for children working in nonhazardous work.

Panel (3) shows the distribution of working children by age at the first job. The survey asked working children about their ages when they first started to work. I use age 12 as a cutoff age consistent with the ILO's definition of child labor. As mentioned before, a working child under age 12 is defined by the ILO as a child laborer regardless of the type or the intensity of work. This implies that children at this age are considered very young to perform any job. Therefore, I consider children who started to work under 12 years old as very young to work. The majority of working children in the data (59 percent) begun to work under age 12. These children tend to live in low-income households. In particular, the household monthly income per capita for children who started work as kids under 12 is 28.4 percent less than the household monthly income per capita for children who entered the labor market as adolescents aged 12-17.

Panel (4) shows the distribution of working children by work pattern. Working children at the time of the survey were asked if they had worked in each single month of the past 12 months. About 55 percent of working children who were economically active during the week before the survey had also worked in the past 12 months. Of these children, about 48 percent reported working most of the months last year (worked ten months or more). The remaining 7 percent reported working only during the summer months (Jun, July, and August). These months coincide with the end of school year in Egypt. Almost all the children (about 98 percent) who work only in the summer go to school. Furthermore, these children live in households where monthly income per capita is 78.2 percent higher than the household monthly income per capita for children who work in full-year jobs. In fact, the average household income per capita for children who work only during their school break is surprisingly 37 percent higher than the household income per capita for children who are not engaged in any work.

Panel (5) in Table 20 shows the distribution of working children by type of employer. The survey asked child workers whether they worked as employees, owned account workers, employers, or were unpaid family workers. The latter excluded household chores. Almost all the children were either employed in the formal labor market or unpaid family workers. About 62 percent of the working children worked for their families; whereas 38 percent were employees in the formal labor market. Table 33 in Appendix C shows the distribution of working children by the type of place at which they work. Among children working for their families, about 70 percent carried out their work on farms. On the other hand, among children engaged in a market work, only 26 percent carried out their work on farms. The remaining market work was carried out in factories, offices, retail establishments and shops, and construction sites.

As can be seen from Panel (5), the income of households where children are engaged in family production is quite lower than the income of households where children work in the formal market. This could be due to the fact that I focus the analysis on monthly cash income. Parents of unpaid family workers are themselves less likely to have jobs in the formal market. Besides, a considerable portion of the farm production is usually consumed by the farming households in developing countries. Any additional sales of agricultural products does not usually occur on a monthly basis, and hence the revenues may not appear as a monthly income. These households are, however, more likely to own lands and livestock compared to the households where children work in the formal labor market.[30]

[30] I control for household's ownership of lands and livestock in all my regression models.

Table 20: Distribution of Working Children and Household Income Per capita (N=6,550)

	%Working children	Average monthly family income per capita *
1) Work intensity		
- Light work	18.98	180.82
- Child laborers (ILO)	81.02	175.36
2) Hazard exposure		
- Nonhazardous work	32.63	193.40
- Hazardous work	67.37	168.16
3) Age at first Job		
- Teens 12-17	41.18	202.81
- kids under 12	58.82	157.91
4) Work pattern**		
- Work in summer only	7.47	317.50
- Work most of the year	48.14	178.22
5) Type of employer		
- Family work	61.71	174.73
- Market work	38.29	179.10

*The average monthly household income per capita for nonworking children is 231.70 EGP.
**about 55 percent of children who worked last week had participated in the labor market in the past 12 months.

It is worth mentioning that there is some overlaps between these work dimensions. Table 34 in Appendix C shows the joint distribution of these groups. As can be seen from the table, there is an overlap between work intensity, hazard exposure, and age at first job. In particular, the majority of child laborers, as defined by the ILO, are engaged in hazardous work and had started to work when they were kids under 12. In contrast, none of children who are engaged in light workloads face hazardous conditions during work and the majority of them had started to work when they were adolescents age 12-17. This implies that child laborers and children engaged in light work are not only different in the intensity of work but they are also differ in exposure to hazards and their ages at first job. Therefore, the difference in the effect of income between these two groups will capture the difference in three dimensions without being able to isolate the effect of each. This is not, however, expected to be problematic as the differences between the two groups are consistent in a way that describes the unfavorable work conditions. Therefore, the estimated combined effect will be quite meaningful.

Table 34 also shows an overlap between type of the employer and the age at first job. In particular, children who started to work as kids are mostly working for their families, while children who started to work as adolescents mostly work in the formal market. This case raises a concern of mixed effects that might cancel each other or revise the expected effects. Children working for their families are generally considered to be better off in comparison to children who work in the formal market. However, children starting to work as kids under 12 are considered to be worse off in comparison to children staring to work as adolescents. Isolating the effect of each work dimension (type of employer and age at work) requires comparing groups that are similar except in one dimension. That is, comparing children who work for their families and children who work in the market such that both groups had started to work at the same age. Likewise, comparing children who joined the labor market as kids and those who joined the market as adolescents such that both groups work for the same employer. The limited sample size, however, makes it quite difficult to conduct that detailed subanalyses.

5. Methodology

Most of the empirical studies have modeled child's work and schooling separately using limited dependent variable models such as Logit or Probit models (Edmonds, 2007). This approach, however, may produce biased estimates if the decisions of child's work and schooling are jointly determined by children's unobservable ability and omitted household characteristics. An alternative to the univariate independent models is the bivariate Probit model, which is implemented in this chapter. This model has the advantage of accounting for the correlation between the error terms in schooling and child labor regressions. That is, the bivariate Probit regression takes into account that household and child unobservable characteristics affect both child labor and schooling decisions. In particular, I model child labor, C_{ir}, and child schooling, S_{ir}, jointly as two binary dependent variables. C_{ir} is a binary variable equal to one if child i in household r works and zero otherwise. S_{ir} is a binary variable equal one if child i in household r goes to school and zero otherwise.

These two binary variables correspond to the two continuous latent variables $C^*{}_{ir}$ and $S^*{}_{ir}$, respectively. It is assumed that each observed variable, C and S, takes on the value one if its latent variable takes on a positive value, as follows:

$$C^*{}_{ir} = \alpha_0 + \alpha_1 log(family\ income)_r + \alpha_2 X_{ir} + \alpha_3 X_r + \varepsilon_{ir}, \tag{1}$$
$$S^*{}_{ir} = \beta_0 + \beta_1 log(family\ income)_r + \beta_2 X_{ir} + \beta_3 X_r + \upsilon_{ir}, \tag{2}$$

where

$$C_{ir} = \begin{cases} 1 & if\ C^*{}_{ir} > 0 \\ 0 & \quad otherwise, \end{cases} \tag{3}$$
$$S_{ir} = \begin{cases} 1 & if\ S^*{}_{ir} > 0 \\ 0 & \quad otherwise, \end{cases} \tag{4}$$

and

$$\begin{bmatrix} \varepsilon_{ir} \\ \upsilon_{ir} \end{bmatrix} |\ control\ variables \sim \mathcal{N}\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & \rho \\ \rho & 1 \end{bmatrix}\right). \tag{5}$$

Where $C^*{}_{ir}$ and $S^*{}_{ir}$ are latent variables for child work and child schooling, respectively. log($family\ income$) is the logarithm of household r monthly income. I exclude from this measure all the income brought by children under the age of 18. X_{ir} is a vector of child-level characteristics, which includes child gender and child age. X_r is a vector of household

r characteristics, which includes an urban dummy, family size, education of the head of the household, a dummy for whether the head of the household was a child laborer, a dummy if a child's mother works, a dummy for single-parent household, a dummy for whether a household owns livestock, and the size of agricultural land. Both α and β are vectors of parameters. ε_{ir} and υ_{ir} are errors terms that are assumed to be correlated with correlation ρ.

I estimate the bivariate Probit model specified in Eq.1- Eq.5 for all working children as the first step in my analysis. In the next step, I disaggregate the population of working children and run two separate bivariate Probit regressions for each of the five work dimensions mentioned earlier. For example, for the type of employer dimension, I run two separate bivariate Probit regressions for family work and market work. In the bivariate Probit regression for family work, the dependent variable, C_{ir}, equal to one if a child works in a family business and zero if the child does not work at all. Similarly, in the bivariate Probit regression for market work, the dependent variable, C_{ir}, equal to one if a child works in the market and zero if the child does not work. Therefore, in each regression, the group of nonworking children is used as the comparison group. Using nonworking children as the comparison group for both regressions helps controlling for any differences in child or household characteristics between the groups of market-work child workers and family-work child workers, and therefore, makes it possible to compare the effect of income on child labor between these two separate regressions.

I repeat this analysis for the other work dimensions. Disaggregating the population of working children helps explain the sources of the change in child labor as income increases. For example, if the aggregate bivariate Probit model shows that child work decreases as income increases, the heterogeneous analysis would describe which types of child work are most sensitive to the changes in income.

One concern about this analysis is the potential endogeneity of household income. That is, poor households differ from rich households in many ways that might be associated with income and child labor decision. Parental noncognitive skills and family background are expected to confound the relationship between household income and child labor. To handle the endogeneity of the income, one needs to extract an exogenous variation in family income that is uncorrelated with household's unobservable characteristics.

In this chapter, I use measures of shocks in households' income over the past 12 months to create an exogenous variation in income. In particular the ENCLS survey asked all households if they experienced a fall in their income over the past 12 months due to any of these reasons: loss of employment of any member, bankruptcy of a family business, illness or serious accident of a working member, death of a working member, abandonment by the household head, fire in house/business/property, criminal act by a household member, land dispute, loss of cash support or in-kind assistance, fall in prices of products of the household business, loss of harvest, and loss of livestock. The percentages of households facing each of these income shocks are shown in Table 35 in Appendix C. The most common reasons for the fall in income over the past 12 months are illness or serious accidents of a working member (7.1 percent) and the loss of employment of any member (3.25 percent).

