ABSTRACT

Title of dissertation: SHOCKS AND HUMAN CAPITAL
IN DEVELOPING COUNTRIES

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I begin my dissertation by introducing the following two chapters. I start out by describing the basic theory underlying economists' historical interest in the effects of shocks in developing countries. I then briefly review the empirical literature on household responses to shocks and outline how it is reasonable to expect that even recurring, non-exotic shocks have substantial permanent effects on affected young peoples' completed human capital stocks. Next, I describe the contributions that the following two chapters of this dissertation make and how they are similar with respect to their use of nationally representative household survey data, their policy relevance, and the way they use basic economic theory and contextual knowledge to uncover meaningful impacts of shocks on different groups of young people. Finally, I conclude this introductory chapter by considering whether or not it should be regarded as a surprise that shocks in developing countries can be allowed by households to impact affected children's human capital stocks the way they do, despite the large returns associated with human capital investments.

The second chapter considers the permanent effects of rainfall shocks on adults'

completed human capital stocks. Existing research suggests that health and education investments in children are affected by aggregate income shocks, but there is considerably less evidence on what the effects of many years' worth of such shocks are on individuals' completed human capital stocks. I contribute to this literature by studying the effects of the number and timing of all the unusually wet and dry years over the first 21 years of rural West African individuals' lives on their likelihood of being literate and their completed educational attainment. I use historical rainfall data merged with nationally representative household surveys conducted in 12 countries, and I find that both wet and dry years have substantial negative impacts on women's human capital and smaller positive impacts on men's: The average effect of wet and dry rainfall shocks over the first 21 years of life is a 22 percent decrease in females' likelihood of being literate and a 10 percent increase in males'. I argue that this pattern of results is consistent with existing research on how West African females and males work and acquire human capital.

The third chapter provides evidence on how children in different types of households are affected by food price shocks. While people in poor countries report that inflation is one of their primary concerns, there is not much evidence on how they are actually affected by it. In particular, it is not clear how food (along with other) price inflation affects individuals in net food producing and net food consuming households. I use four highly comparable household surveys collected in Egypt to examine how children's weights-for-height evolved over time and in the face of the food price crisis of 2008, and I utilize data on region of residence and parents' occupation to examine how changes over time differed by household net food consumption sta-

tus. I find that despite several years' worth of solid economic growth in the run-up to 2008, most region- and parental occupation-based groups of Egyptian children's weights-for-height had not increased at all since 2005 (for example). Quantile regression results reveal that the lightest children in 2008 were considerably lighter than the lightest children in 2005, and also that children in those households most likely to have been net food producing seem to have been more negatively affected by high food prices than children in most other kinds of households. High food prices seem to have offset any possible beneficial effects of growth for children in the poorest households.

SHOCKS AND HUMAN CAPITAL
IN DEVELOPING COUNTRIES

by

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Dissertation submitted to the Faculty of the Graduate School of the
University of Maryland, College Park in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
2014

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Dedication

I dedicate this dissertation to Grace Lopez.

Acknowledgments

First and foremost I'd like to thank my advisor, Professor Judy Hellerstein. Any advisor in economics will continually push their students to think more carefully about their work. This is much easier for a student to deal with when the advisor is cool.

I would also like to thank my other committee members for all of the guidance and feedback they've provided me. I am proud to have been trained to understand every detail of my work and to endlessly try to get things right, and of course interactions with economists are a crucial part of the training I've received. In particular, I would like to thank Raymond Guiteras, Jessica Goldberg, Roger Betancourt, Peter Murrell, Seth Sanders and Chris McKelvey for helping me with various aspects of my work. Thank you also to Matthew Neidell for all of the opportunities he has given me. Finally, thank you to The Economist for giving me a sense of what economics was and how powerful a set of tools it is, as well as to my dad for recommending that I pick up a copy.

I would like to thank my fellow graduate students who also became my friends: Abby Alpert, Paul Bailey, Juan Bonilla-Angel, Randy Chugh, Carolina Gonzalez-Velosa, Amy Knaup and Gabriel Lara-Ibarra. You guys are all great and I miss you.

Thank you to my family, who are of course by far the greatest contributors to anything I ever have or will achieve in my life. Thanks also to Grace Lopez, you're the best.

Finally, I would like to thank the big homie Matthew Avedon. Actually, shouts out to everyone who was at St. Vincent's Hospital in New York City the day Matt was born there. Check's in the mail buddy, I'll holler.

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List of Abbreviations

GDP	Gross Domestic Product
ICRISAT	International Crops Research Institute for the Semi-Arid Tropics
OLS	Ordinary Least Squares

Chapter 1: Introduction

1.1 Shocks and Basic Economic Theory

Aggregate shocks in developing countries have been shown to result in a wide variety of effects and responses. This thesis will present evidence of hitherto unknown or underappreciated effects of shocks, but the motivation for the associated empirical analyses of these additional effects is based on consideration of what is already known about shocks. A natural starting point when thinking about the consequences of shocks is to consider their contemporaneous effects on utility. One of the most commonly made and intuitively appealing assumptions in economics is that utility functions are concave. In other words, marginal utility is a decreasing function of the quantity consumed. A direct implication of this assumption is that individuals would prefer quantities consumed to be highly similar across time periods. When marginal utility decreases with consumption, the sum of low consumption period utilities and high consumption period utilities will be less than the sum of period utilities with smooth consumption.

Under this assumption, we might expect that the contemporaneous utility effects of shocks are particularly severe in developing countries. A characteristic feature of developing economies is that certain markets are either incomplete or missing. In particular, formal markets for credit or insurance are typically assumed to be lacking or absent; the sorts of information and record-keeping necessary for these kinds of markets to function has historically simply not been present in developing country settings. More specifically, Besley (1995) cites the specification and enforcement of contracts, legal uncertainty, and inadequate communication-related technology as binding constraints preventing the functioning of proper credit and insurance markets. In the absence of informal responses such as the sales of valuable assets or borrowing from other households, then, individuals and households in these settings would therefore be vulnerable to the sorts of shock-driven intertemporal variation in quantities consumed described above. Also, although missing credit or insurance markets are potentially large parts of the reason why shocks might have serious consequences in developing countries, there are other features of developing economies that likely imply substantial impacts of shocks. For example, almost by definition, opportunities to earn money outside of farming in rural areas of developing countries will typically be limited. This of course implies that chances to compensate for a disappointing harvest via increases in off-farm labor supply will also be limited. Furthermore, the low baseline consumption levels of people in developing countries coupled with the assumption of decreasing marginal utility will mean that the welfare consequences of a given shock will be felt more dearly as well. That is, when marginal utility is high, small changes to consumption quantities will have larger impacts.

How large are the contemporaneous utility effects of shocks likely to be in practice? Despite the absence of formal credit or insurance, empirical evidence shows that household consumption in developing countries is at least partially protected from the effects of shocks. Townsend (1995) finds that while most of the rural Indian households studied using the ICRISAT dataset are effectively insured against idiosyncratic shocks, about one-sixth of them are not. The quality of insurance against idiosyncratic shocks has also been found to vary by shock type, with Gertler and Gruber (2002) and Fafchamps and Lund (2003) finding that a key household member's sickness is less well-insured than is their job loss or death. Also, Lim and Townsend (1998) find that the poor are less well-insured than are the rich. Aggregate shocks have also been found to result in substantial fluctuations in quantities consumed (Frankenburg et al., 2003), even if consumption does not very closely track aggregate income in some cases (Paxson, 1992). In sum, the general conclusion is that consumption in developing country settings is only partly smooth in the face of shocks, which implies in turn that contemporaneous utility will tend to be negatively affected to some extent.

But unsurprisingly in developing country settings, where markets are sometimes poorly functioning and the institutions and responses that pick up the slack are many and varied, contemporaneous utility is not the only outcome affected by shocks. Indeed, poor households are believed to engage in costly "income smoothing", or the ex ante avoidance of profitable but risky income-generating activities (Morduch, 1995). It has been shown, for example, that the riskiness of various agricultural production activities prevents poor households from engaging in those activities, even when expected returns are higher (Rosenzweig and Binswanger, 1993). Bardhan (1983) argues that "tied labor" arrangements, whereby employees receive low but steady wages for long periods of time, can be understood as a rational response to a risky environment. Fink et al. (2014) find evidence that apparently sub-optimally large allocations of off-farm labor are driven by the absence of seasonal savings and credit.

Again, responses to risk and shocks in developing countries have been found to be numerous and creative. Rosenzweig and Wolpin (1993) find evidence that agricultural households in India rely on "buffer stocks" in the form of bullocks to cope with production shocks (although subsequent research by Fafchamps, Udry and Czukas (1998) and Lim and Townsend (1998) failed to find evidence of this kind of behavior). There is also a wide variety of substitutes for formal credit and insurance, such as group lending programs where a number of loan recipients are jointly liable for repayment (Stiglitz, 1990), credit cooperatives that borrow as a collective and then distribute loan money to members (Banerjee, Besley and Guinnane, 1994), and rotating savings and credit associations that allocate group savings to members based on lottery or a bidding process (Besley, Coate and Loury, 1993). Udry (1994) studies informal credit arrangements between friends and family members in Nigeria, where lending and borrowing (and sometimes both simultaneously) are extremely common. Fafchamps and Lund (2003) study risk sharing in the Philippines and find that informal insurance networks largely consist of friends and relatives, while Grimard (1997) finds that ethnic groups constitute the mutual insurance network in Cote d'Ivoire. In addition to sales of jewelry, Frankenburg et al. (2003) find that Indonesian households adjusted their membership sizes and members increased labor supply in response to the economic crisis of 1998. Kochar (1999) also finds that

Indian farmers increase off-farm labor supply in response to harvest shocks. Rosenzweig and Stark (1989) finds that Indian daughters are married off from households in risky areas to households in areas where weather conditions typically differ, so that sending and receiving households each have better transfer sources. Jacoby and Skoufias (1997), Jensen (2000) and Bjorkman-Nyqvist (2013) each find that children's school attendance fluctuates with income. We have seen evidence that shocks in developing countries tend to result in both contemporaneous utility fluctuations as well as costly risk-avoiding productivity choices. Are there other types of costs associated with this wide variety of ways that households in developing countries have been found to respond to shocks?

1.2 Shocks and Investments in Children's Human Capital

Existing research suggests that the last coping strategy described above, whereby households temporarily pull children out of school in response to shocks, does not result in permanently lower human capital stocks for affected children (Jacoby and Skoufias, 1997; Funkhouser, 1999). On the other hand, de Janvry et al. (2006) hypothesize that if not being enrolled in school causes children to forget either the material they last learned or how to learn more generally, then re-enrollment will be sufficiently costly that some children will not do so. Turning back to the possibility that shocks could result in consumption fluctuations, then given that consumption of basic goods like food for young children has an investment component, it follows that this is another way in which shocks can affect investments in children. [1] More generally, investments in children's human capital requires resources just as consumption or other investments do, so it is natural to wonder whether or not they will be affected by income shocks. Also, human capital stocks are sometimes sufficiently low and marginal utility sufficiently high in developing country settings that it seems reasonable to wonder whether or not investments would be sacrificed for current consumption in response to shocks. In sum, it certainly seems reasonable to expect that the costs of shocks could include decreased investments in children's human capital.

The existing evidence makes clear that investments in children's human capital have potentially substantial permanent welfare consequences. Maccini and Yang (2009) find that women's adult height, schooling attainment, and a household durable good index are each positively affected by rainfall in the first year of life, and argue that this is because rural incomes increase with rainfall. In a striking illustration of the value of parental time inputs, Gertler et al. (2013) find that one-hour weekly visits by health workers to the homes of stunted Jamaican children that encouraged mothers to develop their children's cognitive and personality skills resulted in 42 percent increases in those children's earnings 20 years later. Child nutrition has also been found to have consequences for adult outcomes. Maluccio et al. (2009) find that a high-protein energy drink given to Guatemalan children under the age of 8 results in increases in cognitive ability amongst adults of 0.24

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[1] In principle, children's nutritional intake could be protected even if shocks decrease household consumption. It is therefore an empirical question as to whether or not investments are affected.

standard deviations. Hoddinott et al. (2008) study the same intervention and find an increase in male wages of 46 percent as a result of nutritional supplementation in the first two years of life. The disease environment during childhood is also important: Bleakley (2010) estimates that 100 percent malaria eradication in several Latin American countries increased adult incomes by more than 40 percent. Moving onto the returns to education, Duflo (2001) uses a large school construction program in Indonesia to identify a return to education on men's wages of between 6.8 and 10.6 percent. [2] Foster and Rosenzweig (1996) find that better educated farmers profited more from the introduction of Green Revolution technologies than other farmers did. The benefits of education are also believed to persist across generations: Thomas, Strauss and Henriques (1991) and Glewwe (1999) each find that paternal education positively impacts child health, and Osili and Long (2008) find that an additional year of schooling decreased Nigerian women's number of births before age 25 by 0.26 births. Many of the results in these papers are generated from either plausibly exogenous or experimental variation in the key explanatory variables, and it is therefore clear that human capital investments can have large, extremely persistent effects.

1.3 Contribution

It seems clear that the study of the effects of shocks on affected individuals' human capital stocks is a worthwhile pursuit. This literature is incomplete with

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[2] On the other hand, Uwaifo Oyelere (2010) uses a similar identification strategy and finds a return of only 2.8 percent in Nigeria.

respect to both of the types of shocks that I study in the following two chapters of this dissertation. The main chapter (2) studies the effects of the number and timing of rainfall shocks experienced while young on adults' human capital stocks. As mentioned above, there does exist research on the effects of various kinds of aggregate shocks on the contemporaneous enrollment in school of affected children, and there is also research suggesting that these lapses in education investments are made up for later. [3] But there is less evidence on what the permanent effects of an entire childhood's worth of those kinds of shocks might be. The following chapter in this dissertation provides this kind of evidence by studying the effects of rainfall shocks experienced over the first 21 years of life on West African adults' literacy and highest grade completed at school. This chapter takes econometric identification issues seriously and provides policy-relevant evidence on how an objective measure of human capital is permanently affected by a type of shock that is common throughout the developing world.

The third and final chapter studies the food price crisis of 2008. While there exists a substantial amount of work on the impacts food price shocks have on outcomes like hunger and poverty, most of it is based on simulations; there is less evidence that uses household survey data and less still that relies on objective measures of well-being from survey data. Moreover, as far as I know, there is no research that uses the kind of anthropometric data I do to identify how different types of households, and in particular net food producing and consuming households, seem

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[3] In addition to the aforementioned papers by Jacoby and Skoufias (1997), Jensen (2000) and Bjorkman-Nyqvist (2013) on the effects of rainfall shocks on enrollment, Frankenburg et al. (2004), Rucci (2003) and Funkhouser (1999) found that enrollment declined following macroeconomic crises.

to fare in response to food price crises. This chapter cannot be considered evidence of the causal effects of high food prices on different households' well-being, due to the fact that the main analysis does not involve an estimation of how any group of children's weights-for-height would have evolved over time in the absence of high food prices (i.e., the counterfactual scenario). But the chapter seems to be unique in that it uses a relatively objective measure of children's well-being–their weight-for-height–to provide evidence of how food prices seem to have affected children as a function of their region of residence and parent's occupations. Moreover, it does this for children throughout the conditional weight-for-height distribution using quantile regression techniques.

Both of these chapters use nationally representative household survey data collected over multiple years to show that the shocks I study likely result in substantial welfare effects for certain individuals (and in the case of rainfall shocks, I show that these effects are permanent). The two chapters are also similar in how they deal with shocks that are common. Again, rainfall shocks matter in any location where peoples' livelihoods depend at least in part on rainfed agriculture. Food price inflation is probably similarly familiar to the individuals throughout the developing world who are affected by it. These shocks are also both aggregate shocks, and this distinction is worth noting because we would expect these types of shocks to be more difficult to cope with. To the extent that informal insurance networks are locally geographically concentrated, then since everyone in the network will have been hit with the shock, there is no one to provide any kind of insurance "pay-out" (Townsend, 1994). The insurance value of "buffer stock" savings is also likely particularly low following aggregate shocks, due to the increase in the number of households who would sell assets, which in turn would result in a ”fire sale” price (Lim and Townsend, 1994). By the same logic, we would expect off-farm wages to be particularly low following an aggregate shock, given the increase in labor supply. There are simply fewer available coping mechanisms to draw on following aggregate shocks.

The two chapters are also similar in that they use basic economic logic and contextual understanding to motivate analyses of how different groups of children might be affected by shocks. The second chapter considers the permanent effects of rainfall shocks experienced while young on the completed human capital stocks of women and men separately. One look at the differences in literacy rates and the average highest grade completed at school between the women's and men's samples makes it clear that investment processes differ substantially along gender lines. Men are more than three times as likely to be literate as women are, for example. Female and male children in West Africa allocate their time differently outside of school as well. Existing research shows that girls work inside and outside the household, whereas boys basically only work outside of it. The results I obtain show that the prioritization of education investments in boys is actually exacerbated by both wet and dry shocks, which each decrease completed human capital stocks for females but (if anything) increase them for males. The third chapter is more or less based on the idea that children in net food producing households are expected to have been affected by food price inflation differently than children in net food consuming households were. Since I do not actually observe net food production status, I use region of residence (which determines rural-urban status, as well as whether or not high-priced crops were being harvested in large quantities), parental occupation (e.g., self-employed farmer, agricultural employee or other), and finally a child's implied place in the conditional weight-for-height distribution, as proxies for net food producing household status. Existing research suggests that net food producing households should benefit from high food prices, but in the case of the 2008 food price crisis in Egypt I do not find evidence in support of this. In fact, along with the lightest (and, I assume, the most economically disadvantaged) children in most region and parental occupation-based groups, it was the group of children that I argue were most likely to have been members of net food producing households who seemed to have been the most negatively affected by food price inflation. (I argue that the most likely explanation for this is the high cost of fertilizer and other inputs.) In the case of both chapters, then, the allowance that the analyses make for these different groups of children possibly being affected differently ends up providing results that are somewhat surprising yet quite plausible.

Also, both chapters are policy-relevant, given the existing stock of research and policy concerns. In the case of the second chapter, by complementing existing research on how some households respond to shocks by pulling children out of school and providing evidence on the permanent effects of this coping strategy, this chapter speaks to outcomes that are at the heart of multiple Millennium Development Goals (MDGs). In particular, the second and third MDGs call for universal primary school enrollment and the promotion of gender equality, respectively, and the chapter clearly speaks to both of these issues. Also, as debates about how much development aid should be devoted to the management of the effects of climate change begin, evidence of the sort that I provide that ordinary weather variation can have substantial permanent consequences will be useful. The food price inflation chapter speaks to a topic that is likely to remain important for the medium term; FAO (2011) concludes that food prices will likely exhibit higher volatility and on average remain relatively high for the next several years at least. Both of the following chapters therefore have to do with policy issues that will continue to be important.

Finally, given the magnitude of the potential returns to investments in children's human capital, we might ask why parents ever sacrifice such investments (for smoother consumption, presumably). In particular, we might first ask whether parents are selfish rather than altruistic. [4] Existing economic research suggests that parents are indeed altruistic, at least with respect to children's time allocation to work and school. Manacorda (2006) uses state-specific child labor laws to identify who in the household benefits from child labor in early 20th century America. He finds no evidence of any relationship between the proportion of working children in a household and their parents' labor supply, and that the returns from child labor are redistributed to other children in the household in the form of less time working. Using data from Pakistan, Bhalotra (2004) finds that adult consumption and children's school participation covary positively, and concludes that the hypothesis

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[4] This is not meant to suggest that parental selfishness alone would imply that parents would forego human capital investments in their children for higher household income thanks to their labor. Given the widespread tendency in the developing world for elderly parents to depend on their children for support, it would seem to be in parents' self-interest to invest in their children's education in a way that could help the parents themselves later.

of altruistic parents is not rejected. We might expect, in turn, that if parents would prefer to send their children to school rather than work as long as they could afford to do so, then school attendance would be positively affected by household wealth and work would be negatively affected by it. In fact, there is a substantial amount of evidence in favor of these sorts of income effects (Edmonds and Schady, 2012; Attanasio et al., 2006; Edmonds, 2006; Filmer and Schady, 2008; Ravallion and Wodon, 1998; Schultz, 2004; Cogneau and Jedwab, 2012). [5]

It would seem, therefore, that parents would prefer to invest in their children's human capital when resources permit. Given the kinds of fluctuations in children's human capital stocks that shocks are known to cause, it seems reasonable to expect that it is the inability to smooth consumption perfectly in the face of shocks that drives these investment fluctuations. We might also note that under the assumption of concave utility functions, the marginal utility of consumption will be particularly high in poor places (especially following negative income shocks), which might help rationalize the way valuable investments are sometimes sacrificed for the sake of trying to maintain current consumption. Because as we will see in the following chapter, the costs that are incurred in the attempt to smooth can indeed by high, at least in the case of rural West African women who experienced higher numbers

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[5] The positive relationship between household wealth and children's school enrollment is not as straightforward as one might suspect. Basu, Das and Dutta (2010) find that child labor supply is low for children in landless rural Indian households, that it is high for children in households that own a moderate amount of land, and that it is once again low for children in households with very large amounts of land. This is consistent with their model, which shows that labor market imperfections will mean children in landless households do not work, while children in households with some but not a large amount of land will work (thanks to the fact that their land offers them tasks to perform). Even in this kind of situation, however, higher household wealth does eventually decrease the amount of work children do.

of rainfall shocks before the age of 21.

Chapter 2: Rainfall Shocks and Human Capital

2.1 Introduction

Risk is a central aspect of life in developing countries, given the widespread existence of incomplete formal insurance and credit markets. Perhaps the most common types of shocks for households in agricultural areas of the developing world are rainfall shocks, and given the extent to which informal insurance networks are typically geographically concentrated, households will likely not be insured against these shocks. The variability in income that rainfall shocks cause can in turn affect education and health investments in children, despite the potentially high returns to such investments. In fact, there is evidence that the kinds of rainfall shocks I will be analyzing have measurable effects on (contemporaneous) human capital investments for both school-aged as well as preschool-aged children. Investments in children are thought to be affected by rainfall shocks primarily through some set of competing income and substitution effects, the exact nature of which depend on the type of investment (and the age of the potential recipient) under consideration. However, as existing research suggests is the case, temporary investment shocks could be made up for later, and it is still mostly an open question as to what the long-term human capital costs of these rather commonplace shocks are. [1] This chapter attempts to provide an answer using rainfall shocks in West Africa.

More specifically, I study the literacy and highest grade completed of rural West African women and men who were at least 21 years old at the time of collection of the household surveys on which I rely, and I consider the effects on those outcomes of the numbers and timing of all of the unusually wet and dry years experienced over the first 21 years of life. (Years are considered wet or dry if the total amount of rainfall is sufficiently far above or below, respectively, the local annual average over the previous 25 years.) In other words, I consider how the human capital stocks of adults were affected by rainfall shocks that occurred before the age of 21. Given the differences in the ways West African females and males allocate their time, I study the effects of shocks on the completed human capital stocks of women and men separately. My main empirical analysis allows the effects of additional wet and dry years to vary depending on the age range in which they occurred and controls for (among other things) time-invariant, location-specific factors and (country-specific) temporal factors. Intuitively, my main analysis compares changes in human capital outcomes across birth cohorts across different locations (where rainfall patterns differed over the first 21 years of individuals' lives). Thus, here I provide the first estimates of the permanent effects of all of the annual rainfall shocks that people experienced over the course of their educational career (and before) on an objective measure of human capital (literacy). I do this by studying 39 cohorts worth of women and men using as many as 26 nationally representative household

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[1] See Section 2.2.2.1 below for a discussion of these existing findings.

surveys from 12 West African countries merged with (as far as I know) the most geographically disaggregated precipitation data available.

This chapter is related to the aforementioned literature on the contemporaneous investment effects of covariate shocks (including rainfall shocks), which is summarized by Ferreira and Schady (2008). One of the main conclusions of that paper is that in poorer places, education and health investments in school-aged and preschool-aged children, respectively, tend to be procyclical. I build on these results by considering the lasting effects of a whole childhood's worth of covariate shocks.

This project is also related to research dealing with the long-term consequences of shocks experienced at formative ages. For example, economists have shown that lower rainfall in the first year of life (Maccini and Yang, 2009), pandemics experienced while in utero (Almond, 2006), being one of a set of twins (Behrman and Rosenzweig, 2004; Royer, 2005), drought and civil war before the age of 3 (Alderman et al., 2006) and exposure to political violence (Leon, 2009) each lead to worse adult outcomes, including completed educational attainment. Shah and Millett Steinberg (2013) also study the permanent effects of rainfall shocks on Indian adolescents and adults, but their main long-term outcomes are the highest grade completed at school and adult wages rather than a direct measure of human capital. Most of these papers either focus on unusual shocks or on shocks at very particular ages only (or both). In contrast, I consider the effects of common weather shocks and how those effects differ over the entire period from infancy to young adulthood; this chapter is about a basic, recurring climatic feature as opposed to the kind of shock that only affects individuals at critical ages. Understanding the effects of rainfall shocks therefore allows us to estimate what affected adults' human capital stocks would be in the absence of a major source of aggregate productivity risk that (as we will see) affects individuals over the entire course of their educational career. The fact that the main outcome under study here is literacy also helps set this chapter apart. As Bleakley (2010) argues, it is possible for shocks to have particular effects on the highest grade completed and differently-signed effects on the kind of outcome we are oftentimes ultimately interested in, human capital, and so it is important to have a valid measure of the latter. Another contribution of my chapter is that it considers the effects of unusually rainy as well as dry seasons in West Africa. Wet shocks in particular are likely to become a topic of greater interest if the predictions of the Intergovernmental Panel on Climate Change turn out to be true and these shocks become more common in the region over the next several decades (Christensen et al., 2007).

My baseline results for females are that both additional wet and dry years decrease the likelihood of being literate as well as the highest grade completed (between birth and age 20, depending on the type of shock, for both outcomes). An extra wet (dry) year between age 7 and age 13 (birth and age 6) decreases the likelihood of being literate by 1.2 (0.6) percentage points, which are economically significant effects when considered alongside the women's sample literacy rate of about 17 percent. Also, an additional wet (dry) year between age 14 and age 20 lowers the highest grade completed by 0.120 (0.033), which are again substantial compared to the women's sample average highest grade completed of 1. Given the average numbers of shocks experienced during the different age intervals and the baseline estimates of the effects of shocks, the set of all shocks experienced over the first 21 years of life decrease women's likelihood of being literate by 22.1 percent, and their highest grade completed at school by 19.1 percent (on average). Note that these estimated effect sizes are seriously large, especially given the relatively small number of women in the sample who are literate or who completed even one year of school. For men, each additional wet (dry) year between the ages of 14 and 20 (birth and age 6) increases the likelihood of being literate by 1.5 (1.1) percentage points (which might be compared to the men's sample literacy rate of 44 percent). Also, an additional dry year between birth and age 6 increases the highest grade completed by 0.075 (with the sample mean for this outcome being 4.10). On average and as a whole, shocks increase men's likelihood of being literate by 9.9 percent and their highest grades completed by 5.6 percent. For women especially, shocks have large effects on completed human capital stocks.

As will be made clear, however, many of these baseline estimates could be affected by sample selection bias. In particular, under strongly unfavorable assumptions the negative effects of wet years on women's human capital could be either understated or overstated and the negative effects of dry years could be overstated, while the beneficial effects of both types of shocks on men's human capital stocks could be overstated. For example, under strong assumptions about how many females live long enough to enter the sample because of wet (dry) shocks between the ages of 7 and 13 (birth and age 6), each additional shock would decrease women's likelihood of being literate by only 0.007 (0.003) percentage points. The magnitudes of these selection-free estimates are about 42 and 50 percent smaller, respectively, than their baseline analogs. But even under highly unfavorable assumptions, there remains solid evidence that women's human capital stocks are substantially negatively affected by both wet and (to a lesser extent) dry years and that wet shocks increase men's human capital.

In an attempt to understand the channels through which preschool-aged shocks are having their effects, I consider the hypothesis that shocks affect children's health, which in turn later affects the kinds of education investments that parents make in their children. I test for evidence in favor of this hypothesis by examining how rainfall shocks affect individuals' height (using the same baseline methodology as described above), which I take to be a measure of early-life health conditions (Bozzoli et al., 2009) (and which I have data on for a subset of the women in my data only). I find no evidence that shocks between birth and age 6 affect women's height. On the other hand, I find that the negative (positive) effects of preschool-aged dry shocks on women's (men's) human capital are driven by shocks at ages 1-3 (2-4). While this is not necessarily evidence in favor of the health shocks explanation of preschool-aged shocks, it seems inconsistent with the alternative hypothesis that preschool-aged shocks persistently affect household income or access to productive resources, and therefore parents' ability to finance their children's education when those children are old enough to attend school. It is therefore not clear whether shocks at preschool ages are affecting individuals through a health channel or in another way.

I argue that this pattern of results is broadly consistent with existing evidence on how young, rural West Africans allocate their time in general and in response to rainfall shocks in particular. More specifically, the finding that additional wet years decrease females' human capital stocks can plausibly be explained by the stylized fact that both on- and off-farm economic activities are higher following the kinds of bumper harvests that wet years tend to result in. As for the negative effects of dry years on females' human capital, I argue that the most likely explanation for this finding is that parents are simply not willing to finance girls' school enrollment when household income is low (as it is following particularly dry rainy seasons). At the same time, given the facts that boys seem to work mostly in agriculture and that their parents seem more willing to finance their education in general, I argue that the low labor demand that dry years cause offers boys the chance to spend more time learning rather than working. Finally, wet years likely mean dominant income effects for boys, with parents taking the opportunity to invest in the human capital of their sons when income is higher.

Section 2.2 provides background on the agricultural labor cycle in West Africa as well as considers conceptual issues. Section 2.3 describes the empirical strategy, the data and the sample selection criteria. Section 2.4 presents descriptive statistics and results. Section 2.5 explores robustness and Section 2.6 concludes.

2.2 Background and Conceptual Issues

2.2.1 The Agricultural Labor Cycle in West Africa

As mentioned above, I study the effects of shocks in 12 West African countries. The region is sufficiently large that it naturally plays host to variation in rainfall conditions. For instance, the wet season (which lasts from either March or May until October or September, respectively) is generally shorter and annual rainfall levels typically lower at higher latitudes. [2] Also, the onset of the rainy season tends to be less predictable, and the distribution of precipitation over the course of the rainy season less uniform, in areas further north. There is also considerable variation in the types of crops that are grown across the region. For example, crops that require less rainfall to thrive and that tend to be less sensitive to dry shocks such as bulrush millet, cassava, groundnuts and sorghum are grown in areas to the north where rainfall levels are lower and more erratic. To the south, where rainfall levels are higher and dry shocks are less common, crops such as coffee, cocoa, yams and maize are grown.

In important ways, however, agricultural production methods and labor market conditions in general and over the course of the agricultural cycle do not tend to vary across the region. For example, agriculture is the main sector in which most individuals in West Africa work, with 60-80 percent of all people there being engaged in agricultural work of some sort. Also, rainfall is a critical input on the farm, as the vast majority of agriculture in the region is rain-fed (Hayward and Oguntoyinbo, 1987). Child labor is also common throughout the region, with labor force participation rates for African children between the ages of 5 and 14 of 41 percent (ILO, 1998). [3] Most child labor is believed to be family-controlled in the

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[2] Roughly speaking, Benin, Cote dIvoire, Ghana, Liberia, Nigeria, Sierra Leone and Togo have a longer rainy season (interrupted by a brief dry spell near the end of August) from March to October, while Burkina Faso, Guinea, Mali, Niger and Senegal have a shorter wet season from May to September.

[3] Although this result is based on data collected near the end of the time period that I am concerned with, Bhalotra (2003) notes that it likely reflects child labor supply conditions in previous decades, as Africa did not experience the kind of post-war secular decline in child labor that other regions did.

sense that children work in family-run enterprises (Andvig, 2001).

Also, agricultural labor demand is higher during the wet season throughout the region (Hayward and Oguntoyinbo, 1987; ICRISAT, 1980; Canagarajah and Skyt Nielsen, 1999). During the wet season, crops are planted, weeded, tended to, and eventually harvested (and it is important that each of these tasks be performed at very particular times). After the harvest is complete and crops have been prepared for consumption and sale, household food supplies and income levels-and through multiplier effects, off-farm economic activities such as petty trading-are at their highest (Mortimore, 1989; Thomas, 1997). Moreover, labor demand (both on and off the farm) and household incomes are higher still during and after those wet seasons where rainfall was plentiful. [4]

In contrast, the dry season is characterized by relatively slack on- and off-farm labor demand. Although tasks such as clearing farmland and preparing it for planting need to be done before the beginning of the wet season, the time frame within which these tasks have to be performed is not necessarily so tight that, for example, child workers cannot afford to wait and do them on the weekend (when school is not in) (Robson, 2004). The dry season is also when seasonal migration takes place, with men leaving rural areas in substantial numbers to earn money in urban areas since earning opportunities are scarce in rural areas. Moreover, seasonal migration is more common following those wet seasons where rainfall was particularly scarce (Mortimore, 1989). These stylized facts are consistent with the

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[4] See Jensen (2000), Webb and Reardon (1992), Sillah (2009), Bjorkman-Nygvist (2013), Bhalotra and Umana-Aponte (2010), Bengtsson (2010), Dercon (2004), Levine and Yang (2006) and Paxson (1992) for evidence of a positive relationship between rainfall and income. See Iliya (1999) and Reardon (1997) for evidence of increased labor demand following wet shocks.

ideas that agriculture largely drives economic activity in rural areas and that rural West Africans have a limited number of other sectors that they can work in.

2.2.2 Conceptual Framework and Existing Evidence

2.2.2.1 Shocks and School-Aged Children

I start out by considering how rainfall shocks affect contemporaneous investments in school-aged children's human capital and follow up by discussing how temporary investment shocks could have permanent effects. For school-aged children, it is expected that dry shocks (for example) will lead to income and substitution effects. On the one hand, a dry year is expected to decrease household income. This will increase the marginal utility of consumption of whatever goods a child's labor can buy, which pushes children to devote more time to working and less to attending school or learning. On the other hand, a dry year also means lower agricultural labor productivity and so lower demand for labor in a setting where most people work in agriculture. Thus, the opportunity cost of attending school or spending time studying is lower, which of course pushes children to devote more time to learning.

We might also consider the direct costs of attending school, which can be substantial in the countries I study. For instance, Jensen (2000) finds that in Cote dIvoire in 1986, the median cash outlay associated with sending children to school was about one-third the median household income per capita. We would expect higher school costs to reinforce the income effect of dry years: They should be more dear in terms of lost utility from foregone consumption during dry years compared to normal years. Of course, it is an empirical question as to which of these effects-substitution or income (augmented by school fees)-dominate in practice, i.e. this simple framework does not offer predictions as to whether a negative shock like a dry year will decrease or increase investment in young peoples' human capital.

In addition to dry years I will also be considering the effects of wet years, which will also (tend to) have competing income and substitution effects: For most households in what I have defined to be wet years, the marginal utility of consumption will be lower (thanks to a bountiful harvest and the higher household incomes that accompany it) which will draw children toward learning, but the demand for labor will also be higher which will draw them toward work. For those households whose farm plots are especially prone to flooding, however, wet years will have negative income effects (which, again, will encourage children to work). Also, to the extents that agricultural labor markets are poorly functioning or that children work more or less exclusively in household-run enterprises, wet years will have the same kinds of substitution effects as dry years for children from households negatively affected by floods in that demand for these children's labor will be low. [5] Once again, the basic theory does not offer a prediction as to how children's human capital should be affected.

Ferreira and Schady (2008) summarize and interpret the results of the existing literature on the effects of covariate shocks on (school-aged) children's contemporaneous human capital investment. They note that school enrollment tends to decline

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[5] There is evidence to suggest that labor was not commonly traded across farms in West Africa over the time period being studied here (Binswanger and McIntire, 1987; Oyekale, 2009; ICRISAT, 1980; Yaro, 2006; Reardon et al., 1988; Bhalotra, 2003; Fafchamps et al., 1998).

during or following negative covariate shocks in poorer countries such as Cote dIvoire (Jensen, 2000), Malawi (World Bank, 2007) and Indonesia (Thomas et al., 2007), but for the most part tends to rise during negative shocks in less-poor Latin American countries including Brazil (Duryea and Arends-Kuenning, 2003), Mexico (Binder, 1999; McKenzie, 2003) and Peru (Schady, 2005). [6] (It should also be noted that the depressed economic conditions in Cote d'Ivoire and Malawi were driven by rainfall deficits.) The authors argue that this pattern of results is consistent with the aforementioned basic theoretical framework, and that the reason why enrollment is mostly procyclical in poorer nations and mostly countercyclical in less-poor nations is because in the former-where marginal utilities of consumption are higher-income effects should be stronger and therefore more likely to dominate substitution effects. They also argue that people in poorer nations are more likely to be subject to the kinds of credit market imperfections that prevent households from being able to borrow for the sake of financing their children's schooling when incomes are temporarily low.

It is possible that these kinds of temporary shock-driven changes to investments do not lead to permanent effects. For example, if investments are easily substitutable over time then we would expect the effects of a temporary lapse to be undone by increased investment later. Indeed, Jacoby and Skoufias (1997), Funkhouser (1999) and Bjorkman-Nyqvist (2013) each suggest that enrollment lapses are made up for later, and that there are not substantial permanent human capital effects

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[6] Costa Rica, however, is a less-poor nation that does not fit this pattern. There, enrollment was found to have decreased during the 1981-1983 recession (Funkhouser, 1999).

associated with shocks. But it is not clear that getting non-enrolled children caught up would be so easy. De Janvry et al. (2006) hypothesize that re-enrolling in school following a period of non-enrollment might be especially costly for a few reasons: Children could learn to appreciate other aspects of life or lose some of their ability to study when not enrolled, and children might remember sufficiently little of whatever material they previously learned that enrolling becomes more trouble than it is worth. More generally, it seems reasonable to suppose that the cost of enrolling in school is decreasing in the amount of learning that has recently taken place, for reasons similar to those just given. For instance, learning less in one year could make an additional year in school particularly costly if children become less interested in learning relative to pursuing other things, or if new material simply becomes too hard to learn then. In these ways, temporary shocks could result in permanent effects on individuals' human capital stocks.

2.2.2.2 Shocks and Preschool-Aged Children

Rainfall shocks that occur during preschool ages can also affect individuals' completed human capital stocks. In particular, rainfall shocks can affect health investment in young children in ways that have long-term health effects, and if parents decide how much to invest later in the education of children as a function of how healthy those by-then school-aged children are (as in Maccini and Yang, 2009; Alderman et al., 2006; Alderman et al., 2001; and Hoddinott and Kinsey, 2001), then shocks at early ages can have permanent human capital effects. [7] More specifically, we might suppose that child health depends on: privately purchased health-promoting goods (e.g., food, clothing and medicine); parental time inputs (for things like preventive health care visits, breastfeeding and the collection of clean water); and the disease environment, which can be directly affected by rainfall (Wang et al., 2009). Of course, rainfall shocks can affect all of these factors. The household's ability to purchase health-promoting goods would be diminished following a dry year, for instance, while the amount of time parents devote to maintaining children's health will depend on whether parents supply more or less labor during dry years. In other words, parental time inputs will be subject to the same kinds of income and substitution effects that school-aged children's labor supply is, and so it is in turn an empirical question as to whether preschool-aged childrens health is helped or harmed by dry years. So once again, the theory does not offer predictions as to how a covariate shock might affect contemporaneous investments in children's health.

The evidence on the effects of negative covariate shocks on young children's health investments is once again summarized and interpreted in Ferreira and Schady (2008). The authors argue that health outcomes are more likely to worsen during bad economic times in poorer countries, where the effect of the marginal dollar (not) spent on health-promoting private goods is more likely be large enough to overwhelm the effect of any possible increase in the amount of time that parents devote to chil-

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[7] Of course, shocks that occur while individuals are still in utero can also have substantial permanent effects (Almond, 2006). In results not shown, however, I find no evidence that shocks in the 12 months preceding birth affect either of the human capital outcomes I focus on here, for either females or males.

dren's health. And once again, the results of the empirical literature are consistent with the authors' argument: Negative aggregate income shocks lead to worse health outcomes in poor countries such as Cote dIvoire (Jensen, 2000), Zimbabwe (Alderman et al., 2006), Ethiopia (Yamano et al., 2005), Tanzania (Alderman et al., 2009) and Cameroon (Pongou et al., 2006), while children's health improved in developed countries such as the US (Chay and Greenstone, 2003; Dehejia and Lleras-Muney, 2004) during downturns.