Some of the reasons for the decrease in income might, however, be correlated with households' behaviors. For example, the loss of employment of any member, bankruptcy of a family business, abandonment by the household head, criminal act by household member, fire in house/business/property, and land dispute are all expected to be consequences of some actions taken by households. Whether a household experiences any of these shocks is not expected to be entirely random of households' characteristics. Additionally, some of the others reasons are expected to affect only a specific group of households based on their eligibility or ownership of assets such as loss of cash support or in-kind assistance, fall in prices of products of the household business, loss of harvest, and loss of livestock.

In this chapter, I focus on two sources of income decline as the source of the exogenous variation in income. Specifically, I use illness/serious accident of a working member and the death of a working member. In particular, I create a dummy variable that equal 1 if a household faced one of these two accidents over the past 12 months and zero otherwise. About 7.73 percent of households in the sample (7.1 percent faced illness/serious accidents while 0.63 percent faced a death of a working member) faced one of these two events in the last year. I us this dummy variable to instrument family income in Eq. 1 and Eq.2 above.

One concern about this instrumental variable is that despite the fact that the probability of death is expected to affect both rich and poor people households, old people have higher probability of death than young people. Table 21 below shows that rich parents are slightly older than poor parents because the rich have, on average, higher life expectancy. To account for this difference, I control for the age of the head of household in the regressions. Even with controlling for the differences in age, poor parents are expected to have higher probabilities of sickness and death due to lack of healthcare access. To account for this possibility, I create a wealth index using households' ownerships of assets and durable goods and add the quintiles of this index to the set of control variables. Controlling for the wealth index quintiles allows me to examine the effect of a recent shock in monthly income on child labor within a wealth quintile. That is, the exogenous variation that is being extracted in this chapter comes from the change in the monthly income (the flow) conditional on the wealth (the stock).

The ENCLS survey asked households many questions about their house characteristics and their ownerships of durable goods. Table 36 in Appendix C provides a detailed description of the variables used in the computation of the wealth index. These variables include the type of dwelling; the ownership of dwelling; the size of dwelling; the number of rooms per household member; the availability of bathrooms and kitchens; the type of toilet; the source of energy for cooking, heating, cooling, and lightening; the main source for drinking water; and a set of indicator variables for household ownership of durable goods such as air-condition, automobile, washing machine, TV, and mobile phone. I use the principal component analysis to combine these variables into a single index called the wealth index[31].

Altogether, I re-estimate the bivariate Probit regression of Eq.1- Eq.5 using the created instrumental variable to instrument family income, controlling for the age of the head of the household and the wealth quintiles, as follows.

$$C^*_{ir} = \gamma_0 + \gamma_1 log(family\ income)_r + \gamma_2 X_{ir} + \gamma_3 X_r + \gamma_4 ag\_head_r + \gamma_k \sum_{k=5}^{8} wealth\_quintile_{rk} + \omega_{ir}, \quad (7)$$

$$S^*_{ir} = \pi_0 + \pi_1 log(family\ income)_r + \pi_2 X_{ir} + \pi_3 X_r + \pi_4 ag\_head_r + \pi_k \sum_{k=5}^{8} wealth\_quintile_{rk} + \varphi_{ir}, \quad (8)$$

The first stage is specified as follow,

$$log(family\ income)_r = \delta_0 + \delta_1 IV_r + \delta_2 X_{ir} + \delta_3 X_r + \delta_4 ag\_head_r + \delta_k \sum_{k=5}^{8} wealth\_quintile_{rk} + \epsilon_r \quad (9)$$

[31]The wealth index is a measure of the living standards, similar to the one computed in the DHS survey. For more information on the construction of the wealth index, please see Rutstein and Johnson (2004). I implement the principal components analysis using the Stata factor analysis procedure, implemented using the Stata program “pca”. In this analysis and in all the regressions in this chapter, I account for the complexity of the survey setting through using the Stata prefix command “svy” to correct variance estimates.

where C^{*}_{ir}, S^{*}_{ir}, $log(family\ income)_r$, X_{ir}, and X_r are defined as before. IV_r is an instrumental variable that equals one if household r faced either illness/serious accident or a death of a working member over the past 12 months and zero otherwise. ag_{head_r} is the age of the head of household r. $\sum_{k=5}^{8} wealth_{quintile_{rk}}$ is a set of four indicator variables representing the upper four quintiles of the wealth index (the excluded lowest quintile represents the baseline group). ω_{ir}, φ_{ir}, and ϵ_r are error terms.

I apply the instrumental variable (IV) analysis using the two-stage residual inclusion (2SRI) approach introduced by Terza, Basu, and Rathouz (2008). The 2SRI is an IV-based approach to correcting for endogeneity bias in nonlinear models. Terza et al. (2008) show that extending the popular linear two-stage least squares (2SLS) estimator to nonlinear models (such as bivariate Probit models) results in inconsistent estimates. The 2SRI estimator has the same first-stage (Eq.9 above) as the linear 2SLS, where I regress the endogenous variable (logarithm of family income) on the instrumental variable along with other control variables from the main equation. In the second-stage regression, however, the logarithm of family income variable is not replaced with its predicted values from the first stage, as is the case with the linear 2SLS. Instead, the first-stage residuals are included as additional regressors in the second-stage estimation[32]. In symbols, the second stage bivariate Probit regression under Terza's approach is specified as follow,

[32] Following the recommendation of Terza et al. (2008), the standard errors of the 2SRI estimator are obtained via bootstrapping using 2,000 replications.

$$C^*{}_{ir} = \gamma_0 + \gamma_1 log(family\ income)_r + \gamma_2 X_{ir} + \gamma_3 X_r + \gamma_4 ag\_head_r + \gamma_k \sum_{k=5}^{8} wealth\_quintile_{rk} + \gamma_9\ first\_stage\_residual_r + u_{ir}, \quad (10)$$

$$S^*{}_{ir} = \pi_0 + \pi_1 log(family\ income)_r + \pi_2 X_{ir} + \pi_3 X_r + \pi_4 ag\_head_r + \pi_k \sum_{k=5}^{8} wealth\_quintile_{rk} + \pi_9\ first\_stage\_residual_r + \tau_{ir}, \quad (11)$$

where $first\_stage\_residual_r$ is the first stage residuals generated from Eq.9 above.

I estimate the bivariate Probit model above for all working children and for the subpopulations of working children, separately, as specified before. It is worth mentioning that using this instrumental variable limits the ability to explore the heterogeneous effects of income by dimension of child's age at first job. To illustrate, the exogenous variation in income is created from an income shock over the past 12 months prior to the survey. The average working child in the sample has started to work three years prior to the time of the survey[33]. Thus, children have joined the labor market before the occurrence of the income shock, and so this exogenous variation may not predict child's age at first job.

Table 21: Age of the Head of the Household, Child Labor, and Child Schooling

Quintiles of family monthly income per capita	**Average age of household head**	**% Children going to school**	**% Children going to work**
1st income quintile	43.52	15.70	85.20
2nd income quintile	43.52	13.44	88.83
3rd income quintile	44.18	11.90	90.30
4th income quintile	45.05	9.74	91.89
5th income quintile	46.74	8.54	94.19

This table is computed using the NCLS Survey.

[33] Children who started to work under age 12 have spent about four years in the labor market by the time of the survey; whereas, children who started to work as adolescent have spent about two years in the labor market by the time of the survey. Thus, both groups had started to work before the occurrence of the income shock.

6. Results

6.1. Baseline Results: Aggregate Analysis Ignoring Endogeneity

Table 22 below shows the results of the bivariate Probit regression ignoring the endogeneity of income. The first row combines all the working children in one group in a way similar to the majority of previous studies. The following five panels disaggregate the population of working children based on work intensity, hazard exposure, age at the first job, work pattern, and the type of employer. The estimates in this table represent the average marginal effects (AME) of the logarithm of family monthly income on the marginal probabilities of child labor and schooling. The AME for all regressors are presented in Tables 37 - 47 in Appendix C.

The first two columns of Table 22 present the result of independent Probit models; whereas the last two columns show the findings from the bivariate Probit regressions. The results of the two models are very close with slight changes in magnitudes. The estimated correlation parameter between the error terms of the schooling and child labor equations (ρ) is, however, statistically significant. This suggests that the bivariate Probit model is a better fit than the independent Probit models. Therefore, I focus my discussion on the results of the bivariate Probit regression.

The results from the aggregate analysis in the first row show that if family monthly income increases by one percent, other factors held constant, a child will be 0.018 percentage points less likely to work and 0.009 percentage points more likely to go to school. These effects are statistically significant at 99 confidence level. This finding is consistent with the literature that found that poverty or negative economic shocks are important factors driving children to work. In particular, the result in this section is consistent with Beegle et al. (2006), Edmonds (2005), and Wahba (2006).

As can be seen, the effect of income on school attendance is smaller than the effect of income on child labor. That is, there is a small difference between the poor and the rich in the probability of enrolling their children into school compared to the difference in the probability of child labor. This can be justified by the fact that the majority of children (90 percent) in the sample go to school (Table 18)[34]. There are some differences in school enrollment rate across income levels as can be seen from the second column of Table 21. These differences, however, are relatively smaller than the differences in child labor rates (the third column of Table 21).