Any possible persistent health shocks that rainfall shocks cause could later result in either compensatory or reinforcing education investment decisions on the part of parents. In particular, Rosenzweig and Zhang (2009) find evidence of reinforcing education investments in response to health shocks, while Liu et al. (2009) and Bharadwag et al. (2010) find evidence of compensating education investment behavior. It should also be kept in mind that rainfall shocks during preschool ages could affect later education investments in ways that have nothing to do with children's health. For example, rainfall shocks have been shown to have persistent effects on rural households' consumption , which implies that shocks early on could affect the timing of initial enrollment in school if the direct costs of attending school are substantial. [8] It is also possible that income shocks at preschool ages affect eventual education investments through their persistent effects on households' access to agricultural inputs and capital. For example, it is believed that one of the ways farmers attempt to smooth consumption following an agricultural output

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[8] Dercon (2004) finds that 10 percent lower rainfall 4-5 years before decreases current consumption growth by 1 percentage point for a sample of rural Ethiopian households.

shock is by selling livestock (Fafchamps et al., 1998; Mortimore, 1989). Thus, if dry years are simultaneously responsible for persistently low incomes and decreased livestock numbers, it is reasonable to expect that those numbers will remain low for a sustained period following a shock. [9] Since it is typically the responsibility of children (and in particular boys) to care for livestock (Bolwig and Paarup-Laursen, 1999; Robson, 2004), rainfall shocks at preschool ages could affect children's labor supply even after those children are old enough to attend school. It could also be that rainfall shocks affect households' abilities to purchase agricultural inputs such as fertilizer years into the future, which, to the extent that such inputs and child labor are complementary, would also affect the supply of the latter. Also, it is possible that individuals' educational careers could either start by a particular age or not at all, perhaps because formal learning is more difficult when it begins at more advanced ages. Or it could be that children who do not enroll as soon as possible become sufficiently productive outside of school that the opportunity cost of enrolling is prohibitively high by the time households do have the resources to finance those children's enrollment. These possibilities imply that not enrolling early on because of rainfall shocks could have permanent consequences.

Thus, we have reason to believe that temporary rainfall shocks at any age over the course of individuals' educational careers (and before) could end up affecting those individuals' completed human capital stocks. Also, while the papers described above are important in that they show that temporary shocks-including

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[9] In addition to sales-driven decreases in livestock counts, livestock also perish following dry rainy seasons due to lack of pasture (Mortimore, 1989).

rainfall shocks-can have contemporaneous human capital investment effects, we may ultimately be more concerned with the long-term effects of these types of shocks. I now turn to the econometric strategy that I will use to estimate these permanent effects.

2.3 Empirical Strategy, Data and Sample

2.3.1 Empirical Strategy

I use the following specification to try and learn about how additional wet and dry years at different ages affect human capital outcomes:

$$H_{ipt} = \alpha + \sum_{j=1}^{3} \beta_j \#wetyearsj_{pt} + \sum_{j=1}^{3} \rho_j \#dryyearsj_{pt} + \delta_p + \theta_{ct} + \lambda_r(t) + X'_{ipt}\Gamma + \varepsilon_{ipt}. \tag{2.1}$$

Here H_{ipt} is the human capital outcome (literacy or highest grade completed) of individual i from rainfall neighborhood (i.e., place) p born in year t. [10] The $\#wetyearsj_{pt}$ and $\#dryyearsj_{pt}$ variables are equal to the numbers of wet and dry years, respectively, that individuals from place p and born in year t experienced during the jth age interval, $j \in \{1, 2, 3\}$. These three different age intervals are as follows: the birth year until the age of 6, age 7 to age 13 and age 14 to age 20. These intervals correspond to the preschool period, a period when individuals' attachment to school might be relatively strong, and a time period when those attachments

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[10] The term rainfall neighborhood refers to the fact that individuals are assigned rainfall shocks data based on which geographic location I have historical rainfall data for that they live closest to. Thus, all individuals who live closer to a given one of these locations than any other are said to live in that location's rainfall neighborhood.

are likely weaker. [11] The coefficients associated with these shocks variables are meant to tell us what the effects of experiencing an additional shock during each age interval are.

We might also expect, for example, that places where rainfall shocks are more common are either better or worse equipped to deal with those shocks. In this case, if we ignore these sorts of unobservable time-invariant, location-specific factors and simply regress a literacy indicator on the shocks variables, the resulting estimates of the wet and dry years effects could be biased. To deal with this kind of concern, I include rainfall neighborhood fixed effects (denoted by δ_p) in my specification. Also, to identify wet and dry years effects separately from time series effects, I include country-specific birth year fixed effects (θ_{ct}) in my specification as well. In an attempt to deal with the possibility that distinct changes in literacy tended to happen at a sub-national level in places that became more or less dry over time, I also include controls for region-specific linear time trends ($\lambda_r(t)$) (where each individual in my samples lives in one of 55 regions). The X vector includes full sets of dummy variables in my specification corresponding to individuals religion (Muslim and Christian, with 'other' being the omitted category) and ethnicity (of which there are 71 in all). Standard errors are clustered at the rainfall neighborhood level.

Regressions are unweighted, which, under the assumption that the baseline model is correctly specified, will lead to consistent estimates of the effects of rainfall shocks on human capital (Deaton, 1997). To test whether or not this assumption

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[11] The qualitative features of my baseline results are robust to changes in the choice of age intervals (results available upon request).

holds, I would have to augment the sample weights that the DHS provides in multiple ways, since these only work to make respondents for each survey representative at the rural level (for the year the survey was conducted only). I pool together surveys from multiple countries and years, however, and so in order for my sample to be representative of the rural population of the 12 West African countries that I study as a whole, I would (at least) need to multiply the weights that the DHS provides by the inverse of the rural sampling rate (again, for each survey).

Only after doing this would individuals in my sample be assigned weights such that, for example, individuals in smaller countries would not count for too much or too little relative to individuals in larger countries. Since I am not aware of any available data on rural populations between the ages of 21 and 49 across years, however, I use data from the World Population Prospects online database on the total populations in that age range for each country I study. [12] These data are only available for every year that is a multiple of 5, and I therefore assume constant rates of population growth in the intervening years to obtain estimates of populations sizes then. I then calculate sampling rates for every country- and year-specific household survey I use data from, and multiply the within-country weights that the DHS data contains by the inverse of these sampling rates. The hope is that these steps give weights that can be used to make my sample roughly representative of the rural population of the countries I study as a whole. (Indeed, it is these augmented weights that I use to calculate the descriptive statistics in Table 2.2.)

It should be noted, however, that given the possibility of unobserved poten-

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[12] Available at http://esa.un.org/wpp/Excel-Data/population.htm.

tially non-random migration, then even having exact data on rural sampling rates would not be sufficient to generate the kinds of weights that would make my sample of non-migrants representative of the entire cohorts to which they belong (since these cohorts include migrants as well as non-migrants). In other words, given the likelihood that rural migrants are different in unobservable ways from rural non-migrants, it is not at all clear what other kinds of adjustments would need to be made to the weights that the DHS provides–in addition to incorporating sampling rate differences across surveys–for the weighted sample of rural non-migrants to be truly representative of rural individuals as a whole. Again, the hope is that the sample weights I apply do a reasonably good job of making my sample more or less representative of the rural population of the 12 countries I study.

In any case, the augmented weights in question are used to test whether or not the baseline results are sensitive to the use of sample weights. The results are qualitatively similar if quantitatively different, and in particular sometimes substantially larger in magnitude (as well as available upon request). For example, both wet and dry shocks still negatively affect women's human capital (although during fewer age intervals), but the magnitudes of the effects are between 100 and 157 percent larger relative to the baseline results. For men, results are unchanged except that wet shocks increase the likelihood of being literate between the ages of 7 and 13 (but not between 14 and 20).

The consistency of my estimates of the wet and dry years effects rests on the assumption that the rainfall neighborhood-specific deviations in numbers of shocks around the long-term neighborhood average, after controlling for birth year effects and region-specific time trends, are independent from idiosyncratic errors. In other words, I assume that the variation left over in the wet and dry years variables after controlling for time-invariant rainfall neighborhood characteristics, cohort effects and regional trends is orthogonal to unobserved determinants of individuals human capital outcomes. For example, if rainfall neighborhoods which became more dry over time also simultaneously played host to residents with lower human capital stocks (for reasons that had nothing to do with rainfall shocks), then the estimated effects of wet and dry years would be biased downwards. On the other hand, if those places that became more dry over time would have played host to the same human capital trends (had they not actually become more dry) as places that did not become more dry, then these comparison neighborhoods would serve as good counterfactuals for the treated neighborhoods, and the resulting wet and dry years estimates would be unbiased.

2.3.2 Data

The household surveys I rely on are Demographic and Health Surveys (DHS) collected in Benin, Burkina Faso, Cote dIvoire, Ghana, Guinea, Liberia, Mali, Niger, Nigeria, Senegal, Sierra Leone and Togo between 1988 and 2008 (see Table 2.1 for the complete list of surveys used). These surveys are nationally representative (at rural and urban levels) and they contain information on a wide assortment of socioeconomic and demographic variables, including respondents' literacy, highest grade completed, year of birth, religion, ethnicity, and whether or not respondents have ever migrated. I append the surveys with the aim of obtaining enough power to identify the effects of shocks separately from local, time-invariant factors and time series effects (and because, as described earlier, in many ways the circumstances under which individuals' human capital stocks were formed were broadly similar across countries). In sampled households each woman between the age of 15 and 49 was selected to be interviewed, while some fraction of the men between the ages of 15 and 59 were selected (with that fraction varying by survey), so that my women's sample is substantially larger than my men's sample. [13]

The human capital outcomes I consider include an indicator equal to 1 if an individual is literate (and zero otherwise) and a variable equal to the individual's highest grade completed. For surveys collected before 2000, individuals literacy status is self-reported. In particular, individuals were asked if they could read and understand a letter or a newspaper with ease, with difficulty or not at all. I consider individuals in these pre-2000 surveys to be literate if they reported at least being able to read with difficulty. For surveys collected in and after 2000, individuals were instead asked to try and read a sentence printed on a card while survey workers decided whether individuals could read the whole sentence, only parts of the sentence or none of the sentence. [14] I consider individuals in these surveys literate if they could read at least parts of the sentence. As mentioned previously, I merge the pre-2000 and post-1999 surveys together for my main analyses as if the data used to construct the literacy variable was collected the same way in both

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[13] Those women and men who either usually lived in the household or were visitors who slept there the previous night were eligible for interview (ICF Macro, 2011).

[14] Literacy data is missing for 0.32 percent of women and 1.49 percent of men as a result of interviewers not having sentences printed in respondents spoken language.

periods, but I should note that the estimated effects of the key coefficients in my main specifications-those corresponding to the rainfall shocks covariates-are robust to the inclusion of a post-1999 survey year fixed effect. [15] The highest grade completed variable is defined to be just what it sounds like: the highest grade that an individual finished (regardless of whether or not they attempted but failed to complete a higher grade).

I study how these human capital outcomes vary as a function of historical rainfall shocks. To measure these, I rely on the availability (in the DHS data) of the longitude and latitude of the geographic center of the primary sampling unit in which respondents live, which is normally the respondent's village in rural areas. Knowing relatively precisely where individuals reside allows me to assign to them historical rainfall data that likely does a good job of characterizing the rainfall shocks conditions that they actually experienced while growing up. The rainfall data I use comes from Wilmott and Matsuura's Gridded Monthly Rainfall Time Series, version 2.01. [16] These climate researchers constructed their data set by using historical rainfall data for each month from January, 1900 until December, 2008 from actual rainfall measurement stations to predict rainfall levels (again, for each month over the same time period) at each of 900-plus geographic locations (grid points) within the 12 countries I study. They did this for each grid point by using data from (on average) the nearest 20 rainfall measurement stations. The grid points are 0.5 degrees apart from one another in both the east-west and north-south directions,

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[15] Note that this fixed effect is added to account for survey-specific factors, as opposed to cohort-specific factors (which the country-specific fixed effects already control for).

[16] Available at http://climate.geog.udel.edu/ climate/.

which works out to about 35 miles on average. Thus, for all the respondents in my data the nearest rainfall data set grid point is at most about 25 miles away.

This gridded rainfall data was constructed using climatologically aided as well as more traditional interpolation techniques to estimate monthly total precipitation quantities. The actual station data (for the West African region) that the interpolation scheme relies on comes from the Global HIstorical Climatology Network (GHCN) and Professor Sharon Nicholson's archive of African precipitation data. The number of monthly rainfall totals that underlie the gridded data varies by year, with there being between at least 190 and 385 stations for the period and region I study (only the GHCN data is publicly available so that its stations can be counted). This quantity of stations implies that on average, no point was any more than between about 71 and 50 miles from a station, respectively. [17]

Finally, I will define wet and dry years. For each rainfall neighborhood, for each year between 1940 and 1998, I compute the annual average level of rainfall over the previous 25 years, and similarly for the standard deviation. A year is considered wet if the level of rainfall in that year is at least 1 standard deviation above the local mean level over the past 25 years, and a year is considered dry if rainfall that year is at least 1 standard deviation below that level. The 1 standard deviation threshold, which roughly translates into a 44 percent difference relative to the mean rainfall level on average, allows for enough variation in the numbers of shocks that the effects of those shocks can be identified but not so much variation that rainfall

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[17] With this in mind, and given that 99 percent of the geographic coordinates of sample clusters in the DHS data are no more than about 3 miles from the sample's actual location, it seems highly unlikely that this offset is responsible for a quantitatively important proportion of however much measurement error exists.

shocks no longer warrant the name.

The 1 standard deviation threshold follows the examples of Jensen (2000) and Grimard and Hamilton (1999), who both study the effects of rainfall shocks in West Africa. [18] It should be kept in mind, however, that the qualitative features of the main results are robust to changes in the definition of what constitutes a shock. In particular, results are mostly qualitatively unchanged for shock thresholds between 0.8 and 1.3 standard deviations of local, recent rainfall (and are available upon request).

2.3.3 Sample Selection

The quality of the rainfall data that I use depends positively on how geographically close the actual rainfall measurement stations underlying it were to the grid points: A large number of rainfall measurement stations nearby makes it more likely that predicted monthly rainfall levels at a given location will be close to the true levels. The rainfall stations supporting the data set were relatively numerous until the late 1990s. Thus, since my main specification will consider the effects of

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[18] In both of these studies, however, the areas being studied were no larger than single countries, whereas I study an entire region spanning multiple agroecological zones. Given this, it should be noted that the baseline results are robust to an alternative shock definition that is inspired by the Standardized Precipitation Index (SPI). The SPI fits historical rainfall data to a chosen distribution, and the parameters associated with the fitted distribution are used to rescale rainfall realizations so that they are in terms of the standard normal distribution (Edwards and McKee, 1997)). Shocks might then be defined to be rainfall levels that are below the 16th percentile of the historical rainfall distribution or above the 84th percentile–thresholds that roughly correspond to the points in the distribution that are 1 standard deviation below or above the historical mean, respectively. In the case of the alternative definition that I use, wet (dry) shocks are simply defined to be rainfall amounts that are more than (roughly) the 84th percentile (less than the 16th percentile) of the local rainfall distribution over the previous 25 years. This definition allows for the possibility of assymetrically distributed rainfall in a way that the simple 1 standard deviation threshold that the baseline results rely on does not, and it remains in keeping with the SPI in the sense that it defines shocks relative to the actual historical distribution of rainfall. The results are qualitatively unchanged (and available upon request).

rainfall shocks at ages as high as 20, I will not be including in my samples women or men born after 1978 (so that my youngest cohort turned 20 in 1998). Also, 1940 is nearly the earliest year in which anyone in my data was born, so that year will bound my sample on the early birth year side. Given that I consider the effects of shocks that occurred during the birth year and the subsequent 20 years, a large number of cohorts is necessary to generate within-location variation in the numbers of shocks experienced at different ages.

In order to maximize the likelihood that sufficiently many of the people in my sample had actually completed their educational career by the time they were interviewed, I only consider women and men who were at least 21 years old then. In addition, given that rainfall shocks affect residents of agricultural areas most directly, I restrict my sample to women and men who lived in rural areas as children.

While the DHS data does allow me to observe how long a respondent has lived in their current location (which, combined with age data, allow me to observe whether or not they had lived in their current location their entire life), it contains practically no information on where respondents previously lived. Thus, if I want to assign people who are at least 21 years old the proper shocks variables, then my sample needs to consist of non-migrants only. According to a simple Heckman sample selection-type model, if people chose to migrate-and therefore exit the universe of potential respondents from which my sample is drawn-because of the kinds of rainfall shocks conditions they experienced between birth and the time they turned 21, and these rainfall shock-induced migrants were either more or less likely to be literate (for example), then my estimates of the effects of shocks on the literacy for my sample of non-migrants will be biased.

Finally, it is also possible that my estimates of the effects of rainfall shocks on human capital outcomes will be biased because of selective mortality. More specifically, if rainfall shocks during the first 21 years of life affect individuals' mortality then once again, amongst my sample (of living individuals), the numbers of rainfall shocks experienced and error terms could be correlated. Given the evidence described in Section 2.2 on the procyclicality of young children's health outcomes in developing countries (and since all of that evidence was based on the effects of negative aggregate income shocks) we might expect that dry years during preschool ages would be the type of shock most likely to have caused children to die before they had a chance to end up in my sample. If we assume also that these deceased individuals would have been particularly unlikely to end up being literate or well-educated, then there will be a positive correlation between the numbers of dry shocks that individuals in my sample experienced early on and their idiosyncratic error terms, which would bias the estimated effects of dry years on individuals' human capital to the right on the real number line. [19]

In Section 2.5, I will discuss these issues in more detail and attempt to place bounds on the extent to which my baseline estimates might be biased as a result of selective, rainfall shock-induced migration and mortality. In all that follows, however, it should be kept in mind that under strong assumptions on how strong the survival effects of shocks are, many of the baseline estimates could be overstated

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[19] It should also be kept in mind that the baseline results are robust to the exclusion of individuals born before 1950, i.e. the exclusion of the subset of the sample that we might expect to be most heavily subject to mortality-based sample selection bias (results available upon request).

to a non-trivial extent. But as mentioned in the introduction, even under strong assumptions selection bias is not large enough to alter the qualitative features of the baseline findings, which are that both wet and dry years have large negative effects on women's human capital stocks and (more modest) positive effects on men's.

2.4 Descriptive Statistics and Results

2.4.1 Descriptive Statistics

Descriptive statistics (calculated using the weights described in Section 2.3.1) for the samples of women and men can be seen in Table 2.2. Sample literacy rates (of 17 percent and 44 percent for women and men) and highest grades completed (of 1.71 and 4.10 for women and men) are low, particularly for women, with nearly 84 percent of women not having completed a single year of schooling (see Figure 2.1). There is substantial variation in human capital stocks across countries, with higher than average literacy rates and mean highest grades completed in Cote d'Ivoire, Ghana, Liberia, Nigeria and Togo, but stocks are fairly low in all 12 countries (again, particularly for women). Literacy rates are also presented separately for respondents interviewed before 2000 (when literacy was self-reported) and respondents interviewed in or after 2000 (when interviewers made their own decisions on how literate respondents were). The birth year distributions show that the men's sample is a bit older than the women's; this is due to the fact that, again, men as old as 59 were interviewed while only women under the age of 50 were. My earlier claim regarding the frequency of the types of shocks that I consider is also borne out by the descriptive statistics: People experienced around 2 and 5.5 of what I have defined to be wet and dry years over the first 21 years of their lives, respectively. The gender discrepancies in numbers of shocks experienced can be explained by the facts that the men's sample is older and that regional precipitation levels began a marked decline starting in the late 1960s (see Hulme et al., 2001, as well as Figure 2.2). Similarly, the fact that the birth year distributions of the women's and men's samples are as skewed towards later years as they are explains the greater frequency of dry years experienced relative to wet years experienced. [20]

To get a sense of the kinds of rainfall conditions that generate my results, refer to Figure 2.3, which displays the averages (across individuals) of the numbers of rainfall shocks of different intensities that people experienced over the first 21 years of their lives. As the figure makes plain, the great majority of annual rainfall levels were within 2 standard deviations of the local mean (over the previous 25 years). The figure also reflects the aforementioned sustained decline in annual rainfall levels in the way drier-than-average years were more common than years when rainfall was above average.

Also, as Figure 2.4 makes clear, becoming literate can be a long process. Completing another year of schooling seems to add about 10 percentage points to the likelihood of being literate on average, for both women and men. Thus, even if a child is enrolled every year starting from the year she turns 6, she might still be learning to read at the age of 16. This implies that individuals' likelihoods of be-

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[20] Baseline results are not sensitive to the truncation of the men's sample so that the sample only includes men younger than age 50, similarly to the women's sample (results available upon request).

ing literate (as well as their highest grades completed) could be affected by shocks experienced throughout their teenage years.

Finally, Figure 2.5 provides suggestive evidence of the presence of age heaping. In settings like rural West Africa, it is reasonable to expect that (for older cohorts in particular) ages and birth years are not recalled with perfect accuracy. This would have serious consequences for the analysis here if sufficiently many people reported being born in the same year (e.g., a year ending in '0' or '5'). However, as Figure 2.5 shows, this is not obviously a first order problem. For example, while a particularly large number of women report being born in 1960, it seems unlikely that the results would change drastically if the distribution of birth years was more even around that time, especially since the key independent variables change relatively slowly as birth years increase.

2.4.2 Results

2.4.2.1 Baseline Results

Table 2.3 contains the baseline results for the literacy and highest grade completed outcomes, for women and men separately. [21] For women, both wet and dry years (significantly) decrease the likelihood of being literate and the highest grade completed (see columns 1 and 2, respectively). Literacy and the highest grade complete are each affected by wet shocks during the latter two age intervals and dry

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[21] Once again, since rainfall shocks before age 21 might have affected who ended up in my sample of (living) non-migrants, it is possible that some of the results that will be discussed here and in the following sections are subject to sample selection bias (see Section 2.5.2).

shocks during all three. The F-statistics corresponding to the test of the hypothesis that the shocks coefficients are jointly zero are greater than 4 for both outcomes.

The magnitudes of the point estimates are (again) seriously large, with (for example) additional wet (dry) years between the ages of 7 and 13 (birth and age 6) decreasing women's likelihood of being literate by 1.2 (0.6) percentage points. These estimated effect sizes are substantial compared to the women's sample literacy rate of 17 percent. Moreover, the implied average effects of shocks are very large as well: The set of all shocks experienced over the first 21 years of life decrease women's likelihood of being literate by 22.1 percent. The magnitudes for the highest grade completed outcome imply that, for each additional wet (dry) year between the ages of 7 and 13 (birth and age 6), roughly 1 in 13 (18) women completed 1 less year of schooling. In percentage terms, these effect sizes are considerably larger than those implied by Shah and Millett Steinberg (2013), where the implied effects of, say, an additional wet shock between the ages of 6 and 13 imply a 2.4 percent decrease in the highest grade completed outcome, as compared to the 4.4 percent decrease in that outcome here following a wet shock between the ages of 7 and 13. [22] Finally, given that, as Figure 2.4 shows, an additional year of schooling is associated with a 10 percentage point increase in the likelihood of being literate, the magnitudes of the wet and dry years estimates for the highest grade completed outcome seem consistent with their analogs for the literacy outcome if a substantial proportion of the literacy effects is being driven through decreases in time spent in school. In

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[22] Although Shah and Millett Steinberg (2013) define shocks differently, this is unlikely to explain the difference cited here, since my baseline results are robust to reasonable changes in shock definition thresholds (as discussed in Section 2.3.2).

particular, the highest grade completed effects are reasonably close to 10 times as large as the literacy effects.

How might rainfall shocks affect women's human capital stocks? Here I consider the effects of shocks after the age of 6 (and separately analyze shocks at preschool ages in the next sub-section). The negative effects of wet years on women's human capital can be explained by the likelihood that wet years increase female labor demand (both on and off the farm as well as inside the home), and that girls respond by working more and going to school less; in terms of the conceptual framework outlined in Section 2.2, substitution effects dominate income effects for females during wet years. This interpretation is consistent with existing findings on the beneficial effects of rainfall on agricultural output and household incomes (Webb and Reardon, 1992; Jensen, 2000), which in turn drive economic activity in rural areas (Iliya, 1999; Mueller and Zevering, 1969). Moreover, Bhalotra and Umana-Aponte (2010) find that African women's labor supply is procyclical, and other research shows that young females in West Africa tend to work on the farm, off the farm in small family-owned firms, and at home doing chores and taking care of younger siblings (Herz et al., 1991; Ajani, 2008; Canagarajah and Skyt Nielsen, 1999; Robson, 2004; Bhalotra, 2003; Glick and Sahn, 2000). [23] All of this would also be consistent with the results of Edmonds (2005), who finds that children work in the household in ways that their comparative advantages would suggest they would.

But given the aforementioned evidence on the procyclicality of female labor

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[23] This interpretation of substitution effects dominating is consistent with that of Shah and Millett Steinberg (2013), who find that wet shocks worsen Indian students' test score performance and decrease Indian adults' highest grade completed and wages. In contrast to the findings here, however, the substitution effect dominates the income effect for dry as well as wet shocks.

supply in Africa, it is likely that adolescent girls were working less during and after dry shocks. Indeed, evidence suggests that opportunities for work in rural West Africa are few and far between at such times (Fafchamps et al., 1998; Mortimore, 1988; ICRISAT, 1980). The most likely explanation for the negative effects of dry years on women's human capital stocks, then, is that parents simply were not willing to finance their daughters' educations when harvests were poor and incomes were low. [24] Indeed, the low levels of female human capital-in absolute terms as well as relative to males-certainly suggest that girls' school fees might have commonly been one of the first expenses cut after negative income shocks.

Switching to the men's results in columns 3 and 4 of Table 2.3, we see qualitatively different effects of shocks. In particular, additional wet years between the ages of 14 and 20 positively affect men's likelihood of being literate, and dry years between birth and age 13 have beneficial effects on both human capital outcomes. The effects of wet shocks on the highest grade completed before age 14 are less straightforward: They are positive at preschool ages but negative between the ages of 7 and 13. The hypothesis that shocks do not jointly affect men's human capital stocks is rejected for both outcomes.

The magnitudes of the estimated effects of shocks are not particularly large for men, with an additional wet (dry) year from age 14 to age 20 (birth to age 6) increasing the likelihood of being literate by 1.5 (1.1) percentage points. Given the men's sample literacy rate of 44 percent, these magnitudes imply that additional

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[24] Again, these negative effects of dry shocks on completed human capital stocks are in contrast to the findings of Shah and Millett Steinberg (2013).

wet and dry shocks during the age intervals in question increase men's likelihood of being literate by about 3.4 and 2.5 percent, respectively. [25] Both wet and dry shocks at preschool ages also significantly increase men's highest grade completed, with an extra wet (dry) year then meaning that about 1 in 16 (13) men eventually complete an additional year of schooling.

How might wet shocks after age 6 be affecting men's literacy? From the perspective of the conceptual framework, it would seem that substitution effects dominate income effects between the ages of 7 and 13 (and that men spend less time in school as a result), but that income effects dominate afterwards until age 20. Given that wet shocks between the ages of 7 and 13 do not decrease men's likelihood of being literate, however, perhaps it is men who were either already literate or who would not have gone on to become literate whose enrollment was affected. On the other hand, since wet shocks between the ages of 14 and 20 have positive effects on men's literacy but not their schooling attainment, these shocks seem to have caused men to spend more time learning while enrolled in school rather than increased the number of years they were enrolled. Perhaps wet shocks cause some younger men to increase their labor supply and decrease their school enrollment before age 14, but afterwards result in the kinds of higher household incomes that offer a last chance for other kinds of men to learn how to read. These positive human capital effects of wet shocks during the third age interval stand in contrast to the negative effects we have observed for females. Given the differences in human capital stocks

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[25] Given men's average numbers of shocks experienced, average effects are also moderately sized relative to the sample average literacy rate: Shocks as a whole increase men's likelihood of being literate by 9.9 percent.

across genders, it is perhaps not surprising that young men's time spent learning would have increased (between the ages of 14 and 20) thanks to rainfall-induced lower marginal utilities of consumption, even as young women responded to the increased demand for labor by working more. After all, cheap labor is scarce during wet years, so parents choosing to have girls work and boys study more then would simply be another example of parents favoring boys' education when resources are scarce (Chibiko Offorma, 2009).

More generally, the fact that shocks negatively affect females' literacy in a way that they do not negatively affect males' is consistent with Bjorkman-Nyqvist (2013), who finds that dry shocks decrease female school enrollment but not male school enrollment. Also, de Vreyer et al. (2012) find that the completed schooling attainment of girls is more severely negatively affected by locust swarms than that of boys was in Mali (one of the countries my sample is drawn from). The pattern of results in this chapter is also consistent with the findings of Manacorda (2006), which show that the main beneficiaries of co-resident children's labor are other children themselves. In other words, the fact that some children work frees up other children from the responsibility to do so, as seems to be the case here (along gender lines strictly). A final reason why it might not be surprising that shocks have effects on female human capital that are more negative than the effects on males is that most West African tribes are patrilocal. Parents can therefore expect to receive more of the benefits of the human capital investments they make in their sons, while daughters' husbands' families will be the chief beneficiaries of investments in daughters.

It is important to keep in mind, however, that the results are merely consistent with these explanations, and there might have been other phenomena at work. For example, following wet shocks, and from the perspective of the conceptual framework, the (magnitudes of) substitution effects relative to income effects are larger for females than they are for males. In principle, however, there could have been a number of other factors at work following wet shocks: The opportunity cost of teachers' time–and therefore their attendance–could have been affected; school lunches could have increased with public budgets; large amounts of rain on tin roofs could have made learning difficult; violent conflict could have become less likely; and certain individuals' migration propensities could have been affected. Moreover, these other effects could have worked alongside competing income and substitution effects. For example, to the extent that large amounts of rainfall, say, negatively affected teacher attendance, then the effects of wet shocks on both females' and males' completed human capital stocks were each pushed further to the left on the real number line. These other kinds of effects could be working across years as well, so that the estimated effects of shocks are the result of events that occurred in the period immediately after shocks occurred as well as other events in later periods. For example, if shocks before age 7 result in the migration of household members that in turn mean higher remittance income for the sending household years later, then dynamic effects over the medium term could be important. Unfortunately, the retrospective nature of the analysis makes it very difficult to determine what the most important mechanisms driving the permanent effects of shocks might be. In any case (and with this kind of limitation in mind), I next consider the mechanisms through which rainfall shocks at preschool ages might be affecting women's and men's human capital stocks.

2.4.2.2 Shocks at Preschool Ages

As the conceptual framework demonstrates, rainfall shocks during preschool ages can have permanent effects on human capital if those shocks have persistent health effects which later influence parents' education investment decisions. I test for evidence of this mechanism by considering how rainfall shocks affect women's height, which is commonly taken to be a measure of early-life health conditions. Unfortunately, height data was only collected for a subset of the women in my sample-which precludes the possibility of analyzing how shocks might have affected men's health at preschool ages. Also, the women for whom height data was collected were not always randomly selected. For example, in older surveys height data tended only to be collected for women who had given birth in the last five years. Thus, it is possible that rainfall shocks before the age of 21 affected who was eligible to be in my sample of women for whom height data was collected (if, for example, rainfall shocks affect fertility), which could lead to biased estimates of the effects of shocks on women's height. To see if rainfall shocks did in fact affect who could have ended up in my sample of women with height data, I regressed an indicator for whether height data was available on all of the covariates from my baseline specification along with survey year fixed effects and controls for the kinds of rainfall shocks conditions women experienced between the year they turned 21 and the year they were surveyed. [26] The results (available upon request) indicate that the only shocks to have significant effects on the likelihood of having had height data collected were wet years between the ages of 14 and 20, with the estimate of the corresponding coefficient equal to 0.005. [27] Thus, if wet shocks caused (for example) particularly tall (short) women to enter the sub-sample of women with height data, then the estimated effects of wet shocks between age 14 and age 20 will be biased to the right (left) on the real number line. In short, the third age interval wet shocks estimate for the height outcome should be interpreted with caution.

To conduct the actual analysis, I simply regress a variable equal to an individual woman's height (in millimeters) on the same set of regressors as in my baseline analysis. The results can be seen in Table 2.4, with columns 1 and 2 containing literacy and highest grade completed results for the subset of women for whom height data is available and column 3 containing the results for the height outcome. As the first two columns show, the wet and dry shocks estimates are broadly similar to their baseline analogs in Table 2.3, with both wet and dry years having significantly negative effects (leading to a rejection of the null hypothesis that shocks do not jointly affect literacy and schooling attainment).

The results in column 3 give very little evidence that rainfall shocks during preschool ages lead to persistent health effects: The magnitudes of both the wet and

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[26] Since rainfall shocks are slightly serially correlated and rainfall shocks after age 21 could have affected whose height was measured (through their effect on fertility), neglecting to control for shocks conditions for the post-age 21 period could lead to biased estimates of the main covariates of interest, the wet and dry shocks for the three age intervals between birth and age 20.

[27] One explanation for this could be that wet shocks increase the likelihood of becoming pregnant (and so the likelihood of having had height data collected).

dry estimates are small and far from significant. [28] Moreover, as described below in Section 2.5, the effects of dry shocks before age 7 could be biased to the left on the real number line due to survival- and migration-based sample selection, in which case the true effects of shocks on women's height are if anything more positive than the results in Table 2.4 suggest. There is therefore no evidence of a negative effect of shocks on women's height, and the positive effects of dry shocks before age 7 on women's survival discussed in Section 2.5 suggests that if anything, dry shocks result in positive health shocks for girls. Also, according to results described in Appendix A, the effects of dry shocks between birth and age 6 on women's human capital are driven primarily by shocks during the first 4 years of life. This seems more consistent with some type of health channel rather than, say, a lagged income effect of shocks at preschool ages preventing households from having the resources to send female children to school later. [29] It is also possible that dry shocks early on did in fact result in health shocks that subsequent educational investment decisions took into account, but those health effects were not permanent and therefore are not reflected in adult women's heights. If the negative effects of dry shocks before age 7 on human capital did in fact operate through a health channel, then it seems more likely that dry shocks negatively affected young girls' health and that parents invested less in the education of those females in response. [30] Thus, while these results do not

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[28] The magnitudes of the shocks coefficients point estimates are each considerably less than one-tenth of 1 percent of the size of the sample average height of about 1,596 millimeters.

[29] Existing evidence on the persistent income effects of dry shocks suggests that shocks at preschool ages could affect women's human capital stocks in a way that is similar to the way shocks at more advanced ages do, namely through their (lagged) effects on parents' willingness to finance girls' school enrollment (see Dercon, 2004).

[30] It need not be the case that dry shocks negatively affect female human capital, however. As the conceptual framework states, it is possible that dry shocks result in fewer health-promoting

strongly support the possibility that shocks affect completed human capital stocks through effects on early-life health, they are consistent with that possibility and they do not seem consistent with some alternative explanations.

While wet shocks at preschool ages do not seem to affect women's height, the estimated effects of wet years between the ages of 7 and 13 and between 14 and 20 are significantly positive. During each age range, an additional wet year is estimated to increase height by at least 2 millimeters. [31] It is possible that wet shocks at these advanced ages result in the kinds of positive nutrition shocks that increase women's final, adult heights. This would be consistent with the results of Aguero and Deolalikar (2012), who find that exposure to the Rwandan genocide (and the negative nutrition shocks that followed) substantially negatively affected the adult heights of females as old as 18 at the time of the conflict.

For men, while the baseline results from Table 2.3 suggest that wet shocks at preschool ages positively affect schooling attainment, the results in Table A.1 suggest that these effects are driven more or less entirely by shocks that occur at

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private goods but more parental time inputs, and that the latter effect dominates the former. Also, as Skoufias et al. (2011) note, breeding conditions for vector-borne diseases are hindered by insufficient rainfall. It is therefore possible that dry shocks improved young females' health, and parents invested less in these more health females as a result.

[31] It is possible that the sign and magnitude of these estimates could be driven to some extent by two types of sample selection bias. First, there is the just-mentioned possibility that the estimates of the third age interval wet shocks coefficient could be subject to sample selection bias. Also, as Section 2.5.2 shows, wet shocks between the ages of 14 and 20 increase the numbers of non-migrant women in my sample. If we make the usual assumption that the would-be migrants who wet years kept from moving tend to be high types, and in particular that they tend to be taller than the average non-migrant woman in my sample, then there will be a positive correlation between the numbers of wet shocks experienced over these two age ranges and idiosyncratic error terms (amongst the women in my sample). This would of course lead to estimates of the effects of wet shocks on height that are biased to the right on the real number line, which could help explain the sign and size of the estimates in question. But according to the results in Section 2.5.2, if anything, wet shocks between the ages of 7 and 13 are causing female 'high types' from migrating out of the sample and causing 'low types' to survive. Thus, the positive effects of wet shocks from age 7 to age 13 on women's height are potentially understated.

age 5 only. Also, the beneficial effects of dry shocks between birth and age 6 are driven by shocks at ages 2, 3 and 4. It is possible that following both types of shock, material resources are concentrated on boys and their health improves, and that parents invest more in boys' education later. Alternatively, it could be that shocks result in negative health shocks for boys but that parents compensate for this later in the form of greater education investments. With no direct measure of men's early-life health conditions, however, the evidence in favor of shocks permanently affecting literacy or the highest grade completed at school through a health channel is of course quite limited. But the data do not seem to support the possibility of early-life shocks affecting later school attendance through a lagged asset or input availability channel.

2.4.2.3 Shocks and Schooling Along the Extensive Margin

Given the high proportion of the individuals in my sample who have not completed a single year of schooling, I look for evidence that certain women's and men's educational careers never even began because of shocks. To do this, I regress an indicator equal to 1 if an individual completed at least 1 year of schooling (and zero otherwise) on my baseline covariates.

The results for women and men can be seen in columns 1 and 2, respectively, of Table 2.5, where we see that dry years between birth and age 6 do in fact affect educational attainment at the extensive margin for both women and men. The estimated effect for women of an additional dry year before age 7 implies that each additional dry year then decreases the likelihood of having completed at least one year of schooling by six-tenths of a percentage point, which is substantial given that only about 25 percent of the women in my sample completed at least 1 year of schooling. For men, dry shocks at preschool ages increase the likelihood of having completed at least 1 year of schooling by 1.1 percentage points, which, once again, is not particularly large compared to the men's sample average for this outcome of about 49 percent.

There is also evidence that wet shocks between the ages of 14 and 20 significantly affect educational attainment along the extensive margin for both women and men, and similarly for dry shocks at that age for women. Once again, the estimated effects are negative for women and positive for men. An additional wet shock during this third age interval decreases the likelihood of completing at least 1 year of schooling by 1.2 percentage points for women and increases that likelihood by 0.9 percentage points for men. [32] An additional dry shock after the age of 13 decreases women's likelihood of having completed one year of schooling by 0.4 percentage points. It seems surprising that rainfall shocks could affect this outcome at ages this advanced-we would normally expect the decision to attend school at all or not to have been made at younger ages. One possible explanation for these results is that, following some shock to school availability (which was unrelated to rainfall

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[32] Note that these estimated effect sizes are quite large, especially for females. In particular, out of the roughly 25 percent of all women in the sample who completed at least 1 year of schooling, the magnitude of the effect of experiencing an additional wet shock between the ages of 14 and 20 implies that a surprisingly large proportion of women must have been only beginning their formal education above the age of 13 (and that a very large proportion of these must have been affected by wet shocks). Also, again, under strong assumptions both the women's and men's estimates of shocks effects could be non-trivially biased because of self-selection into the sample of living non-migrants.

shocks), demand for schooling was unusually high amongst currently teen-aged individuals eager to take advantage of previously unavailable education opportunities. [33] Obviously, in a situation like this individuals' educational careers were potentially just beginning, which would explain how the outcome in question could have been affected during the advanced stages of adolescence.

2.4.2.4 Shocks Numbers Indicators Results

The baseline results assume that the effects of additional wet and dry years are linear. In order to learn whether this linearity assumption might be justified and whether more shocks always have more severe effects than fewer, I will now substitute series' of dummy variables for each of the shocks variables in the baseline specification. More specifically, for each of the three wet (dry) years shocks variables, I construct three dummy variables that are equal to 1 when the number of wet (dry) years experienced during the age range in question is equal to 1, 2 and at least 3, respectively (and equal to zero otherwise). In other words, zero shocks experienced will be the omitted category for both types of shocks and for each of the three age ranges.