6.2. Baseline Results: Heterogeneous Analysis Ignoring Endogeneity

Panel (1) through panel (5) of Table 22 present the results of the subpopulation analysis. In particular, panel (1) divides the population of working children based on the ILO's definition of child labor. That is, I run the bivariate Probit regression, separately, for child laborers-ILO and children performing light economic activity. For each group, the C_{ir} is equal to one if a child is engaged in a work defined by that group and zero if the child does not work at all. The results in panel (1) indicate that, on average, a one-percent increase in household monthly income reduces the probability that a child is only economically active –relative to not performing any work— by 0.003 percentage points, other factors held constant. The reduction in the probability of child labor-ILO relative to the nonworking status, however, is six times greater than this estimate (0.018 versus 0.003 percent). Both estimates are statistically significant. These results suggest that most of the reduction in child's work as income increases (the findings of the aggregate analysis) comes from the reduction in the worst forms of child, as defined by the ILO[35].

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[34] In fact, primary school enrolment in Egypt reached 96 percent in the school year 2008/2009 (United Nations Development Programme (UNDP), 2010).

[35] Considering the overlapping between different dimensions of child' work, discussed earlier, the worst forms of child labor combine heavy workloads, hazardous work, and work performed by kids under 12.

The findings from the schooling equations show that a child is more likely to attend school as household income increases, other factors held constant. The increase in the probability of school attendance for child laborers-ILO is higher than the increase in the probability of school attendance for children engaged in light economic activity. This is consistent with the results from the child work equation. In particular, there are small differences in income and in school attendance between nonworking children and children working in light economic activity. There are, however, substantial differences in income and school attendance between nonworking children and child laborers[36].

In panel (2), I divide working children by hazard exposure during work. The results show that as income increases, other factors held constant, children are less likely to work regardless of the level of hazard at work. The reduction in the probability of hazardous work, however, is more than twice as much as the reduction in the probability of nonhazardous work (0.016 percentage points versus 0.006 percentage points). As can be seen, the results from the work intensity and the hazard exposure dimensions are very consistent due to the overlapping between them. Along similar lines, results in panel (3) show that as incomes increase, children are less likely to enter the labor market at young ages. The reduction in the probability of starting work as a child under 12 is almost twice as large as the reduction in the probability of starting work as an adolescent.

[36]This intuition is also true for other work dimensions. That is, there are small differences in school attendance between nonworking children and children who work in family businesses and in jobs not highly physical, jobs nonhazardous, and jobs during school breaks. In contrast, there are considerable differences in school attendance between nonworking children and children who work in market work, jobs that are physical, hazardous jobs, and full-year jobs.

The results in panels (4) and (5) classify working children based on work pattern and type of employer, respectively. As can be seen from panel (4), there are no significant differences between the poor and the rich in the probability of engaging their children in work during their summer school break. The rich, however, are eight times less likely than the poor to engage their children in full-year jobs. The last panel shows the disaggregation of working children by type of employer. The results in panel (5) show that the rich are one and one-half times less likely to engage their children into a market work relative to family work[37].

[37] As discussed earlier, there is some overlap between the type of employer and the age at first job dimensions. That is, children who started to work as kids mostly work for their families, while children who started work as adolescents mostly participate in the formal labor market. This could justify the smaller differential effects for these two dimensions compared to the other dimensions. Controlling for the type of employer in panel (3) could increase the differential effect of income in the favor of work started after age 12. Similarly, controlling for the difference in ages at first job in panel (5) could increase the differential effect of income in the favor of family work.

Table 22: Bivariate Probit Model: AME of Household Monthly Income

	Independent Probit		Bivariate Probit	
Sample	**Work**	**School**	**Work**	**School**
All Working children (N=54,994)	-0.018***	0.008***	-0.018***	0.009***
S.E.	(0.003)	(0.003)	(0.003)	(0.003)
P-Value	[0.000]	[0.000]	[0.000]	[0.000]
1) Work intensity				
Light economic activity (n=49,687)	-0.003*	0.005**	-0.003*	0.005**
S.E.	(0.002)	(0.002)	(0.002)	(0.002)
P-Value	[0.082]	[0.011]	[0.082]	[0.011]
Child laborers (ILO) (n=53,751)	-0.018***	0.007***	-0.018***	0.008***
S.E.	(0.003)	(0.003)	(0.003)	(0.003)
P-Value	[0.000]	[0.002]	[0.000]	[0.002]
2) Hazard exposure				
Nonhazardous work (n=50,581)	-0.006***	0.006***	-0.006***	0.006***
S.E.	(0.002)	(0.002)	(0.002)	(0.002)
P-Value	[0.005]	[0.008]	[0.005]	[0.008]
Hazardous work (n=52,857)	-0.016***	0.007***	-0.016***	0.008***
S.E.	(0.003)	(0.003)	(0.003)	(0.003)
P-Value	[0.000]	[0.001]	[0.000]	[0.001]
3) Age at first Job				
Teens 12-17 (n=51,141)	-0.008***	0.006**	-0.008***	0.006***
S.E.	(0.002)	(0.002)	(0.002)	(0.002)
P-Value	[0.000]	[0.008]	[0.000]	[0.008]
Kids under 12 (n=52,297)	-0.014***	0.007***	-0.014***	0.007***
S.E.	(0.003)	(0.002)	(0.003)	(0.002)
P-Value	[0.000]	[0.002]	[0.000]	[0.002]
4) Work pattern				
Summer work (n=48,933)	-0.001	0.004**	-0.001	0.004**
S.E.	(0.002)	(0.002)	(0.002)	(0.002)
P-Value	[0.369]	[0.041]	[0.369]	[0.041]
Work all the year (n=51,597)	-0.008***	0.007***	-0.008***	0.007***
S.E.	(0.002)	(0.002)	(0.002)	(0.002)
P-Value	[0.000]	[0.003]	[0.000]	[0.003]

Table 22: Bivariate Probit Model: AME of Household Monthly Income (cont'd)

	Independent Probit		Bivariate Probit	
Sample	**Work**	**School**	**Work**	**School**
5) Type of employer				
Unpaid Family work (n=52,486)	-0.008**	0.004*	-0.008**	0.005**
S.E.	(0.003)	(0.002)	(0.003)	(0.002)
P-Value	[0.011]	[0.027]	[0.011]	[0.027]
Market work (n=50,952)	-0.012***	0.009***	-0.012***	0.009***
S.E.	(0.002)	(0.003)	(0.002)	(0.003)
P-Value	[0.000]	[0.001]	[0.000]	[0.001]

In all regressions I control for child gender, child age, an urban dummy, a dummy for single-parent household, family size, education of the head of the household, a dummy for whether the head of the household was a child laborer, a dummy if a child's mother works, a dummy for whether a household owns livestock, and size of agricultural land. I account for the complexity of survey design through prefixing the Stata estimation commands with "svy". Standard errors are shown in parentheses. *** refers to the 99 percent confidence level, ** refers to the 95 percent confidence level, and * refers to the 90 percent confidence level.

6.3. Results from the Instrumental Variable Analysis

This section presents the results of the instrumental variable analysis using the two-stage residual inclusion approach introduced by Terza et al. (2008). In particular, I create an indicator variable equal to one if a household faced a shock in income over the past 12 months due to a death of a working member or illness/serious accident, and zero otherwise. As explained in section 4, I also create a wealth index and add it to the set of control variables to account for the differences between the poor and the rich in the probabilities of death and sickness. The results of the first stage and second regressions are shown in Tables 23 and 24, respectively. The first two columns in each table present the results without controlling for the wealth index; whereas, the last two columns control for the wealth index. As can be seen, controlling for wealth slightly reduces the magnitude and the significance of the coefficients but does not change the main results. The discussion in this section is focused on the findings of the IV regressions controlling for the wealth index to account for potential selection into illness, serious accidents, or death.

Table 23 shows that, holding other things constant, a household with a working member who was sick, had a serious accident, or died last year experienced 14 percent reduction in its monthly income in comparison to a household who did not experience this shock. Controlling for wealth reduces this estimate from 15.6 percent to 14 percent. Both estimates are statistically significant at the 99 percent confidence level.

Table 23: First Stage Regression: the Effect of a Recent Shock on Household Income

	Without controlling for wealth	**Controlling for wealth**
Estimate	-0.156***	-0.140***
S.E.	(0.022)	(0.022)
P-value	[0.000]	[0.000]
R-squared	0.1459	0.177
No. of observations	54,994	54,994

I control for child gender, child age, an urban dummy, family size, education of the head of the household, a dummy for whether the head of the household was a child laborer, the age of the head of the household, a dummy if a child's mother works, a dummy for single-parent household, a dummy for whether a household owns livestock, size of agricultural land, the age of the head of the household, and the highest four quintiles of the wealth index. Standard errors are shown in parentheses *** refers to the 99 percent confidence level, ** refers to the 95 percent confidence level, and * refers to the 90 percent confidence level.

The results of the bivariate Probit regressions using the instrumental variable are shown in Table 24. As can be seen from the aggregate analysis of all working children, each one percent increase in household monthly income reduces the probability of child's work by 0.101 percentage points, which is more than five times bigger than the estimate (0.018 percent) obtained from the baseline bivariate Probit model in Table 22. This estimate is statistically significant at 99 percent confidence level. The subpopulation analysis is presented in panel (1) through panel (4). I focus the analysis in this section on four work dimensions: work intensity, hazard exposure, work pattern, and the type of employer. As discussed before, the age at first job dimension is excluded from this analysis because children had entered the labor market before the occurrence of the income shock and therefore, the exogenous variation is not appropriate to predict the variations in this outcome.