The literacy and highest grade completed results for women can be seen in columns 1 and 2, respectively, of Table 2.6. The results show that more wet years between birth and age 6 do not always seem to have more severely negative effects on the literacy outcome, and similarly for both outcomes between the ages of 7

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[33] Osili and Long (2008) and Oyelere (2010) find that educational attainment was positively affected when school fees were waived as part of a Universal Primary Education program in Nigeria.

and 13. For example, while women who experienced 1 wet year between birth and age 6 had a 0.9 percentage point lesser chance of being literate than women who experienced no wet shocks then, there is no difference in the likelihood of being literate between women who experienced 2 wet shocks and women who experienced none (statistically speaking). Also, F-tests reject the hypothesis that additional shocks effects have equally-sized effects for wet shocks during both of the first two age intervals. More wet shocks are consistently worse than fewer for both outcomes between the ages of 14 and 20 (with the effect sizes of each additional shock once again being statistically indistinguishable from one another). For this third age interval, the negative effects of wet shocks seems to be driven in large part by the third-or-higher category, with affected women having a likelihood of being literate that is 5.2 percentage points lower than the analog for women who experienced no wet shocks then. [34]

Turning to the women's dry years results, we see that additional dry years between birth and age 6 do not always have more severely negative effects than fewer for the literacy outcome, but that they do for the highest grade completed (with, for example, a third-or-higher dry shock implying that the average highest grade completed of affected women was lower than that of women who experienced zero dry shocks then by more than 13 percent). Given the possibility that dry shocks early on affect women's human capital through a kind of health effect, the relatively

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[34] One possible explanation for the relative strength of higher-order wet shocks during the 14-20 age range is that the cost of re-enrolling following some period of time spent out of school is increasing in that time. In this case, if enough women spent sufficiently long periods of time outside of school thanks to multiple wet shocks, we would expect higher-order shocks to have particularly large effects.

large magnitude of the third-or-higher effect here could reflect the possibility that it takes multiple shocks for young girls' health–and later education investments–to be affected. More dry years between the ages of 7 and 13 and between 14 and 20 also tend to be consistently worse than fewer for both outcomes. Consistent with the baseline results, the effects of dry years during this third age interval are smaller than dry years effects at preschool ages, with a third-or-higher shock estimate implying that affected women were 1.3 percentage points less likely to be literate compared to women who experienced zero shocks. Once again, the effects of a third-or-higher shock seem relatively large, which could imply that it takes multiple dry years during this age range for household incomes to fall sufficiently far that girls' education investments are curtailed.

For men (see columns 3 and 4), we see that wet shocks during the first age interval seem to affect the highest grade completed in a very particular way, with the estimate of the first shock effect being positive and large, the second estimate being positive and slightly larger than the first, and the third not being statistically significant. From age 7 to age 13, there is no evidence that wet shocks substantially affect men's literacy, but the signs and magnitudes of the point estimates for the highest grade completed outcome show that more wet years then decrease schooling attainment. Also, more shocks between the ages of 14 and 20 tend to have more strongly positive effects on both human capital outcomes than fewer (though none of the estimates for this age interval are significantly positive for the highest grade completed outcome). [35] The magnitude of the third-or-higher point estimate

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[35] Effect sizes for the literacy outcome are, however, statistically distinguishable according to the

implies that going from experiencing zero wet shocks to experiencing at least 3 increases the likelihood of being literate by 5.5 percentage points. Also, the fact that the effect of the second and third-or-higher shocks are so much larger than the effect of the first could mean that it takes a few wet shocks for income effects to dominate and for men to actually start spending substantially more time learning; perhaps men are only able to devote more tim to learning after household incomes have been high for a number of years.

Similarly, more dry shocks always have stronger positive effects on men's human capital than fewer, with (for example) the likelihood of being literate estimated to be 2.7 percentage points higher for those men who experienced at least 3 dry shocks as compared to those men who experienced zero. For the second and third age interval dry years effects, the results in Table 2.6 are consistent with the baseline results in that there is no good evidence that more dry years have stronger effects than fewer. Thus, for a substantial proportion of the (women's and men's) significant shocks numbers estimates from the baseline results, we see evidence that more shocks do in fact have stronger effects than fewer. To sum up the baseline results, it would seem that in contrast to what Jacoby and Skoufias (1997), Funkhouser (1999) and Bjorkman-Nyqvist (2013) each suggest, there are substantial permanent effects of shocks on affected individuals' human capital.

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results of an F-test.

2.4.3 Effects by Agroecological Zone

West Africa is a large place that includes multiple agroecological zones. In particular, as mentioned in Section 2.2, there is substantial variation in average rainfall levels across latitudes, with areas farther to the north typically being more dry. It is possible that shocks in drier regions have different effects than do shocks in wetter regions. For example, it could be that wet shocks in dry areas do not increase agricultural productivity, perhaps because of weaker relationships between the crops typically grown there and quantities of water. Similarly, dry shocks in drier regions could have less severely negative consequences for household incomes if crop choices are such that any associated risks are minimized ex ante.

I explore how shocks effect differ by agroecological conditions by interacting the shocks numbers variables from the baseline specification with indicators equal to 1 if the individual's rainfall neighborhood is in a region that is at least 50 percent arid or semi-arid according to the Food and Agriculture Organization's Agroecological Zones Framework. [36] This includes all of Burkina Faso, Mali, Niger, Senegal and (the more northern) regions of Benin and Nigeria. The results can be seen in Table 2.7. It seems clear that for women, both wet and dry shocks have more severely negative effects in wetter and more humid regions (if anything). This is only true in a statistical sense when it comes to the effects of wet shocks between the ages of 7 and 13, but the way the magnitudes vary by arid/semi-arid region status for the other shocks main terms relative to the baseline results also appear to be

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[36] See http://www.fao.org/wairdocs/tac/x5756e/x5756e0j.htm (accessed on November 27, 2014) for more information.

consistent with this conclusion. For men, it appears that the baseline wet birth-age 6 results mask heterogeneity by agroclimatic zone, with men in more humid regions benefiting from these shocks then and men in drier regions being negatively affected by them. Otherwise, it appears that, if anything, the human capital stocks of men in more arid regions are more positively affected by both wet and dry shocks (during the third and first age intervals, respectively) than are men in less arid regions. It appears that households in more arid regions are more likely to focus resources exclusively on sons following shocks, perhaps because of higher baseline household poverty rates, which (again) would be consistent with households choosing to invest in boys only when resources are scarce.

Overall, these results are consistent with the possibility that in arid regions, the crops typically planted are less dependent on or sensitive to rainfall. It is also possible, however, that the effects of shocks on women's human capital seem to be more severely negative in more humid regions because these are the only regions where there is meaningful variation in the outcome variables. In particular, as Table 2.2 makes clear, human capital levels are particularly low in countries that are either entirely or mostly arid or semi-arid. Women's human capital stocks in these places might have therefore been particularly unlikely to have been negatively affected by rainfall shocks.

2.5 Robustness

2.5.1 Region-Specific Cohort Fixed Effects

We might worry that, even after controlling at the country level for cohort-specific human capital levels, the resulting rainfall shocks estimates are to some extent still capturing place-specific human capital trends along with the effects of rainfall shocks. To address this, I estimate the baseline specification with region-specific birth year dummies in place of their country-specific analogs. Thus, this version of the specification will include as many as 55 complete sets of birth year fixed effects-one for each region-as opposed to the 12 (countrys' worth) from the baseline version of the specification. [37]

The results for women's literacy and highest grade completed can be seen in columns 1 and 2, respectively, of Table 2.8. They are highly similar to the baseline results, with both wet and dry years once again having significantly negative effects on both outcomes and the null hypothesis of shocks not jointly affecting human capital rejected (for both outcomes). Indeed, for the literacy (highest grade completed) outcome, the F statistic associated with the test of the null hypothesis that the shocks numbers coefficients are jointly equal across specifications is about 1.033 (1.803), and the null hypotheses are therefore not rejected at the 5 percent significance level. [38] Increasing the flexibility of the baseline specification with

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[37] Since there are no men from Cote d'Ivoire in my sample, the men's sample is of course spread over fewer than 55 regions.

[38] The null hypothesis that the shocks coefficients are jointly equal across specifications for the highest grade completed is rejected at the 10 percent level, however.

respect to how time series effects are controlled for does not result in substantial (or significant) changes to the results for women in the sense that both wet and dry years are still estimated to have substantial, negative effects on women's human capital.

Turning to the men's results in columns 3 and 4, we see that they are highly similar regardless of how cohort effects are controlled for. In particular, additional wet shocks between age 14 and age 20 still increase the likelihood of being literate and wet shocks between age 7 and age 13 still decrease the time spent in school. On the other hand, the estimated effects of additional dry years during preschool ages have decreased in magnitude and are no longer significantly positive. This could reflect the possibility that trends in men's literacy formation between birth and age 6 were more positive in those regions that became more dry over time relative to trends in other regions. Once again, however, the null hypothesis that shocks effects differ across specifications which control for birth year fixed effects at a finer or coarser geographic level is not rejected for either outcome at conventional levels. [39]

2.5.2 Sample Selection Bias Robustness Check

2.5.2.1 Shocks, Migration and Mortality: Theory

As mentioned previously, since my sample consists of living non-migrants only, then if rainfall shocks before the age of 21 caused people who were especially likely or unlikely to be well-educated to migrate (or not), or survive (or not), then my

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[39] The associated F statistics for the literacy and highest grade completed outcomes are about 0.968 and 1.242, respectively.

baseline results will be biased. To illustrate the point, I will rely on a framework that is loosely related to a Heckman selection model (Heckman, 1979).

I begin by considering how rainfall shock-induced migration could result in the baseline estimates being affected by sample selection bias. Suppose that the utility that individual i receives from migrating is denoted by M_{i1} and the utility they receive from not migrating is denoted by M_{i0}. Then individual i will migrate if and only if $M_{i1} - M_{i0} > 0$. Let M_i be an indicator equal to 1 if individual i migrates and zero otherwise (so that M_i is the observable outcome associated with individual i's migration decision). Also, let $M_{i1} - M_{i0}$ be given by the right-hand side of the following equation:

$$M_{ipt} = a + \sum_{j=1}^{3} B_j \#wetyearsj_{pt} + \sum_{j=1}^{3} C_j \#dryyearsj_{pt} + d_p + h_{ct} + l_r(t) + X'_{ipt}G + e_{ipt}, \tag{2.2}$$

where once again p indexes individuals' rainfall neighborhoods and t their years of birth. Migration-and therefore the chance that an individual could possibly be in my sample of non-migrants-is modelled here to depend on the same rainfall shocks variables, rainfall neighborhood fixed effects, country-specific birth year fixed effects, regional time trends and demographic variables as in the baseline specification.

Assume also that, conditional on observables, individuals with larger human capital stocks are more likely to migrate, i.e. that $Cov(\varepsilon_{ipt}, e_{ipt}) > 0$. (Recall that ε_{ipt} denotes the idiosyncratic error term from the baseline human capital specification.) This is the standard assumption that migrants tend to be particularly highly motivated or especially able. This assumption is reasonable here since, for example, migrant women are about 50 percent and migrant men 37 percent more likely to be literate than their respective non-migrant analogs.

If individuals can only be in my sample if they have never migrated and they migrate if and only if $M_{i1} - M_{i0} > 0$, then my sample of non-migrants will only consist of individuals for whom the following condition holds:

$$e_{ipt} \leq -a - \sum_{j=1}^{3} B_j \#wetyearsj_{pt} - \sum_{j=1}^{3} C_j \#dryyearsj_{pt} - d_p - h_{ct} - l_r(t) - X'_{ipt}G. \quad (2.3)$$

If, for example, dry years cause individuals to move (i.e., if $B_j > 0$ for some $j \in \{1, 2, 3\}$), then individuals with high e_{ipt} terms will tend to have experienced fewer numbers of dry years in order for this condition to still hold and for those individuals to still have a chance of being in my sample of non-migrants. Intuitively, since individuals with high e_{ipt} terms were always more likely to migrate and dry years increase the likelihood of migrating, then individuals in my sample of non-migrants with high e_{ipt} will tend to have experienced fewer dry years during age interval j. In other words, conditional on all other independent variables the covariance of $\#dryyearsj_{pt}$ and e_{ipt} will be negative amongst the non-migrants that comprise my sample. Then, under the assumption that $Cov(\varepsilon_{ipt}, e_{ipt}) > 0$, $B_j > 0$ will also imply that the covariance of $\#dryyearsj_{pt}$ and ε_{ipt} (conditional on controls) will be negative in my sample of non-migrants. This will lead to estimates of the effects of dry shocks during age interval j on human capital outcomes that are biased to the left on the real number line. Obviously, shocks estimates will also be biased if instead dry years decrease the likelihood of migrating or if wet years affect individuals' migration decisions (again, as long as my sample only consists of non-migrants and we assume that migrants tend to be high types relative to non-migrants).

In theory, a similar type of sample selection bias could arise if rainfall shocks affect the survival of certain types of individuals. It is possible that rainfall shocks experienced during the first 20 years of life affect the likelihood that individuals die before they are old enough to possibly end up in the data I use. Moreover, it is reasonable to expect that any individuals whose survival would be affected by rainfall shocks are less likely to have turned out to have been literate. In this case, then amongst the (living) individuals in my sample, there would be a correlation between the numbers of shocks experienced and the idiosyncratic error term from the baseline human capital specification. For example, if dry shocks kill low types, then in my sample there will be a positive correlation between the numbers of dry shocks experienced and error terms, which will bias dry shocks estimates to the right on the real number line. Thus, in the same way that rainfall shock-induced migration could create sample selection bias, shock-induced mortality could also do so.

Finally, it is very important to note that under reasonable assumptions, the co-occurrence of migration-based sample selection and mortality-based sample selection could have particularly important implications for the direction and magnitude of the net sample selection bias. For example, if a particular type of rainfall shock simultaneously causes literate people to migrate and illiterate people to survive, then each of these individual sample selection forces would be working to make the sample less literate. As we will see below, however, the number of migrants will have much stronger effects on the extent of sample selection bias than will the number of survivors. It will therefore be important to obtain estimates of the numbers of individuals entering or exiting the sample via both migration- and mortality-based channels.

2.5.2.2 Shocks and Total Changes in Neighborhood-Cohort Sizes: Empirical Evidence

To do this, I will first obtain estimates of the net effects of rainfall shocks on rainfall neighborhood-cohort sizes, i.e. the sums of changes in neighborhood-cohort sizes from both shock-induced migration (or its absence) and mortality (or survival). Then, in the next sub-section, I will obtain estimates of the mortality effects of shocks, which can then be combined with the net effects to obtain implied estimates of the migration effects of shocks. [40]

One way to examine sample selection due to the sum of migration and mortality would be to consider whether or not the number of living non-migrants (in my sample) who live in a particular place and who were born in a particular year depends on the kinds of rainfall shocks that someone born in that same time and place would have been exposed to. I do exactly this by estimating the following equation:

$$NLNM_{pts} = \alpha + \sum_{j=1}^{3} \beta_j \#wetyearsj_{pt} + \sum_{j=1}^{3} \rho_j \#dryyearsj_{pt} + Z'_{pts}\Pi + \delta_p + \theta_{ct} + \tau_s + \lambda_r(t) + \varepsilon_{pts}. \tag{2.4}$$

Here $NLNM_{pts}$ is defined to be the total number of living rural non-migrants in my

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[40] Although slightly inelegant, this two-pronged approach possesses the virtue of being feasible given readily available data (the DHS data itself).

sample who are from (and currently living in) rainfall neighborhood p, born in year t and surveyed in year s. To be clear, if a rainfall neighborhood was sampled in more than one survey year (which is quite common), then there will be more than one set of observations for that neighborhood (since, as described below, this specification also controls for rainfall shocks conditions up until the year before survey year s). For instance, if a place was surveyed once in 1990 and again in 1995, then there will be one set of 30 observations corresponding to that first survey year–one for each birth year between 1940 and 1969, the last year for which there will be people from that place who were at least 21 years old in the survey year–and another set of 35 corresponding to the second survey year for that place (one for every birth year from 1940 to 1974).

The $\#wetyearsj_{pt}$ and $\#dryyearsj_{pt}$ variables are all defined as before, and the estimates of the β_j and ρ_j coefficients will indicate whether or not wet and dry years tended to either push people to leave a place (or die) or cause them to remain there (or survive). More specifically, the estimated magnitudes of these variables will measure how the size of the typical cohort (in the typical rainfall neighborhood) was affected by wet and dry shocks during the three age intervals. The rainfall neighborhood fixed effects (δ_p) control for each rainfall neighborhoods long-term average population level (amongst other location-specific, time-invariant unobservables), while the (country-specific) cohort fixed effects (θ_{ct}) and regional time trend controls ($\lambda_r(t)$) are meant to control for temporal factors common to all locations (within each country and region, respectively). Again, most neighborhoods were sampled in more than one of the household surveys that I rely on, so to control for survey year-specific unobservables I include survey year fixed effects (τ_s) as well. ε_{pts} is a mean-zero error term.

The Z_{pts} vector contains controls for rainfall shocks before cohort t was born and after they turned 21. Specifically, the vector contains two indicators for whether or not the year before cohort t's birth was wet or dry, since rainfall conditions in these years might have affected fertility and rainfall shocks are (slightly) serially correlated. The Z vector also controls for rainfall shocks conditions after age 20 and before survey year s in two ways. The first pair of controls simply consists of variables equal to the numbers of wet and dry years between the year in which cohort t turned 21 and the year before survey year s, while another pair consists of variables equal to the proportions of years in that same post-21 time period (before survey year s) that were wet and dry. I estimate the specification separately for each of these types of post-21 shocks controls. The identification assumption here is similar to that of the baseline analysis: Trends in the numbers of living non-migrants in rainfall neighborhoods which became more or less dry over time have to be assumed to be the same as trends in places that did not become more or less dry over time for the estimated effects of rainfall shocks to be consistent.

The results can be seen in Table 2.9, with the two sets of womens results in columns 1 and 2 and mens results in columns 3 and 4. In general, both wet and dry shocks tend to decrease neighborhood-cohort sizes for women and men. Dry shocks that occur between the ages of 14 and 20 are an exception for women, however. In order to know how these net effects of shocks on neighborhood-cohort sizes break down into migration and mortality effects, we need estimates of the effects of shocks on at least one of these. I seek estimates of the mortality effects of shocks next.

2.5.2.3 Shocks and Mortality: Empirical Evidence

I attempt to identify the extent of mortality-based sample selection bias by using data available for a subset of the females in the DHS data on the mortality status of their siblings. Data on siblings' birth years, survival status, gender, birth order and (if applicable) death year are used to study whether or not rainfall shocks affect individuals' likelihood of dying before reaching each of the ages of 20, 30, and the gender-specific average age of the individuals in my sample. (These are the ages of 35 and 39 for women and men, respectively.) Examining the effects of shocks on the likelihood of surviving to these particular ages only will not necessarily provide a set of estimates that can precisely estimate how large or small mortality-based sample selection bias might be; shocks can be expected to have smaller impacts on the mortality of individuals younger than average and larger impacts on older individuals' mortality, for example. In an attempt to compensate for this imprecision, therefore, I will choose estimates of the effects of shocks on mortality (or survival) from that outcome that happens to present the most severe test for my baseline results (given the assumptions I make on the human capital stocks of migrants/non-migrants and survivors/the deceased). I will describe this choice process in further detail below.

The effects of rainfall shocks on mortality will be analyzed using the following specification:

$$Mort_{ipt} = \alpha + \sum_{j=1}^{3} \beta_j \#wetyearsj_{pt} + \sum_{j=1}^{3} \rho_j \#dryyearsj_{pt} + \delta_p + \theta_{ct} + \lambda_r(t) + X'_{ipt}\Gamma + \varepsilon_{ipt}. \tag{2.5}$$

Here $Mort_{ipt}$ is an indicator equal to 1 if individual i from rainfall neighborhood p and born in year t died before a particular age. (Again, there are separate mortality outcomes for ages 20 and 30 for both women and men, as well as age 35 for women and 39 for men.) All other controls are the same as in the baseline specification. After once again conditioning on rainfall neighborhood and country-specific birth year fixed effects, plus region-specific linear trend controls, the β_j and ρ_j coefficients will indicate whether or not wet and dry years tended to either aid or harm survival.

Sibling mortality data is collected in DHS surveys in an attempt to understand the prevalence and determinants of pregnancy-related deaths. [41] In the surveys that contain sibling mortality modules, all women interviewed are asked to list all children born to their mothers, to provide data on the birth year and mortality status of their siblings, and if applicable, siblings' death years. The sample consists only of siblings of non-migrant women from the countryside who were born between 1940 and 1978 (who were also sufficiently old by the time the data was collected for the mortality outcome variables to be properly defined). [42]

The results in Table 2.10 suggest that wet (dry) shocks between the ages of 7 and 13 (birth and age 6, and age 14 and 20) aid the survival of women, while

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[41] Due to the difficulty of collecting sibling mortality information, only a subset of DHS' contain the associated modules. The countries (and years) for which sibling mortality is available are these: Benin (1996); Burkina Faso (1998); Cote d'Ivoire (1994); Guinea (1999, 2005); Mali (1995, 2001, 2006); Nigeria (2008); Niger (1992); Senegal (2008); Sierra Leone (1992, 2005); and Togo (1998).

[42] I assume that siblings grew up in the same places where the (non-migrant) women respondents were surveyed by DHS survey workers.

wet shocks between birth and age 6 have significantly negative effects on men's survival. Unfortunately, mortality estimates based on these data are also subject to sample selection bias. As mentioned above, this data only allows us to analyze the survival of siblings of living women. In other words, the sample of siblings is selected similarly to the way that the baseline human capital sample is selected, namely with respect to survival status (of the women who provided the sibling data).

In the interest of moving forward despite imperfect data, I first assume that the results of this analysis in Table 2.10 are not so affected by this type of mortality-based selection bias that the signs of the point estimates are incorrect. This assumption will end up implying that the baseline human capital effects of wet shocks for women–which are some of the most striking results I obtain–are too negative. It should therefore probably be considered an unfavorable assumption. I also assume that siblings' idiosyncratic survival factors are positively correlated, i.e. if one sibling survived then their brothers and sisters were more likely to survive. When this assumption is combined with the first assumption that the signs of the mortality effects of shocks in Table 2.10 are correct, then this second assumption is also in many ways unfavorable. Finally, I assume that age differences between siblings are not so great that the shocks that siblings experienced are insufficiently highly correlated with the shocks that the women who provided the data experienced. In other words, I assume that the women who provided the data on their siblings' mortality statuses tended to experience sufficiently many of the same shocks that their siblings did. Assuming otherwise would once again imply that the baseline human capital estimates were less biased than I will conclude they could be.

Under these assumptions, then amongst the siblings in my sample, there will be a positive correlation between each of the wet shocks and dry shocks variables and the idiosyncratic error term in the mortality equation above. In other words, in order for (females and males) to end up in the sample of siblings, the living women who provided the data and who experienced fewer wet or dry shocks will tend to have lower idiosyncratic mortality factors themselves. Assuming siblings' idiosyncratic survival factors are positively correlated therefore implies that each of the wet and dry shocks coefficients estimates in Table 2.10 will be biased to the right on the real number line. For women, then, the survival effects of wet and dry shocks during these age intervals will be understated, while men's estimated dry shocks mortality effects will be overstated. Therefore, in the interest of being conservative, I will assume that the actual mortality effects of shocks are 3 times larger than the estimates I obtain in Table 2.10. [43] This is likely a strong assumption, and it will end up being quite unfavorable.

2.5.2.4 Sample Selection and Selection-Free Estimates

Given the mortality estimates of shocks in Table 2.10 and the net effects of shocks on the numbers of living non-migrants in my sample in Table 2.9, I can back out the migration effects of shocks. The implied numbers of individuals who enter or exit my sample because of the effects of rainfall shocks on mortality and migration can be seen in Table 2.11. As mentioned above, the values in Table 2.11 represent

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[43] As the results will show, 3 is the largest integer for which the sample selection-free analogs to many of the most important baseline estimates would still be significantly different from zero at conventional levels (assuming no change in standard errors).

the combinations of mortality and migration estimates that present the most severe test for the validity of the baseline results. In particular, for each type of shock and each age interval, the mortality estimate of shocks (out of the 3 possible ones in Table 2.10) is assumed to be the one with the largest magnitude. This is because, first and foremost, all individuals whose migration choices and mortality are affected by shocks are assumed to be literate and illiterate, respectively, with certainty. [44] Between the two sources of sample selection bias under consideration here, the type that is certainly more powerful is that which is driven by migration. This is because the sample average human capital stocks are so low. Intuitively, while both migration and mortality affect sample selection through their effects on, for example, the literacy rate–the number of literate individuals relative to the total number of individuals–mortality/survival only affects the relatively large denominator and so only results in fairly small changes to the literacy rate. Migration, on the other hand, affects the comparatively small numerator (as well as the denominator), and so causes much more sample selection bias. The mortality estimates from Table 2.10 that are assumed to be the true mortality effects of shocks are therefore those that either minimize the number of individuals who do not migrate because of shocks, or maximize the number of individuals who do migrate because of them. In both of these cases, migration-based sample selection decreases the literacy rate to the greatest extent. Similarly, given these chosen mortality estimates, the total

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[44] Again, migrants in the DHS data are considerably more likely to be literate than non-migrants, and it also seems reasonable to expect that individuals whose very survival depends to some extent on shocks would turn out to be illiterate. Assuming that everyone in these two groups of individuals are literate and illiterate, respectively, maximizes the extent of the sample selection biases, and these are also likely strong assumptions.

numbers of females and males who enter or exit the sample due to either migration or mortality in Table 2.9 are chosen so that non-migration is minimized or migration is maximized.

Table 2.11 contains the results of these choices. The values in the table refer to changes in the numbers of individuals in the average rainfall neighborhood in response to shocks, and values are only calculated when there is (statistically significant) evidence in either Table 2.9 or Table 2.10 that shocks cause individuals to either enter or exit the sample of living non-migrants. Columns 1 and 2 give implied changes in the number of individuals in the sample due to mortality and migration, respectively, assuming that the mortality estimates in Table 2.10 are correct, while columns 3 and 4 of Table 2.11 give the analogs of these under the assumption that the true mortality effects of shocks are 3 times as large as the estimates in Table 2.10.

Table 2.12 contains the resulting sample selection-free human capital estimates of shocks. More specifically, the table contains the baseline literacy results for comparison purposes (in column 1), information on the signs of estimated sample selection effects of mortality and migration (in columns 2 and 3), and the implied selection-free estimates of the effects of shocks on literacy (in columns 4 and 5). The column 4 estimates assume that mortality effects are only as large as the estimates in Table 2.10, while the column 5 estimates assume that those estimates understate the truth by a factor of 3. For women, the negative effects of dry shocks experienced between the ages of 14 and 20 decrease substantially in magnitude when possible selection effects are taken into account. On the other hand, wet shocks during the latter 2 age intervals and dry shocks in the first 7 years of life would still be significantly negative (at the 10 percent level at least, and assuming no changes in standard errors) and economically significant, even under the assumption of strong survival effects of shocks. For men, the positive effects of wet shocks from age 14 to age 20 and dry shocks before age 7 are only slightly diminished by sample selection bias under strong assumptions.

Table 2.13 contains the highest grade analogs to the information in Table 2.12. The negative effects of wet and dry shocks on women's highest grade completed are less robust than they are for the literacy outcome. In particular, while there are only fairly small changes in the effects of shocks under the assumption that mortality effects are only as large as the estimates in Table 2.10 (in column 4), assuming they are larger results in substantial changes to the magnitudes and significance of wet shocks from age 7-13 and dry shocks from birth-age 6 and from age 14-20 (column 5). For men, the positive effects of wet shocks over the first 7 years of life are quite sensitive to possible sample selection effects; the magnitude of this point estimate drops substantially after taking into account the potential effects of shock-induced migration. This is less true for the dry shocks from birth-age 6 estimate.

It is important to note that weakening the assumption on the percentage of individuals whose migration decisions were affected by shocks were literate results in selection-free estimates of the effects of shocks that are much closer to the baseline estimates than the estimates in column 5 of each of Table 2.12 and 2.13. Note that this is consistent with the aforementioned power of migration-based sample selection in particular to potentially seriously affect the baseline estimates. For example, assuming that women who migrated in response to wet shocks experienced between age 7 and age 13 were only as likely to be literate as the average migrant in the DHS data implies a selection-free estimate on literacy of -0.011, even under the stronger assumption on how large the survival effects of these shocks are. [45] (Compare this point estimate to the baseline estimate of -0.012 and the strong survival effect selection-free estimate in Table 2.12 of -0.007.) Finally, it is important to realize how large the mortality effects of shocks must be for the mortality effects of shocks to actually be 3 times as large as the point estimates in Table 2.10. For example, the number of women who survive until age 35 has to be more than 5 percent higher following an additional wet shock between the ages of 7 and 13 for the strong survival assumption to be true. This would represent remarkably strong survival effects of shocks.

2.6 Conclusion

I have examined the effects of annual rainfall shocks over the first 21 years of women's and men's lives on their likelihood of being literate and their highest grade completed. Given the wage (Glewwe, 1996; Psacharopolous, 1994), health (Strauss and Thomas, 1995; De Walque, 2007) and intergenerational (Thomas et al., 1991; Oreopolous, 2006) benefits of human capital, knowing whether and to what extent rainfall shocks affect individuals' stocks of it means knowing something more about how poverty might be perpetuated in developing countries. In addition to what

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[45] Selection-free estimates under these weaker literacy assumptions for other shocks available upon request.

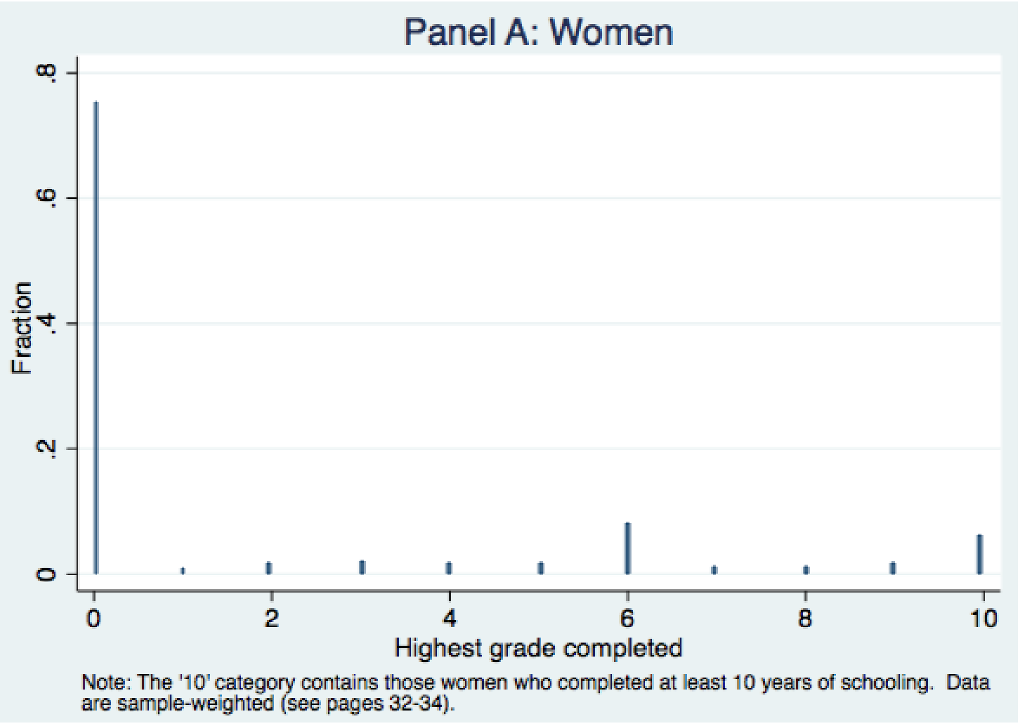
it might teach us about households' abilities to cope with everyday-type shocks, the answer to this question will be of interest to policy makers; if rainfall shocks have permanent effects on peoples' human capital stocks, then social policy might optimally take rainfall conditions into account. Given that one of the Millennium Development Goals is to achieve universal primary enrollment and another is to increase gender equality, this chapter is clearly quite relevant for policy.

In the West African context, I find that unusually wet and dry years have economically significant effects on women's and men's human capital. An additional wet (dry) year between the ages of 7 and 13 (birth and age 6) decreases women's likelihood of being literate by about 7 (3.5) percent, while an additional wet (dry) year between age 14 and age 20 (birth and age 6) increases men's likelihood of being literate by as much as 3.4 (2.5) percent. The baseline shocks effects estimates imply that thanks to shocks, women are 22.1 percent less likely to be able to read, while men are 9.9 percent more likely to be literate. While some of these estimates might be affected by sample selection bias, even under strongly unfavorable assumptions there remains evidence that both wet and dry shocks have substantial negative effects on women's human capital and that wet shocks have positive effects on men's human capital. These results contradict the suggestions of some earlier research papers that temporary lapses in human capital investments following shocks are made up for later.

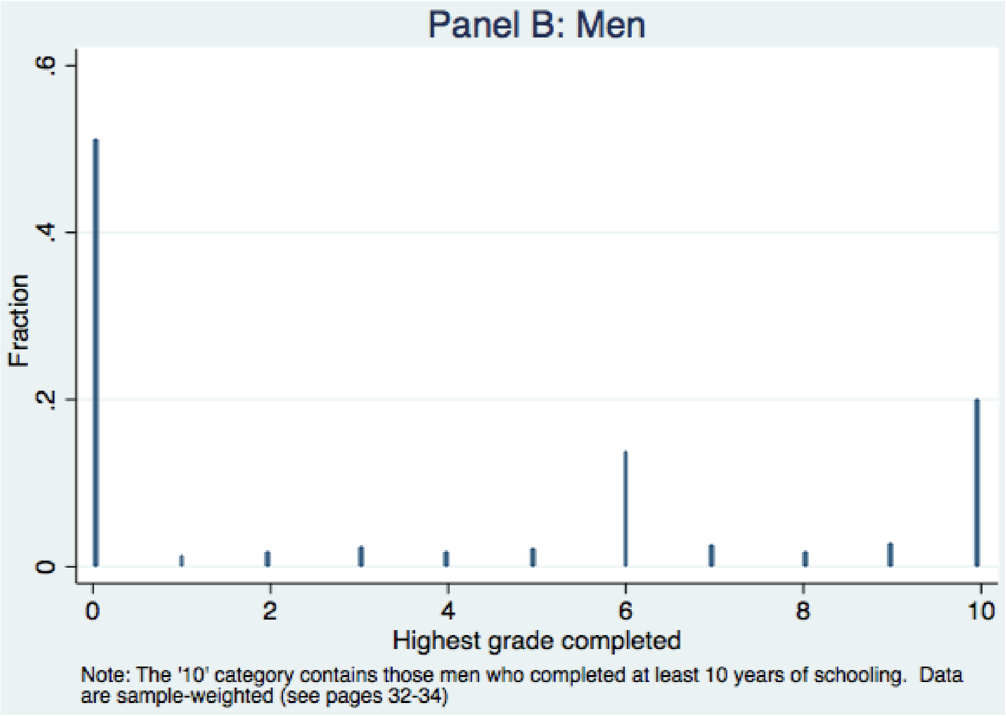
I have argued that the most plausible explanation for this pattern of results is that, following wet years females work more and spend less time in school while males likely do just the opposite, while dry years decrease both household income (to the point where parents are no longer willing to finance girls school enrollment) as well as boys opportunity cost of attending school. These results relate to the sizable development economics literature on household responses to shocks and provide evidence that whatever consumption smoothing that households manage to achieve comes at the expense of some affected individuals' completed human capital stocks. In particular, this chapter complements existing research on how investments in children are affected by temporary income shocks by showing that those contemporaneous investment effects are not necessarily undone later, and that the permanent effects can be substantial. Moreover, income smoothing strategies that avoid risk at the expense of profits with respect to productivity or labor supply choices are not the only cost associated with attempts to smooth consumption in risky environments–investments in children can also be substantially affected by shocks.

The results here suggest that the welfare effects of temporary shocks could be extremely persistent and that policies designed to help particular household members stay in school following rainfall shocks could have substantial returns. In particular, if local institutional phenomena and economic conditions and behavior are such that rainfall shocks affect the way children use their time, then it will be important to consider the potential human capital benefits of social insurance schemes, weather insurance or conditional cash transfers whose payouts are triggered by rainfall shocks. On the other hand, as Shah and Millett Steinberg (2013) note, programs that increase wages could have the effect of decreasing human capital investments in children. This would be consistent with the findings of Basu et al. (2010), who find that child labor initially increases with household wealth, given the lack of labor market opportunities for the poorest children and the way those opportunities at first increase with wealth.

Figure 2.1: Distribution of Women's and Men's Highest Grades Completed

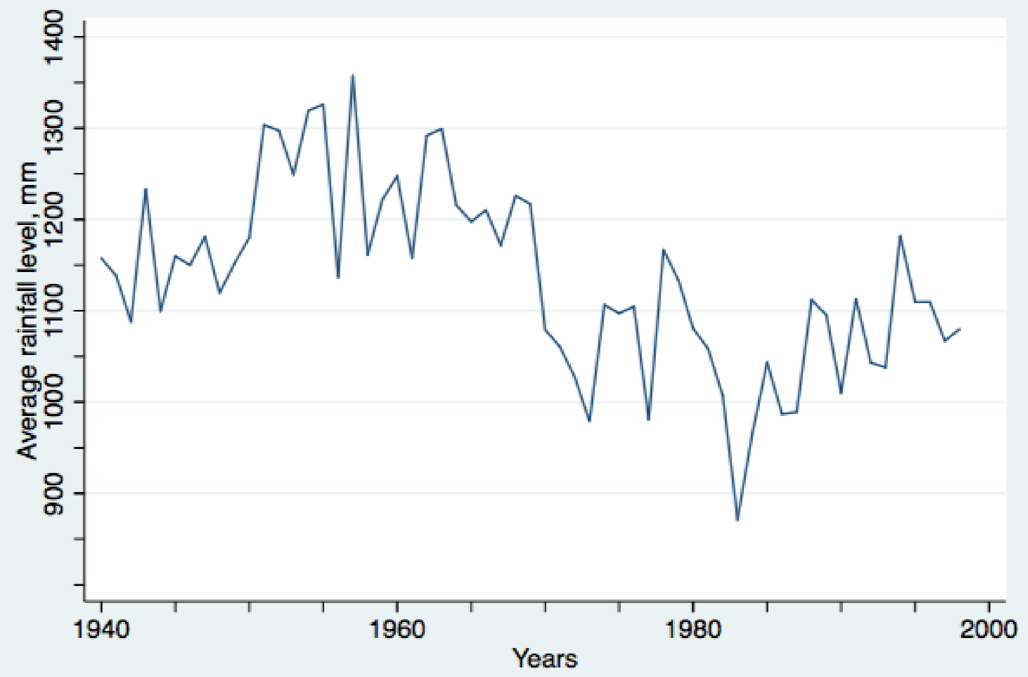


Note: The '10' category contains those women who completed at least 10 years of schooling. Data are sample-weighted (see pages 32-34).