Disaggregating the population of working children in panels 1 through 4 shows that work intensity and hazard exposure are the most significant dimensions where the effect of income differs. In particular, panel (1) shows that each one percent increase in income reduces the probability that a child is engaged in a heavy workload (Child labor –ILO) by 0.094 percentage points, which is statistically significant at 95 percent confidence level. The increase in income has an insignificant effect on the probability that a child is engaged in a light economic activity. In particular, the effect of income on child labor-ILO is almost seven times as large as the effect on light economic activity. Panel (2) shows the results of the subpopulation analysis by hazard exposure. The results show that each one percent increase in income reduces the probability that a child is engaged in a hazardous work by 0.104 percentage points, which is again statistically significant at 95 percent confidence level. The increase in income, however, has a small and insignificant effect on the probability of engaging a child in nonhazardous work.

It is worth mentioning that the effect of income on the probability of school attendance is statistically insignificant in the regression of all working children and for all subpopulations regressions. One potential explanation might be related to the nature of the instrumental variable that is being used to create the exogenous variation in household income. In particular, I use recent accidents (illness, serious accidents, or death) occurred to working members in households as the source of exogenous variation in household monthly income. Given the universal primary school enrollment in Egypt, parents are less likely not to enroll their children in school or withdraw them out of the school in a response to a recent income shock that might be temporarily. That is, school attendance is nonresponsive to income shocks in the short run. Another explanation is that the effect of income on school attendance might have opposite effects for subpopulations of children (males versus females; urban versus rural), that are canceled out at the aggregate level. To investigate this explanation and to further explore the heterogeneous effects of income on child's work, the next sections present the results of separate analyses by child's gender and region.

Table 24: Bivariate Probit Model (with IV): AME of Household Monthly Income

	Without controlling for wealth		Controlling for wealth	
Sample	**Work**	**School**	**Work**	**School**
All Working children (N=54,994)	-0.105***	0.025	-0.101**	0.008
S.E.	(0.036)	(0.029)	(0.040)	(0.033)
P-value	[0.004]	[0.382]	[0.011]	[0.818]
1) Work intensity				
Light work (n=49,687)	-0.014	0.010	-0.014	-0.005
S.E.	(0.019)	(0.024)	(0.022)	(0.027)
P-value	[0.459]	[0.662]	[0.530]	[0.854]
Child laborers (ILO) (n=53,751)	-0.101***	0.023	-0.094**	0.030
S.E.	(0.031)	(0.029)	(0.036)	(0.037)
P-value	[0.001]	[0.427]	[0.010]	[0.417]
2) Hazard exposure				
Nonhazardous work (n=50,581)	-0.008	0.005	-0.004	-0.012
S.E.	(0.023)	(0.025)	(0.027)	(0.028)
P-value	[0.728]	[0.854]	[0.873]	[0.673]
Hazardous work (n=52,857)	-0.103***	0.030	-0.104***	0.014
S.E.	(0.029)	(0.028)	(0.034)	(0.031)
P-value	[0.000]	[0.282]	[0.002]	[0.657]
3) Work pattern				
Summer work (n=48,933)	0.009	0.009	0.012	-0.005
S.E.	(0.012)	(0.023)	(0.014)	(0.025)
P-value	[0.450]	[0.699]	[0.383]	[0.832]
Work all the year (n=51,597)	0.008	0.019	0.017	-0.001
S.E.	(0.027)	(0.028)	(0.031)	(0.031)
P-value	[0.765]	[0.496]	[0.583]	[0.981]

Table 24: Bivariate Probit Model (with IV): AME of Household Monthly Income (cont'd)

	Without controlling for wealth		Controlling for wealth	
Sample	**Work**	**School**	**Work**	**School**
4) Type of employer				
Unpaid Family work (n=52,486)	-0.065**	0.037	-0.066*	0.027
S.E.	(0.029)	(0.025)	(0.034)	(0.029)
P-value	[0.028]	[0.137]	[0.050]	[0.353]
Market work (n=50,952)	-0.044*	0.001	-0.040	-0.020
S.E.	(0.022)	(0.026)	(0.025)	(0.030)
P-value	[0.044]	[0.974]	[0.108]	[0.506]

This model is estimated using the two-stage residual inclusion approach introduced by Terza et al. (2008) (see the text). I control for child gender, child age, an urban dummy, family size, education of the head of the household, a dummy for whether the head of the household was a child laborer, the age of the head of the household, a dummy if a child's mother works, a dummy for single-parent household, a dummy for whether a household owns livestock, size of agricultural land, the age of the head of the household, the highest four quintiles of the wealth index, and the first-stage residuals from Eq.9. Standard errors are shown in parentheses. *** refers to the 99 percent confidence level, ** refers to the 95 percent confidence level, and * refers to the 90 percent confidence level.

6.4. Exploring Gender Differences

This section presents the results of the instrumental variable analysis by gender. Table 48 in Appendix C shows the result of this analysis. The results of this analysis show that the increase in income decreases the probability of child's work for both males and females. The reduction, however, is higher for males than females. This can be explained by the fact that the percentage of child's work among males is more than three times as large the percentage of female child workers (17.7 percent versus 5.8 percent). Thus, the increase in income is expected to be more pronounced and influential in the population of male child workers compared to female child workers. Despite the bigger magnitudes, the effect of income on child's work is less statistically significant for males compared to females. This might be because the population of male child workers is larger than the population of female child workers, and so, the variations in parental income is expected to be larger in the male equation. As can be seen from the results, the standard error of the AME of income in the male equation is 0.082; whereas, the standard error in the female equation is 0.05, despite the fact the number of male child worker is more than three times the number of female child's workers.

The disaggregation of child workers by work dimensions, for both males and females confirms the same finding of Table 24 above. That is, both work intensity (child labor-ILO versus light activity) and hazard exposure (hazardous versus nonhazardous work) are the most two significant criteria where parental income has heterogonous effects. As can be seen, for male equation, the reduction in the probability of child labor-ILO is more than sixteen times as large as the reduction in the probability of light economic activity; and the reduction in the probability of hazardous work is more than five times as large as the reduction in the probability of nonhazardous work. The same pattern also holds in case of female equation.

Examining the effect of income on the probability of school attendance shows that the changes in income have insignificant effects for both male and female children. The estimated effects for the female equation are, however, negative, whereas, the estimated effects for the male equation are mostly positive. This might explain the insignificant effects of income on school attendance in the aggregate equation, as the opposite signs might cancel each other at the aggregate level. The opposite signs of these estimates might indicate that parents are more likely to respond to an income shock by withdrawing male children from school to help in the labor market but they might be reluctant to withdraw their young girls due to the lack of suitable jobs for girls. Parents in this case might also compensate for the reduction in total household education by educating more girls. The effects of income on school attendance in both male and female equations are, however, small and not statistically significant.

6.5. Exploring Regional Differences

This section presents the results of the instrumental variable analysis by region. The results are shown in Table 49 in Appendix C. As can be seen, the increase in income reduces the probability of child's work in both urban and rural areas. The magnitude of the reduction is, however, higher in rural areas than urban areas (-0.119 versus -0.080). Similar to the argument in the previous section, the percentage of child workers in rural areas is higher than urban areas (17.3 percent versus 6.7 percent), and thus, the effect of income is expected to be more influential in rural areas. Despite the large magnitude, the estimated effect in rural areas is not however statistically significant compared to the estimated effect in urban areas. Similar also to the argument in the previous section, the insignificant results might be driven by the fact that the large population of working children in rural areas exhibits large variations in household income compared to the urban areas. As can be seen, the standard error of the AME of the income effect in the rural equation is 0.083 compared to 0.035 in urban areas, despite the fact that the number of working children is larger in rural areas.

The results of the disaggregation analysis in panel (1) through panel (4) are also consistent with the findings in the previous sections. In particular, both work intensity and hazard exposure are the most important dimensions where the effect of income differs. In rural areas, the reduction in the probability of child labor-ILO as income increases is 131 times as the large as the reduction in the probability of light economic activity; and the reduction in the probability of hazardous work is more than six times as large as the reduction in the probability of nonhazardous work. The similar relative relationships also hold in urban areas with smaller magnitudes.

It is worth mentioning that the effect of household income on the probability of school attendance is negative in rural areas while it is positive in urban areas. The magnitudes are quite larger in rural areas but they are not statistically significant compared to the urban areas due to larger standard errors. These findings indicate that the increase in income significantly increases the probability of school attendance in urban areas but it seems to reduce school attendance in rural areas. The latter effect is not however statistically significant.

7. Conclusion

In this chapter, I use data from the recently available 2010 Egypt Child Labor Survey to explore the heterogeneous effects of parental income on child labor. I disaggregate the population of working children based on several dimensions to differentiate between favorable versus unfavorable work conditions and analyze the effect of parental income on child labor within each group. The results from the bivariate Probit regressions show that as incomes increase, parents are less likely to send their children to work, other factors held constant. The magnitude of the income effect, however, varies substantially across subpopulations of working children. In particular, the effect of parental income is minimal among children with light workloads compared to those with excessive workloads, children who do not face hazards during work compared to those who work in hazardous conditions, children who entered the labor market as adolescents age 12-17 compared to children who entered the labor market as kids under 12, children who work only during school break compared to those who work full-year jobs, and children who work in unpaid family businesses compared to those who work in the formal labor market.

The results of the instrumental variable analysis show that both hazard exposure and the intensity of work are the most significant work dimensions where household income has heterogeneous impacts. In particular, the results show that almost all the reduction in child labor as income increases comes from the reduction in hazardous work and heavy workload. This finding applies for male and female working children in both urban and rural areas.

The implications from these results is threefold. First, higher family incomes clearly deter types of child labor most likely to be harmful to children. Second, among households with moderate or high incomes, child labor is less likely to be harmful and quite possibly helpful to children and their families. A third implication regards public policy. Prior studies examining the relationship between income and child labor raised concerns about the effectiveness of income support policies. The results in this chapter lessen such concerns. Programs that increase incomes among very poor households who engage their children in the worst forms of child labor are highly likely to improve child well-being.

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Appendix A: Additional Tables and Graphs for Chapter I

Table 25: Percentages of Internal Migration in Egypt in 1996 and 2006

	Total Sample	Males	Females
Egypt Population Census 1996			
% Rural-urban migration	1.6	1.4	1.7
% Urban-rural migration	2.5	2.1	2.8
Total Population	2,064,421	1,036,520	1,027,901
Egypt Population Census 2006			
% Rural-urban migration	2.8	2.7	3.0
% Urban-rural migration	1.2	1.1	1.3
Total Population	2,784,612	1,406,178	1,378,434

This table is computed by the authors using data from the Egypt Population, Housing, and Establishment Census 1996 and 2006. I restrict the age of individuals to the same age interval of the analysis: 22-49.

Table 26: Estimating the Discontinuities in Baseline Characteristics: RD Models

Variable	Urban=1	Muslim=1	Mother's years of education	Father's years of education
Discontinuity estimate	-0.004	0.001	-0.066	-0.693
Standard error	(0.013)	(0.007)	(0.38)	(0.584)
P-value	[0.736]	[0.869]	[0.862]	[0.235]
Sample mean	{0.447}	{0.949}	{2.189}	{4.247}
Local sample	26,681	20,808	2,006	1,365
Total sample	97,314	74,847	5,813	3,493

This table is estimated using seven waves of the DHS (1992-2014).

Table 27: The Effect of Female Education on Fertility: Controlling for Husband Education

Outcomes	**Local Linear**	**Local Poisson (for count dependent variables)**
(a) Discontinuity in education		
Estimate	-0.829	-
Standard error	0.137	-
P-value	0.000	-
Mean	6.245	-
Local Sample	26,673	-
(b) Effect of female education		
1) Number of children born		
Estimate	-0.08	-.116
Standard error	0.045	.047
P-value	0.077	0.014
Mean	3.5	3.5
Local sample	18,788	18,788
2) Age at birth (in months)		
Estimate	0.143	-
Standard error.	0.116	-
.1365757	0.217	-
Mean	267.154	-
Local sample	16,937	-
3) Ideal number of children		
Estimate	0.012	.012
Standard error	0.047	.051
P-value	0.795	0.815
Mean	3.027	3.027
Local sample	17,330	17,330

This table is estimated using the EDHS data (seven waves), women age 22-49. I estimate local regression models with a triangular kernel and a bandwidth of 60 months. Standard errors are clustered by primary sampling unit. In all the regressions, I control for husband education and age at the time of the survey for fertility outcomes except age at first birth.

Table 28: Average Years of Education of Women by Marital Status and Age: DHS Survey

Age groups	Never-married women	Childless ever-married women	Ever-married with children
22-26	10.40	8.74	6.45
27-31	8.94	7.99	6.67
32-36	7.13	6.51	5.82
37-41	5.92	5.51	5.18
41-49	4.10	4.73	4.11
Total Average	**8.46**	**7.48**	**5.59**
Total sample	**9,932**	**5,098**	**72,100**

This table is computed using the DHS survey (six rounds). I used both the ever-married questionnaire and the household member questionnaire. I restrict women age to 22-49, analogous to the analysis in the chapter.

Table 29: Average Years of Education of Women by Marital Status and Age: Census 2006

Education Level	Never-married Women	Ever-married Women
Total sample: age 22-49		
% No Education	22.91	51.42
% Secondary & below	44.74	37.8
% College & above	32.35	10.79
Total Population	171,880	1,204,915
Age: 22-26		
% No Education	17.18	39.08
% Secondary & below	47.91	49.74
% College & above	34.9	11.18
Total Population	118,726	255,343
Age: 27-31		
% No Education	27.13	42.42
% Secondary & below	41.44	44.61
% College & above	31.43	12.97
Total Population	31,519	253,747
Age: 32-36		
% No Education	40.44	50.55
% Secondary & below	36.78	38.92
% College & above	22.78	10.54
Total Population	10,959	223,537
Age: 37-41		
% No Education	52.78	60.14
% Secondary & below	31.2	30.78
% College & above	16.01	9.08
Total Population	5,932	213,237
Age: 41-49		
% No Education	59.21	65.18
% Secondary & below	23.54	24.8
% College & above	17.25	10.02
Total Population	5,178	277,841

This table is computed using the Egypt Population, Housing, and Establishment Census 2006.I restrict women age to 22-49, analogous to the analysis in this chapter.

Figure 16: 95% CI for the Estimated Discontinuity in Education and Fertility (Restricted Sample)

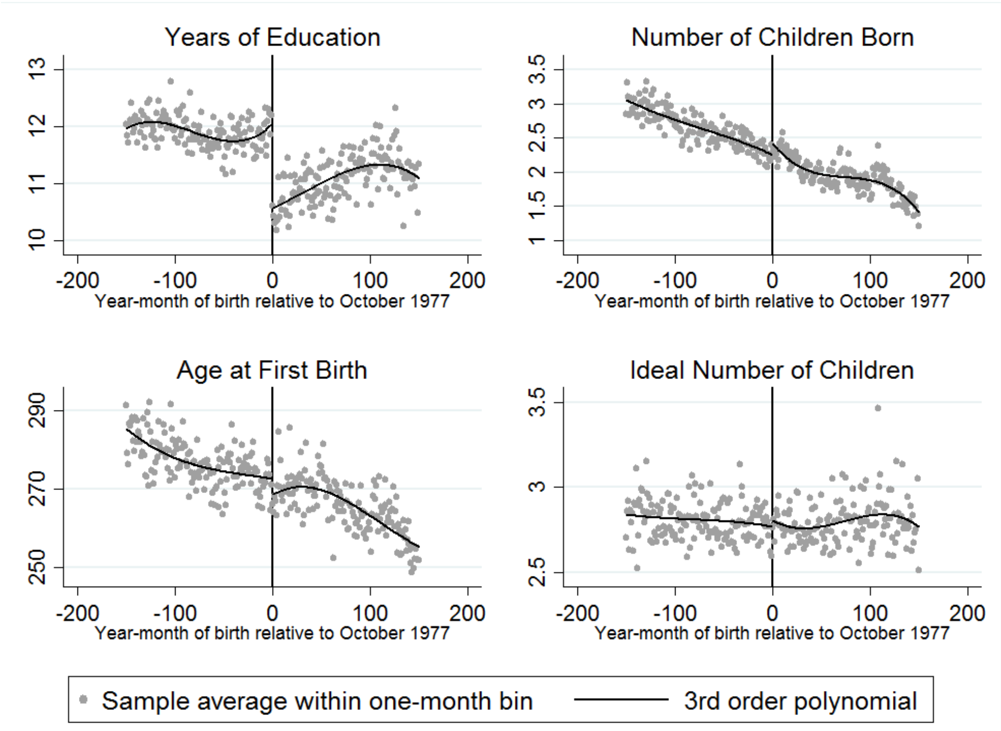


Figure 17: 95% CI for the Estimated Effect of Education on Fertility (Restricted Sample)

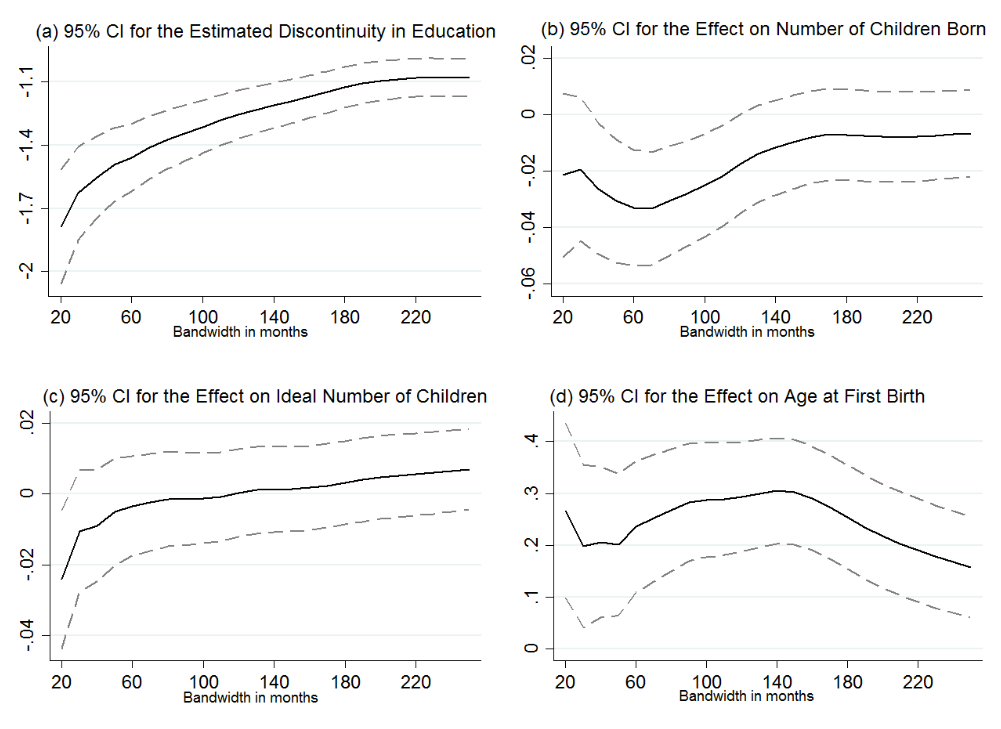
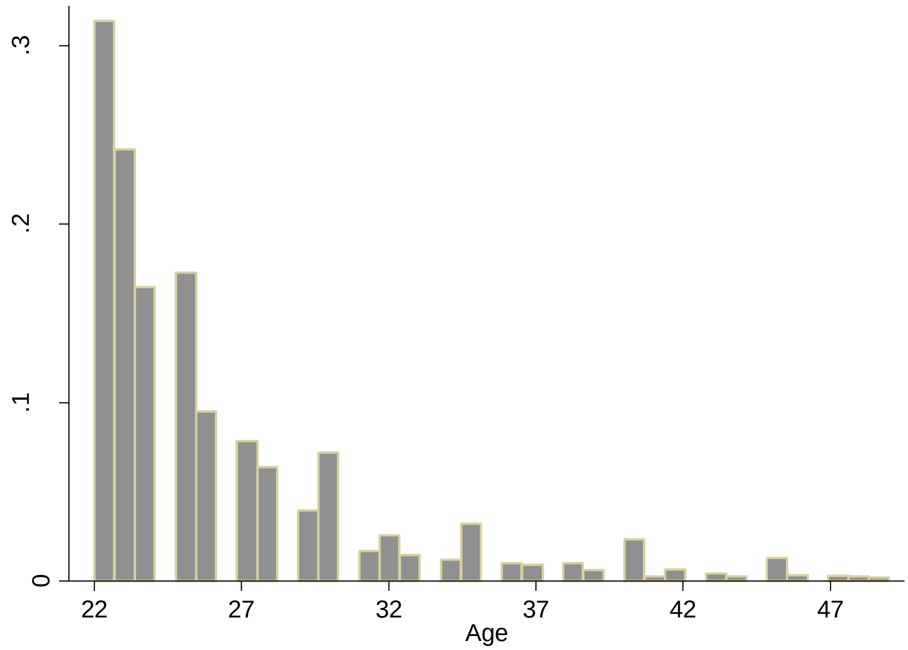