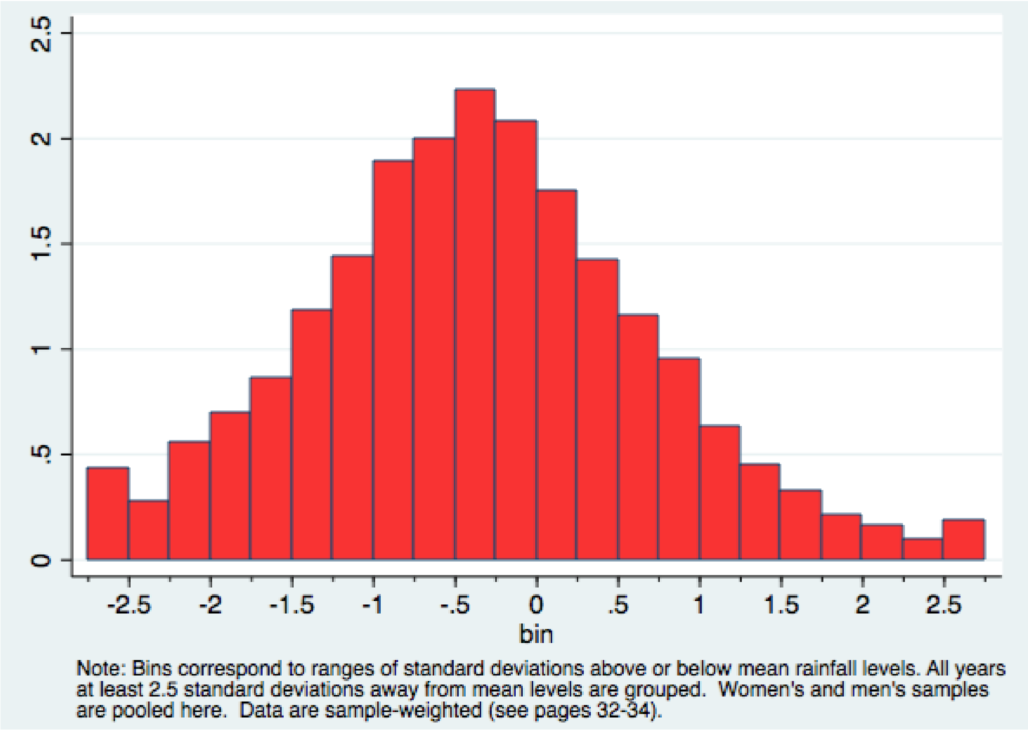
Note: The '10' category contains those men who completed at least 10 years of schooling. Data are sample-weighted (see pages 32-34)

Figure 2.2: Average Rainfall Levels Over Time



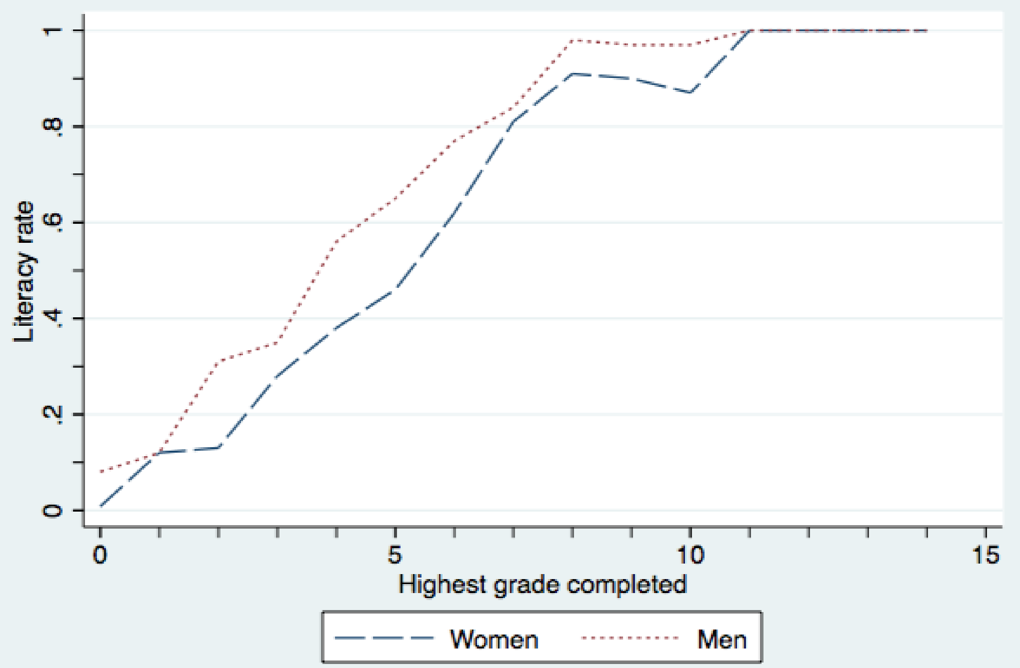
Note: Average values are calculated for all rainfall data set grid points within years after sample-weighting (see pages 32-34).

Figure 2.3: Mean Numbers of Annual Rainfall Levels Experienced Between Birth and Age 21



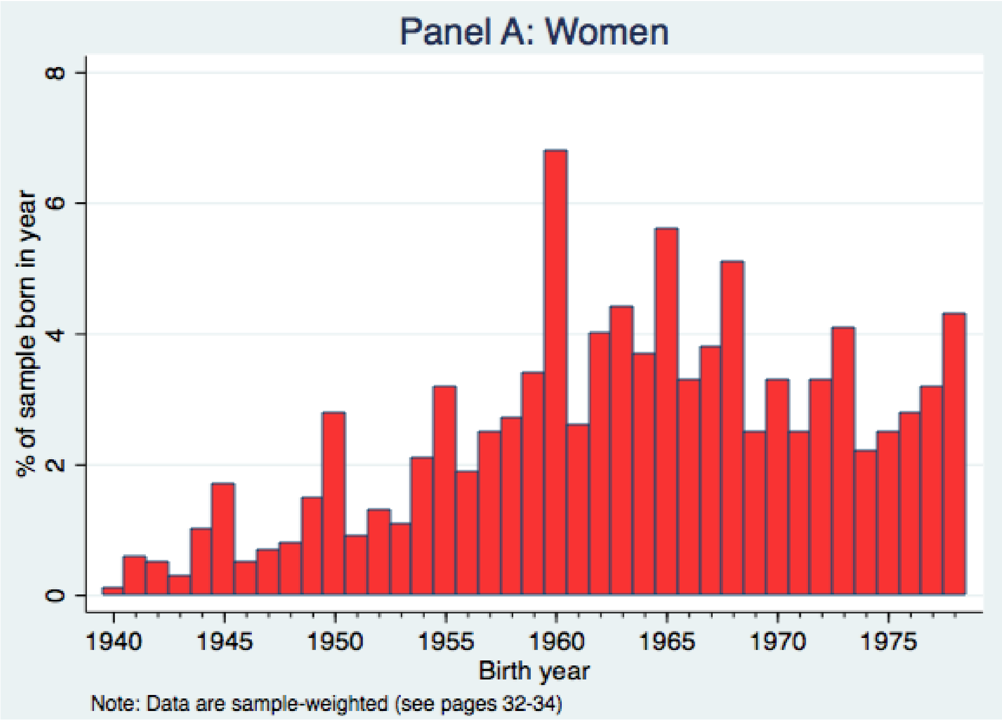
Note: Bins correspond to ranges of standard deviations above or below mean rainfall levels. All years at least 2.5 standard deviations away from mean levels are grouped. Women's and men's samples are pooled here. Data are sample-weighted (see pages 32-34).

Figure 2.4: Literacy Rates by Highest Grade Completed

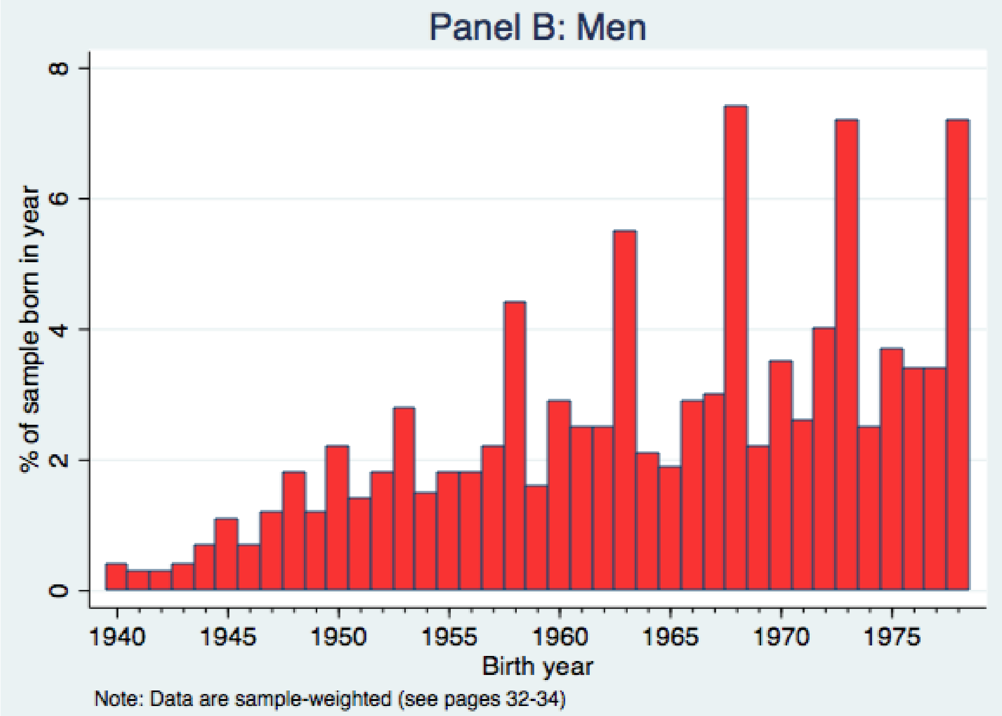


Note: Individuals are considered literate if they can read with ease or with difficulty. Data are sample-weighted (see pages 32-34).

Figure 2.5: Age Heaping for Women's and Men's Samples

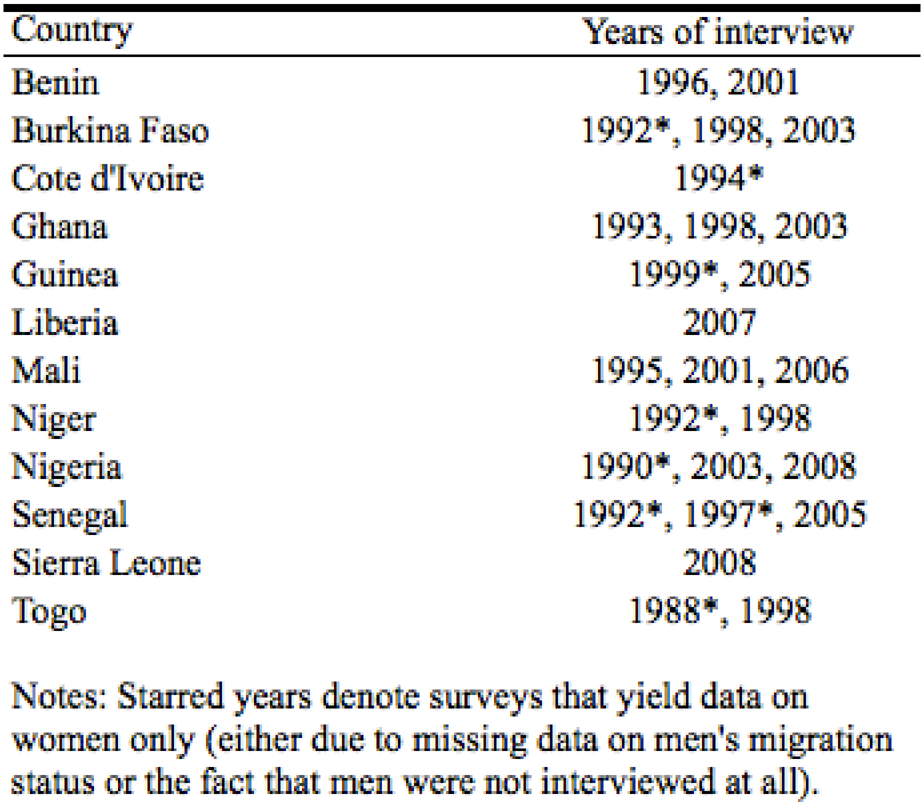


Note: Data are sample-weighted (see pages 32-34)



Note: Data are sample-weighted (see pages 32-34)

Table 2.1: Household Survey Countries and Years of Interview



Country	Years of interview
Benin	1996, 2001
Burkina Faso	1992*, 1998, 2003
Cote d'Ivoire	1994*
Ghana	1993, 1998, 2003
Guinea	1999*, 2005
Liberia	2007
Mali	1995, 2001, 2006
Niger	1992*, 1998
Nigeria	1990*, 2003, 2008
Senegal	1992*, 1997*, 2005
Sierra Leone	2008
Togo	1988*, 1998

Notes: Starred years denote surveys that yield data on women only (either due to missing data on men's migration status or the fact that men were not interviewed at all).

Table 2.2: Descriptive Statistics

	Women	Men
Literacy rate:		
All countries	0.17	0.44
Benin	0.06	0.22
Burkina Faso	0.02	0.11
Cote d'Ivoire	0.13	-
Ghana	0.28	0.48
Guinea	0.02	0.13
Liberia	0.18	0.54
Mali	0.04	0.12
Niger	0.03	0.11
Nigeria	0.20	0.57
Senegal	0.10	0.30
Sierra Leone	0.05	0.16
Togo	0.20	0.52
Literacy rate, pre-2000 surveys	0.14	0.26
Literacy rate, post-1999 surveys	0.19	0.48
Average highest grade completed:		
All countries	1.71	4.10
Benin	0.42	1.42
Burkina Faso	0.20	0.57
Cote d'Ivoire	0.82	-
Ghana	3.88	5.24
Guinea	0.25	0.92
Liberia	1.47	4.78
Mali	0.33	0.74
Niger	0.25	0.53
Nigeria	2.03	5.35
Senegal	0.61	1.70
Sierra Leone	0.62	1.60
Togo	1.38	3.56
Percent of sample born between:		
1940 and 1945	4.28	3.19
1946 and 1950	6.30	7.18
1951 and 1955	8.80	9.21
1956 and 1960	17.24	12.94
1961 and 1965	20.39	14.57
1966 and 1970	17.99	19.04
1971 and 1975	14.72	19.93
1976 and 1978	10.27	13.94
Mean number of wet years between birth and age 6	0.80	0.83
Mean number of wet years between age 7 and age 13	0.54	0.59
Mean number of wet years between age 14 and age 20	0.65	0.74
Mean number of dry years between birth and age 6	1.61	1.69
Mean number of dry years between age 7 and age 13	2.00	1.95
Mean number of dry years between age 14 and age 20	1.85	1.80
Sample size	41,991	16,496
Number of rainfall neighborhoods	978	885
Average number of respondents per rainfall neighborhood	42.94	18.64

Notes: All means are weighted to account for sampling weights and population differences across countries. See pages 32-34 for details.

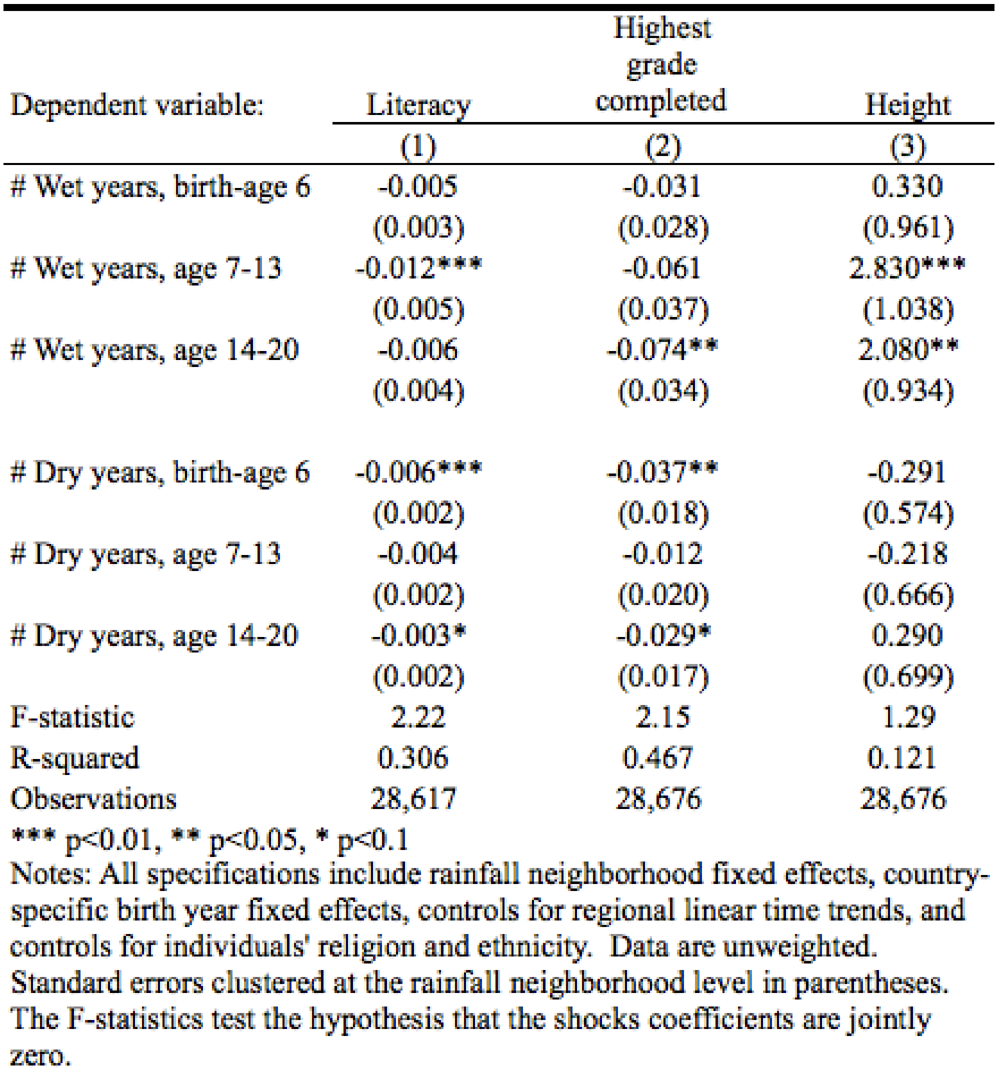
Table 2.3: Baseline Human Capital Results for Women and Men

	Women		Men	
Dependent variable:	Literacy	Highest grade completed	Literacy	Highest grade completed
	(1)	(2)	(3)	(4)
# Wet years, birth-age 6	-0.003	-0.021	-0.001	0.064*
	(0.002)	(0.021)	(0.005)	(0.038)
# Wet years, age 7-13	-0.012***	-0.076**	-0.005	-0.098**
	(0.004)	(0.031)	(0.005)	(0.048)
# Wet years, age 14-20	-0.012***	-0.120***	0.015***	0.052
	(0.004)	(0.031)	(0.005)	(0.047)
# Dry years, birth-age 6	-0.006***	-0.056***	0.011***	0.075**
	(0.002)	(0.018)	(0.004)	(0.032)
# Dry years, age 7-13	-0.004**	-0.029*	0.007*	0.056*
	(0.002)	(0.017)	(0.004)	(0.034)
# Dry years, age 14-20	-0.003**	-0.033**	0.001	0.012
	(0.002)	(0.014)	(0.004)	(0.029)
F-statistic	4.70	4.38	3.05	1.71
R-squared	0.289	0.433	0.392	0.513
Observations	41,991	42,062	16,496	16,676

*** p<0.01, ** p<0.05, * p<0.1

Notes: All specifications include rainfall neighborhood fixed effects, country-specific birth year fixed effects, controls for regional linear time trends, and controls for individuals' religion and ethnicity. Data are unweighted. Standard errors clustered at the rainfall neighborhood level in parentheses. The F-statistics test the hypothesis that the shocks coefficients are jointly zero.

Table 2.4: Women's Height Results

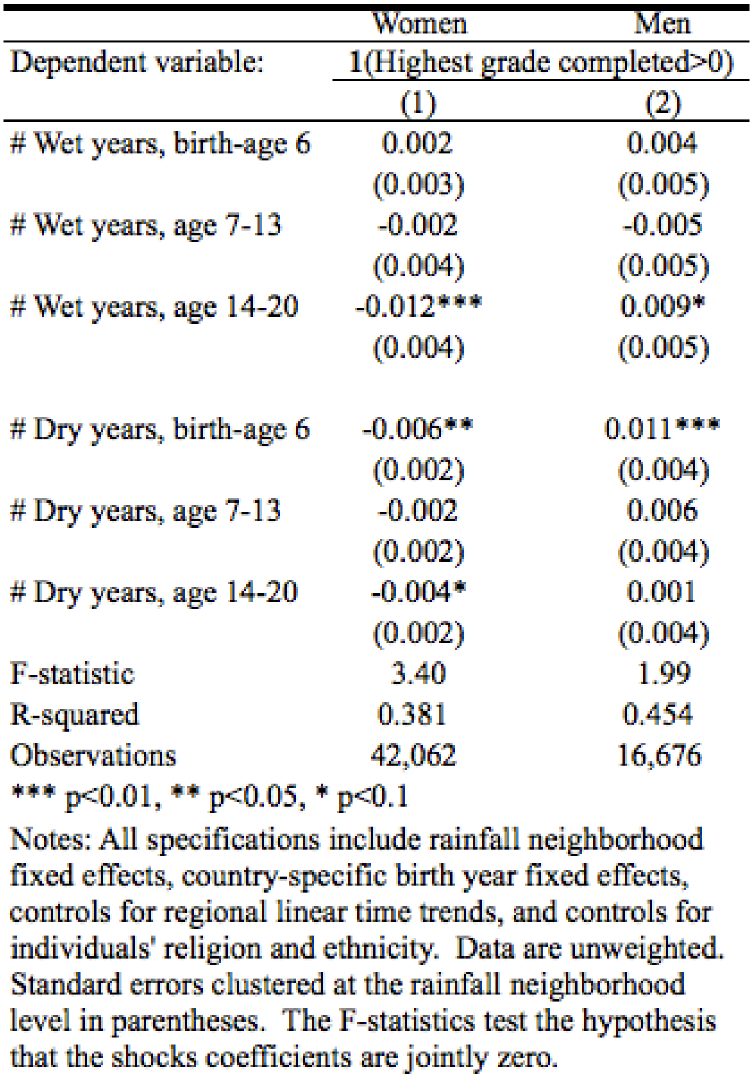


Dependent variable:	Literacy	Highest grade completed	Height
	(1)	(2)	(3)
# Wet years, birth-age 6	-0.005	-0.031	0.330
	(0.003)	(0.028)	(0.961)
# Wet years, age 7-13	-0.012***	-0.061	2.830***
	(0.005)	(0.037)	(1.038)
# Wet years, age 14-20	-0.006	-0.074**	2.080**
	(0.004)	(0.034)	(0.934)
# Dry years, birth-age 6	-0.006***	-0.037**	-0.291
	(0.002)	(0.018)	(0.574)
# Dry years, age 7-13	-0.004	-0.012	-0.218
	(0.002)	(0.020)	(0.666)
# Dry years, age 14-20	-0.003*	-0.029*	0.290
	(0.002)	(0.017)	(0.699)
F-statistic	2.22	2.15	1.29
R-squared	0.306	0.467	0.121
Observations	28,617	28,676	28,676

*** p<0.01, ** p<0.05, * p<0.1

Notes: All specifications include rainfall neighborhood fixed effects, country-specific birth year fixed effects, controls for regional linear time trends, and controls for individuals' religion and ethnicity. Data are unweighted. Standard errors clustered at the rainfall neighborhood level in parentheses. The F-statistics test the hypothesis that the shocks coefficients are jointly zero.

Table 2.5: Schooling Attainment (Extensive Margin) Results



	Women	Men
Dependent variable:	1(Highest grade completed>0)	
	(1)	(2)
# Wet years, birth-age 6	0.002	0.004
	(0.003)	(0.005)
# Wet years, age 7-13	-0.002	-0.005
	(0.004)	(0.005)
# Wet years, age 14-20	-0.012***	0.009*
	(0.004)	(0.005)
# Dry years, birth-age 6	-0.006**	0.011***
	(0.002)	(0.004)
# Dry years, age 7-13	-0.002	0.006
	(0.002)	(0.004)
# Dry years, age 14-20	-0.004*	0.001
	(0.002)	(0.004)
F-statistic	3.40	1.99
R-squared	0.381	0.454
Observations	42,062	16,676

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: All specifications include rainfall neighborhood fixed effects, country-specific birth year fixed effects, controls for regional linear time trends, and controls for individuals' religion and ethnicity. Data are unweighted. Standard errors clustered at the rainfall neighborhood level in parentheses. The F-statistics test the hypothesis that the shocks coefficients are jointly zero.

Table 2.6: Shocks Numbers Indicators Results

	Women		Men	
Dependent varaible:	Literacy	Highest grade completed	Literacy	Highest grade completed
	(1)	(2)	(3)	(4)
1(#WetYears, birth-age 6=1)	-0.009*	-0.058	0.002	0.255***
	(0.005)	(0.043)	(0.010)	(0.079)
1(#WetYears, birth-age 6=2)	0.004	0.053	0.014	0.261**
	(0.007)	(0.061)	(0.013)	(0.111)
1(#WetYears, birth-age	-0.014*	-0.120*	-0.010	0.127
	(0.008)	(0.071)	(0.015)	(0.133)
1(#WetYears, age 7-13=1)	-0.019***	-0.125***	-0.009	-0.079
	(0.005)	(0.047)	(0.010)	(0.082)
1(#WetYears, age 7-13=2)	-0.017*	-0.114	-0.012	-0.180
	(0.010)	(0.086)	(0.015)	(0.128)
1(#WetYears, age 7-13=3+)	-0.027**	-0.166	-0.018	-0.309*
	(0.013)	(0.103)	(0.019)	(0.180)
1(#WetYears, age 14-20=1)	-0.012**	-0.137***	0.001	0.042
	(0.005)	(0.042)	(0.009)	(0.076)
1(#WetYears, age 14-20=2)	-0.017*	-0.214***	0.033**	0.159
	(0.009)	(0.081)	(0.014)	(0.120)
1(#WetYears, age 14-20=3+)	-0.052***	-0.402***	0.055***	0.238
	(0.013)	(0.106)	(0.019)	(0.166)
1(#DryYears, birth-age 6=1)	-0.011**	-0.100**	0.011	0.124
	(0.005)	(0.043)	(0.010)	(0.086)
1(#DryYears, birth-age 6=2)	-0.006	-0.107*	0.023*	0.192*
	(0.006)	(0.055)	(0.012)	(0.110)
1(#DryYears, birth-age	-0.024***	-0.228***	0.027**	0.183
	(0.007)	(0.069)	(0.014)	(0.123)
1(#DryYears, age 7-13=1)	-0.001	-0.018	0.011	0.060
	(0.007)	(0.051)	(0.011)	(0.103)
1(#DryYears, age 7-13=2)	-0.006	-0.039	0.009	0.006
	(0.007)	(0.064)	(0.013)	(0.125)
1(#DryYears, age 7-13=3+)	-0.010	-0.083	0.023	0.137
	(0.008)	(0.070)	(0.015)	(0.140)
1(#DryYears, age 14-20=1)	-0.001	-0.023	0.002	0.111
	(0.005)	(0.043)	(0.011)	(0.098)
1(#DryYears, age 14-20=2)	-0.003	-0.036	0.008	0.073
	(0.006)	(0.052)	(0.013)	(0.111)
1(#DryYears, age 14-20=3+)	-0.013**	-0.111**	-0.007	0.005
	(0.006)	(0.056)	(0.015)	(0.124)
F-statistic	2.92	2.18	1.24	1.24
R-squared	0.289	0.434	0.392	0.513
Observations	41,991	42,062	16,496	16,676

*** p<0.01, ** p<0.05, * p<0.1

Notes: All specifications include rainfall neighborhood fixed effects, country-specific birth year fixed effects, controls for regional linear time trends, and controls for individuals' religion and ethnicity. Data are unweighted. Standard errors clustered at the rainfall neighborhood level in parentheses. The F-statistics test the hypothesis that the shocks coefficients are jointly zero.

Table 2.7: Baseline Results by Agroecological Zone

	Women		Men	
Dependent variable:	Literacy	Highest grade completed	Literacy	Highest grade completed
	(1)	(2)	(3)	(4)
# Wet years, birth-age 6	-0.004	-0.084**	0.012*	0.117*
	(0.004)	(0.042)	(0.007)	(0.063)
# Wet years, birth-age 6*Arid	0.002	0.107**	-0.024***	-0.086
	(0.005)	(0.049)	(0.009)	(0.075)
# Wet years, age 7-13	-0.020***	-0.166***	-0.009	-0.086
	(0.006)	(0.055)	(0.008)	(0.072)
# Wet years, age 7-13*Arid	0.015**	0.161**	0.005	-0.017
	(0.007)	(0.065)	(0.010)	(0.091)
# Wet years, age 14-20	-0.016***	-0.171***	0.012*	0.030
	(0.006)	(0.047)	(0.007)	(0.062)
# Wet years, age 14-20*Arid	0.006	0.089	0.005	0.042
	(0.007)	(0.056)	(0.009)	(0.085)
# Dry years, birth-age 6	-0.005	-0.046	0.009	0.101*
	(0.004)	(0.033)	(0.006)	(0.053)
# Dry years, birth-age 6*Arid	-0.002	-0.021	0.004	-0.052
	(0.004)	(0.037)	(0.008)	(0.067)
# Dry years, age 7-13	-0.005	-0.040	0.004	0.063
	(0.003)	(0.031)	(0.006)	(0.054)
# Dry years, age 7-13*Arid	0.001	0.018	0.006	-0.013
	(0.004)	(0.036)	(0.008)	(0.066)
# Dry years, age 14-20	-0.005*	-0.066**	-0.004	-0.030
	(0.003)	(0.028)	(0.006)	(0.052)
# Dry years, age 14-20*Arid	0.003	0.059*	0.009	0.077
	(0.004)	(0.033)	(0.008)	(0.065)
R-squared	0.289	0.434	0.392	0.513
Observations	41,991	42,062	16,496	16,676

*** p<0.01, ** p<0.05, * p<0.1

Notes: All specifications include rainfall neighborhood fixed effects, country-specific birth year fixed effects, controls for regional linear time trends, and controls for individuals' religion and ethnicity. Data are unweighted. Standard errors clustered at the rainfall neighborhood level in parentheses.

Table 2.8: Region-Specific Birth Year Fixed Effects Results

	Women		Men	
Dependent variable:	Literacy	Highest grade completed	Literacy	Highest grade completed
	(1)	(2)	(3)	(4)
# Wet years, birth-age 6	-0.004	-0.041*	-0.004	0.060
	(0.002)	(0.023)	(0.005)	(0.044)
# Wet years, age 7-13	-0.013***	-0.084**	-0.006	-0.132**
	(0.004)	(0.034)	(0.006)	(0.051)
# Wet years, age 14-20	-0.010***	-0.105***	0.012**	0.033
	(0.004)	(0.035)	(0.006)	(0.052)
# Dry years, birth-age 6	-0.004**	-0.034**	0.005	0.038
	(0.002)	(0.016)	(0.004)	(0.032)
# Dry years, age 7-13	-0.004*	-0.039**	0.005	0.057*
	(0.002)	(0.017)	(0.004)	(0.033)
# Dry years, age 14-20	-0.003*	-0.024	0.000	-0.013
	(0.002)	(0.015)	(0.004)	(0.030)
F-statistic	3.56	2.64	2.04	1.75
R-squared	0.309	0.453	0.436	0.547
Observations	41,701	41,772	16,248	16,426

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: All specifications include rainfall neighborhood fixed effects, region-specific birth year fixed effects, and controls for individuals' religion and ethnicity. Data are unweighted. Standard errors clustered at the rainfall neighborhood level in parentheses. The F-statistics test the hypothesis that the shocks coefficients are jointly zero.

Table 2.9: Rainfall Shocks and Numbers of Living Non-Migrants

Dependent variable:	Number of non-migrant women		Number of non-migrant men	
	(1)	(2)	(3)	(4)
1(Year before birth wet)	-0.009	-0.012	0.001	-0.001
	(0.013)	(0.013)	(0.010)	(0.010)
# Wet years, birth-age 6	-0.003	-0.006	-0.001	-0.003
	(0.006)	(0.006)	(0.004)	(0.004)
# Wet years, age 7-13	0.015**	0.010*	0.006	0.003
	(0.006)	(0.006)	(0.005)	(0.004)
# Wet years, age 14-20	0.023***	0.016**	0.016***	0.009*
	(0.007)	(0.007)	(0.005)	(0.005)
# Wet years, age 21-survey year	0.029***		0.019***	
	(0.007)		(0.006)	
Average number of wet years between age 21 and the survey year		0.218***		0.082*
		(0.059)		(0.049)
1(Year before birth dry)	-0.004	-0.005	-0.001	0.004
	(0.012)	(0.012)	(0.009)	(0.009)
# Dry years, birth-age 6	0.005	0.002	0.008*	0.013***
	(0.006)	(0.005)	(0.004)	(0.003)
# Dry years, age 7-13	0.002	-0.002	-0.004	0.002
	(0.006)	(0.005)	(0.005)	(0.004)
# Dry years, age 14-20	-0.007	-0.011**	-0.007	-0.001
	(0.006)	(0.005)	(0.005)	(0.004)
# Dry years, age 21-survey year	0.015**		-0.004	
	(0.006)		(0.005)	
Average number of dry years between age 21 and the survey year		0.243***		0.146***
		(0.056)		(0.051)
F-statistic	4.99	5.38	3.37	3.03
R-squared	0.232	0.232	0.173	0.173
Observations	68,943	68,943	52,578	52,578
Number of rainfall neighborhoods	978	978	885	885

*** p<0.01, ** p<0.05, * p<0.1

Notes: All specifications include rainfall neighborhood fixed effects, country-specific birth year fixed effects, controls for regional linear time trends, and survey year fixed effects. Data are unweighted. Standard errors in parentheses. The F-statistics test the hypothesis that the shocks coefficients are jointly zero.

Table 2.10: Rainfall Shocks and (Sibling) Mortality Results

	Women			Men		
Dependent variable:	Died by age 20	Died by age 30	Died by age 35	Died by age 20	Died by age 30	Died by age 39
	(1)	(2)	(3)	(4)	(5)	(6)
# Wet years, birth-age 6	-0.002	-0.006	-0.001	0.008**	0.008*	0.007
	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.006)
# Wet years, age 7-13	-0.007*	-0.014***	-0.014**	0.006	0.007	0.001
	(0.004)	(0.005)	(0.006)	(0.004)	(0.005)	(0.008)
# Wet years, age 14-20	0.001	-0.004	-0.010	-0.003	-0.002	-0.015
	(0.004)	(0.005)	(0.007)	(0.003)	(0.006)	(0.009)
# Dry years, birth-age 6	-0.005*	-0.008**	-0.007	-0.001	0.001	0.003
	(0.002)	(0.003)	(0.005)	(0.003)	(0.004)	(0.007)
# Dry years, age 7-13	-0.001	-0.002	0.001	-0.001	-0.002	0.006
	(0.002)	(0.003)	(0.004)	(0.003)	(0.004)	(0.006)
# Dry years, age 14-20	-0.005*	-0.007**	-0.006*	0.002	0.000	0.003
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.005)
F-statistic	0.147	0.006	0.091	0.197	0.462	0.402
R-squared	0.069	0.081	0.099	0.071	0.084	0.121
Observations	55,948	38,115	25,164	60,920	41,938	18,742

*** p<0.01, ** p<0.05, * p<0.1

Notes: All specifications include rainfall neighborhood fixed effects, country-specific birth year fixed effects, controls for regional linear time trends, and controls for individuals' religion and ethnicity. Data are unweighted. Standard errors clustered at the rainfall neighborhood level in parentheses. The F-statistics are associated with tests of the hypothesis that the shocks coefficients are jointly zero.

Table 2.11: Effects of Shocks on Mortality and Implied Effects on Migration, Per Average Rainfall Neighborhood

	Mortality change in number of individuals in average rainfall neighborhood from shock (1)	Implied effect of shock on number of individuals in average rainfall neighborhood who migrate (2)	Mortality change in number of individuals in average rainfall neighborhood from shock, times 3 (3)	Implied effect of shock on number of individuals in average rainfall neighborhood who migrate (4)
	Women			
# Wet years, birth-age 6	-	-	-	-
# Wet years, age 7-13	0.0806	0.0104	0.2426	0.1715
# Wet years, age 14-20	0	-0.1102	0	-0.1102
# Dry years, birth-age 6	-0.0450	-0.0450	-0.1351	-0.1351
# Dry years, age 7-13	-	-	-	-
# Dry years, age 14-20	-0.0396	0.1148	-0.1377	0.1941
	Men			
# Wet years, birth-age 6	0.0224	-0.0224	0.0672	-0.0672
# Wet years, age 7-13	-	-	-	-
# Wet years, age 14-20	0	-0.0637	0	-0.0637
# Dry years, birth-age 6	0	-0.0553	0	-0.0553
# Dry years, age 7-13	-	-	-	-
# Dry years, age 14-20	-	-	-	-

Notes: Mortality changes are based on the maximum point estimate across columns in Table 9. Implied migration changes are based on the difference between the point estimate from Table 8 that minimizes emigration or maximizes immigration and the mortality change. All results derived using unweighted data.

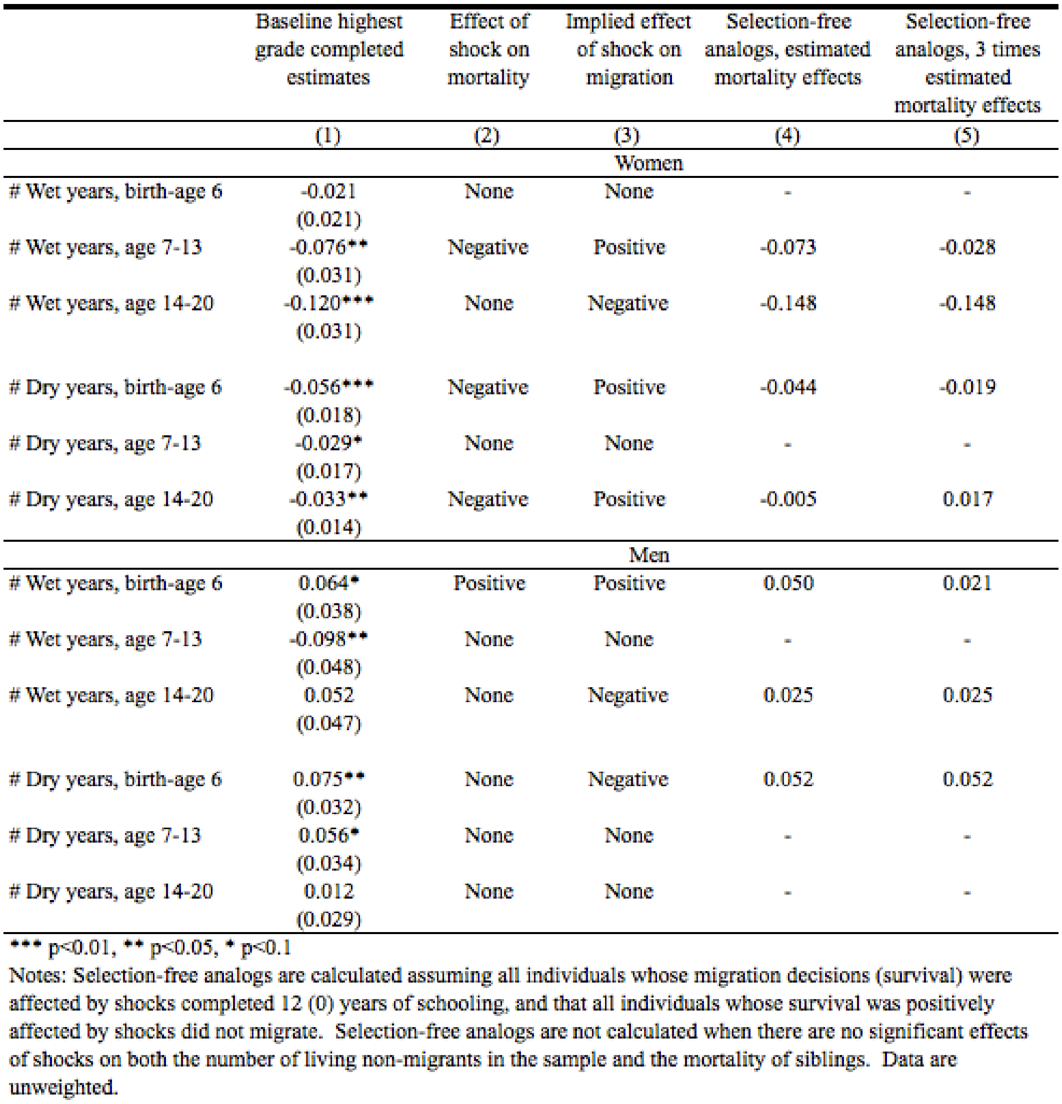
Table 2.12: Baseline Literacy Estimates and Their Sample Selection-Free Analogs

	Baseline literacy estimates	Effect of shock on mortality	Implied effect of shock on migration	Selection-free analogs, estimated mortality effects	Selection-free analogs, 3 times estimated mortality effects
	(1)	(2)	(3)	(4)	(5)
			Women		
# Wet years, birth-age 6	-0.003 (0.002)	None	None	-	-
# Wet years, age 7-13	-0.012*** (0.004)	Negative	Positive	-0.011	-0.007
# Wet years, age 14-20	-0.012*** (0.004)	None	Negative	-0.014	-0.014
# Dry years, birth-age 6	-0.006*** (0.002)	Negative	Positive	-0.005	-0.003
# Dry years, age 7-13	-0.004** (0.002)	None	None	-	-
# Dry years, age 14-20	-0.003** (0.002)	Negative	Positive	-0.001	0.001
			Men		
# Wet years, birth-age 6	-0.001 (0.005)	Positive	Positive	-0.002	-0.004
# Wet years, age 7-13	-0.005 (0.005)	None	None	-	-
# Wet years, age 14-20	0.015*** (0.005)	None	Negative	0.013	0.013
# Dry years, birth-age 6	0.011*** (0.004)	None	Negative	0.009	0.009
# Dry years, age 7-13	0.007* (0.004)	None	None	-	-
# Dry years, age 14-20	0.001 (0.004)	None	None	-	-

*** p<0.01, ** p<0.05, * p<0.1

Notes: Selection-free analogs are calculated assuming all individuals whose migration decisions (survival) were affected by shocks are literate (illiterate), and that all individuals whose survival was positively affected by shocks did not migrate. Selection-free analogs are not calculated when there are no significant effects of shocks on both the number of living non-migrants in the sample and the mortality of siblings. Data are unweighted.