Figure 18: Age Distribution of Never-Married Women Age 22-49 (DHS Survey)



Appendix B: Additional Tables and Graphs for Chapter II

Table 30: The Effects of Parent Education on Child Malnutrition: RD Models

Variables	Local Linear
Exogenous variation in mother education	-0.796***
	(0.183)
Exogenous variation in father education	-1.601***
	(0.202)
(a) Probability of Stunting	
Effect of mother education	-0.003
	(0.024)
Effect of husband education	-0.001
	(0.023)
(b) Probability of Underweight	
Effect of mother education	-0.002
	(0.013)
Effect of husband education	0.007
	(0.013)
(c) Probability of Overweight	
Effect of mother education	0.009
	(0.019)
Effect of husband education	-0.011
	(0.018)
Observations (local sample)	21,947

In all regressions, I use a 60-month bandwidth and a triangle weighting function. I also control for child's age, child gender, a binary variable for urban, set of binary variables for region of residence (upper rural, upper urban, lower rural, lower urban, urban governorates, and frontier governorates), and survey-fixed effects. *** refers to the 99 confidence level. ** refers to the 95 confidence level, and * refers to the 90 confidence level.

Table 31: The Effects of Parent Education on Child Mortality: RD Models

Variables	**Local Linear**
(a) Under-five-year mortality	
Exogenous variation in mother's education	-0.813***
	(0.175)
Exogenous variation in father's education	-1.493***
	(0.197)
Estimated effect of mother's education	0.004
	(0.012)
Estimated effect of father's education	-0.011
	(0.012)
Local sample	26,916
Total Sample	76,311
(b) Under-one-year mortality	
Exogenous variation in mother education	-0.830***
	(0.175)
Exogenous variation in father education	-1.506***
	(0.198)
Estimated effect of mother education	0.004
	(0.011)
Estimated effect of father education	-0.009
	(0.011)
Local Sample	26,611
Total Sample	72,129
(c) Under-one-month mortality	
Exogenous variation in mother education	-0.865***
	(0.178)
Exogenous variation in father education	-1.522***
	(0.197)
Estimated effect of mother education	0.010
	(0.010)
Estimated effect of father education	-0.014
	(0.010)
Local sample	25,593
Total Sample	62,231

In all these regressions, I control for child's year of birth, child gender, a binary variable for urban, set of binary variables for region of residence (upper rural, upper urban, lower rural, lower urban, urban governorates, and frontier governorates), and survey-fixed effects. *** refers to the 99 confidence level. ** refers to the 95

Appendix C: Additional Tables and Graphs for Chapter III

Table 32: Percentage of Child Laborers Facing Each Type of Hazardous Condition at Work

Types of hazardous work	**% Working children**
Exposed to - dust, fumes at work	44.68
Exposed to – exhaustion	34.56
Exposed to - bending for long time	29.61
Exposed to - extreme cold or heat at work	16.18
Exposed to - no bathroom available	13.93
Exposed to - chemicals (pesticides, glues, etc.)	12.88
Exposed to - loud noise or vibration at work	7.36
Exposed to - insufficient ventilation	5.8
Exposed to - dangerous tools (knives, etc.) at work	5.51
Exposed to - fire, gas, flames at work	3.68
Exposed to - work in water/lake/pond/river	2.99
Exposed to - other things	5.94
Observations	6,550

Table 33: The Distribution of Child Laborers by the Places to Carry Out their Work

Place to carry out child's work during past week	**Market Work**	**Family work**
Plantations/farm/garden	25.68	69.64
Factory/Atelier	24.28	1.98
Shop/kiosk/coffee house/restaurant/hotel	13.76	7.6
Construction sites	12	0.37
At (his/her) family dwelling	1.2	14.13
Different places (mobile)	7.38	1.53
Client's place	6.46	0.62
Formal office	3.83	0.82
Others	5.43	3.32
Observations	2,508	4,042

Table 34: Joint Distribution of Five Work Dimensions (columns percentages)

		Intensity %		Hazard level %		Age at job %		Type of employer %		Pattern %	
		heavy	**light**	**hazard**	**nonhazard**	**kid**	**teen**	**market**	**family**	**summer**	**annual**
Intensity	**Heavy**	-	-	100	41.8	87.3	72	79.7	81.8	78.1	81.8
	light	-	-	0	58.2	12.7	28	20.3	18.2	21.9	18.2
	Total	-	-	*100*	*100*	*100*	*100*	*100*	*100*	*100*	*100*
Hazard level	**hazard**	83.2	0	-	-	67.7	67	72.5	64.2	61.2	69.9
	nonhazard	16.9	100	-	-	32.3	33	27.5	35.8	38.9	30.1
	Total	*100*	*100*	-	-	*100*	*100*	*100*	*100*	*100*	*100*
Age at job	**kid**	63.4	39.3	59.1	58.3	-	-	38.6	71.4	61.4	53.2
	teen	36.6	60.7	40.9	41.7	-	-	61.4	28.6	38.7	46.8
	Total	*100*	*100*	*100*	*100*	-	-	*100*	*100*	*100*	*100*
Type of employer	**market**	37.7	40.9	41.2	32.2	25.1	57.1	-	-	33.5	45.2
	family	62.3	59.1	58.8	67.8	74.9	42.9	-	-	66.5	54.8
	Total	*100*	*100*	*100*	*100*	*100*	*100*	-	-	*100*	*100*
Pattern	**summer**	12.9	15.7	12	16.7	15.2	11.3	10.3	15.8	-	-
	annual	87.1	84.3	88.1	83.3	84.8	88.7	89.7	84.2	-	-
	Total	*100*	*100*	*100*	*100*	*100*	*100*	*100*	*100*	-	-

Table 35: Distribution of Households' Reasons to Decrease in Income

Reasons to Decrease in Income over the past 12 months	% of Children
Fall in income - loss of employment of any member	3.25
Fall in income - bankruptcy of a family business	1.73
Fall in income - illness or serious accident of a working member	7.10
Fall in income - death of a working member	0.63
Fall in income - abandonment by the household head	0.22
Fall in income - fire in house/business/property	0.22
Fall in income - criminal act by household member	0.05
Fall in income - land dispute	0.13
Fall in income - loss of cash support or in-kind assistance	0.24
Fall in income - fall in prices of products of the household business	1.04
Fall in income - loss of harvest	0.56
Fall in income - loss of livestock	1.69
Fall in income - other	1.11
Total Children	54,994

Table 36: Description of the Variables Included in the Wealth Index

Six indicator variables for the type of dwelling:

1. Apartment/flat
2. More than one apartment
3. Villa or house
4. Countryside house
5. Room or more in a house unit
6. Separate room or more

Six indicator variables for the ownership of dwelling:

1. Ordinary low rent
2. New rent
3. Furniture rent
4. Property
5. Donation
6. Gift

Five indicator variables for the size of dwelling:

1. less than 20 square meters
2. 20 to 39 square meters
3. 40 to 69 square meters
4. 70 to 99 square meters
5. 100 square meters or more

A continuous variable for the number of rooms per household member

Five indicators variables for the availability of kitchen: 1. Inside house and exclusive 2. Inside house and shared 3. Outside house and exclusive 4. Outside house and shared 5. Not available
Five indicators variables for the availability of bathroom: 1. Inside house and exclusive 2. Inside house and shared 3. Outside house and exclusive 4. Outside house and shared 5. Not available
Five indicators variables for the type of toilet: 1. Inside house and exclusive 2. Inside house and shared 3. Outside house and exclusive 4. Outside house and shared 5. Not available
Four indicator variables for the source of energy – cooking: 1. Nothing 2. Gas 3. Natural Gas 4. Electricity
Four indicator variables for the source of energy- heating: 1. Nothing 2. Gas 3. Electricity 4. other
Three indicator variables for the source of energy cooling: 1. Nothing 2. Gas 3. Electricity
Three indicator variables for the source of energy lightening: 1. Nothing 2. Gas 3. Electricity
Four indicator variables for the main source for drinking water: 1. Pipe-borne inside house 2. Pipe-borne outside house 3. Borne-hole tubewell 4. Other
Sixteen indicator variables for whether a household owns: 1. Air-condition 2. Fireplace 3. Heater