Table 2.13: Baseline Highest Grade Completed Estimates and Their Sample Selection-Free Analogs



	Baseline highest grade completed estimates	Effect of shock on mortality	Implied effect of shock on migration	Selection-free analogs, estimated mortality effects	Selection-free analogs, 3 times estimated mortality effects
	(1)	(2)	(3)	(4)	(5)
			Women		
# Wet years, birth-age 6	-0.021 (0.021)	None	None	-	-
# Wet years, age 7-13	-0.076** (0.031)	Negative	Positive	-0.073	-0.028
# Wet years, age 14-20	-0.120*** (0.031)	None	Negative	-0.148	-0.148
# Dry years, birth-age 6	-0.056*** (0.018)	Negative	Positive	-0.044	-0.019
# Dry years, age 7-13	-0.029* (0.017)	None	None	-	-
# Dry years, age 14-20	-0.033** (0.014)	Negative	Positive	-0.005	0.017
			Men		
# Wet years, birth-age 6	0.064* (0.038)	Positive	Positive	0.050	0.021
# Wet years, age 7-13	-0.098** (0.048)	None	None	-	-
# Wet years, age 14-20	0.052 (0.047)	None	Negative	0.025	0.025
# Dry years, birth-age 6	0.075** (0.032)	None	Negative	0.052	0.052
# Dry years, age 7-13	0.056* (0.034)	None	None	-	-
# Dry years, age 14-20	0.012 (0.029)	None	None	-	-

*** p<0.01, ** p<0.05, * p<0.1

Notes: Selection-free analogs are calculated assuming all individuals whose migration decisions (survival) were affected by shocks completed 12 (0) years of schooling, and that all individuals whose survival was positively affected by shocks did not migrate. Selection-free analogs are not calculated when there are no significant effects of shocks on both the number of living non-migrants in the sample and the mortality of siblings. Data are unweighted.

Chapter 3: Food Price Inflation and Children's Health

3.1 Introduction

People in developing countries often list inflation as one of their chief concerns (Easterly and Fischer, 2001). This is perhaps not surprising since typical household strategies for maintaining adequate consumption levels, such as borrowing from relatives or friends or increasing labor supply, might be unavailable or inadequate when price increases are sufficiently large. This is especially likely to be true when the goods whose prices have increased do not have close substitutes and they are an extremely important part of the typical consumption basket, as with food. Indeed, the expected changes in consumption habits that accompany food price inflation–including the consumption of smaller portions, fewer meals, or cheaper (per unit) but less nutritious substitute foods (Cohen and Garrett, 2009)–could negatively affect health, perhaps permanently in the case of children (Alderman et al., 2006). Indeed, the food price crisis of mid-2008 brought with it reports of increased rates of malnutrition and under-nutrition (IFAD, 2008). In the case of Egypt (the setting of this chapter), this is reflected in the 90 percent increase in the proportion of children whose weight-for-height was at least 2 standard deviations lower than that of an international reference group of children between earlier years and 2008 (see Figure

3.1). But despite all the attention from the press, policymakers and international organizations that this crisis received, it is still not clear whether different kinds of households are helped or hurt by this inflation (Headey, 2011).

In particular, little is known about whether urban or rural households are more severely affected or whether the latter are negatively affected at all. While urban households are typically richer than rural households and therefore better placed to cope with food price inflation, urban households of course tend to be net food buying households. [1] Rural households, on the other hand, are typically more vulnerable due to their lower household incomes, but these households could benefit from higher food prices if higher output prices make agriculture sufficiently profitable. The common expectation, however, is that urban households would decrease quantities consumed (on average, at least) while at least some rural households would benefit from higher food prices (Fujii, 2011; OECD, 2008; Cohen and Garrett, 2009; Ivanic and Martin, 2014). Consistent with this, Ferreira et al. (2011), who study household welfare in Brazil in 2008, find that the poorest net food buyers were substantially negatively affected by high food prices, while the rural poor actually benefited from the higher food prices. On the other hand, Gibson and Kim (2013) find that given consumers' tendencies to switch to cheaper but equally nutritious rice, the elasticity of calories with respect to rice prices is quite low. If quality adjustments are in fact empirically important, then perhaps households can cope with high food prices rather easily. This chapter examines the evolution over time of Egyptian children's

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[1] This is particularly true in Egypt, where only about 4 percent of workers classified as residing in urban areas are employed in agriculture according to the 2008 Demographic and Health Survey.

weight in urban and rural areas to shed light on how children in different types of households seem to actually be affected by rapid food price increases.

More specifically, the evolution of Egyptian children's weight-for-height for children from urban, rural agricultural and rural non-agricultural households is analyzed here. This is done using four highly comparable household surveys conducted between 2000 and 2008, a year in which world food (and other) price inflation was especially high. Data on parents' occupation along with information on when and where unusually high-priced crops were being harvested allow for consideration of how the weights of children from what were likely net food selling households evolved. In addition to examining the evolution over time of the weights-for-height of children in different types of households, I also use quantile regression techniques to examine changes in the (conditional) weight-for-height distribution as a function of these same factors. This is highly appropriate given what are likely widely diverging household coping capabilities across the weight-for-height distribution; while heavier children might have lived in households that were able to maintain consumption despite inflation, lighter children were likely more vulnerable to food price rises. Also, we might naturally be more interested in the evolution of something like weight-for-height for the lightest children, given their stronger need for some sort of assistance. To be clear, I do not claim to identify the causal effect of high food prices on the weight-for-height of different children. Rather, I compare changes in different groups of children's weight-for-height over time under the assumption that food prices had effects that were powerful enough that these effects will be large relative to other time-varying factors, and it will therefore be clear which children were affected most severely and in which directions.

This chapter is related to the literature on the effects of food price inflation in developing countries. Most of the work in this literature relies on simulations or, when household survey data is used, subjective measures of well-being (Headey, 2011). Thus, one of this chapter's main contributions is to use a relatively objective measure of child well-being from nationally representative household survey data to examine who might have been affected and how by the food price crisis of 2008. This chapter is also novel in that it simply tracks the evolution of different groups' of children's weights-for-height over time, in contrast to the papers based on simulation results that it complements. Many of those papers rely heavily on assumptions and do not account for the full range of factors that can change over time along with food prices, for example. This chapter therefore provides a sort of record that simulation results might usefully be compared to. I do this by using Demographic and Household Survey data collected in Egypt–a country seriously affected by the crisis due to its heavy dependence on imported staple foods–in 2000, 2003, 2005 and 2008 (in April-June, the height of the crisis).

I find that the effects of food price inflation tend to have been negative for children at the bottom of the conditional weight-for-height distribution, and this is true for most of the region- and parental occupation-based groups I consider. While most of the decreases in weight-for-height at the first decile of the conditional distribution of weight-for-height between 2005 and 2008 are fairly small, given the growth of the Egyptian economy over this time period it is somewhat striking that there were any decreases at all. (Indeed, lower rates of growth over a shorter period of time resulted in substantial gains in weight-for-height in most cases by the run-up to measurement in 2005.) I also find that, if anything, the set of children expected to have been most likely to have benefited from high food prices seem to have been substantially negatively affected by them. While the change in weights-for-height amongst these children between 2005 and 2008 is not statistically significant at conventional levels, it is close to being so, and the magnitudes of the changes for these children are large. More specifically, children from the region of rural Egypt where high-priced staple crops were being harvested and sold around the time of measurement in 2008, and whose parents were self-employed farmers, registered what appear to be some of the largest decreases in weight-for-height between 2005 and 2008. On the other hand, several groups of children from net food buying households actually gained weight over this time period, including heavier urban children and rural children whose parents did not work in agriculture. Overall, these results suggest that for the most disadvantaged Egyptian children, the negative effects of high food prices outweighed any possible beneficial effects of economic growth. Also, for rural net food selling households, the results are consistent with the possibility that high input prices had negative effects that were stronger than the beneficial effects of high agricultural output prices.

Section 3.2 provides background on the 2008 food price crisis in Egypt and considers conceptual issues. Section 3.3 describes the empirical analysis, the data and the sample selection criteria. Section 3.4 presents descriptive statistics and main results. Section 3.5 concludes.

3.2 Background and Conceptual Issues

3.2.1 The 2008 Food Price Crisis in Egypt

Between 2005 and mid-2008, world prices of wheat, coarse grains and rice all roughly doubled (OECD, 2008). The causes of these price increases included droughts in major grain-producing areas, low baseline stocks of cereals, increased demand for maize for the production of biofuels, rising oil prices and the continuing devaluation of the US dollar (the currency in which prices for these commodities are usually quoted). Although the (real) prices in mid-2008 of these staples were far from unprecedented, the price of wheat, for example, had not been as high as it was then since the early 1980s. According to the FAO, between 2006 and 2008, the world prices of cereals and oils increased by as much as 140 percent, while wheat prices rose by 250 percent. It was not only the prices of foods that were rising so rapidly, however; petroleum and agricultural input prices (which includes fertilizer prices) increased by 160 percent in the year to August, 2008. This is noteworthy, since Egyptian farmers use nitrogenous fertilizers highly intensively (Kherallah et al., 1999).

Egypt's most important staple foods are wheat, maize and rice. Moreover, Egyptian consumers are highly vulnerable to increases in the world prices of wheat and maize, as the country is the world's largest importer of the former and is a major importer of the latter. At the household level, more than half of all household consumption expenditure is on food. On the other hand, Egypt is a net exporter of rice (Arab Republic of Egypt Initiative on Soaring Food Prices, 2008). The food price crisis of 2008 was widely reported to have taken a serious toll on Egyptian consumers. According to Egypt's Central Agency for Public Mobilization and Statistics, overall (annual) consumer price inflation peaked in August, 2008, at 25.6 percent, and year-on-year food price inflation then stood at 35.5 percent, with reportedly higher food inflation rates in rural areas (Arab Republic of Egypt Initiative on Soaring Food Prices, 2008). The accuracy of these official inflation rates has been called into question, however, and informal surveys suggest that flour and rice prices might have risen by over 100 percent while vegetable oil prices might have increased by over 200 percent over the period in question (Wikileaks Discussion Forum, 2011). Also, according to the World Food Program, the prices of cereals in Egypt rose by 129 percent between 2006 and 2008 (WFP, 2008). It should be emphasized again, however, that food prices were not the only ones to rise substantially in Egypt in 2008; the prices of petroleum and construction materials also exhibited large increases (Wikileaks Discussion Forum, 2011).

Annual real per capita growth (as well as food price inflation) rates over the time period in question can be seen in Figure 3.2. As the economy slowed down in the early 2000s after several years of fairly high growth rates (with particularly low annual growth rates in 2001 and 2002), so did employment levels. Since the end of 2004, however, as growth picked up it affected more Egyptians' employment prospects positively (Hasan and Sassanpour, 2008). Wage growth also increased after 2003, so that growth likely began increasing household earnings for employed Egyptians around this time. Unemployment statistics, however, are mostly informative with respect to the labor supply of young, relatively well educated Egyptians seeking entry into the labor market. In fact, according to the 2005 Labor Force Sample Survey, 92 percent of unemployed Egyptians were younger than 30 years of age, while recorded unemployment for older Egyptians is very low. For many Egyptian households headed by individuals old enough to have young children, therefore, growth after 2003 would have brought increases in household income. Also, unemployment was evenly spread between rural and urban areas in the early 2000s. Finally, Ersado and Aran (2014) document substantial increases between 2000 and 2008 in access to and consumption of a wide variety of health care treatments for young children in Egypt, which suggests that improved health care was working alongside growth to improve children's physical conditions.

The Egyptian government responded to the food price crisis by expanding its food aid programs, increasing public sector wages and the value of pensions and making changes to trade policies. Egypt's ration card system (which offers eligible households a monthly quota of rice, flour, tea, sugar and oil) went from covering 11.1 million to 26.1 million Egyptians in the second quarter of 2008. However, this program is not well-targeted to the poor and is reportedly riddled with corruption (Wikileaks Discussion Forum, 2011). Existing evidence indicates that all but 20 percent of the poorest and most vulnerable Egyptians fail to take advantage of the ration cards, primarily because poor Egyptians rarely possess the national identification cards required to sign up and they live in sufficiently remote areas (Arab Republic of Egypt Initiative on Soaring Food Prices, 2008). Also, the goods meant to be sold to ration card holders were sold by merchants who sold the same goods at market prices, and this reportedly presented too tempting an arbitrage opportunity to pass up (Wikileaks Discussion Forum, 2011). There is also a subsidized bread program that is open to everyone. It is not clear, however, that this program does much to bolster the food security of those Egyptians who need help the most. Reportedly, as corn prices rose and the gap between the prices of subsidized and unsubsidized flour widened, an increasing number of bakers and millers began selling their quotas' worth of subsidized flour on the black market, leading to shortages for all would-be program beneficiaries (Wikileaks Discussion Forum, 2011).

The Egyptian government took other steps to help particular groups. In May, 2008, public sector workers' wages were increased by 30 percent and public sector retirees' pension payments were increased by 20 percent. Also, like many other developing countries, Egypt made changes to its trade policies in mid-2008 in an attempt to shield its citizens from the effects of higher food prices. In particular, import tariffs for food products were lowered to 6.5 percent on average (the lowest level in over a decade) in the first half of 2008 and the government banned the export of rice in April, 2008. The latter policy change is believed to have reduced domestic rice prices to some extent.

3.2.2 Agriculture in Egypt

Agriculture in Egypt is highly geographically concentrated on about 3 percent of the country's land, much of which is near the Nile river. About 35 percent of all Egyptians live in farm households (Siam, 2002). The most common type of farm is slightly larger than one (highly productive) acre in size and is located in canal-irrigated areas near the Nile. This Nile-fed irrigation allows Egyptian farmers to harvest as many as 3 crops per year. Most Egyptians who work in agriculture either work on small farms or work as tenants or shareholders, and the majority of village residents either have no or little land, and cultivate as tenants or smallholders. In addition, for many agricultural workers, farm work is merely one of multiple types of casual labor performed over the course of a year (Arab Republic of Egypt Initiative on Soaring Food Prices, 2008).

Climatic variations within Egypt imply that different crops tend to be grown in different locations. For example, Lower Egypt (i.e., northern Egypt) is relatively humid and therefore suitable for the cultivation of cotton and rice. Upper Egypt, on the other hand, is hotter and drier and is more suited to the cultivation of sugarcane, onions and lentils (Metz, 1990). (The terms Lower and Upper refer to the altitude of the Nile River, which flows north from the East African highlands–and relatively high-altitude southern Egypt–to the Mediterranean Sea.) More importantly for the purposes of this chapter, while wheat is grown all over Egypt in the winter season (which lasts from December to the beginning of April), the warmer temperatures in Upper Egypt restrict wheat yields substantially while yields are relatively high in Lower Egypt. Thus, beginning in April, 2008, farmers (who sold any wheat) in Lower Egypt in particular should have begun benefiting from high wheat prices. As far as the production of other staples goes, rice and maize are each sowed in May and are not harvested until October. Thus, to the extent that farmers in Lower Egypt (the only region where rice and maize are cultivated) previously stored their surplus production to sell when prices for these crops increased sharply in early 2008, then the benefits of high food prices should be reflected in children's weights as of the second quarter of 2008 (when that year's household survey was collected). Farmers in Lower Egypt are therefore more likely to have benefited from high food prices compared to other Egyptians, as they were more likely to be net sellers of the staple crops whose prices increased the most. Moreover, in addition to having a climate that is in many ways less hospitable toward the cultivation of crops, Upper Egypt has historically been a poorer and more food-insecure region.

3.2.3 Conceptual Issues and Existing Evidence

Food price inflation would only be harmful if wage inflation was relatively low and, in the event that it was not, households could not easily switch to some substitute set of foods. Alas, while in principal nominal wages could have grown fast enough for the real price of food to have remained stable, evidence suggests that this did not occur. In particular, nominal wage growth in 2008 was roughly 25 percent (Al-Nashar, 2008), which is lower than the food price inflation rate.

The staples whose prices increased so rapidly between 2006 and 2008 are extremely important to the Egyptian diet. Kherallah et al. (1999) note that the average annual consumption of wheat and wheat products is about 200 kilograms per capita. It is not clear precisely how price elastic demand for cereals is; Fayyad et al. (1995) reports price elasticities of wheat, corn and rice of -0.12, -0.24 and -0.45, respectively, while Dawoud (2005) finds an own-price elasticity of cereals as a whole of -0.58. The consumption of cereals therefore likely decreased to some extent. Also, staple food prices were not the only food to become more expensive; the prices of other, non-staple foods such as meat, poultry, fish, fruits and vegetables increased substantially as well, and there is evidence that consumption of the first three of these foods declined between 2005 and 2008 (WFP, 2008), which would be consistent with the (relatively high) own-price elasticity estimates from Fayyad et al. (1995) and Dawoud (2005) for these goods. Thus, it is not obvious that households could have simply switched from more expensive staples to these other foods, and thereby maintained sufficiently high levels of nutritional intake. Of course, there are a wide variety of other coping mechanisms that net food buying households could have utilized to deal with these price changes. For example, there is anecdotal evidence that Egyptian households cut back outlays on other essentials (such as health- and education-related goods) (IFAD, 2008). It is an empirical question as to whether or not these types of strategies proved sufficient to maintain normal food consumption levels and children's weights.

There are reasons to expect that high food prices were most likely to have negatively affected the poorest households. First, food makes up a larger proportion of consumption baskets for poorer households. Also, the economic growth that took place in the years preceding 2008 is believed to have protected richer households from the effects of food price inflation in a way that it did not for poorer households. Indeed, while economic growth decreased the proportion of Egyptians who fell under the country's poverty line fell from 23.4 percent in 2005 to 18.6 percent in 2008, the proportion of Egyptians living in extreme poverty increased from 5.4 to 6.4 percent

(Arab Republic of Egypt Initiative on Soaring Food Prices, 2008). [2] Also, the rate of wastage–i.e., the proportion of children whose weight-for-height was at least 2 standard deviations lower than that of an international reference group of children–increased by more than 90 percent from 3.4 percent before 2008 to 6.5 percent during it (see Figure 3.1).

Given this possibility that different types of households and parts of the income distribution were affected differently by inflation, I will examine the evolution of the conditional distribution of children's weights-for-height over time as a function of region- and parental occupation-based factors. Since one of the crops whose price was considerably higher in 2008 (wheat) was being harvested around the time the 2008 data was collected in large quantities in (rural) Lower Egypt, the region of residence should help determine who might have benefited from high food prices and who might have been harmed by them. Similarly, parental occupation data, and in particular data on whether or not children had a parent who was either a self-employed farmer or an agricultural employee, allow for an even more focused examination of potential beneficiaries and others. The hypothesis that children in Lower rural Egypt with parents who were more likely to have sold greater amounts of high-priced wheat were relatively heavy in 2008 will be tested. More specifically, I assume that children in rural Lower Egypt with self-employed farmer parents at the top of the conditional weight-for-height distribution were most likely to have been members of net (high-priced) food selling households. Thus, it is this group

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[2] Here, a household is considered poor if daily per capita consumption is less than 2 USD/day at purchasing power parity, and a household is considered to be extremely poor if it cannot meet its basic food needs (Arab Republic of Egypt Initiative on Soaring Food Prices, 2008).

of children who should have had the highest weights-for-height in 2008 (relative to observationally similar children in previous years). Additionally, since wheat is cultivated in rural Upper Egypt, it is also possible that children with self-employed farmer parents at the top of the conditional weight-for-height distribution in that region were also relatively heavy (given height) in 2008. Given the less productive climate of Upper Egypt for wheat cultivation, however, it is expected that fewer children would have belonged to net wheat selling households in this region than did in Lower Egypt. [3] In keeping with the general expectation from the literature that high food prices are beneficial for at least some net food selling households, the assumption here is that the beneficial effects of high food prices on children's weight-for-height dominated the harmful effects of having to pay more for inputs such as fertilizer.

There are also net food buying households in rural Egypt to consider. On the one hand, the standard approach to understanding the impacts of high food prices would typically conclude that children in these households should have been negatively affected by high food prices, but for farm employee households in particular, there are reasons to doubt this. In Lower Egypt, children with self-employed farmer parents at the lower part of the conditional weight-for-height distribution, children with agricultural employee parents, and children with parents employed outside of agriculture could have all benefited to some extent from high food prices (despite the fact that they lived in net food buying households). In particular, members of

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[3] Also, children in what could have been net food selling households in rural Upper Egypt would not have benefited in late 2007 and afterwards from higher prices for maize and rice like their counterparts in Lower Egypt might have, since those crops are not cultivated in Upper Egypt.

these types of households in Lower Egypt might have either: sold some quantity of high-priced wheat in the second quarter of 2008; earned (substantially) higher wages as agricultural workers; or earned more working in the rural non-agricultural sector (thanks to increased demand for non-farm goods and services). Again, the same things are true for observationally similar households in rural Upper Egypt, but are less likely to have occurred. Under the assumptions of the standard approach, however, children in these types of households in rural Lower and especially Upper Egypt are therefore expected to have been negatively affected by high food prices in 2008.

It is important to emphasize, however, that for children in all 3 of these parental occupation-based groups in both rural regions, the poorest children could have been negatively affected by high food prices. This vulnerability is driven by the prominence of (what in 2008 were) high-priced staple foods in poorer households' consumption baskets. Again, it is the conditionally lightest children in these groups who are assumed to have been the poorest ones, and we therefore should expect these lightest children to be more likely to have been negatively affected by high food prices. Finally, given their net food buying status, urban children–and, once again, urban children near the bottom of the conditional weight-for-height distribution in particular–are expected to been relatively negatively affected by high food prices in 2008.

To sum up these standard approach-based predictions, since high-priced wheat was being sold around the time that children were being measured in 2008, and assuming the negative effects of higher input prices were not sufficiently large, children in net food selling households are expected to have registered the largest increases in weight-for-height between 2005 and 2008. In terms of the observable characteristics in the data, these are the conditionally heaviest children of self-employed agricultural workers in rural Egypt, and they are expected to have benefited from higher food prices in 2008 in a way that children from (within-region) net food buying households did not. [4] This latter group includes the lighter children of self-employed agricultural workers, children of agricultural employees, children of non-agricultural workers, and urban children. Note that once again under the standard assumptions, the fact that agricultural employees are members of net food buying households will have stronger effects than the increase in wages for these workers will, and members of these households are expected to be negatively affected by higher food prices. A second prediction is that children from net food selling and net food buying households in Lower rural Egypt should have benefited from higher food prices to a greater extent than their Upper rural counterparts, since higher-priced crops were produced and sold in greater amounts in the former region. A third and final prediction is that, in all regions and for all parental occupations, the conditionally lightest children are considered particularly likely to have been substantially harmed by high food prices.

The analysis of the conditional distribution of children's weights-for-height will be done using quantile regression analyses (in addition to ordinary linear regression analyses of the same factors over time). The assumption motivating the choice to use

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[4] As I will discuss shortly, and since I do not observe household wealth in my data, I assume that children's weight-for-height is positively correlated with household wealth.

quantile regression techniques, of course, is that child weight is positively correlated with household income. In this case, then consistent with the evidence described above, it could have been children at the left tail of the conditional weight-for-height distribution who were affected most severely by inflation. The household surveys on which I rely do not contain income data. However, as the results in Tables 3.3, 3.4, 3.6 and 3.9 show, parental highest grades completed at school, which are of course typically positively related to income, are also positively associated with weight-for-height. I also assume rank preservation: The weight-for-height of a child at a particular conditional quantile of the distribution in one year would also be at that same conditional quantile if they were observed in any other year. Intuitively, this assumption will allow the empirical results to be interpreted as indicative of the evolution of weights-for-height for different groups of children (e.g., low-weight children of self-employed farmers in Lower Egypt), rather than the evolution of the different conditional quantiles (corresponding to the weights-for-height of children who were only temporarily at that location of the conditional distribution).

Again, in keeping with the predominant existing view, I expect that those agricultural households who were most likely to have been net sellers of high-priced crops would have benefited from the sale of those crops in the second quarter of 2008. It is important to note, however, that food prices were not the only prices to exhibit large increases in 2008: High petroleum costs reportedly pushed the prices of fertilizer and diesel fuel up substantially (Wikileaks Discussion Forum, 2011), which in turn would have pushed up the costs of agricultural production, harvesting, processing and transportation. Also, along with many other agricultural products, livestock feed became more expensive. Thus, in principle it is not obvious how this group of rural agricultural households will have been affected by the food price crisis of 2008.

Most of the evidence on how different kinds of households in developing countries were affected by food price increases in 2008 comes from simulation studies, which normally conclude that the numbers of poor or hungry people increased by tens of millions of people worldwide (Headey, 2011). The conclusions from these types of studies normally rest on the assumptions that only food prices changed and that household incomes remain fixed. Both of these assumptions are not applicable for the case of Egypt, however, as we have seen. Also, in practice, food and oil prices commonly move together (Arshad and Hameed, 2009), and so it is not clear that understanding the effects of solely food prices on well-being is preferable from a policymaking perspective than understanding the effects of inflation more generally, or how well-being is affected during a typical high food (and other) price episode. Headey (2011) is an exception to these simulation studies in that it is based on household survey data (from a cross-section of countries). This paper relies on a subjective measure of well-being in the form of self-reported food affordability. In particular, respondents were asked whether their household had any problems affording food and whether the household had experienced episodes of hunger in the previous12 months. As the author notes, however, this kind of outcome has been found to be subject to test-retest unreliability, and question phrasing and placement have been shown to have large effects on responses. Interestingly, the author finds that self-reported food insecurity decreased rather than increased between 2005 and 2008 (with most of Africa and Latin America being exceptions to this finding). Ferreira et al. (2011) also relies on household survey data, and estimates the welfare consequences of food price increases in Brazil in 2008 using simulation techniques. The authors find that net food buyers–and in particular the poorest of these consumers–experienced large, negative effects, while the rural poor benefited from the higher food prices. This chapter is intended to add to this literature by relying on a relatively objective measure of well-being in the form of children's weights-for-height, and going beyond a focus on average outcomes in favor of distribution-wide analyses.

3.3 Empirical Analysis, Data and Sample

3.3.1 Empirical Approach

As Section 3.2 makes clear, Egypt offers a good opportunity to consider the hypothesis that rising food prices benefit households that sell food and negatively affects households that do not. This will be tested by comparing changes in children's weights-for-height in regions where high priced food was being sold (rural Lower Egypt) to changes in children's weights-for-height where food was not being sold (urban Egypt and, to a lesser extent than in rural Lower Egypt anyway, rural Upper Egypt). It is not obvious, however, that we might learn something by comparing weight-for-height changes over time across these regions. Put another way, economic growth or other time-varying factors could have had different effects on the different regions regardless of what happened to prices in 2008. In this case, to the extent that these (region-specific) changes would have affected children's weight-for-height, a comparison of changes in the different regions would confuse the effects of inflation with what were merely region-specific changes. This would obviously make for a flawed test of the hypothesis in question.

As Figure 3.3 shows, however, the assumption that children's weight-for-height evolved similarly in the different regions is plausibly not too strong. While levels differ substantially across regions, for 2000, 2003 and 2005, changes appear comparable. In particular, child weight-for-height levels for children in rural Upper Egypt are the lowest of the three regions, which is consistent with existing research (Arab Republic of Egypt Initiative on Soaring Food Prices, 2008). On the other hand, it is surprising that levels for children in rural Lower Egypt are higher than levels for urban children. But more importantly for the purposes of this chapter, the directions of changes in weight-for-height between surveys are constant across all 3 regions. First, weight-for-height drops substantially in all three regions between the 2000 and 2003 surveys. This large drop is likely to have been driven mainly by drops in GDP growth per capita, which had been fairly high for several years before 2002 (see Figure 3.2). A second, less important factor behind the drop in weights-for-height in 2003 was likely the devaluation in January, 2003 of the Egyptian pound, which resulted in non-trivial increases in the (relative) price of food and has been found to have affected poor households more strongly than other households (Kraay, 2007). Indeed, the seeming severity of the events of 2003 compared to the food price crisis of 2008 is striking and is an interesting finding in its own right. In any case, after 2003, economic growth rates picked back up and this is once again reflected in children's health in all three regions. Thus, for the years prior to 2008 anyway, weight-for-height changes seem comparable, and this should perhaps ease concerns over the usefulness of comparing child weight-for-height changes across regions.

My empirical analysis is based on the following baseline specification:

$$WFH_{it} = \alpha + \sum_{t=1}^{3} \beta_t Urbant_i + \sum_{t=1}^{4} \rho_t LoEgyptt_i + \sum_{t=1}^{4} \theta_t UpEgyptt_i + X_{it}'\Gamma + \varepsilon_{it}. \quad (3.1)$$

Here WFH_{it} is the weight-for-height of child i observed in year t. $Urbant_i$, $LoEgyptt_i$ and $UpEgyptt_i$ are dummy variables corresponding to residence in either urban areas of Egypt or rural areas of Lower and Upper Egypt in year t, respectively. Note that $t \in \{2000, 2003, 2005, 2008\}$ for the rural Lower and Upper Egypt year indicators, but $t \in \{2003, 2005, 2008\}$ for the urban Egypt indicators since urban children measured in 2000 are the omitted category. An additional specification replaces the sets of dummies for the pair of rural regions with dummies for 3 categories of rural Egyptian children (again, for all four survey years for the rural Egypt year indicators and for three survey years for urban children): children with no parents who work in agriculture, children with at least one parent who works as a self-employed farmer and children with at least one parent who works as an agricultural employee (e.g., a casual farm laborer). The sets of dummies from the two specifications are included to identify changes in children's weight-for-height for the region- and region and occupation-based groups of interest, respectively. [5]

If the hypothesis that high food prices increase income in net food selling households and decrease quantities of food consumed in net food buying households

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[5] For linear regression analyses, sample weights are not used. The results in Tables 3.3 and 3.4 are robust to the use of sample weights, however.

is true, then the coefficient estimates of the dummies equal to one for rural children measured in 2008 should take on larger positive values than the estimates of the dummies for urban children measured in 2008. Moreover, the $LoEgypt2008$ coefficient should have exhibited a larger jump relative to analogous values for earlier years than did the $UpEgypt2008$ coefficient, since wheat yields would have been relatively high in Lower Egypt (due to more favorable climatic/growing conditions). Finally, if the predictions set forth in Section 3.2 are correct and children's weight-for-height in 2008 is especially high (relative to earlier years) in rural Lower Egypt, then the second version of the specification should show that these large positive changes in weights-for-height were driven by children whose parents work as self-employed agricultural workers (i.e., children who are likely to reside in net food selling households). In particular, the estimate of the rural Lower Egypt self-employed farmer parent indicator for the upper conditional quantiles of the conditional weight-for-height distribution should exhibit relatively high growth between 2005 and 2008.

The X_{it} vector contains controls for individual and household-level factors. Child-specific controls include a dummy equal to one if child i is a male and zero otherwise. I also include quadratic terms for mother's and father's ages; variables equal to mother's and father's highest grades completed at school; variables equal to the numbers of females and males in the household in each of several age groups; and the household dependency ratio. [6] Finally, I include a dummy variable equal to one if child i's mother works.

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[6] The age group categories are defined for householders between birth and age 6, age 7-age 15, age 16-25, age 26-45, age 46-60 and age 61-plus. The household dependency ratio is defined to be the number of householders between the ages of zero and 15 plus the number of householders above the age of 45 divided by the number of householders between the ages of 16 and 45.

The specification above will be used to conduct linear regression as well as quantile regression analyses. As the conceptual framework section makes clear, there is reason to believe that lighter children in net food buying households were more severely negatively affected by food price inflation than other children in these households. Also, it could be that only agricultural households containing the heaviest children benefited from high food prices. The specification above will be estimated for various conditional quantiles of the conditional weight-for-height distribution to explore these possibilities. For quantile regressions, standard errors are bootstrapped.

3.3.2 Data

The household surveys I rely on are Demographic and Health Surveys (DHS) collected in 2000, 2003, 2005 and 2008. These surveys were all collected in the first two quarters of the year (see Table 3.1 for the months of data collection for each survey). These surveys are nationally representative at the rural and urban levels and they contain a wide variety of socioeconomic and demographic data, such as anthropometric data for all children living in sampled households under the age of 6, region of residence, respondent occupation, highest grade completed, and the age and gender of all household members. In sampled households each woman between the ages of 15 and 49 was selected to be interviewed, and all of these women who were married were also asked about the occupation and highest grade completed of their husband. Thus, for all of the children in my sample, I observe the parental variables of interest.

The sampling scheme of the four household surveys was designed to provide information on "various population and health indicators of interest for... six major subdivisions" of the country", which are aggregated here to urban, rural Lower and rural Upper Egypt regions. Each survey also relies on the same type of three-stage probability sampling scheme.

The outcome I consider is children's weight-for-height. I consider this outcome because I am interested in using a relatively objective measure of the short-term effects of food price inflation on children's health and well-being. [7] A valid measure of this is one that is potentially affected one way or another quickly, and weight-for-height is therefore preferable to another objective measure of child health such as, say, height-for-age.

My sample consists of all children below the age of 6 who resided in sampled households and whose parents' occupational status and highest grade completed data is not missing. This leaves me with a total of 40,233 children (out of a total of 40,573 children with non-missing weight-for-height data), 20,600 (51.2 percent) of whom are male. [8]

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[7] While at least 97 percent of all of the children under age 6 had their weights measured in each survey, a small proportion of those children–as much as six percent–were judged to have "implausibly high or low" weights that were consequently not included in the version of the data released to the public.

[8] A total of 451 children are missing weight-for-height data.

3.4 Descriptive Statistics and Results

3.4.1 Descriptive Statistics

Descriptive statistics for the sample of children can be seen in Table 3.2. Beginning with Panel A, the mean (standard deviation) of weight-for-height for the entire sample is about 0.142 (0.028). Also, when we examine mean weight-for-height for children in urban and rural Upper Egypt, we see that both groups of children have slightly lower weights-for-height relative to the sample average. But while rural children as a whole still weigh less (given their age) than urban children, the fact that children in rural Lower Egypt are the heaviest of all three of these groups is once again surprising. Evidently the usual finding that rural Egyptian children weigh less than urban Egyptian children does not tell the whole story. Also, in both rural Lower and Upper Egypt, we can also see that average differences in weights-for-height across parental occupation categories are small.

The sample average mother's highest grade completed is about 6.7, while that of father's is moderately higher at 8.3. About 36 (6) percent of children's mothers (fathers) were between the ages of 15 and 25, about 50 (50 percent) percent were between the ages of 26 and 35, while the rest of the sample children's mothers (fathers) were older. The sample average number of very young and elderly householders relative to the number of prime-aged adults is about 0.83, and about 15 percent of all sample children's mothers work. Panel B of Table 3.2 contains descriptive statistics across years for the proportions of children whose parents worked in different occupations and whose weight-for-height data was missing. Also, there is data on the numbers of children that mothers gave birth to in the previous 5 years, as well as the numbers of children under the age of 5 who died in the previous 5 years. These statistics make clear that there did not seem to be uncommonly large changes between 2005 and 2008, which might ease concerns over the possibility that the high food price inflation of the latter year substantially affected sample composition.

Returning to Figure 3.3, it is striking that three years' worth of substantial growth in real GDP per capita beginning in 2005 did not result in larger increases in children's weights-for-height by 2008. Moreover, it seems that it is rural (Lower Egyptian) and not urban children who lost the most weight (relative to height) in the three years preceding measurement in 2008. This figure is not obviously consistent with the main hypothesis motivating this chapter–namely, high food prices were beneficial for rural children and harmful for urban children. Next we move on to the main results for a more careful examination.

3.4.2 Ordinary Least Squares Results

3.4.2.1 Region-Specific OLS Results

The main results for the version of the specification that considers region-specific changes in weights-for-height can be seen in Table 3.3. The linear regression results can be seen in columns 1 (which only additionally controls for child gender and age) and 2 (which adds controls for parents' education and age, household composition, and an indicator for whether the child's mother works). The results are quite similar for both versions of the specification, and I will discuss the less parsimonious specification's results here.

The omitted group is male children in urban Egypt in 2000 who are less than one year old. This group of children has an average weight-for-height of 0.115. The results make clear that relative to these children, urban children were lighter (given height) in 2003 compared to 2000, their relative weights were on the way to recovery in 2005, and urban children had more than recovered their weights-for-height by 2008. While the changes in average urban children's weights-for-height are commonly highly statistically significant, the magnitudes in question are normally small. For example, holding all else equal, urban children in 2003 were -0.00794 kilograms (given height) lighter than their counterparts in 2000 (on average), which represents a bit more than one-quarter of a standard deviation of the weight-for-height variable. Note that this is an example of a relatively large inter-survey change in weights-for-height, i.e. most changes in weight-for-height between surveys were fairly modest. It is also worth noting that 3 years' worth of solid economic growth in the run-up to measurement in 2008 was not as beneficial, for example, as the events leading up to measurement in 2003 were harmful (according to the results of a t-test). These relatively weak gains between 2005 and 2008–which might be contrasted with the gains between 2003 and 2005, when the economy was growing at a moderate rate but inflation was much lower–are consistent with food price inflation having had a substantially negative effect on urban children's weights-for-height.

Lower rural Egyptian children in 2000 were heavier (given height) than their urban counterparts, and these children were hit relatively hard by the events leading up to measurement in 2003. By 2005, however, these children were well on their way to back to their (conditionally average) 2000 weights-for-height. But not only did the economic growth between 2005 and 2008 not result in substantial increases in Lower rural Egyptian children's weights-for-height over that period, but (conditional) weights-for-height were actually lower in 2008 than they were in 2005 (though not significantly so, according to a t-test). Given that high-priced wheat was being harvested and sold in relatively large quantities in Lower rural Egypt around the time of measurement in 2008, children there might have been expected to benefit more than other children from food price inflation. The observed decrease in average weights-for-height between 2005 and 2008 is therefore surprising. Of course, it remains to be seen which types of children in Lower rural Egypt in 2008 are driving this result.

In contrast to Lower rural Egyptian children, Upper rural Egyptian children who were measured in 2000 were lighter (given height) than their urban counterparts. These children were also considerably lighter (given height) in 2003, but had made a moderate recovery by 2005. Again, however, the 3 years' worth of substantial economic growth that Egypt experienced before 2008 appears unable to have fully counteracted other, negative effects, and Upper rural children grew only slightly (but statistically significantly) heavier between 2005 and 2008.

Female children had slightly lower weights-for-height than male children, and the age dummy coefficients make it clear that weight-for-height increases substantially with age. The magnitudes of the age coefficients are quite large relative to the typical region-year indicator estimate. Weight-for-height is increasing with father's (but not mother's) highest grades completed at school. [9] There is some evidence that the more young–and likely less economically productive–household members there are, the lower is children's weight-for-height. On the other hand, holding all else equal, an increase in the number of males between the ages of 26 and 45 is associated with higher weights-for-height. Finally, weights-for-height are higher when the child's mother works and, surprisingly, the higher is the household dependency ratio. These results are somewhat consistent with weights-for-height being higher when more household members engage in economically productive activities.