4. Automobile
5. Tractor
6. Motor bike
7. Tok Tok
8. Bicycle
9. VCD/DVD player
10. Standard washing machine
11. Oven
12. Dishwasher
13. Sewing machine
14. Satellite/cable TV
15. Mobile phone
16. Radio

Table 37: Bivariate Probit Model: AME of Household Income for all Child Workers

VARIABLES	Pr(Work=1)	Pr(School=1)
Ln(income)	-0.018***	0.009***
	(0.003)	(0.003)
Child is male = 1	0.115***	0.006*
	(0.004)	(0.003)
Child age	0.021***	-0.018***
	(0.001)	(0.001)
Urban = 1	-0.031***	0.011***
	(0.005)	(0.004)
HH size	0.008***	-0.009***
	(0.001)	(0.001)
Education of head	-0.006***	0.009***
	(0.000)	(0.000)
Head was a child laborer = 1	0.040***	-0.008**
	(0.005)	(0.004)
Mother work = 1	0.094***	-0.005
	(0.005)	(0.004)
Size of agriculture land	0.003***	0.000
	(0.001)	(0.001)
Own livestock = 1	0.060***	-0.001
	(0.006)	(0.004)
Observations	54,994	54,994

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 38: Bivariate Probit Model: AME of Household Income for Light Activity

VARIABLES	Pr(Light Work=1)	Pr(School=1)
Ln(income)	-0.003*	0.005**
	(0.002)	(0.002)
Child is male = 1	0.027***	0.023***
	(0.002)	(0.003)
Child age	0.011***	-0.011***
	(0.001)	(0.001)
Urban = 1	-0.004	0.010***
	(0.002)	(0.004)
HH size	0.001	-0.008***
	(0.001)	(0.001)
Education of head	-0.001***	0.006***
	(0.000)	(0.000)
Head was a child laborer = 1	0.009***	-0.004
	(0.002)	(0.003)
Mother work = 1	0.023***	0.001
	(0.002)	(0.003)
Size of agriculture land	0.000	0.000
	(0.000)	(0.000)
Own livestock = 1	0.019***	0.003
	(0.003)	(0.004)
Observations	49,687	49,687

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 39: Bivariate Probit Model: AME of Household Income for Child Labor

VARIABLES	Pr(Child Labor=1)	Pr(School=1)
Ln(income)	-0.018***	0.008***
	(0.003)	(0.003)
Child is male = 1	0.103***	0.007**
	(0.004)	(0.003)
Child age	0.015***	-0.016***
	(0.001)	(0.001)
Urban = 1	-0.031***	0.012***
	(0.005)	(0.004)
HH size	0.007***	-0.009***
	(0.001)	(0.001)
Education of head	-0.005***	0.009***
	(0.000)	(0.000)
Head was a child laborer = 1	0.036***	-0.008**

	(0.005)	(0.004)
Mother work = 1	0.082***	-0.005
	(0.005)	(0.004)
Size of agriculture land	0.002***	0.000
	(0.001)	(0.000)
Own livestock = 1	0.050***	-0.001
	(0.005)	(0.004)
Observations	53,751	53,751

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 40: Bivariate Probit Model: AME of Household Income for Nonhazardous Work

VARIABLES	Pr(Nonhazardous Work=1)	Pr(School=1)
Ln(income)	-0.006***	0.006***
	(0.002)	(0.002)
Child is male = 1	0.043***	0.021***
	(0.003)	(0.003)
Child age	0.008***	-0.011***
	(0.000)	(0.001)
Urban = 1	-0.006*	0.009**
	(0.004)	(0.004)
HH size	0.003***	-0.008***
	(0.001)	(0.001)
Education of head	-0.002***	0.007***
	(0.000)	(0.000)
Head was a child laborer = 1	0.018***	-0.005
	(0.003)	(0.003)
Mother work = 1	0.044***	0.001
	(0.003)	(0.003)
Size of agriculture land	0.001**	0.000
	(0.000)	(0.000)
Own livestock = 1	0.027***	0.004
	(0.004)	(0.004)
Observations	50,581	50,581

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 41: Bivariate Probit Model: AME of Household Income for Hazardous Work

VARIABLES	Pr(Hazardous Work=1)	Pr(School=1)
Ln(income)	-0.016***	0.008***
	(0.003)	(0.003)
Child is male = 1	0.093***	0.007**
	(0.003)	(0.003)
Child age	0.016***	-0.016***
	(0.001)	(0.001)
Urban = 1	-0.030***	0.013***
	(0.004)	(0.004)
HH size	0.006***	-0.009***
	(0.001)	(0.001)
Education of head	-0.005***	0.008***
	(0.000)	(0.000)
Head was a child laborer = 1	0.030***	-0.008**
	(0.005)	(0.004)
Mother work = 1	0.067***	-0.005
	(0.005)	(0.004)
Size of agriculture land	0.002***	0.000
	(0.001)	(0.000)
Own livestock = 1	0.044***	-0.001
	(0.005)	(0.004)
Observations	52,857	52,857

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 42: Bivariate Probit Model: AME of Household Income for Working as a Kid

VARIABLES	Pr(started to work as a kid=1)	Pr(School=1)
Ln(income)	-0.014***	0.007***
	(0.003)	(0.002)
Child is male = 1	0.075***	0.017***
	(0.003)	(0.003)
Child age	0.007***	-0.012***
	(0.000)	(0.001)
Urban = 1	-0.023***	0.012***
	(0.005)	(0.004)
HH size	0.006***	-0.008***
	(0.001)	(0.001)
Education of head	-0.004***	0.008***
	(0.000)	(0.000)
Head was a child laborer = 1	0.042***	-0.008**

	(0.005)	(0.004)
Mother work = 1	0.076***	-0.004
	(0.004)	(0.004)
Size of agriculture land	0.002***	0.000
	(0.000)	(0.000)
Own livestock = 1	0.050***	0.001
	(0.005)	(0.004)
Observations	52,297	52,297

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 43: Bivariate Probit Model: AME of Household Income for Working as a Teen

VARIABLES	Pr(started to work as a teen=1)	Pr(School=1)
Ln(income)	-0.014***	0.006**
	(0.003)	(0.002)
Child is male = 1	0.077***	0.017***
	(0.004)	(0.003)
Child age	0.007***	-0.012***
	(0.000)	(0.001)
Urban = 1	-0.025***	0.011***
	(0.005)	(0.004)
HH size	0.006***	-0.008***
	(0.001)	(0.001)
Education of head	-0.004***	0.007***
	(0.000)	(0.000)
Head was a child laborer = 1	0.047***	-0.008**
	(0.005)	(0.004)
Mother work = 1	0.074***	-0.004
	(0.004)	(0.004)
Size of agriculture land	0.002***	0.000
	(0.001)	(0.000)
Own livestock = 1	0.053***	0.003
	(0.005)	(0.004)
Observations	47,538	47,538

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 44: Bivariate Probit Model: AME of Household Income for Summer Work

VARIABLES	Pr(Summer Work=1)	Pr(School=1)
Ln(income)	-0.001	0.004**
	(0.002)	(0.002)
Child is male = 1	0.017***	0.027***
	(0.002)	(0.003)
Child age	0.002***	-0.009***
	(0.000)	(0.001)
Urban = 1	-0.002	0.010***
	(0.002)	(0.003)
HH size	0.000	-0.007***
	(0.001)	(0.001)
Education of head	-0.000*	0.006***
	(0.000)	(0.000)
Head was a child laborer = 1	0.005***	-0.003
	(0.002)	(0.003)
Mother work = 1	0.014***	0.004
	(0.002)	(0.003)
Size of agriculture land	0.000*	0.000
	(0.000)	(0.000)
Own livestock = 1	0.007***	0.005
	(0.002)	(0.003)
Observations	48,933	48,933

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 45: Bivariate Probit Model: AME of Household Income for Annual Work

VARIABLES	Pr(Annual Work=1)	Pr(School=1)
Ln(income)	-0.008***	0.007***
	(0.002)	(0.002)
Child is male = 1	0.073***	0.004
	(0.003)	(0.003)
Child age	0.015***	-0.016***
	(0.001)	(0.001)
Urban = 1	-0.017***	0.011***
	(0.004)	(0.004)
HH size	0.004***	-0.009***
	(0.001)	(0.001)
Education of head	-0.004***	0.008***
	(0.000)	(0.000)
Head was a child laborer = 1	0.023***	-0.009**

	(0.004)	(0.004)
Mother work = 1	0.040***	-0.005
	(0.004)	(0.004)
Size of agriculture land	0.001***	0.000
	(0.000)	(0.000)
Own livestock = 1	0.030***	-0.003
	(0.004)	(0.004)
Observations	51,597	51,597

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 46: Bivariate Probit Model: AME of Household Income for Family Work

VARIABLES	Pr(Family Work=1)	Pr(School=1)
Ln(income)	-0.008**	0.005**
	(0.003)	(0.002)
Child is male = 1	0.072***	0.025***
	(0.004)	(0.003)
Child age	0.011***	-0.012***
	(0.001)	(0.001)
Urban = 1	-0.030***	0.011***
	(0.005)	(0.004)
HH size	0.005***	-0.008***
	(0.001)	(0.001)
Education of head	-0.003***	0.007***
	(0.000)	(0.000)
Head was a child laborer = 1	0.036***	-0.006*
	(0.005)	(0.003)
Mother work = 1	0.087***	-0.002
	(0.005)	(0.003)
Size of agriculture land	0.002***	-0.000
	(0.001)	(0.000)
Own livestock = 1	0.066***	-0.001
	(0.005)	(0.004)
Observations	52,486	52,486