Overall, the results are consistent with Figure 3.3 in that they show that it was rural Lower Egyptian children who seemed to have been hit hardest by the events in the run-up to 2008. Again, this is somewhat surprising. Recall that it was agricultural households in rural Lower Egypt who were expected to have benefited from high food prices the most, as wheat was being harvested there in relatively large amounts as the 2008 survey was being conducted. Thus, it is striking to see that rural Lower Egyptian children's weights-for-height in 2005 were well on their way back up to their pre-2003 levels, but that those weights-for-height were then actually lower (if anything) by 2008. In contrast, children's weights-for-height in urban and rural Upper Egypt increased between 2005 and 2008. Also, out of all the groups of children considered, urban children were the only ones to have weighed more (given height) in 2008 than 2000. This seems to contradict the predictions set forth in the conceptual framework, which predicts that as net food

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[9] Given the positive relationship between parental education and a wide variety of measures of household socioeconomic status and well-being, the fact that father's education is positively related to weight-for-height suggests that the latter is a useful measure of children's health and well-being.

buying households, urban households should have been relatively severely affected by high food prices. The hypotheses that high food prices are better for rural or net food selling households than for urban or net food buying ones (respectively) will be more properly assessed, however, when we examine what happened to children's weights-for-height as a function of parents' occupations as well as region of residence (and all this at different conditional quantiles of the weight-for-height distribution).

3.4.2.2 Region and Parental Occupation-Specific OLS Results

We turn now to the results for the version of the specification that allows for the region-specific effects over time to vary with parents' occupations (see Table 3.4). Once again, the results are highly similar regardless of whether or not controls for parental education, household composition and maternal labor supply are included (column 2 contains these additional controls). Since the urban indicators do not vary by parental occupation, the results for urban children's weights-for-height are quite similar to the results described in the previous sub-section. In particular, urban children's weights-for-height were substantially negatively affected by the events leading up to measurement in 2003, they were heading for recovery to their 2000 weights-for-height in 2005 but were not quite there yet, and they had more than recovered to baseline levels by 2008.

Turning to Lower rural Egyptian children, we see more results that seem to contradict what was expected. In particular, the children of self-employed agricultural workers–the group of children considered the most likely to have benefited from high food prices in 2008–started out heavier (given height) than their urban counterparts in 2000, and they were hit quite hard by the events leading up to 2003 but had recovered by 2005. In 2008, however, these children's average weights-for-height decreased substantially, and were actually not statistically distinguishable from their weights-for-height in 2003. For the children of agricultural employees and non-agricultural workers, however, weights-for-height were on average higher in 2000 than they were for urban children, the events leading up to 2003 took a substantial toll, and they were (at least) well on their way to recovering to their 2000 weights-for-height by 2005. But while weights-for-height in 2008 were statistically indistinguishable from their 2005 levels (according to t-tests) for the children of parents who did not work in agriculture, the children of agricultural employees also lost weight between 2005 and 2008.

In other words, weights-for-height for the children of non-agricultural workers in 2008 had merely not grown relative to 2005, but weights-for-height for the children of self-employed agricultural workers and agricultural employees decreased substantially. Thus, the group of children in Lower rural Egypt who were expected to have benefited the most from high food prices in 2008 seem to have been harmed by them. One possible explanation for this is that self-employed farmers had especially high input costs to attempt to manage, and therefore household consumption might have decreased as a result.

Many aspects of the results for Upper rural Egyptian children will look familiar by this point. In particular, the children of self-employed agricultural workers, agricultural employees and non-agricultural workers in Upper rural Egypt each had average weights-for-height that were roughly comparable to their urban counterparts in 2000, those weights-for-height were substantially negatively affected by events in the run-up to measurement in 2003, and substantial recoveries were under way in 2005. However, while that recovery for the children of non-agricultural workers had continued in 2008 (if not statistically so, according to a t-test of equality of the 2005 and 2008 coefficients), weights-for-height in 2008 were significantly lower for self-employed farmers' and agricultural employees' children than in 2005. Thus, agricultural workers' children in Upper rural Egypt were harmed by the events leading up to measurement in 2008 in a way that other children in that region were not. Again, it is possible that higher input costs make agriculture less lucrative for these children's parents.

As for the control variables (in column 2 of Table 3.4), females and younger children are (unsurprisingly) still lighter (and once again substantially so in the case of younger children). Also, father's highest grade completed at school is still significantly positively related to children's weights-for-height. Having more young females in the household still tends to be negatively associated with weights-for-height, while higher numbers of prime-aged males in the household is associated with higher ones.

In conclusion, the linear regression results seem to contradict several of the predictions set forth in the conceptual framework. In particular, the children regarded as most likely to have benefited from high food prices in 2008–the children of self-employed agricultural workers in Lower rural Egypt–seemed to have been hit rather hard by the events leading up to measurement then. Also, while urban children's weights-for-height did not exhibit a very large positive change between 2005 and 2008, they were statistically significantly higher. Again, this was the only group of children considered whose weights-for-height were significantly higher in 2008 than they were in 2000. We turn next to an analysis of conditional quantiles of the weight-for-height variable to see if the average changes over time described in this section hold for different parts of the conditional distribution.

3.4.3 Conditional Quantile Regression Results

3.4.3.1 Region-Specific Quantile Regression Results

The conditional quantile regression results for the baseline set of covariates can be seen in Table 3.5. The results in Table 3.5 correspond to the specification that only controls for region-year effects, child gender and child age. There are not substantial changes in the magnitudes of the key right-hand side variables after controls for household members' education and age and household composition are added to the model (see Table 3.6), and there are certainly not changes in the qualitative nature of the findings. Quantile regressions were run for the 5th, 10th, 25th, 50th, 75th, 90th and 95th percentiles. Also, Figures 3.4-3.6 provide a graphical summary of the results for the key independent variables (the region-specific year indicators) in Table 3.6, and so the discussion below will focus on the results from that table. The omitted group is male children in urban Egypt in 2000 were were less than a year old, whose mother did not work (with average values of the other control variables). The results described below will mostly describe changes over time in other children's weights-for-height relative to this omitted group of children.

Figure 3.4 displays the results for the urban indicators in 2003, 2005 and 2008. The results in this figure should be interpreted as changes in conditional quantiles of the weight-for-height distribution over time for urban children (relative to weights-for-height in 2000, holding children's gender and age and other factors constant). For example, the 0.05 conditional quantile for urban Egyptian children measured in 2003 is 0.00580 units below its 2000 analog (see column 1 of Table 3.6), which implies that this point of the conditional distribution of weight-for-height decreased moderately for urban children between 2000 and 2003. This change represents slightly more than one-fifth of the sample standard deviation of the weight-for-height outcome.

As Figure 3.4 shows, the OLS results mostly do a good job of characterizing the effects of the events leading up to 2003, in the sense that the conditional quantiles are not substantially different from one another then. (It might be worth noting, however, that according to the results in Table 3.7, the 0.05 and 0.95 conditional quantiles are statistically significantly different from one another, if not highly economically significantly different.) Turning to urban children measured in 2005, we see that the economic growth over the preceding couple of years resulted in the conditionally heaviest children's weights-for-height surpassing those of urban children in 2000 (as evidenced by the positivity of the slope for the upper conditional quantiles). On the other hand, while conditionally lighter urban children in 2005 were very slightly heavier than they were in 2003, they had certainly not recovered to their 2000 weights-for-height like the heaviest children had. It would appear that economic growth in the run-up to measurement in 2005 did not benefit conditionally lighter children in urban Egypt the way it did conditionally heavier children there. This is certainly true in a statistical sense for the 0.05 and 0.95 conditional (for example) according to the results in Table 3.7, which show that the differences between heavy and light children went from being significantly negative in 2003 to being significantly positive in 2005.

This is even more true for the events leading up to measurement of urban children in 2008. In particular, despite 3 years' worth of solid economic growth between 2005 and 2008, the bottom decile of urban children was actually slightly lighter (given height) in the latter year than they were in the former. This is clearly consistent with food price inflation having had negative effects on the poorest urban children's well-being. Also, the higher the conditional quantile, the greater were the benefits of economic growth in the run-up to 2008, and the less important were the negative effects of high food prices then. Indeed, quantiles near the top of the conditional weight-for-height distribution for urban children in 2008 were substantially higher than their 2005 analogs, and were considerably higher than their 2000 analogs as well. It would appear therefore that the net effects of food price inflation and economic growth were negative for the conditionally lightest urban children and positive for other urban children in the run-up to 2008. Again, this spreading out of the conditional weight-for-height distribution is confirmed by the results of tests of the equality of the inter-quantile regression urban children's coefficients across years in Table 3.7, which show that the differences between heavy and light urban children became larger over time.

Finally, while Figure 3.4 makes it clear that the mean (i.e., OLS) effect does a poor job of describing changes in weight-for-height throughout much of the conditional weight-for-height distribution in general, the inadequacy of the OLS result is particularly severe in the case of urban children's weights-for-height in 2008. As the figure shows (and as we will see), the 0.05 and 0.95 conditional quantiles, for example, for urban children in 2008 were simply much farther from one another than was the case in earlier years (statistically speaking as well as with respect to magnitudes). These results are consistent with food price inflation and economic growth each having qualitatively different effects on the different ends of the conditional weight-for-height distribution.

Turning now to the conditional quantile results for Lower rural Egyptian children in Figure 3.5, although conditional weights-for-height for the heaviest kids were statistically larger than those of the lightest kids, in substantive terms the OLS results are mostly quite informative for most of the conditional weight-for-height distribution for Lower rural children in 2000. In particular, Lower rural children were slightly heavier (given height) throughout the conditional weight-for-height distribution in 2000 compared to urban children then. Figure 3.5 also shows that children in this region throughout the conditional distribution were fairly seriously negatively affected by the events leading up to measurement in 2003. Lower rural Egyptian children at the 0.75 conditional quantile, for example, were roughly 0.00833 units lighter in 2003 than they were in 2000, which represents a substantial drop.

By 2005, however, most Lower rural children were well on their way back up to their 2000 weights-for-height. Also, once again similarly to urban children, this was only barely true for the conditionally lightest Lower rural children in 2005, while the 0.95 conditional quantile in 2005 was already higher than this region's 2000 analog, for example. (And weight gains over time were statistically larger for heavier children, according to tests of the equality of the coefficients in Table 3.7, which displays inter-quantile regression results.) For much of the conditional distribution in 2005, however, the OLS result is a somewhat accurate indicator of what was happening to Lower rural children's weights-for-height.

Figure 3.5 also displays conditional quantile results for Lower rural children in 2008, and shows first and foremost that like urban children then, the conditionally lightest children were substantially negatively affected by the events leading up to the second quarter of 2008, and were lighter then than their counterparts were in 2005. Again, this is striking given that the Egyptian economy grew substantially during this time period. Beginning at the conditional median, however, Lower rural children were heavier (given height) in 2008 than in 2005. Importantly, this is with the exception of the 0.95 conditional quantile, which decreased over this time period. There is actually no statistically significant difference between the 2005-2008 weight changes that took place at the top and bottom of the conditional distribution–the conditional weight-for-height distribution for Lower rural children stopped spreading out in 2008, at least when comparing the 0.05 and 0.95 conditional quantiles.

The conditionally lightest Lower rural children therefore had substantially lower weights-for-height in 2008 than their counterparts in 2005 did, which is consistent with high food prices having negative effects on the health and well-being of members of the poorest households in the region. Moreover, the decrease in weights-for-height between 2005 and 2008 in this region for the conditionally lightest children was considerably larger and affected a greater proportion of the conditional distribution than was the case for urban children. In particular, the 0.05-0.25 conditional quantiles for Lower rural children in 2008 were at lower levels than their 2005 counterparts, while this was only true for the 0.05 and 0.10 conditional quantiles for urban children. Otherwise, the beneficial effects of economic growth in the run-up to measurement in 2008 seem to have dominated any negative effects associated with high food prices for (most of) the rest of the conditional weight-for-height distribution of Lower rural Egyptian children. We see, therefore, that lower average weights-for-height in 2008 compared to 2005 in this region were mostly driven by changes at the bottom and top of the conditional distribution, and that this average decrease occurred despite what happened over the rest of the conditional distribution. It remains to be seen whether these results hold for each of the three parental occupation-based groups that we will consider in the next sub-section.

Turning to Upper rural children in Figure 3.6, we see that these children had lower weights-for-height than urban children did in 2000 (conditional on gender and age and other factors) and that the OLS estimate is mostly informative throughout the conditional distribution (although, again, there are statistically significant differences in the weights-for-height of Upper rural children at the 0.95 and 0.05 conditional quantiles). In 2003, once again, every conditional quantile analyzed was lower than it was in this region in 2000. By 2005, on the other hand, the conditional distribution was higher everywhere, but recovery was considerably more pronounced at the top of the conditional distribution. This can be seen in Table 3.7, which shows that the difference between the 0.95 and 0.05 conditional quantiles went from being significantly negative in 2003 to being significantly positive in 2005.

In 2008, the conditionally lightest Upper rural children had lost weight, and so the net effect of food price inflation and economic growth in the run-up to measurement was once again negative for the conditionally lightest children. Above the 0.25 conditional quantile up, however, weights-for-height were higher in 2008 than in 2005, but the difference is quite modest below the 0.90 conditional quantile. Thus, while the conditionally lightest children were lighter in 2008 (despite 3 years' worth of economic growth), and the conditionally heaviest children were substantially heavier, most of the conditional distribution was not much heavier than it had been in 2005. According to tests of equality of the Upper rural 2005 and 2008 inter-quantile differences, moreover, the heaviest children gained significantly more weight than did the lightest children.

We can see in Table 3.6 that the conditional mean effect for females is mostly informative throughout the conditional distribution. We also see that the conditionally lightest of each cohort of children is heaviest relative to the omitted group, infants. Thus, it seems that some of the variation in weights-for-height amongst infants disappears over time. For all other estimates, the OLS results are normally quite informative. In particular, what are more likely higher numbers of economically productive household members are positively associated with higher weights-for-height.

Many of the main results described here are also reflected in Table 3.7, which displays results from inter-quantile regressions (between the 0.05 and 0.95 quantiles). The results in this table denote the estimated differences between the top and bottom of the conditional distributions of each of the covariates in the model. For Lower rural and Upper rural Egypt, the coefficients corresponding to the year 2000 indicator variables have some of the smallest magnitudes, which reflects the fact that differences between the top and bottom of the conditional distributions in these regions were small (relative to this inter-quantile difference for the omitted group, urban male infant children in 2000). Inter-quantile differences in 2003 for urban and Upper rural children were (statistically) negative and larger, which reflects the facts that the downturn during that period affected different parts of the conditional weight-for-height distributions differently, and that it was conditionally heavier children who lost the most weight in or before 2003. The 2005 coefficients for all 3 regions are positive and significant, which reflects the fact that during the economic recovery period following the downturn of 2003, it was the conditionally heaviest children whose weights-for-height increased the most. The same is true for each region's 2008 coefficient estimates–they are each significant and positive–and this likely reflects the relative benefits of economic growth for conditionally heavier children and the relative costs of high food prices for the conditionally lightest children.

Another striking feature of the 2008 estimates for each region is how large they are relative to the estimates for most earlier years; the conditionally heaviest children were especially heavy relative to the conditionally lightest children in 2008, statistically and substantively speaking (in urban and Upper rural Egypt anyway). Again, this is consistent with both economic growth and high food prices each having different effects on weights-for-height at different parts of the conditional distribution. These results make it clear that the OLS results mask substantial variation across conditional quantiles in estimate sizes, and in so doing justify the use of quantile regression for the analysis.

3.4.3.2 Region and Parental Occupation-Specific Quantile Regression Results

The conditional quantile regression results for the more flexible, region- and parental occupation-specific specification can be seen in Tables 3.8 (for the version of the specification that only contains additional controls for child gender and age) and 3.9 (for the enhanced specification's results). The results in the two tables are quite similar, and for simplicity's sake I will focus on the results in Table 3.9. Once again, quantile regressions were run for the 5th, 10th, 25th, 50th, 75th, 90th and 95th percentiles of the conditional weight-for-height distribution. Graphical depictions of the results for the key coefficients can be seen in Figures 3.7-3.13. Also, for each conditional quantile considered, and for each of the 7 parental occupation-based groups of children considered (i.e., urban children, the 3 groups of rural Lower children and the 3 groups of rural Upper children), Table 3.10 contains differences between the children measured in 2008 and the children measured in 2005. (The stars next to some of the differences correspond to the results of t-tests of equality of the 2005 and 2008 estimates.) So, for example, children at the 0.05 conditional quantile of the weight-for-height distribution who lived in urban areas in 2008 were 0.00267 units lighter than their counterparts in 2005, and this difference is significantly different than zero at the 0.01 level. Since this chapter is primarily concerned with what happened to different types of children's weights-for-height in 2008, Table 3.10 contains many of the most important results.

Figure 3.7 displays the results corresponding to the estimates for the various conditional quantiles of the urban resident indicators for the years 2003, 2005 and 2008. (The omitted group is once again male, urban, infant children in 2000.) The results here are (unsurprisingly) highly comparable to the results for these same coefficients in the region-specific quantile regression results (from Tables 3.5 and 3.6 and Figure 3.4). In particular, while the conditionally lightest urban children had barely gained any weight between 2003 and 2005, the conditionally heaviest had more than recovered. Also, the events leading up to measurement in 2008 were once again found to have decreased the weights-for-height of the bottom of the conditional distribution while the top three-quarters of the conditional distribution were heavier in 2008 than they were in 2005 (see Table 3.10). Although the magnitude of the decreases in weight-for-height between 2005 and 2008 that the lightest urban children sustained are not particularly large, they are striking given the Egyptian economy's growth over that period. These results are consistent with high food prices having negative effects on those urban households least able to cope with shocks, and the beneficial effects of economic growth dominating for heavier urban children.

We turn now to the 3 parental occupation-specific groups of Lower rural Egyptian children: children with at least one parent who was a self-employed farmer, children with at least one parent who worked as an agricultural employee, and children with no parent who worked in agriculture. We begin by considering these groups' of children's weights-for-height in the year 2000. As can be seen in Figures 3.8, 3.9 and 3.10, the OLS results for these indicators are mostly informative for the conditional quantile-specific estimates, in the sense that the magnitudes of the latter are normally relatively close to one another. While each of these 3 groups of children are heavier on average than urban children were in 2000, we see that different parts of the distribution are driving this differential for each group of children. In particular, it is the conditionally heaviest children with self-employed farmer parents and the conditionally lightest children with agricultural employee parents who are heaviest relative to their urban counterparts, while it is the middle of the conditional distribution of children with parents who work outside of agriculture in Lower rural Egypt who are relatively heavy (compared to urban children, for example).

We have seen that each of these 3 Lower rural Egyptian groups of children were substantially lighter in 2003 than they were in 2000 on average. We see from the results in Table 3.9 and in Figures 3.8-3.10 that it was in fact the conditionally lightest children with self-employed farmer parents and (to a lesser extent) non-agricultural sector parents who were least negatively affected by the events leading up to measurement in 2003. This is not true for the children of agricultural employees, however, as it was the lightest of these children that had lost the most weight relative to the omitted group.

We also saw before that in 2005, children in Lower rural Egypt had nearly recovered to their 2000 weights-for-height on average, that it was children with parents working in the agricultural sector who were driving this result, and that it was the conditionally heaviest Lower rural children who had gained the most weight (relative to height) between 2003 and 2005. The results in Table 3.9 and Figures 3.8-3.10 show that it was these conditionally heavier children in each of the 3 parental occupation-based groups that gained the most weight between 2003 and 2005. Regardless of parental occupation, conditionally heavier children in Lower rural Egypt gained more weight between 2003 and 2005 than their lighter counterparts. This is shown clearly in Table 3.10, which displays 0.95-0.05 inter-quantile regression results, and shows that differences between the heaviest and lightest children went from being (not significantly) negative in 2003 to being significantly positive in 2005.

Previously it was shown that it was the children of parents who worked in the agricultural sector who were responsible for the fact that Lower rural Egyptian children were lighter on average in 2008 than they were in 2005. It was also shown that it was the weight-for-height losses of the conditionally lightest and heaviest children that drove this effect. Beginning with the children of self-employed agricultural workers (the group of Lower rural children who lost the most weight on average between 2005 and 2008), we can see in Table 3.10 and Figure 3.8 that, while estimates for all conditional quantiles were lower in 2008 than in 2005, it appears that it was the conditionally lightest and conditionally heaviest of these children who lost the most weight. For example, the 0.05 (0.95) conditional quantile shrank by 0.01357 (0.00945) units between 2005 and 2008, which, again, is striking given the extent to which the Egyptian economy grew over that period (the difference for the heavier children is not significant, however). (As Table 3.10 shows, the conditional third quartile for the children of these self-employed farmers' children only decreased by 0.00401 units, in contrast, which represents a significantly smaller decrease than took place at the 0.05 conditional quantile but a difference that is not statistically different at conventional levels from that which took place at the 0.95 conditional quantile.) Comparing these larger changes at the tails of the conditional distribution to the sample standard deviation in the weight-for-height outcome (0.028) makes it clear that these were substantial decreases for these children of self-employed farmers.

Recall the prediction from Section 3.2.3 that the heaviest children of self-employed farmers in Lower rural Egypt were considered most likely to have gained weight between 2005 and 2008. Table 3.10 shows, however, that these children seem to have fared worse between 2005 and 2008 than the entire conditional weight-for-height distribution of urban children, all but the lightest Lower rural children of self-employed farmers, and the entire conditional distributions for the Lower rural children of agricultural employees and non-agricultural workers. One possible explanation for this surprising decrease in weights-for-height for the conditionally heaviest of these self-employed farmers' children is that, while agricultural output was worth more and so should have helped children in what were most likely to be net wheat selling households to gain (or at least not lose) weight, inputs such as fertilizer and labor–which are heavily used in Egyptian wheat farming–were also more expensive. Indeed, Kherallah et al. (1999) note that fertilizer application rates in Lower Egypt are some of the highest in the world, and find that on average, 79 person-days of labor were used on the typical hectare dedicated to the cultivation of wheat. Moreover, IFAD (2012) reports that in 2008 agricultural wages were 4 times as high as they were a year earlier. World fertilizer prices were also about twice as high in the second quarter of 2008 than they were a year earlier. Under these circumstances, it seems plausible that costs could have increased by a greater factor than did revenues, and profits could have decreased. On the other hand, lower profits in 2008 for wheat farmers would contradict another finding from IFAD (2012), namely that medium- and large-scale farmers reported benefiting from high food prices. Finally, given higher output and input prices, it is also possible that farmers' children's weights-for-height decreased to the extent they did in 2008 because of increases in the opportunity cost of the time that would otherwise have been taken to ensure that young children were well-nourished throughout the day.

As for the especially large losses that the conditionally lightest of these Lower rural children of self-employed farmers sustained, IFAD (2012) finds that some households with farms too small for positive net food sales to occur sold output (at high prices) that they otherwise would have consumed. Members of these households reportedly consumed subsidized bread in place of home-grown wheat-based products but they also cut back consumption of non-staple foods such as meat, poultry and fruits (which were also reportedly more expensive in 2008). Also, these children's weight-for-height losses could be partly explained by their households' initial poverty levels. In other words, it could be that the conditionally lightest of these children come from relatively poor and more vulnerable households, and so might have been particularly hard hit by high food prices. These lighter children of self-employed farmers could have been hit by multiple forces, then–high input prices and high food prices–and it is therefore plausible that the inflation observed in 2008 would have had especially severe negative effects on them.

Initially high poverty levels are also potentially why the bottom 15 percent of the weight-for-height distributions of children with agricultural employee parents or non-agricultural worker parents seem to have lost at least as much weight between 2005 and 2008 as children at the rest of most of the conditional distributions did (see Table 3.10). In particular, above the 0.15 conditional quantile (where, it should be noted, losses were not significantly negative according to t test results), most of the children of agricultural employees lost only a small amount of weight (given height). Also, some conditional quantiles were even higher in 2008 than in 2005 (though not statistically so). Similarly, some of the conditionally lightest 15 percent of children with non-agricultural employee parents were significantly lighter than their counterparts in 2005; but most higher conditional quantiles were actually higher for these children in 2008. It should be emphasized again, however, that it was the conditionally lightest children of self-employed farmers in Lower rural Egypt who really drove the region-wide decrease at the bottom of the conditional distribution described in Section 3.4.2.2; the magnitudes of the changes are considerably larger for the lightest children of self-employed farmers than they were for their agricultural employee and non-agricultural worker analogs. Also, along with the conditionally heaviest children of agricultural employees, it was once again the children of self-employed farmers who really drove the weight loss amongst heavier children. Thus, it was in fact self-employed farmers' children (at the bottom and top of the conditional weight-for-height distribution) who were mainly responsible for the region-wide decrease between 2005 and 2008.

Turning now to Upper rural Egyptian children, recall that in 2000, these children were slightly lighter (given height) than urban children then, and that it was the children of agricultural employees and non-agricultural worker parents who were lightest. From Table 3.9 and Figures 3.11, 3.12 and 3.13, we can see that for the children of self-employed farmers, it is the top quarter of the conditional weight-for-height distribution that was lighter than their urban counterparts in 2000 were. For the children of agricultural employees and non-agricultural workers, it is most of the rest of the conditional weight-for-height distribution that is lighter than urban children in 2000.

We saw earlier that Upper rural children throughout the conditional weight-for-height distribution lost a substantial amount of weight (given height) between 2000 and 2003 on average, and that children with self-employed farmer parents were hit hardest. According to Table 3.9 and Figure 3.11, for the children of self-employed farmer parents, conditional quantiles throughout the entire conditional weight-for-height distribution were substantially lower in 2003 than in 2000. Also, across conditional quantiles there is not a lot of variation in the magnitude of these inter-year differences overall, but if anything, conditionally heavier children lost more weight (given height) than other children. This is also true for the children of agricultural employees and non-agricultural employee parents (see Figures 3.12 and 3.13); if anything, therefore, conditionally heavier children of these types of workers lost slightly more weight (given height) between 2000 and 2003 than lighter children did (though any differences between the 0.95 and 0.05 conditional quantiles were not statistically distinct, for example).

By 2005, Upper rural children–and in particular Upper rural children with an agricultural employee parent–had made a moderate amount of progress back up towards their 2000 weights-for-height on average. Moreover, it was generally conditionally heavier (given height) Upper rural children who gained the most weight between 2003 and 2005. Table 3.9 and Figures 3.11-3.13 show that for all 3 groups of children, it was the conditionally lightest children who registered the smallest increases in weight-for-height. Once again, conditionally heavier children gained considerably more weight than did their lighter counterparts between 2003 and 2005, and the differences in weights gained between the 0.95 and 0.05 conditional quantiles are significant for all 3 groups of children.

We have also seen that, while Upper rural children as a whole were heavier (given height) in 2008 than in 2005 on average, this was only true for the children of non-agricultural worker parents. Moreover, this same overall gain in weights-for-height on average masks the fact that the bottom quarter of the conditional distribution was actually lighter in 2008. According to Table 3.10 and Figures 3.11-3.13, children from what were most likely to have been net food selling households in Upper rural Egypt seem to have actually faired worse in the run-up to measurement in 2008 than children from the most disadvantaged net food buying households, as well as their heavier net food buying counterparts. In particular, the heaviest children of self-employed farmers weighed considerably less in 2008 than their counterparts did in 2005 (and statistically so), while the lightest children of self-employed farmers, agricultural employees and non-agricultural workers lost a small-moderate amount of weight. (Also, according to t test results, differences in the inter-quantile regression coefficients are significant and the heaviest children of self-employed farmers lost significantly more weight than did the lightest children of these farmers.) The heaviest children of non-agricultural workers gained a substantial amount of weight between 2005 and 2008, however, and once again the heaviest urban children gained weight as well. Thus, once again, we see that the group of children mostly likely to have been members of net food selling households lost more weight than other children in their region and urban children. Again, this could be driven by the possibility that net food selling households were hit with substantially higher input costs as well as higher output prices.

Also, for the children of agricultural employees and non-agricultural workers, the bottom 10 and 5 percent (respectively) of the conditional weight-for-height distribution was significantly lighter in 2008. Indeed, the entire conditional distribution of agricultural employees' children's weights-for-height were lower in 2008 than in 2005 (if not always statistically so). For example, children with agricultural employee parents at the 0.05 conditional distribution were 0.00521 units lighter (given height) in 2008 (compared to an insignificant decrease of 0.00387 for the children of self-employed farmers). These results are once again consistent with the negative effects of high food prices outweighing any beneficial effects from economic growth for the lightest (and, presumably, the poorest) children in Upper rural Egypt.

The conceptual framework also predicts that Lower rural net food selling and net food buying households would have benefited from high food prices to a greater extent than their Upper rural counterparts did. This is not the case for children with self-employed farmer parents. Between 2005 and 2008, the changes in Lower rural weights-for-height at all conditional quantiles were negative, while for Upper rural conditional quantiles, they were sometimes positive (and those that were negative were smaller than their Lower rural analogs). For the children of agricultural employees, the conditionally lightest Upper rural children and the conditionally heaviest Lower rural children lost more weight than their analogs in the other region. For the children of non-agricultural workers, results are mixed, with neither region clearly having fared better. Overall, then, it appears that households in Lower rural Egypt did not benefit from the more widespread cultivation of wheat there like they were expected to.

Finally, the estimates of the controls for child gender and age in Tables 3.8 and 3.9 are similar to those in Tables 3.5 and 3.6. In particular, female children are lighter (given height) than males throughout the conditional weight-for-height distribution, and there is not much variation in estimated effects across conditional quantiles.

Table 3.11 displays inter-quantile regression results for the parental occupation-specific set of covariates. Once again, for most of the parental occupation indicators for the year 2000, the estimated differences between the 0.05 and 0.95 conditional quantiles are quite small (relative to the same difference for the omitted group, urban male infant children in the year 2000). The inter-quantile differences for urban and all 3 groups of Upper rural children for the year 2003 are once again significantly negative, which reflects the fact that it was conditionally heavier children in these places whose weights-for-height decreased the most between 2000 and 2003. The fact that it was conditionally heavier children who gained more weight between 2003 and 2005 in all 3 regions is also once again shown in Table 3.11. Indeed, in

Lower rural Egypt in particular, it is clear that it was conditionally heavier children in all 3 parental occupation-based groups who drove this recovery for heavier children. This is less true for Upper rural Egypt, where it was the conditionally heavier children of agricultural employees who drove the earlier, region-wide result. The relative recovery of conditionally heavier children continued in 2008 for urban children and all 3 types of Lower rural children, as well as the children of non-agricultural worker parents in Upper rural Egypt. Thus, once again, for each of these groups of children, the benefits of economic growth seem to have been strongest for conditionally heavier children, while high food prices seem to have taken the largest toll on the weights-for-height of lighter children.

3.5 Conclusion

I have examined changes in Egyptian children's weights-for-height for different region- and parental occupation-based groups to get a sense of how changes differed for (what I assume are) net food selling and net food buying households. Given the prominence of food in the consumption baskets of poor households all over the developing world and the arrival of what appear to be at least occasionally seriously higher food prices, policymakers are naturally concerned about the effects of food prices on individual well-being. Research that speaks to these concerns that utilizes actual household survey data is fairly scarce, however. In particular, there is not enough evidence on what kinds of factors might help households cope with food shocks or what kinds of households need help the most urgently.

I find evidence that, for many children, high food prices in 2008 seem to have largely counteracted whatever beneficial effects several years' worth of respectable economic growth might have brought. This is particularly true for Egyptian children closer to the bottom of the weight-for-height distribution; while economic growth seems to have helped heavier children weather the food price shock, children in what were likely the poorest households were not so fortunate. Moreover, this is despite the Egyptian government's effort to shield more households from the effects of high food prices by expanding previously existing social safety net programs. I also find that those children in what are most likely to have been net food selling households–at the point in the agricultural cycle when they were expected to be cash rich–experienced what appeared to be some of the largest decreases in weight-for-height. (Again, however, these decreases were not statistically different from zero.) While these results do not provide us with a well-quantified relationship between food prices and children's weights-for-height, they do provide a sense of how powerful a force food price inflation is compared to economic growth, and they suggest that it is likely the poorest households that policy should target first and foremost. Also, if agricultural input prices are high along with food prices, then perhaps policies designed to help children in net food selling households should also be considered. This conclusion would be in contrast to the conclusions of a paper like Ivanic and Martin (2014), who find that high food prices are good for farming households in the long run. One of the main novelties of this chapter, however, is its use of a relatively objective measure of household well-being from survey data. Given this feature, the chapter serves as a complement to existing simulation papers. The conclusion here is also different. In particular, one of the main findings is that a child's place in the weight-for-height–and, presumably, household income–distribution is just as important to consider as is their region of residence or their parents' occupations, when thinking about whether they should be targeted for aid.

Figure 3.1: Kernel Density, Weight-for-Height Z-Score, By Year

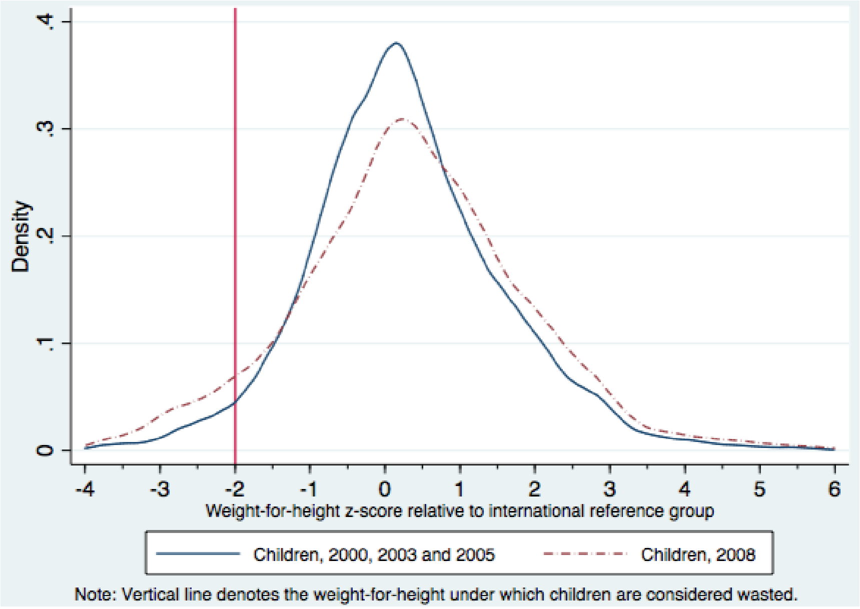
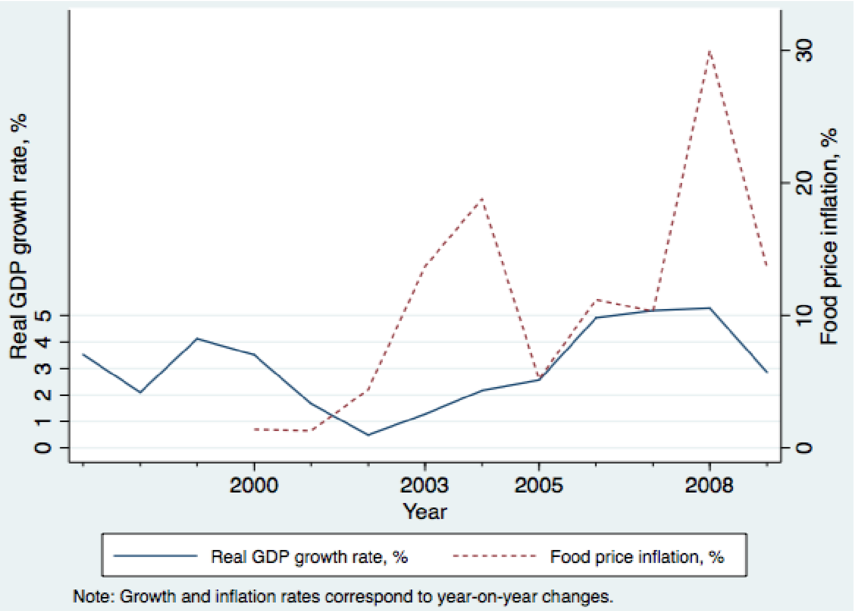


Figure 3.2: GDP Growth Per Capita and Food Price Inflation in Egypt, 1997-2009



Note: Growth and inflation rates correspond to year-on-year changes.

Figure 3.3: Weights-for-Height Over Time, By Region

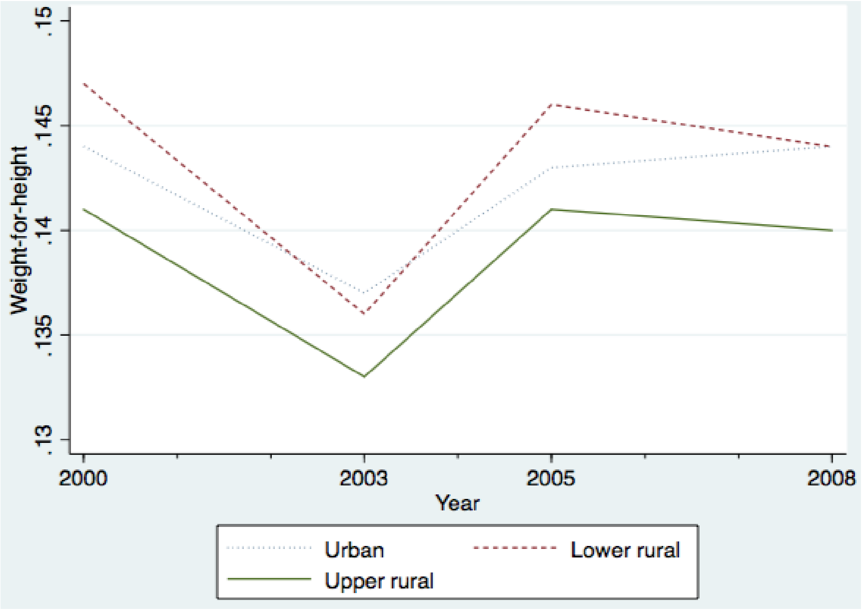


Figure 3.4: Weight-for-Height Changes for Urban Children, By Conditional Quantile

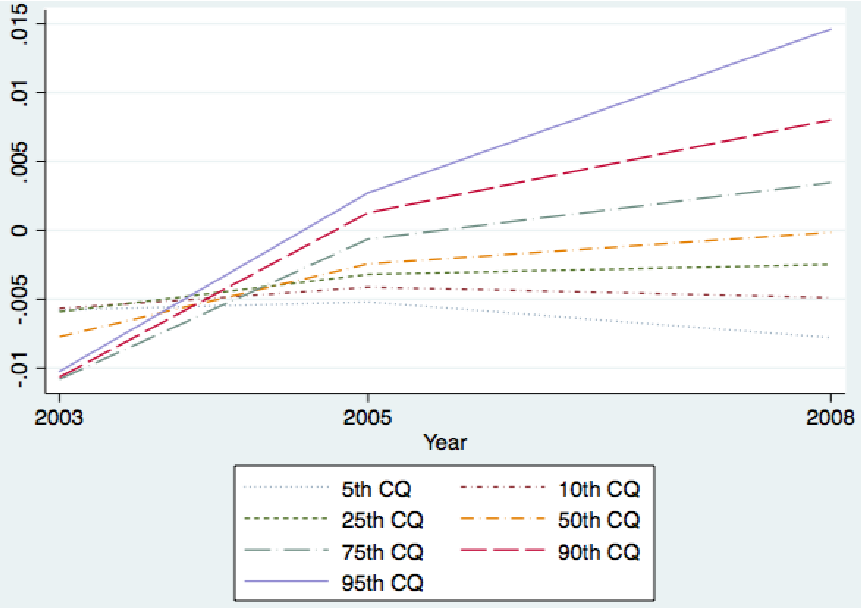


Figure 3.5: Weight-for-Height Changes for Lower Rural Children, By Conditional Quantile

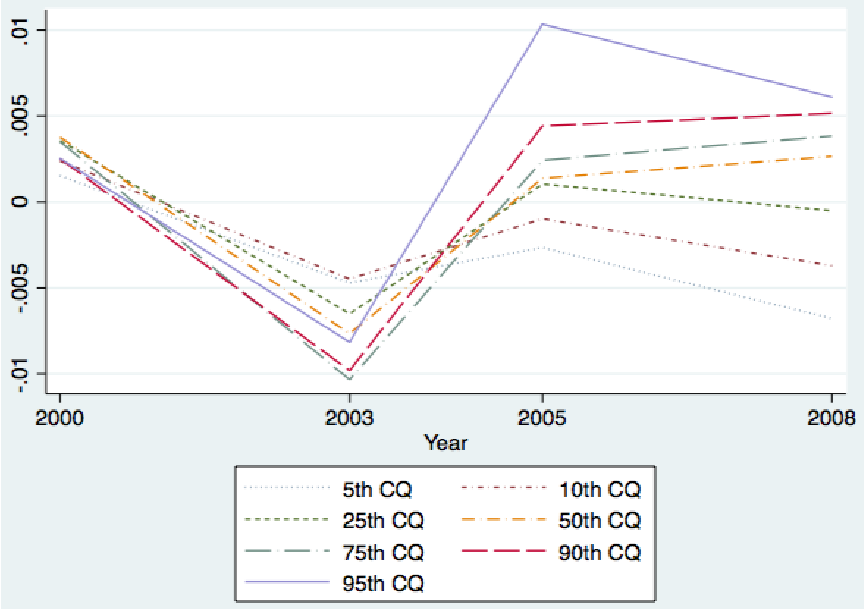


Figure 3.6: Weight-for-Height Changes for Upper Rural Children, By Conditional Quantile

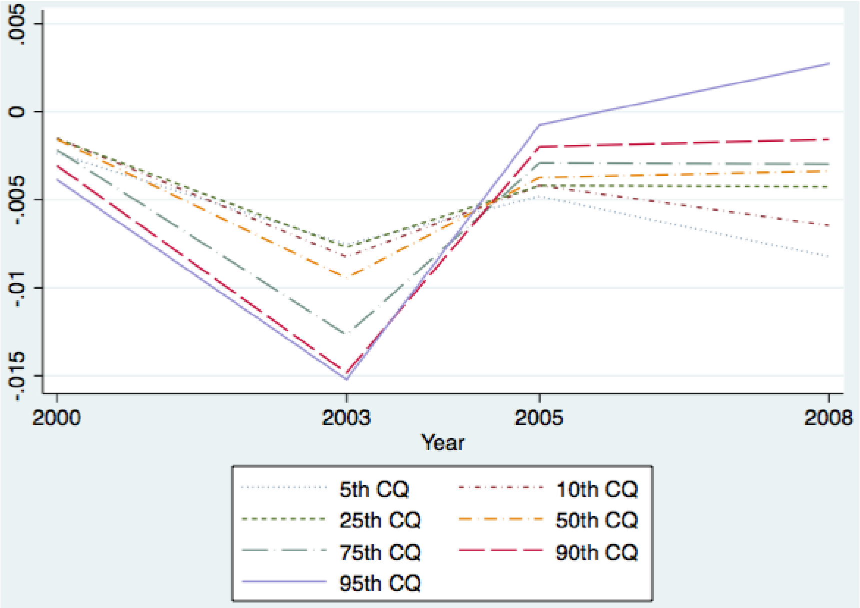


Figure 3.7: Weight-for-Height Changes for Urban Children, By Conditional Quantile

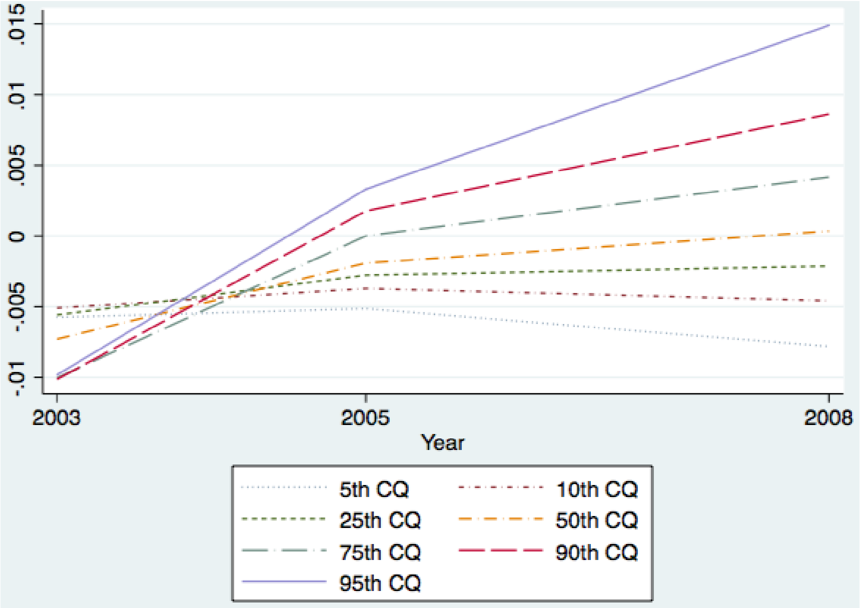


Figure 3.8: Weight-for-Height Changes for Lower Rural Children With Self-Employed Farmer Parents, By Conditional Quantile

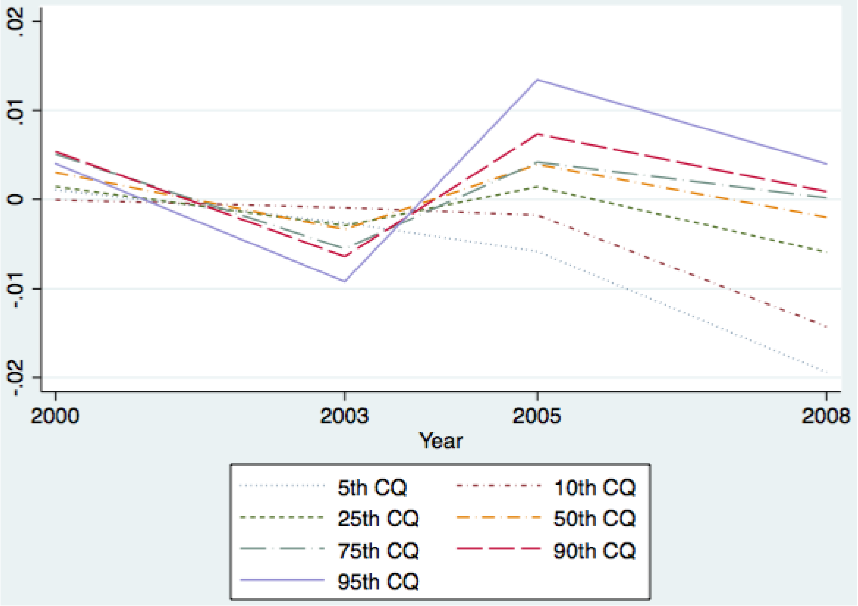


Figure 3.9: Weight-for-Height Changes for Lower Rural Children With Agricultural Employee Parents, By Conditional Quantile

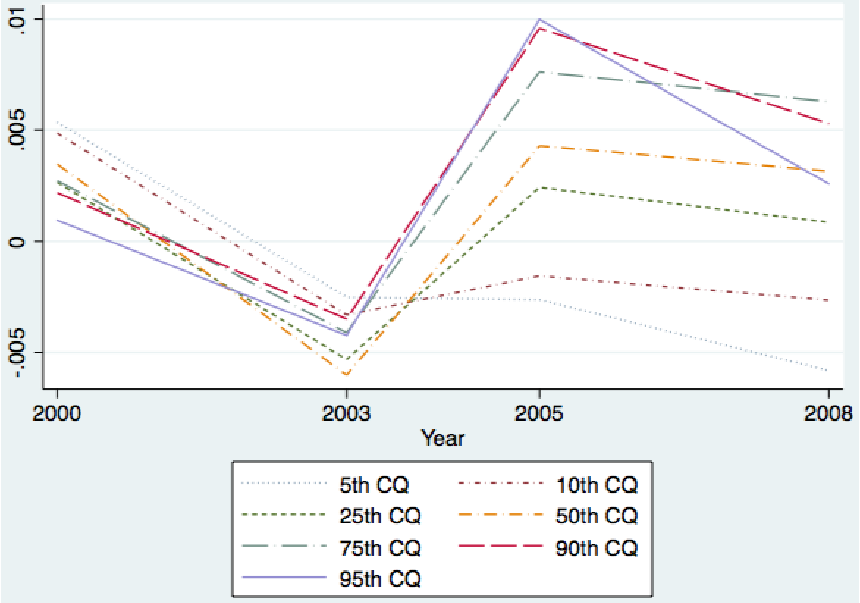


Figure 3.10: Weight-for-Height Changes for Lower Rural Children With Non-Agricultural Worker Parents, By Conditional Quantile

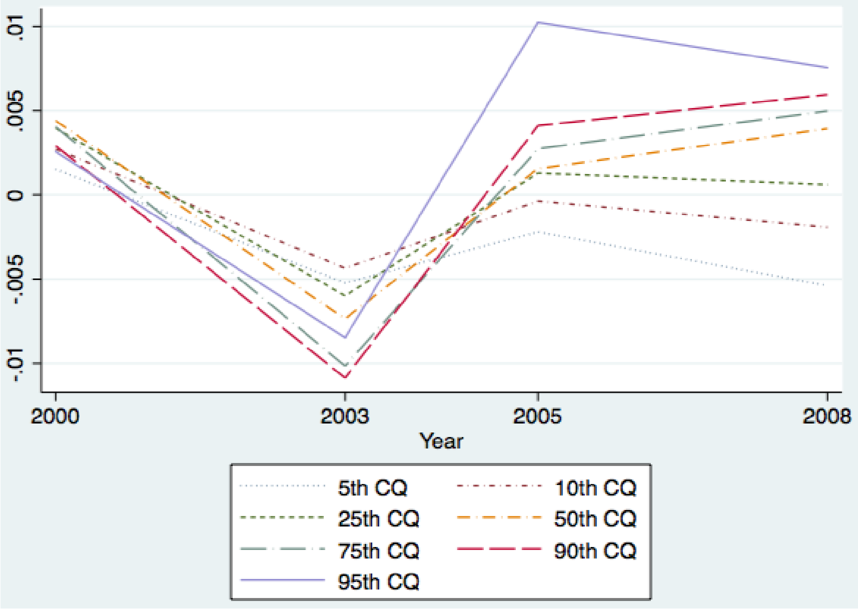


Figure 3.11: Weight-for-Height Changes for Upper Rural Children With Self-Employed Farmer Parents, By Conditional Quantile

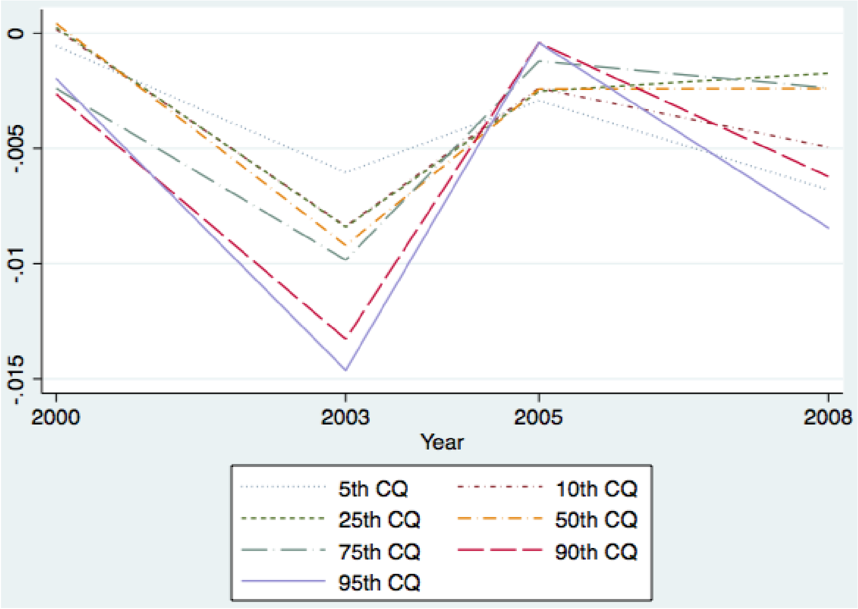


Figure 3.12: Weight-for-Height Changes for Upper Rural Children With Agricultural Employee Parents, By Conditional Quantile

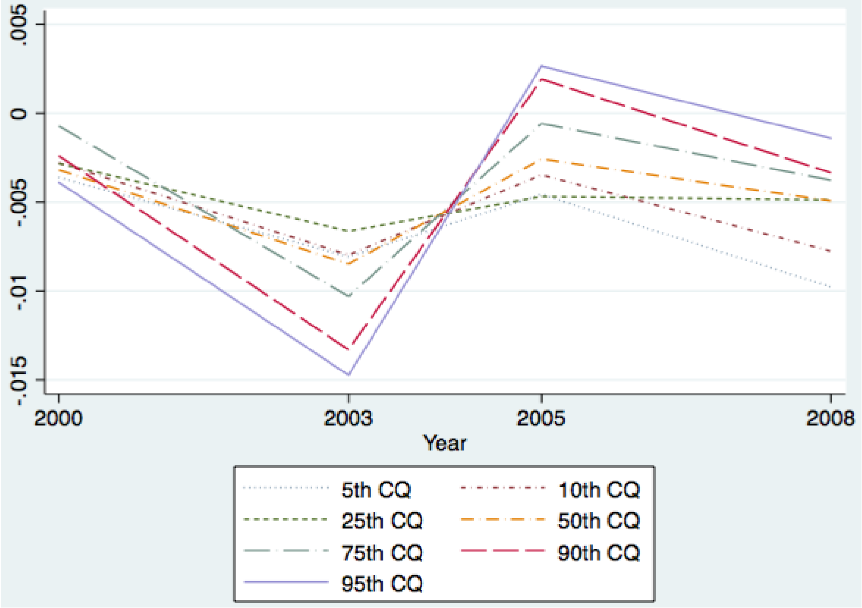


Figure 3.13: Weight-for-Height Changes for Upper Rural Children With Non-Agricultural Worker Parents, By Conditional Quantile

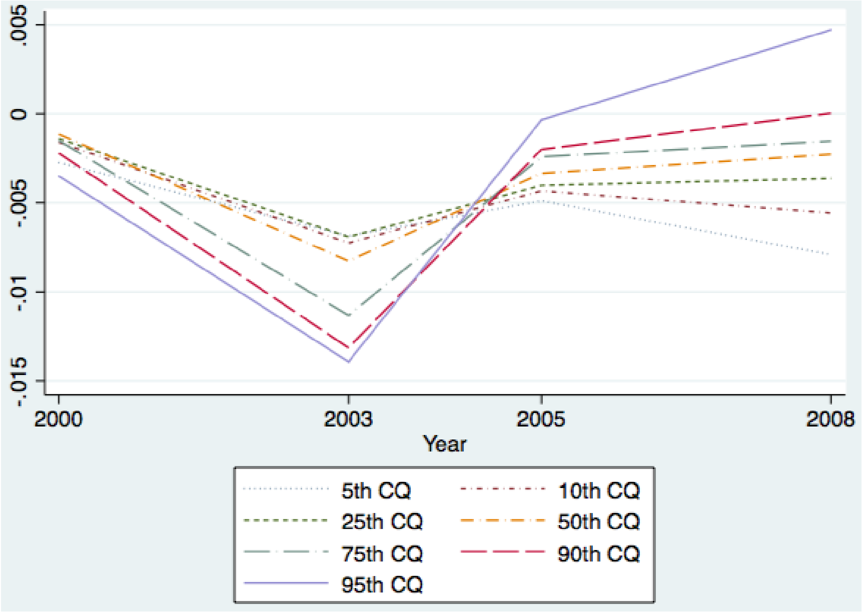


Table 3.1: Demographic and Health Survey Years and Months

2000	February-April
2003	May-June
2005	April-June
2008	March-June

Table 3.2: Descriptive Statistics

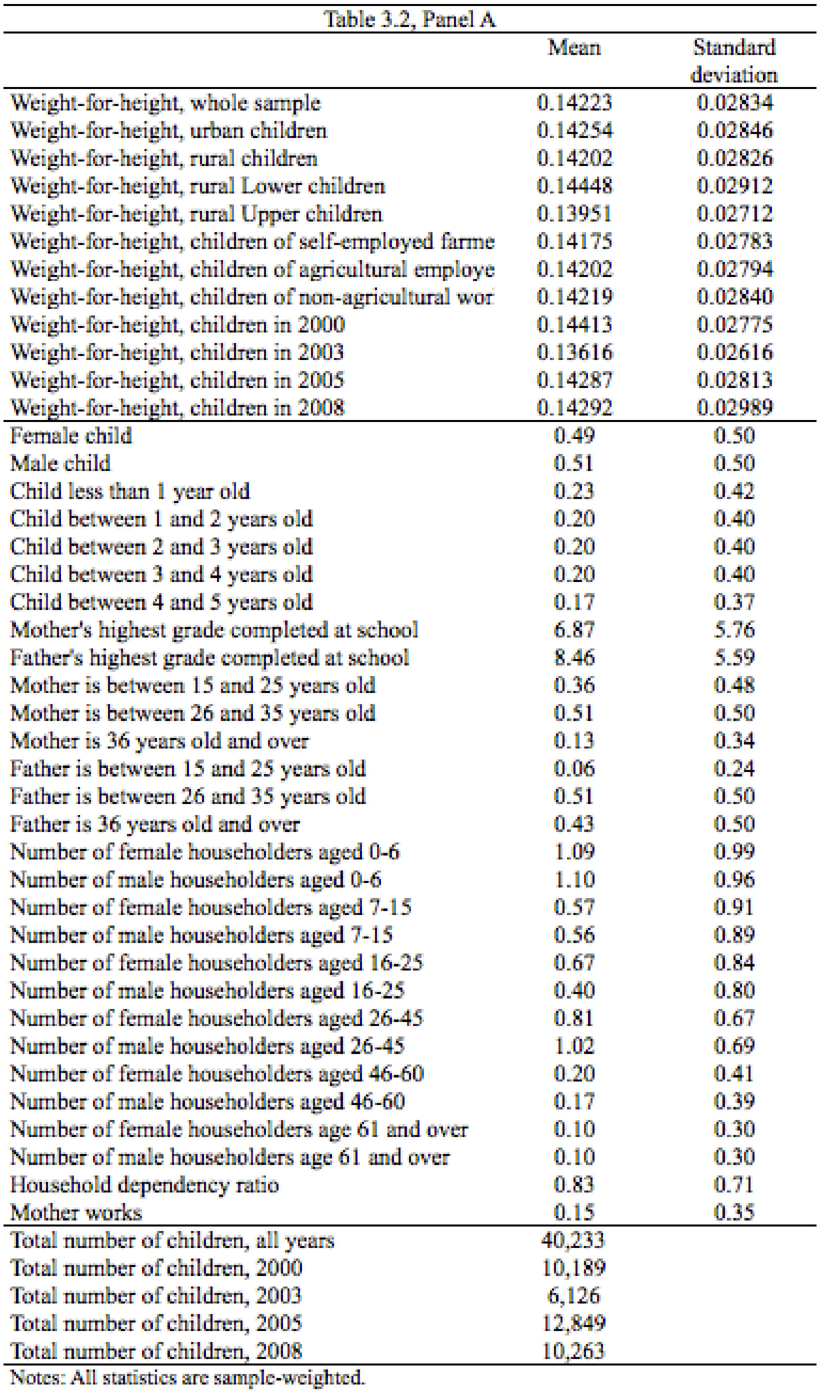


Table 3.2, Panel A

	Mean	Standard deviation
Weight-for-height, whole sample	0.14223	0.02834
Weight-for-height, urban children	0.14254	0.02846
Weight-for-height, rural children	0.14202	0.02826
Weight-for-height, rural Lower children	0.14448	0.02912
Weight-for-height, rural Upper children	0.13951	0.02712
Weight-for-height, children of self-employed farme	0.14175	0.02783
Weight-for-height, children of agricultural employe	0.14202	0.02794
Weight-for-height, children of non-agricultural wor	0.14219	0.02840
Weight-for-height, children in 2000	0.14413	0.02775
Weight-for-height, children in 2003	0.13616	0.02616
Weight-for-height, children in 2005	0.14287	0.02813
Weight-for-height, children in 2008	0.14292	0.02989
Female child	0.49	0.50
Male child	0.51	0.50
Child less than 1 year old	0.23	0.42
Child between 1 and 2 years old	0.20	0.40
Child between 2 and 3 years old	0.20	0.40
Child between 3 and 4 years old	0.20	0.40
Child between 4 and 5 years old	0.17	0.37
Mother's highest grade completed at school	6.87	5.76
Father's highest grade completed at school	8.46	5.59
Mother is between 15 and 25 years old	0.36	0.48
Mother is between 26 and 35 years old	0.51	0.50
Mother is 36 years old and over	0.13	0.34
Father is between 15 and 25 years old	0.06	0.24
Father is between 26 and 35 years old	0.51	0.50
Father is 36 years old and over	0.43	0.50
Number of female householders aged 0-6	1.09	0.99
Number of male householders aged 0-6	1.10	0.96
Number of female householders aged 7-15	0.57	0.91
Number of male householders aged 7-15	0.56	0.89
Number of female householders aged 16-25	0.67	0.84
Number of male householders aged 16-25	0.40	0.80
Number of female householders aged 26-45	0.81	0.67
Number of male householders aged 26-45	1.02	0.69
Number of female householders aged 46-60	0.20	0.41
Number of male householders aged 46-60	0.17	0.39
Number of female householders age 61 and over	0.10	0.30
Number of male householders age 61 and over	0.10	0.30
Household dependency ratio	0.83	0.71
Mother works	0.15	0.35
Total number of children, all years	40,233	
Total number of children, 2000	10,189	
Total number of children, 2003	6,126	
Total number of children, 2005	12,849	
Total number of children, 2008	10,263	

Notes: All statistics are sample-weighted.

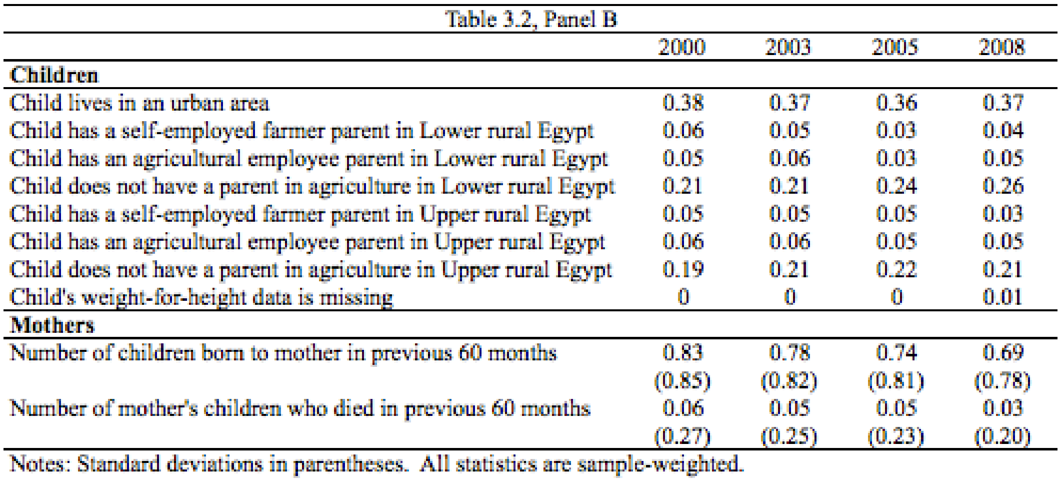


Table 3.2, Panel B

	2000	2003	2005	2008
Children				
Child lives in an urban area	0.38	0.37	0.36	0.37
Child has a self-employed farmer parent in Lower rural Egypt	0.06	0.05	0.03	0.04
Child has an agricultural employee parent in Lower rural Egypt	0.05	0.06	0.03	0.05
Child does not have a parent in agriculture in Lower rural Egypt	0.21	0.21	0.24	0.26
Child has a self-employed farmer parent in Upper rural Egypt	0.05	0.05	0.05	0.03
Child has an agricultural employee parent in Upper rural Egypt	0.06	0.06	0.05	0.05
Child does not have a parent in agriculture in Upper rural Egypt	0.19	0.21	0.22	0.21
Child's weight-for-height data is missing	0	0	0	0.01
Mothers				
Number of children born to mother in previous 60 months	0.83	0.78	0.74	0.69
	(0.85)	(0.82)	(0.81)	(0.78)
Number of mother's children who died in previous 60 months	0.06	0.05	0.05	0.03
	(0.27)	(0.25)	(0.23)	(0.20)

Notes: Standard deviations in parentheses. All statistics are sample-weighted.

Table 3.3: Region-Specific OLS Results

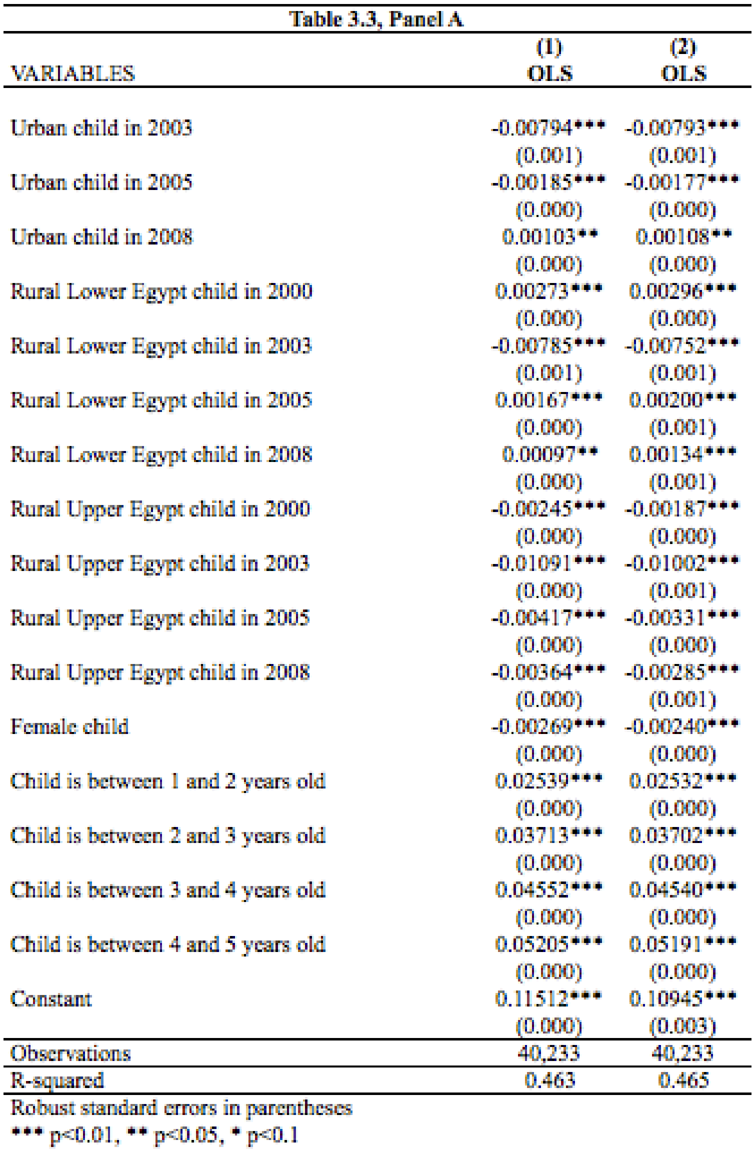


Table 3.3, Panel A

VARIABLES	**(1)** **OLS**	**(2)** **OLS**
Urban child in 2003	-0.00794***	-0.00793***
	(0.001)	(0.001)
Urban child in 2005	-0.00185***	-0.00177***
	(0.000)	(0.000)
Urban child in 2008	0.00103**	0.00108**
	(0.000)	(0.000)
Rural Lower Egypt child in 2000	0.00273***	0.00296***
	(0.000)	(0.000)
Rural Lower Egypt child in 2003	-0.00785***	-0.00752***
	(0.001)	(0.001)
Rural Lower Egypt child in 2005	0.00167***	0.00200***
	(0.000)	(0.001)
Rural Lower Egypt child in 2008	0.00097**	0.00134***
	(0.000)	(0.001)
Rural Upper Egypt child in 2000	-0.00245***	-0.00187***
	(0.000)	(0.000)
Rural Upper Egypt child in 2003	-0.01091***	-0.01002***
	(0.000)	(0.001)
Rural Upper Egypt child in 2005	-0.00417***	-0.00331***
	(0.000)	(0.000)
Rural Upper Egypt child in 2008	-0.00364***	-0.00285***
	(0.000)	(0.001)
Female child	-0.00269***	-0.00240***
	(0.000)	(0.000)
Child is between 1 and 2 years old	0.02539***	0.02532***
	(0.000)	(0.000)
Child is between 2 and 3 years old	0.03713***	0.03702***
	(0.000)	(0.000)
Child is between 3 and 4 years old	0.04552***	0.04540***
	(0.000)	(0.000)
Child is between 4 and 5 years old	0.05205***	0.05191***
	(0.000)	(0.000)
Constant	0.11512***	0.10945***
	(0.000)	(0.003)
Observations	40,233	40,233
R-squared	0.463	0.465

Robust standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3.3, Panel B

VARIABLES	(1) OLS	(2) OLS
Mother's highest grade completed		0.00003
		(0.000)
Father's highest grade completed		0.00010***
		(0.000)
Mother's age		0.00012
		(0.000)
Mother's age, squared		-0.00000
		(0.000)
Father's age		0.00011
		(0.000)
Father's age, squared		-0.00000
		(0.000)
Number of female householders aged 0-6		-0.00050***
		(0.000)
Number of male householders aged 0-6		-0.00023
		(0.000)
Number of female householders aged 7-15		0.00005
		(0.000)
Number of male householders aged 7-15		-0.00021
		(0.000)
Number of female householders aged 16-25		-0.00017
		(0.000)
Number of male householders aged 16-25		0.00014
		(0.000)
Number of female householders aged 26-45		-0.00007
		(0.000)
Number of male householders aged 26-45		0.00060***
		(0.000)
Number of female householders aged 46-60		0.00002
		(0.000)
Number of male householders aged 46-60		-0.00051
		(0.000)
Number of female householders aged 61 and over		-0.00020
		(0.000)
Number of male householders aged 61 and over		-0.00014
		(0.000)
Household dependency ratio		0.00040*
		(0.000)
Mother works indicator		0.00076**
		(0.000)
Observations	40,233	40,233
R-squared	0.463	0.465

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 3.4: Region and Parental Occupation-Specific OLS Results

Table 3.4, Panel A

VARIABLES	(1) OLS	(2) OLS
Urban Egyptian children		
Urban child in 2003	-0.00755***	-0.00749***
	(0.001)	(0.001)
Urban child in 2005	-0.00146***	-0.00134***
	(0.000)	(0.000)
Urban child in 2008	0.00142***	0.00152***
	(0.000)	(0.000)
Rural Lower Egyptian children		
Rural Lower Egypt child with at least one self-employed farmer parent in 2000	0.00244**	0.00306***
	(0.001)	(0.001)
Rural Lower Egypt child with at least one self-employed farmer parent in 2003	-0.00514***	-0.00460***
	(0.002)	(0.002)
Rural Lower Egypt child with at least one self-employed farmer parent in 2005	0.00287**	0.00349***
	(0.001)	(0.001)
Rural Lower Egypt child with at least one self-employed farmer parent in 2008	-0.00441***	-0.00350***
	(0.001)	(0.001)
Rural Lower Egypt child with at least one agricultural employee parent in 2000	0.00252**	0.00303***
	(0.001)	(0.001)
Rural Lower Egypt child with at least one agricultural employee parent in 2003	-0.00509***	-0.00441***
	(0.001)	(0.001)
Rural Lower Egypt child with at least one agricultural employee parent in 2005	0.00362***	0.00449***
	(0.001)	(0.001)
Rural Lower Egypt child with at least one agricultural employee parent in 2008	0.00100	0.00180*
	(0.001)	(0.001)
Rural Lower Egypt child whose parents do not work in agriculture in 2000	0.00330***	0.00325***
	(0.001)	(0.001)
Rural Lower Egypt child whose parents do not work in agriculture in 2003	-0.00777***	-0.00753***
	(0.001)	(0.001)
Rural Lower Egypt child whose parents do not work in agriculture in 2005	0.00175***	0.00204***
	(0.001)	(0.001)
Rural Lower Egypt child whose parents do not work in agriculture in 2008	0.00225***	0.00250***
	(0.001)	(0.001)
Rural Upper Egyptian children		
Rural Upper Egypt child with at least one self-employed farmer parent in 2000	-0.00079	0.00003
	(0.001)	(0.001)
Rural Upper Egypt child with at least one self-employed farmer parent in 2003	-0.01085***	-0.00968***
	(0.001)	(0.001)
Rural Upper Egypt child with at least one self-employed farmer parent in 2005	-0.00325***	-0.00206***
	(0.001)	(0.001)
Rural Upper Egypt child with at least one self-employed farmer parent in 2008	-0.00401***	-0.00293**
	(0.001)	(0.001)
Rural Upper Egypt child with at least one agricultural employee parent in 2000	-0.00289***	-0.00208**
	(0.001)	(0.001)
Rural Upper Egypt child with at least one agricultural employee parent in 2003	-0.01025***	-0.00904***
	(0.001)	(0.001)
Rural Upper Egypt child with at least one agricultural employee parent in 2005	-0.00295***	-0.00166**
	(0.001)	(0.001)
Rural Upper Egypt child with at least one agricultural employee parent in 2008	-0.00536***	-0.00411***
	(0.001)	(0.001)
Rural Upper Egypt child whose parents do not work in agriculture in 2000	-0.00208***	-0.00181***
	(0.001)	(0.001)
Rural Upper Egypt child whose parents do not work in agriculture in 2003	-0.00972***	-0.00898***
	(0.001)	(0.001)
Rural Upper Egypt child whose parents do not work in agriculture in 2005	-0.00387***	-0.00308***
	(0.000)	(0.000)
Rural Upper Egypt child whose parents do not work in agriculture in 2008	-0.00253***	-0.00181***
	(0.000)	(0.001)
Constant	0.11473***	0.10847***
	(0.000)	(0.003)
Observations	40,233	40,233
R-squared	0.463	0.465

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

Table 3.4, Panel B

VARIABLES	(1) OLS	(2) OLS
Female child	0.02538***	0.02530***
	(0.000)	(0.000)
Child is between 1 and 2 years old	0.03714***	0.03700***
	(0.000)	(0.000)
Child is between 2 and 3 years old	0.04553***	0.04538***
	(0.000)	(0.000)
Child is between 3 and 4 years old	0.05207***	0.05190***
	(0.000)	(0.000)
Child is between 4 and 5 years old		0.00004
		(0.000)
Mother's highest grade completed		0.00010***
		(0.000)
Father's highest grade completed		0.00014
		(0.000)
Mother's age		-0.00000
		(0.000)
Mother's age, squared		0.00011
		(0.000)
Father's age		-0.00000
		(0.000)
Father's age, squared		-0.00053***
		(0.000)
Number of female householders aged 0-6		-0.00025*
		(0.000)
Number of male householders aged 0-6		0.00004
		(0.000)
Number of female householders aged 7-15		-0.00023
		(0.000)
Number of male householders aged 7-15		-0.00013
		(0.000)
Number of female householders aged 16-25		0.00014
		(0.000)
Number of male householders aged 16-25		-0.00006
		(0.000)
Number of female householders aged 26-45		0.00062***
		(0.000)
Number of male householders aged 26-45		-0.00003
		(0.000)
Number of female householders aged 46-60		-0.00052
		(0.000)
Number of male householders aged 46-60		-0.00029
		(0.000)
Number of female householders aged 61 and over		-0.00020
		(0.000)
Number of male householders aged 61 and over		0.00055**
		(0.000)
Household dependency ratio		0.00073**
		(0.000)
Mother works indicator		0.00041
		(0.000)
Observations	40,233	40,233
R-squared	0.463	0.465

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

Table 3.5: Region-Specific Quantile Regression Baseline Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	5P	10P	25P	50P	75P	90P	95P
Urban child in 2003	-0.00580***	-0.00569***	-0.00597***	-0.00792***	-0.01059***	-0.01113***	-0.00942***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
Urban child in 2005	-0.00506***	-0.00436***	-0.00358***	-0.00225***	-0.00053	0.00112	0.00323**
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Urban child in 2008	-0.00775***	-0.00523***	-0.00265***	-0.00006	0.00358***	0.00799***	0.01533***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Rural Lower Egypt child in 2000	0.00137	0.00196***	0.00313***	0.00354***	0.00298***	0.00219**	0.00228**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Rural Lower Egypt child in 2003	-0.00473***	-0.00479***	-0.00665***	-0.00793***	-0.01033***	-0.01083***	-0.00755***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Rural Lower Egypt child in 2005	-0.00285***	-0.00141	0.00039	0.00120***	0.00207***	0.00369***	0.01052***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.002)
Rural Lower Egypt child in 2008	-0.00702***	-0.00400***	-0.00105*	0.00238***	0.00359***	0.00424***	0.00595***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Rural Upper Egypt child in 2000	-0.00263***	-0.00218***	-0.00204***	-0.00195***	-0.00300***	-0.00454***	-0.00491***
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Rural Upper Egypt child in 2003	-0.00761***	-0.00887***	-0.00843***	-0.01013***	-0.01344***	-0.01637***	-0.01580***
	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Rural Upper Egypt child in 2005	-0.00515***	-0.00489***	-0.00492***	-0.00451***	-0.00418***	-0.00372***	-0.00163
	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Rural Upper Egypt child in 2008	-0.00875***	-0.00712***	-0.00482***	-0.00390***	-0.00384***	-0.00315***	0.00246
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Female child	-0.00173***	-0.00240***	-0.00256***	-0.00303***	-0.00285***	-0.00265***	-0.00262***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Child is between 1 and 2 years old	0.03888***	0.03740***	0.02941***	0.02291***	0.02025***	0.01978***	0.02211***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Child is between 2 and 3 years old	0.04997***	0.04915***	0.04086***	0.03448***	0.03199***	0.03260***	0.03430***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Child is between 3 and 4 years old	0.05852***	0.05697***	0.04868***	0.04286***	0.04068***	0.04165***	0.04363***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Child is between 4 and 5 years old	0.06459***	0.06262***	0.05467***	0.04910***	0.04753***	0.04892***	0.05343***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Constant	0.07620***	0.08364***	0.10015***	0.11592***	0.13010***	0.14308***	0.14947***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Observations	40,233	40,233	40,233	40,233	40,233	40,233	40,233

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

Table 3.6: Region-Specific Quantile Regression Enhanced Specification Results

Table 3.6, Panel A

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	**5P**	**10P**	**25P**	**50P**	**75P**	**90P**	**95P**
Urban child in 2003	-0.00580***	-0.00568***	-0.00592***	-0.00773***	-0.01081***	-0.01066***	-0.01024***
	-0.001	-0.001	-0.001	0	-0.001	-0.001	-0.002
Urban child in 2005	-0.00521***	-0.00414***	-0.00320***	-0.00244***	-0.00064	0.00127	0.00272*
	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.002
Urban child in 2008	-0.00780***	-0.00488***	-0.00250***	-0.00015	0.00346***	0.00801***	0.01460***
	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.002
Rural Lower Egypt child in 2000	0.00152*	0.00239***	0.00357***	0.00377***	0.00350***	0.00251***	0.00253**
	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001
Rural Lower Egypt child in 2003	-0.00470***	-0.00447***	-0.00650***	-0.00765***	-0.01033***	-0.00980***	-0.00815***
	-0.001	-0.001	-0.001	-0.001	-0.001	-0.002	-0.002
Rural Lower Egypt child in 2005	-0.00265**	-0.00098	0.00104*	0.00138***	0.00242***	0.00441***	0.01034***
	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.002
Rural Lower Egypt child in 2008	-0.00677***	-0.00371***	-0.0005	0.00265***	0.00384***	0.00516***	0.00610***
	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.002
Rural Upper Egypt child in 2000	-0.00237***	-0.00151**	-0.00153***	-0.00156***	-0.00217***	-0.00306***	-0.00386***
	-0.001	-0.001	-0.001	0	-0.001	-0.001	-0.001
Rural Upper Egypt child in 2003	-0.00753***	-0.00825***	-0.00770***	-0.00945***	-0.01269***	-0.01484***	-0.01522***
	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.002
Rural Upper Egypt child in 2005	-0.00481***	-0.00419***	-0.00420***	-0.00374***	-0.00291***	-0.00198**	-0.00074
	-0.001	-0.001	-0.001	0	-0.001	-0.001	-0.001
Rural Upper Egypt child in 2008	-0.00823***	-0.00646***	-0.00426***	-0.00336***	-0.00298***	-0.00157	0.00273
	-0.001	-0.001	-0.001	0	-0.001	-0.001	-0.002
Female child	-0.00176***	-0.00226***	-0.00227***	-0.00275***	-0.00291***	-0.00177***	-0.00203**
	0	0	0	0	0	-0.001	-0.001
Child is between 1 and 2 years old	0.03891***	0.03723***	0.02923***	0.02287***	0.02025***	0.01957***	0.02219***
	-0.001	-0.001	0	0	0	-0.001	-0.001
Child is between 2 and 3 years old	0.05006***	0.04904***	0.04063***	0.03439***	0.03192***	0.03219***	0.03445***
	-0.001	-0.001	0	0	-0.001	-0.001	-0.001
Child is between 3 and 4 years old	0.05847***	0.05682***	0.04845***	0.04272***	0.04067***	0.04130***	0.04361***
	-0.001	-0.001	0	0	-0.001	-0.001	-0.001
Child is between 4 and 5 years old	0.06462***	0.06247***	0.05449***	0.04892***	0.04745***	0.04851***	0.05309***
	-0.001	-0.001	0	0	0	-0.001	-0.001
Constant	0.07886***	0.07909***	0.09469***	0.10875***	0.12718***	0.12961***	0.14125***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.006)	(0.010)
Observations	40,233	40,233	40,233	40,233	40,233	40,233	40,233