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 47: Bivariate Probit Model: AME of Household Income for Market Work

VARIABLES	Pr(Market Work=1)	Pr(School=1)
Ln(income)	-0.012***	0.009***
	(0.002)	(0.003)
Child is male = 1	0.064***	-0.000
	(0.002)	(0.003)
Child age	0.014***	-0.016***
	(0.001)	(0.001)
Urban = 1	-0.007**	0.011***
	(0.003)	(0.004)
HH size	0.004***	-0.009***
	(0.001)	(0.001)
Education of head	-0.004***	0.008***
	(0.000)	(0.000)
Head was a child laborer = 1	0.010***	-0.007*
	(0.003)	(0.004)
Mother work = 1	0.016***	-0.002
	(0.003)	(0.004)
Size of agriculture land	-0.001**	0.001*
	(0.000)	(0.001)
Own livestock = 1	0.003	0.004
	(0.003)	(0.004)
Observations	50,952	50,952

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 48: Bivariate Probit Model (with IV): AME of Household Income by Gender

	Male		**Female**	
Sample	**Work**	**School**	**Work**	**School**
All Working children	-0.133	0.038	-0.075**	-0.013
S.E.	(0.082)	(0.05)	(0.038)	(0.041)
P-value	[0.102]	[0.491]	[0.048]	[0.754]
Observations	28,273	28,273	26,721	26,721
1) Work intensity				
Light economic activity	-0.008	0.001	-0.023	-0.007
S.E.	(0.039)	(0.036)	(0.022)	(0.038)
P-value	[0.835]	[0.970]	[0.313]	[0.857]
Observations	24,141	24,141	25,546	25,546
Child laborers (ILO definition)	-0.133*	0.045	-0.057*	-0.021
S.E.	(0.073)	(0.053)	(0.031)	(0.040)
P-value	[0.070]	[0.395]	[0.064]	[0.610]

Observations	27,398	27,398	26,353	26,353
2) Hazard exposure				
Nonhazardous work	0.030	-0.001	-0.031	-0.008
S.E.	(0.052)	(0.039)	(0.029)	(0.038)
P-value	[0.555]	[0.799]	[0.285]	[0.829]
Observations	24,760	24,760	25,821	25,821
Hazardous work	-0.159**	0.055	-0.053*	-0.019
S.E.	(0.069)	(0.051)	(0.027)	(0.040)
P-value	[0.020]	[0.280]	[0.054]	[0.631]
Observations	26,779	26,779	26,078	26,078
3) Work pattern				
Summer work	0.005	0.009	n.a.*	n.a.
S.E.	(0.031)	(0.032)	n.a.	n.a.
P-value	[0.877]	[0.781]	n.a.	n.a.
Observations	23,686	23,686	25,247	25,247
Work all the year	0.008	0.028	0.022	-0.021
S.E.	(0.062)	(0.051)	(0.025)	(0.040)
P-value	[0.898]	[0.577]	[0.382]	[0.594]
Observations	25,826	25,826	25,771	25,771
4) Type of employer				
Unpaid Family work	-0.095	0.078*	-0.049	-0.018
S.E.	(0.065)	(0.0438)	(0.032)	(0.040)
P-value	[0.145]	[0.070]	[0.129]	[0.644]
Observations	26,101	26,101	26,385	26,385
Market work	-0.032	-0.031	-0.028	-0.006
S.E.	(0.051)	(0.048)	(0.017)	(0.039)
P-value	[0.527]	[0.522]	[0.103]	[0.881]
Observations	25,438	25,438	25,514	25,514

*Only 69 girls work during summer only, and hence the regression is not feasible. This model is estimated using the two-stage residual inclusion approach introduced by Terza et al. (2008) (see the text). I control for child age, an urban dummy, family size, education of the head of the household, a dummy for whether the head of the household was a child laborer, the age of the head of the household, a dummy if a child's mother works, a dummy for single-parent household, a dummy for whether a household owns livestock, size of agricultural land, the age of the head of the household, the highest four quintiles of the wealth index, and the first-stage residuals from Eq.9. Standard errors are shown in parentheses. *** refers to the 99 percent confidence level, ** refers to the 95 percent confidence level, and * refers to the 90 percent confidence level.

Table 49: Bivariate Probit Model (with IV): AME of Household Income by Region

Sample	Urban Work	Urban School	Rural Work	Rural School
All Working children	-0.074**	0.070**	-0.118	-0.070
S.E.	(0.033)	(0.032)	(0.081)	(0.069)
P-value	[0.026]	[0.026]	[0.146]	[0.311]
Observations	27,784	27,784	27,210	27,210
1) Work intensity				
Light economic activity	-0.018	0.068**	-0.004	-0.105*
S.E.	(0.017)	(0.027)	(0.049)	(0.059)
P-value	[0.286]	[0.012]	[0.928]	[0.075]
Observations	26,389	26,389	23,298	23,298
Child laborers (ILO definition)	-0.062**	0.071**	-0.119	-0.070
S.E.	(0.028)	(0.031)	(0.078)	(0.067)
P-value	[0.028]	[0.022]	[0.125]	[0.299]
Observations	27,331	27,331	26,420	26,420
2) Hazard exposure				
Nonhazardous work	-0.020	0.061**	0.020	-0.110*
S.E.	(0.024)	(0.029)	(0.060)	(0.059)
P-value	[0.425]	[0.031]	[0.737]	[0.061]
Observations	26,389	26,389	23,928	23,928
Hazardous work	-0.056**	0.079**	-0.145*	-0.065
S.E.	(0.023)	(0.030)	(0.075)	(0.067)
P-value	[0.017]	[0.010]	[0.053]	[0.329]
Observations	26,779	26,779	25,790	25,790
3) Work pattern				
Summer work	-0.014	0.065**	0.048	-0.102*
S.E.	(0.014)	(0.027)	(0.033)	(0.055)
P-value	[0.324]	[0.014]	[0.143]	[0.063]
Observations	26,103	26,103	22,830	22,830
Work all the year	0.011	0.064**	0.043	-0.087
S.E.	(0.023)	0.032)	(.069)	(0.067)
P-value	[0.614]	[0.046]	[0.529]	[0.190]
Observations	26,911	26,911	24,686	24,686

4) Type of employer				
Unpaid Family work	-0.041*	0.072**	-0.092	-0.027
S.E.	(0.022)	(0.028)	(0.076)	(0.063)
P-value	[0.064]	[0.010]	[0.222]	[0.665]
Observations	26,680	26,680	25,806	25,806
Market work	-0.036	0.066**	-0.046	-0.137**
S.E.	(0.024)	(0.032)	(0.056)	(0.066)
P-value	[0.134]	[0.038]	[0.408]	[0.038]
Observations	27,040	27,040	23,912	23,912

This model is estimated using the two-stage residual inclusion approach introduced by Terza et al. (2008) (see the text). I control for child gender, child age, family size, education of the head of the household, a dummy for whether the head of the household was a child laborer, the age of the head of the household, a dummy if a child's mother works, a dummy for single-parent household, a dummy for whether a household owns livestock, size of agricultural land, the age of the head of the household, the highest four quintiles of the wealth index, and the first-stage residuals from Eq.9. Standard errors are shown in parentheses. *** refers to the 99 percent confidence level, ** refers to the 95 percent confidence level, and * refers to the 90 percent confidence level.

VITA

Fatma Romeh Mohamed Ali was born in Cairo, Egypt on February 22, 1986. Fatma obtained a Bachelor of Arts degree in Economics in 2007 from the Faculty of Economics and Political Science at Cairo University. In 2010, Fatma traveled to the United States to pursue Master and Ph.D. degrees in Economics at the Andrew Young School of Policy Studies, Georgia State University. She finished her Master degree in 2013 and the Ph.D. degree in 2016.

Fatma is an applied microeconomist with research interests in Labor Economics, Development Economics, and Health Economics. Her dissertation entitled “Three Essays on Family and Labor Economics”, examines household decisions that affect the human capital of their children such as fertility, child health, and child labor. Fatma’s research attempts to understand the contextual settings of poor households that shape their incentives to invest in their children. She employs both econometric analysis and program evaluation methods such as regression discontinuity and difference-in-differences (or triple differences) to extract exogenous variations and estimate causal effects. The primary goal of her research is to contribute to a better understanding of the family environments of disadvantaged children and to generate insights that help improve the situation of vulnerable children all over the world. The first chapter of her dissertation, *The Impact of Female Education on Fertility: A Natural Experiment from Egypt*, is currently under review for consideration for publication.

Fatma also has an extensive experience in the classroom and has invested heavily in improving her teaching skills. After serving as an outstanding teaching assistant for graduate Econometrics classes for four years, Fatma taught as a sole instructor at Georgia State University for five semesters from Spring 2015 until Summer 2016. She taught Principles of Microeconomics (Summer 2015 and Summer 2016) and Global Economy (Spring 2015, Fall 2015, Spring 2016, and Summer 2016). Fatma is a very effective and passionate teacher and was rated above the departmental course average on student evaluations.

Fatma has received several awards from Cairo University and Andrew Young School of Policy Studies. In particular, she received the Undergraduate Academic Excellence Award from Cairo University, Egypt, in 2007. She has also received several awards from Andrew Young School of Policy Studies such as the Quantitative Economic Award, Dan Sweat Dissertation Fellowship, Carole Keels Endowed Scholarship, Theodore C. Boyden Excellence in Teaching Economics Award, and the Certificate of Excellence in College Teaching.