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

Table 3.6, Panel B

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	**5P**	**10P**	**25P**	**50P**	**75P**	**90P**	**95P**
Mother's highest grade completed	-0.00001	0.00001	0.00001	0.00004	0.00005	0.00003	0.00004
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Father's highest grade completed	0.00006	0.00008**	0.00009***	0.00009***	0.00014***	0.00016***	0.00009
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mother's age	-0.00011	0.00022	0.00003	0.00011	-0.00005	0.00043	0.00025
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Mother's age, squared	0.00000	-0.00000	-0.00000	-0.00000	0.00000	-0.00001	-0.00000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Father's age	-0.00013	-0.00002	0.00019**	0.00021**	0.00008	0.00018	0.00008
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Father's age, squared	0.00000	0.00000	-0.00000	-0.00000*	-0.00000	-0.00000	-0.00000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of female householders aged 0-6	-0.00005	-0.00014	-0.00029*	-0.00039**	-0.00057***	-0.00091***	-0.00077*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of male householders aged 0-6	-0.00002	-0.00001	0.00003	-0.00015	-0.00050**	-0.00011	-0.00027
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of female householders aged 7-15	0.00013	0.00008	0.00012	0.00016	0.00005	-0.00037	-0.00024
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of male householders aged 7-15	-0.00014	-0.00007	-0.00007	-0.00016	-0.00019	-0.00069**	-0.00114**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of female householders aged 16-25	-0.00002	0.00009	-0.00013	-0.00014	-0.00029	-0.00037	-0.00031
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Number of male householders aged 16-25	-0.00025	-0.00026	0.00005	0.00023	0.00017	0.00067	0.00073
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Number of female householders aged 26-45	-0.00056*	-0.00065*	-0.00028	0.00005	0.00027	0.00054	0.00023
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Number of male householders aged 26-45	0.00083**	0.00054*	0.00029	0.00016	0.00045*	0.00078*	0.00142*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Number of female householders aged 46-60	0.00018	0.00017	-0.00023	-0.00005	0.00051	0.00031	-0.00006
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Number of male householders aged 46-60	-0.00034	-0.00047	-0.00040	-0.00069**	-0.00082*	-0.00029	0.00048
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Number of female householders aged 61 and over	-0.00031	0.00036	-0.00004	-0.00005	-0.00042	-0.00087	0.00013
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Number of male householders aged 61 and over	-0.00006	-0.00009	0.00002	0.00020	-0.00052	-0.00050	-0.00104
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Household dependency ratio	0.00025	0.00024	0.00038	0.00034	0.00041	0.00075*	-0.00005
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Mother works indicator	0.00066	0.00028	0.00056	0.00044	0.00081*	0.00116**	0.00082
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Observations	40,233	40,233	40,233	40,233	40,233	40,233	40,233

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 3.7: Region-Specific Inter-Quantile Regression Results

VARIABLES	(1) **95P - 5P**
Urban child in 2003	-0.00444***
	(0.001)
Urban child in 2005	0.00793***
	(0.002)
Urban child in 2008	0.02241***
	(0.003)
Rural Lower Egypt child in 2000	0.00101
	(0.001)
Rural Lower Egypt child in 2003	-0.00345
	(0.002)
Rural Lower Egypt child in 2005	0.01299***
	(0.002)
Rural Lower Egypt child in 2008	0.01287***
	(0.002)
Rural Upper Egypt child in 2000	-0.00150
	(0.002)
Rural Upper Egypt child in 2003	-0.00769***
	(0.002)
Rural Upper Egypt child in 2005	0.00407**
	(0.002)
Rural Upper Egypt child in 2008	0.01096***
	(0.002)
Female child	-0.00028
	(0.001)
Child is between 1 and 2 years old	-0.01672***
	(0.001)
Child is between 2 and 3 years old	-0.01562***
	(0.001)
Child is between 3 and 4 years old	-0.01486***
	(0.001)
Child is between 4 and 5 years old	-0.01153***
	(0.001)
Constant	0.06239***
	(0.007)
Observations	40,233

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

Table 3.8: Region and Parental Occupation-Specific Baseline Quantile Regression Results

Table 3.8, Panel A

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	5P	10P	25P	50P	75P	90P	95P
Urban Egyptian children							
Urban child in 2003	-0.00567***	-0.00551***	-0.00556***	-0.00751***	-0.00992***	-0.01052***	-0.00905***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
Urban child in 2005	-0.00486***	-0.00406***	-0.00328***	-0.00183***	0.00011	0.00175**	0.00349***
	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Urban child in 2008	-0.00759***	-0.00496***	-0.00225***	0.00034	0.00418***	0.00861***	0.01548***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.002)
Rural Lower Egyptian children							
Rural Lower Egypt child with at least one self-employed farmer parent in 2000	0.00095	0.00018	0.00129	0.00250*	0.00411***	0.00382*	0.00350
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Rural Lower Egypt child with at least one self-employed farmer parent in 2003	-0.00375	-0.00159	-0.00302**	-0.00381***	-0.00713***	-0.00724*	-0.00885**
	(0.003)	(0.002)	(0.001)	(0.001)	(0.002)	(0.004)	(0.004)
Rural Lower Egypt child with at least one self-employed farmer parent in 2005	-0.00520**	-0.00269	0.00092	0.00322**	0.00349**	0.00661*	0.01395***
	(0.003)	(0.002)	(0.001)	(0.001)	(0.002)	(0.004)	(0.005)
Rural Lower Egypt child with at least one self-employed farmer parent in 2008	-0.01788***	-0.01494***	-0.00726***	-0.00336*	-0.00047	-0.00070	0.00263
	(0.002)	(0.003)	(0.002)	(0.002)	(0.001)	(0.003)	(0.004)
Rural Lower Egypt child with at least one agricultural employee parent in 2000	0.00527***	0.00420***	0.00282***	0.00257***	0.00167	0.00112	-0.00078
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)
Rural Lower Egypt child with at least one agricultural employee parent in 2003	-0.00225	-0.00424***	-0.00570***	-0.00620***	-0.00435**	-0.00413	-0.00459
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	(0.004)
Rural Lower Egypt child with at least one agricultural employee parent in 2005	-0.00259	-0.00305	0.00106	0.00387**	0.00489**	0.00830***	0.00989*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.006)
Rural Lower Egypt child with at least one agricultural employee parent in 2008	-0.00608***	-0.00325	-0.00000	0.00242**	0.00475***	0.00333*	0.00114
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)
Rural Lower Egypt child whose parents do not work in agriculture in 2000	0.00162	0.00195*	0.00421***	0.00440***	0.00402***	0.00284***	0.00210
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
Rural Lower Egypt child whose parents do not work in agriculture in 2003	-0.00525***	-0.00486***	-0.00615***	-0.00757***	-0.01048***	-0.01098***	-0.00815***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Rural Lower Egypt child whose parents do not work in agriculture in 2005	-0.00261**	-0.00090	0.00072	0.00126**	0.00246***	0.00356***	0.00985***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.002)
Rural Lower Egypt child whose parents do not work in agriculture in 2008	-0.00541***	-0.00239***	0.00022	0.00354***	0.00454***	0.00549***	0.00780***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Constant	0.07590***	0.08348***	0.09984***	0.11548***	0.12938***	0.14247***	0.14932***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Observations	40,233	40,233	40,233	40,233	40,233	40,233	40,233

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

Table 3.8, Panel B

VARIABLES	(1) 5P	(2) 10P	(3) 25P	(4) 50P	(5) 75P	(6) 90P	(7) 95P
Rural Upper Egyptian children							
Rural Upper Egypt child with at least one self-employed farmer parent in 2000	-0.00044	-0.00046	0.00000	-0.00052	-0.00324***	-0.00401**	-0.00377
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.004)
Rural Upper Egypt child with at least one self-employed farmer parent in 2003	-0.00661***	-0.00911***	-0.00922***	-0.01017***	-0.01146***	-0.01547***	-0.01488***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)
Rural Upper Egypt child with at least one self-employed farmer parent in 2005	-0.00288**	-0.00325***	-0.00315***	-0.00345***	-0.00348***	-0.00246	-0.00098
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Rural Upper Egypt child with at least one self-employed farmer parent in 2008	-0.00572**	-0.00559***	-0.00253*	-0.00287**	-0.00385***	-0.00713***	-0.01076**
	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.005)
Rural Upper Egypt child with at least one agricultural employee parent in 2000	-0.00379**	-0.00368***	-0.00311***	-0.00380***	-0.00216*	-0.00435***	-0.00531*
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)
Rural Upper Egypt child with at least one agricultural employee parent in 2003	-0.00874***	-0.00886***	-0.00766***	-0.00968***	-0.01159***	-0.01407***	-0.01626***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Rural Upper Egypt child with at least one agricultural employee parent in 2005	-0.00544***	-0.00469***	-0.00556***	-0.00365***	-0.00229**	-0.00012	0.00153
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Rural Upper Egypt child with at least one agricultural employee parent in 2008	-0.00981***	-0.00884***	-0.00604***	-0.00604***	-0.00578***	-0.00493**	-0.00171
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.006)
Rural Upper Egypt child whose parents do not work in agriculture in 2000	-0.00256***	-0.00176**	-0.00142**	-0.00125**	-0.00210***	-0.00319***	-0.00444***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
Rural Upper Egypt child whose parents do not work in agriculture in 2003	-0.00661***	-0.00788***	-0.00744***	-0.00907***	-0.01230***	-0.01406***	-0.01488***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.002)
Rural Upper Egypt child whose parents do not work in agriculture in 2005	-0.00513***	-0.00484***	-0.00455***	-0.00420***	-0.00354***	-0.00339***	-0.00170
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
Rural Upper Egypt child whose parents do not work in agriculture in 2008	-0.00823***	-0.00643***	-0.00401***	-0.00280***	-0.00251***	-0.00108	0.00468**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Female child	-0.00184***	-0.00229***	-0.00259***	-0.00297***	-0.00285***	-0.00267***	-0.00262***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Child is between 1 and 2 years old	0.03894***	0.03715***	0.02937***	0.02298***	0.02026***	0.01979***	0.02167***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Child is between 2 and 3 years old	0.05020***	0.04902***	0.04068***	0.03451***	0.03211***	0.03265***	0.03393***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Child is between 3 and 4 years old	0.05870***	0.05681***	0.04858***	0.04282***	0.04071***	0.04159***	0.04351***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Child is between 4 and 5 years old	0.06472***	0.06247***	0.05476***	0.04908***	0.04768***	0.04892***	0.05321***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Observations	40,233	40,233	40,233	40,233	40,233	40,233	40,233

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

Table 3.9: Region and Parental Occupation-Specific Enhanced Specification Quantile Regression Results

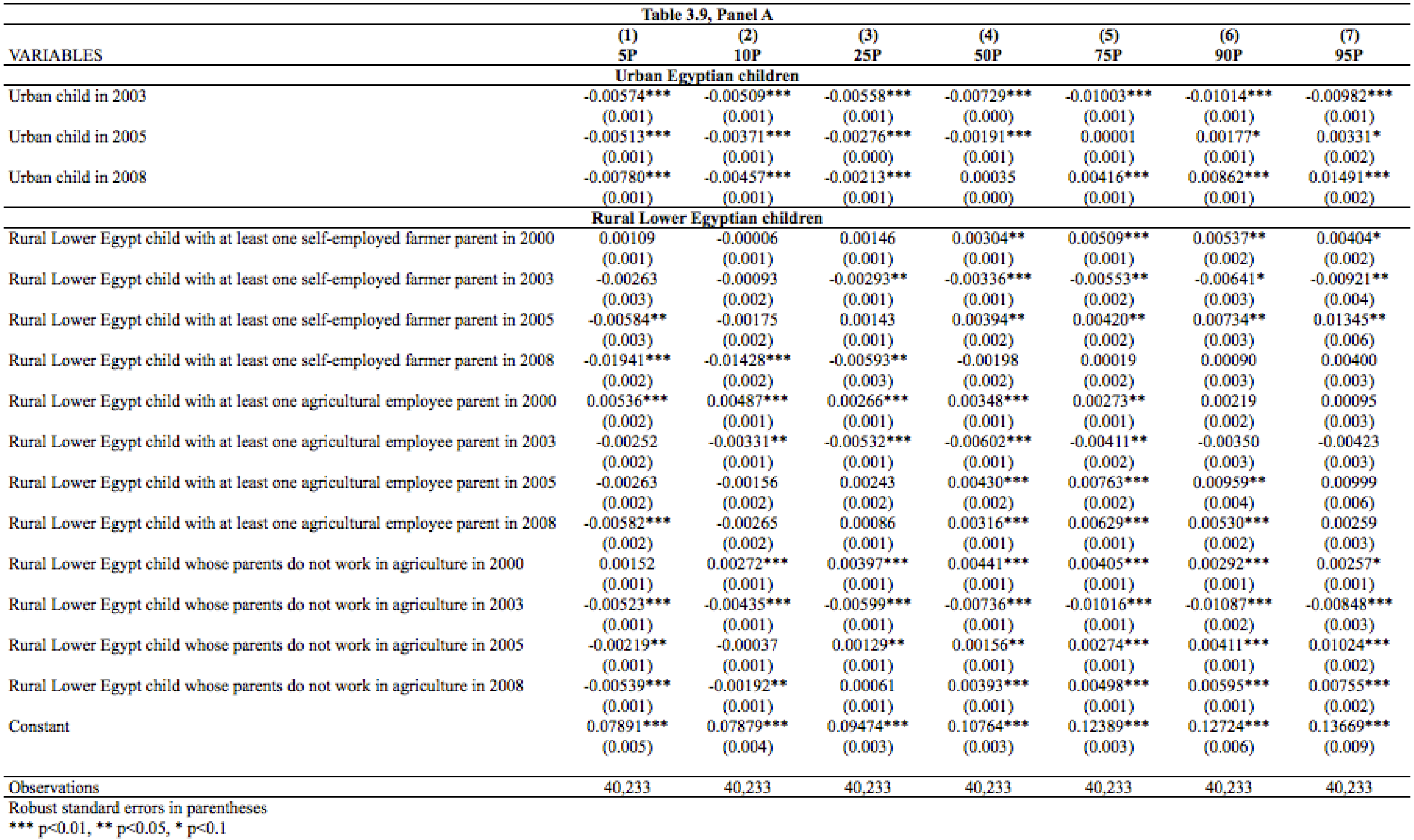


Table 3.9, Panel A

VARIABLES	(1) **5P**	(2) **10P**	(3) **25P**	(4) **50P**	(5) **75P**	(6) **90P**	(7) **95P**
Urban Egyptian children							
Urban child in 2003	-0.00574***	-0.00509***	-0.00558***	-0.00729***	-0.01003***	-0.01014***	-0.00982***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
Urban child in 2005	-0.00513***	-0.00371***	-0.00276***	-0.00191***	0.00001	0.00177*	0.00331*
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.002)
Urban child in 2008	-0.00780***	-0.00457***	-0.00213***	0.00035	0.00416***	0.00862***	0.01491***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.002)
Rural Lower Egyptian children							
Rural Lower Egypt child with at least one self-employed farmer parent in 2000	0.00109	-0.00006	0.00146	0.00304**	0.00509***	0.00537**	0.00404*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Rural Lower Egypt child with at least one self-employed farmer parent in 2003	-0.00263	-0.00093	-0.00293**	-0.00336***	-0.00553**	-0.00641*	-0.00921**
	(0.003)	(0.002)	(0.001)	(0.001)	(0.002)	(0.003)	(0.004)
Rural Lower Egypt child with at least one self-employed farmer parent in 2005	-0.00584**	-0.00175	0.00143	0.00394**	0.00420**	0.00734**	0.01345**
	(0.003)	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)	(0.006)
Rural Lower Egypt child with at least one self-employed farmer parent in 2008	-0.01941***	-0.01428***	-0.00593**	-0.00198	0.00019	0.00090	0.00400
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)
Rural Lower Egypt child with at least one agricultural employee parent in 2000	0.00536***	0.00487***	0.00266***	0.00348***	0.00273**	0.00219	0.00095
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)
Rural Lower Egypt child with at least one agricultural employee parent in 2003	-0.00252	-0.00331**	-0.00532***	-0.00602***	-0.00411**	-0.00350	-0.00423
	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)	(0.003)
Rural Lower Egypt child with at least one agricultural employee parent in 2005	-0.00263	-0.00156	0.00243	0.00430***	0.00763***	0.00959**	0.00999
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.006)
Rural Lower Egypt child with at least one agricultural employee parent in 2008	-0.00582***	-0.00265	0.00086	0.00316***	0.00629***	0.00530***	0.00259
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)
Rural Lower Egypt child whose parents do not work in agriculture in 2000	0.00152	0.00272***	0.00397***	0.00441***	0.00405***	0.00292***	0.00257*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Rural Lower Egypt child whose parents do not work in agriculture in 2003	-0.00523***	-0.00435***	-0.00599***	-0.00736***	-0.01016***	-0.01087***	-0.00848***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)
Rural Lower Egypt child whose parents do not work in agriculture in 2005	-0.00219**	-0.00037	0.00129**	0.00156**	0.00274***	0.00411***	0.01024***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Rural Lower Egypt child whose parents do not work in agriculture in 2008	-0.00539***	-0.00192**	0.00061	0.00393***	0.00498***	0.00595***	0.00755***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Constant	0.07891***	0.07879***	0.09474***	0.10764***	0.12389***	0.12724***	0.13669***
	(0.005)	(0.004)	(0.003)	(0.003)	(0.003)	(0.006)	(0.009)
Observations	40,233	40,233	40,233	40,233	40,233	40,233	40,233

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

Table 3.9, Panel B

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	**5P**	**10P**	**25P**	**50P**	**75P**	**90P**	**95P**
Rural Upper Egyptian children							
Rural Upper Egypt child with at least one self-employed farmer parent in 2000	-0.00054	0.00018	0.00025	0.00043	-0.00239**	-0.00263	-0.00197
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.004)
Rural Upper Egypt child with at least one self-employed farmer parent in 2003	-0.00604***	-0.00837***	-0.00841***	-0.00920***	-0.00984***	-0.01327***	-0.01462***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)
Rural Upper Egypt child with at least one self-employed farmer parent in 2005	-0.00292*	-0.00239***	-0.00252**	-0.00242***	-0.00121	-0.00041	-0.00041
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Rural Upper Egypt child with at least one self-employed farmer parent in 2008	-0.00679***	-0.00494**	-0.00173	-0.00240*	-0.00238**	-0.00622***	-0.00845**
	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.004)
Rural Upper Egypt child with at least one agricultural employee parent in 2000	-0.00361**	-0.00279**	-0.00282***	-0.00319***	-0.00072	-0.00240*	-0.00389
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)
Rural Upper Egypt child with at least one agricultural employee parent in 2003	-0.00807***	-0.00797***	-0.00663***	-0.00848***	-0.01030***	-0.01330***	-0.01473***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Rural Upper Egypt child with at least one agricultural employee parent in 2005	-0.00457***	-0.00347***	-0.00469***	-0.00257***	-0.00058	0.00191	0.00265
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Rural Upper Egypt child with at least one agricultural employee parent in 2008	-0.00978***	-0.00776***	-0.00488***	-0.00493***	-0.00377***	-0.00335*	-0.00140
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.005)
Rural Upper Egypt child whose parents do not work in agriculture in 2000	-0.00274***	-0.00160*	-0.00140***	-0.00115**	-0.00151**	-0.00221**	-0.00350***
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Rural Upper Egypt child whose parents do not work in agriculture in 2003	-0.00687***	-0.00726***	-0.00692***	-0.00827***	-0.01133***	-0.01315***	-0.01393***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Rural Upper Egypt child whose parents do not work in agriculture in 2005	-0.00489***	-0.00433***	-0.00402***	-0.00334***	-0.00241***	-0.00200**	-0.00034
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.002)
Rural Upper Egypt child whose parents do not work in agriculture in 2008	-0.00791***	-0.00557***	-0.00364***	-0.00228***	-0.00154**	0.00003	0.00472*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)
Female child	-0.00184***	-0.00213***	-0.00222***	-0.00275***	-0.00289***	-0.00180***	-0.00159*
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Child is between 1 and 2 years old	0.03905***	0.03705***	0.02927***	0.02298***	0.02020***	0.01970***	0.02183***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Child is between 2 and 3 years old	0.05039***	0.04891***	0.04063***	0.03438***	0.03178***	0.03230***	0.03403***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Child is between 3 and 4 years old	0.05869***	0.05669***	0.04849***	0.04273***	0.04058***	0.04136***	0.04334***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Child is between 4 and 5 years old	0.06479***	0.06238***	0.05462***	0.04899***	0.04741***	0.04855***	0.05282***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Observations	40,233	40,233	40,233	40,233	40,233	40,233	40,233

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 3.9, Panel C

VARIABLES	(1) 5P	(2) 10P	(3) 25P	(4) 50P	(5) 75P	(6) 90P	(7) 95P
Mother's highest grade completed	-0.00002	-0.00001	0.00001	0.00005*	0.00005	0.00004	0.00004
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Father's highest grade completed	0.00007	0.00009***	0.00008***	0.00008***	0.00014***	0.00017***	0.00009
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mother's age	-0.00007	0.00026	-0.00000	0.00013	0.00001	0.00063*	0.00044
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Mother's age, squared	0.00000	-0.00000	-0.00000	-0.00000	-0.00000	-0.00001*	-0.00001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Father's age	-0.00016	-0.00004	0.00020**	0.00022**	0.00014	0.00012	0.00014
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Father's age, squared	0.00000	0.00000	-0.00000	-0.00000*	-0.00000	-0.00000	-0.00000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of female householders aged 0-6	-0.00008	-0.00016	-0.00038***	-0.00041***	-0.00058***	-0.00086***	-0.00103**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of male householders aged 0-6	-0.00006	0.00001	0.00001	-0.00019	-0.00054***	-0.00012	-0.00027
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of female householders aged 7-15	0.00012	0.00007	0.00014	0.00013	0.00000	-0.00018	-0.00018
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of male householders aged 7-15	-0.00010	-0.00008	-0.00012	-0.00019	-0.00031	-0.00053*	-0.00123***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of female householders aged 16-25	-0.00013	0.00005	-0.00012	-0.00004	-0.00016	-0.00045	-0.00004
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Number of male householders aged 16-25	-0.00030	-0.00012	0.00005	0.00028*	0.00015	0.00077**	0.00085
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Number of female householders aged 26-45	-0.00058	-0.00074**	-0.00019	0.00011	0.00034	0.00026	-0.00005
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Number of male householders aged 26-45	0.00085**	0.00071**	0.00036	0.00020	0.00051	0.00074	0.00158**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Number of female householders aged 46-60	0.00020	0.00007	-0.00014	-0.00026	0.00034	-0.00022	-0.00019
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Number of male householders aged 46-60	-0.00006	-0.00049	-0.00051	-0.00079**	-0.00052	-0.00014	0.00038
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Number of female householders aged 61 and over	-0.00009	0.00011	-0.00003	-0.00018	-0.00041	-0.00095	-0.00032
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Number of male householders aged 61 and over	0.00011	-0.00025	-0.00017	0.00014	-0.00061	-0.00010	-0.00140
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Household dependency ratio	0.00015	0.00035	0.00058***	0.00049*	0.00064**	0.00068*	0.00018
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Mother works indicator	0.00069	0.00033	0.00062*	0.00053*	0.00092**	0.00112*	0.00072
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Observations	40,233	40,233	40,233	40,233	40,233	40,233	40,233

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

Table 3.10: Region and Parental Occupation-Specific Conditional Weight-for-Height 2008 - 2005 Differences

Panel A

VARIABLES	**(1)** **5 P**	**(2)** **10 P**	**(3)** **15 P**	**(4)** **20 P**	**(5)** **25 P**	**(6)** **30 P**	**(7)** **35 P**	**(8)** **40 P**	**(9)** **45 P**	**(10)** **50 P**
Urban children, 2008 - 2005 differences	-0.00267***	-0.00086	0.00016	0.00013	0.00063	0.00095*	0.00156***	0.00168***	0.00194***	0.00226***
Lower rural children of self-employed farmers, 2008 - 2005 differences	-0.01357***	-0.01253***	-0.00897**	-0.00983***	-0.00736***	-0.00505**	-0.00478***	-0.00519**	-0.00592**	-0.00592***
Lower rural children of agricultural employees, 2008 - 2005 differences	-0.00319	-0.00109	-0.00305	-0.00234	-0.00157	-0.00167	-0.00165	-0.00124	-0.00004	-0.00114
Lower rural children of non-agricultural workers, 2008 - 2005 differences	-0.0032**	-0.00155	-0.00173**	-0.00130	-0.00068	0.00074	0.00106	0.00149**	0.00215***	0.00237***
Upper rural children of self-employed farmers, 2008 - 2005 differences	-0.00387	-0.00255	0.00004	0.00051	0.00079	0.00075	0.00023	0.00010	-0.00057	0.00002
Upper rural children of agricultural employees, 2008 - 2005 differences	-0.00521**	-0.00429**	-0.00198	-0.00096	-0.00019	-0.00113	-0.00087	-0.00107	-0.00147	-0.00236**
Upper rural children of non-agricultural workers, 2008 - 2005 differences	-0.00302***	-0.00124	0.00008	0.00021	0.00038	0.00051	0.00049	0.00064	0.00079	0.00106**
Observations	40,233	40,233	40,233	40,233	40,233	40,233	40,233	40,233	40,233	40,233

*** p<0.01, ** p<0.05, * p<0.1

Panel B

VARIABLES	**(11)** **55 P**	**(12)** **60 P**	**(13)** **65 P**	**(14)** **70 P**	**(15)** **75 P**	**(16)** **80 P**	**(17)** **85 P**	**(18)** **90 P**	**(19)** **95 P**
Urban children, 2008 - 2005 differences	0.00259***	0.0036***	0.00383***	0.0036***	0.00415***	0.00434***	0.00516***	0.00685***	0.0116***
Lower rural children of self-employed farmers, 2008 - 2005 differences	-0.00623***	-0.00532**	-0.00447**	-0.00425**	-0.00401	-0.00381	-0.00382	-0.00644	-0.00945
Lower rural children of agricultural employees, 2008 - 2005 differences	-0.00136	-0.00097	-0.00048	0.00126	-0.00134	-0.00078	-0.00238	-0.00429	-0.00740
Lower rural children of non-agricultural workers, 2008 - 2005 differences	0.00208***	0.00225***	0.00186**	0.00179*	0.00224***	0.00224**	0.00209**	0.00184	-0.00269
Upper rural children of self-employed farmers, 2008 - 2005 differences	0.00039	0.00012	-0.00020	-0.00050	-0.00117	-0.00087	-0.00190	-0.00581***	-0.00804*
Upper rural children of agricultural employees, 2008 - 2005 differences	-0.00226*	-0.00212**	-0.00301**	-0.00386***	-0.00319**	-0.0041**	-0.00568***	-0.00526**	-0.00405
Upper rural children of non-agricultural workers, 2008 - 2005 differences	0.00132**	0.0014**	0.00153***	0.00123*	0.00087	0.00099	0.00098	0.00203*	0.00506**
Observations	40,233	40,233	40,233	40,233	40,233	40,233	40,233	40,233	40,233

*** p<0.01, ** p<0.05, * p<0.1

Table 3.11: Region and Parental Occupation-Specific Inter-Quantile Regression Results

Table 3.11, Panel A

VARIABLES	**(1)** **95P - 5P**
Urban Egyptian children	
Urban child in 2003	-0.00408***
	(0.002)
Urban child in 2005	0.00843***
	(0.001)
Urban child in 2008	0.02270***
	(0.002)
Rural Lower Egyptian children	
Rural Lower Egypt child with at least one self-employed farmer parent in 2000	0.00296
	(0.003)
Rural Lower Egypt child with at least one self-employed farmer parent in 2003	-0.00658
	(0.005)
Rural Lower Egypt child with at least one self-employed farmer parent in 2005	0.01928***
	(0.005)
Rural Lower Egypt child with at least one self-employed farmer parent in 2008	0.02341***
	(0.004)
Rural Lower Egypt child with at least one agricultural employee parent in 2000	-0.00441
	(0.004)
Rural Lower Egypt child with at least one agricultural employee parent in 2003	-0.00172
	(0.004)
Rural Lower Egypt child with at least one agricultural employee parent in 2005	0.01261**
	(0.005)
Rural Lower Egypt child with at least one agricultural employee parent in 2008	0.00841***
	(0.003)
Rural Lower Egypt child whose parents do not work in agriculture in 2000	0.00105
	(0.002)
Rural Lower Egypt child whose parents do not work in agriculture in 2003	-0.00325
	(0.002)
Rural Lower Egypt child whose parents do not work in agriculture in 2005	0.01243***
	(0.002)
Rural Lower Egypt child whose parents do not work in agriculture in 2008	0.01294***
	(0.002)
Constant	0.05778***
	(0.011)
Observations	40,233

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

Table 3.11, Panel B	
VARIABLES	(1) 95P - 5P
Rural Upper Egyptian children	
Rural Upper Egypt child with at least one self-employed farmer parent in 2000	-0.00144
	(0.005)
Rural Upper Egypt child with at least one self-employed farmer parent in 2003	-0.00858***
	(0.002)
Rural Upper Egypt child with at least one self-employed farmer parent in 2005	0.00251
	(0.002)
Rural Upper Egypt child with at least one self-employed farmer parent in 2008	-0.00166
	(0.004)
Rural Upper Egypt child with at least one agricultural employee parent in 2000	-0.00029
	(0.003)
Rural Upper Egypt child with at least one agricultural employee parent in 2003	-0.00666**
	(0.003)
Rural Upper Egypt child with at least one agricultural employee parent in 2005	0.00722***
	(0.003)
Rural Upper Egypt child with at least one agricultural employee parent in 2008	0.00838
	(0.005)
Rural Upper Egypt child whose parents do not work in agriculture in 2000	-0.00076
	(0.002)
Rural Upper Egypt child whose parents do not work in agriculture in 2003	-0.00706***
	(0.002)
Rural Upper Egypt child whose parents do not work in agriculture in 2005	0.00454***
	(0.002)
Rural Upper Egypt child whose parents do not work in agriculture in 2008	0.01264***
	(0.003)
Female child	0.00026
	(0.001)
Child is between 1 and 2 years old	-0.01722***
	(0.001)
Child is between 2 and 3 years old	-0.01636***
	(0.001)
Child is between 3 and 4 years old	-0.01536***
	(0.001)
Child is between 4 and 5 years old	-0.01197***
	(0.001)
Observations	40,233

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

Chapter A: Effects of Shocks at Particular Ages

The purpose of this appendix is to examine in more detail the exact ages at which wet and dry shocks have their effects on human capital. The following specification will be used to conduct this analysis:

$$H_{ipt} = \alpha + \sum_{j=0}^{20} \beta_j wetyearagej_{pt} + \sum_{j=0}^{20} \rho_j dryyearagej_{pt} + \delta_p + \theta_{ct} + \lambda_r(t) + X'_{ipt}\Gamma + \varepsilon_{ipt}. \tag{A.1}$$

The outcomes being analyzed here are once again the likelihood of being literate and the highest grade completed at school. The *wetyearagej* (*dryyearagej*) variable denotes an indicator equal to one if individual i from rainfall neighborhood p and born in year t experienced a wet (dry) shock at age j, $j \in \{0, 20\}$. All other control variables are the same as the variables in the baseline specification. The estimates for the *wetyearagej* and *dryyearagej* variables will therefore identify the effects of experiencing shocks at age j, as long as these shocks variables are not correlated with the idiosyncratic error terms after conditioning on time-invariant, location-specific factors, country-specific cohort factors, and regional linear trends.

The results can be seen in Table A.1. The results for women mostly reflect the baseline results from Table 2.3: Wet shocks for females aged 7 and over and dry shocks at preschool ages have negative effects on human capital. [1] Shocks effects are not significantly negative at every one of these ages, but estimates are normally negative, including between the ages of 7 and 13 in the case of dry shocks. Importantly, dry shocks effects are significantly negative and substantially sized at ages 1-3. This seems more consistent with the type of health channel explanation for the effects of dry shocks early on described in Section 2.4.2.2 than with a lagged income effect explanation.

For men, the results are consistent with the baseline results in the sense that wet shocks at relatively advanced ages (19 and 20) and dry shocks at preschool ages (2-4) are sometimes significantly positive. [2] Again, the latter set of effects is consistent with the possibility that dry shocks resulted in positive health shocks that are complemented with more education investments later (or negative health shocks that are more than compensated for later).

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[1] These point estimates are subject to the same kinds of sample selection bias as described in Section 2.5.2. In particular, wet shocks from age 7-13 and dry shocks before age 7 might be too large in magnitude, while (if anything) the effects of wet shocks from age 14 to age 20 are understated.

[2] Again, these point estimates are potentially too large in magnitude due to sample selection bias, as discussed in more detail in Section 2.5.2.

Table A.1: Shocks Ages Results

Table A.1, Panel A

	Women		Men	
Dependent variable:	Literacy	Highest grade	Literacy	Highest grade
	(1)	(2)	(3)	(4)
1(Wet shock at age 0)	-0.004	-0.046	0.014	0.117
	(0.006)	(0.043)	(0.011)	(0.099)
1(Wet shock at age 1)	-0.002	-0.023	0.004	0.122
	(0.006)	(0.045)	(0.013)	(0.100)
1(Wet shock at age 2)	0.001	0.018	0.005	0.035
	(0.006)	(0.053)	(0.012)	(0.096)
1(Wet shock at age 3)	-0.007	-0.036	-0.023**	0.087
	(0.006)	(0.050)	(0.011)	(0.092)
1(Wet shock at age 4)	-0.007	-0.009	-0.025**	-0.146
	(0.006)	(0.046)	(0.012)	(0.097)
1(Wet shock at age 5)	-0.008	-0.026	0.012	0.184*
	(0.006)	(0.050)	(0.012)	(0.105)
1(Wet shock at age 6)	-0.007	-0.099*	0.004	0.067
	(0.007)	(0.054)	(0.013)	(0.111)
1(Wet shock at age 7)	-0.013*	-0.049	0.000	0.043
	(0.007)	(0.056)	(0.016)	(0.118)
1(Wet shock at age 8)	-0.010	-0.063	-0.010	-0.221*
	(0.007)	(0.063)	(0.012)	(0.117)
1(Wet shock at age 9)	-0.009	-0.094	0.006	0.019
	(0.008)	(0.070)	(0.014)	(0.119)
1(Wet shock at age 10)	-0.020***	-0.202***	0.035**	0.057
	(0.008)	(0.065)	(0.014)	(0.113)
1(Wet shock at age 11)	-0.004	-0.088	-0.005	-0.174
	(0.009)	(0.070)	(0.013)	(0.122)
1(Wet shock at age 12)	-0.017**	-0.090	-0.013	-0.137
	(0.008)	(0.060)	(0.015)	(0.136)
1(Wet shock at age 13)	-0.013	-0.094	-0.009	-0.270**
	(0.008)	(0.069)	(0.014)	(0.107)
1(Wet shock at age 14)	-0.012	-0.188***	0.013	-0.176
	(0.009)	(0.069)	(0.015)	(0.130)
1(Wet shock at age 15)	-0.017**	-0.141**	0.004	0.032
	(0.008)	(0.062)	(0.013)	(0.111)
1(Wet shock at age 16)	-0.018**	-0.157**	0.021	0.198*
	(0.008)	(0.068)	(0.014)	(0.117)
1(Wet shock at age 17)	-0.006	-0.133**	0.008	0.008
	(0.008)	(0.066)	(0.015)	(0.115)
1(Wet shock at age 18)	-0.022***	-0.186***	0.014	0.026
	(0.007)	(0.063)	(0.014)	(0.108)
1(Wet shock at age 19)	-0.003	-0.004	0.022*	0.095
	(0.007)	(0.061)	(0.013)	(0.111)
1(Wet shock at age 20)	-0.010	-0.108*	0.024*	0.117
	(0.007)	(0.056)	(0.014)	(0.119)
F-statistic	1.80	1.64	1.74	1.24
R-squared	0.309	0.459	0.408	0.514
Observations	41,991	42,062	16,496	16,676

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: All specifications include rainfall neighborhood fixed effects, country-specific birth year fixed effects, controls for regional linear time trends, and controls for individuals' religion and ethnicity. Data are unweighted. Standard errors clustered at the rainfall neighborhood level in parentheses. The F-statistics test the hypothesis that the shocks coefficients are jointly zero.

Table A.1, Panel B

	Women		Men	
Dependent variable:	Literacy	Highest grade	Literacy	Highest grade
	(1)	(2)	(3)	(4)
1(Dry shock at age 0)	0.000	0.009	-0.001	0.035
	(0.005)	(0.036)	(0.010)	(0.083)
1(Dry shock at age 1)	-0.012**	-0.070*	-0.008	0.065
	(0.005)	(0.039)	(0.012)	(0.081)
1(Dry shock at age 2)	-0.017***	-0.178***	0.028***	0.103
	(0.005)	(0.040)	(0.010)	(0.081)
1(Dry shock at age 3)	-0.013***	-0.076**	0.017	0.167**
	(0.005)	(0.037)	(0.011)	(0.074)
1(Dry shock at age 4)	-0.002	-0.035	0.025**	0.057
	(0.005)	(0.037)	(0.010)	(0.078)
1(Dry shock at age 5)	-0.004	-0.026	0.010	0.066
	(0.005)	(0.035)	(0.010)	(0.078)
1(Dry shock at age 6)	-0.004	-0.081**	0.000	0.002
	(0.005)	(0.036)	(0.010)	(0.081)
1(Dry shock at age 7)	-0.005	-0.113***	-0.008	0.006
	(0.005)	(0.040)	(0.011)	(0.077)
1(Dry shock at age 8)	-0.005	-0.053	0.000	0.026
	(0.004)	(0.035)	(0.010)	(0.073)
1(Dry shock at age 9)	-0.004	-0.037	0.017*	0.078
	(0.004)	(0.032)	(0.010)	(0.082)
1(Dry shock at age 10)	-0.002	-0.022	-0.008	0.026
	(0.005)	(0.037)	(0.010)	(0.075)
1(Dry shock at age 11)	-0.007	-0.003	0.019*	0.089
	(0.004)	(0.038)	(0.011)	(0.081)
1(Dry shock at age 12)	0.007	0.025	0.017*	0.195***
	(0.004)	(0.032)	(0.010)	(0.074)
1(Dry shock at age 13)	-0.005	0.006	-0.010	-0.028
	(0.005)	(0.037)	(0.010)	(0.075)
1(Dry shock at age 14)	-0.004	-0.041	0.002	-0.003
	(0.004)	(0.030)	(0.010)	(0.077)
1(Dry shock at age 15)	-0.003	-0.054	-0.002	0.037
	(0.004)	(0.034)	(0.010)	(0.073)
1(Dry shock at age 16)	-0.014***	-0.084**	0.013	0.008
	(0.004)	(0.033)	(0.010)	(0.079)
1(Dry shock at age 17)	0.002	0.024	0.015	0.077
	(0.005)	(0.035)	(0.010)	(0.071)
1(Dry shock at age 18)	0.002	-0.010	-0.016	-0.011
	(0.004)	(0.034)	(0.010)	(0.076)
1(Dry shock at age 19)	-0.005	-0.033	0.013	0.007
	(0.005)	(0.039)	(0.009)	(0.070)
1(Dry shock at age 20)	-0.002	-0.001	0.006	-0.023
	(0.004)	(0.031)	(0.011)	(0.081)
F-statistic	1.80	1.64	1.74	1.24
R-squared	0.309	0.459	0.408	0.514
Observations	41,991	42,062	16,496	16,676

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: All specifications include rainfall neighborhood fixed effects, country-specific birth year fixed effects, controls for regional linear time trends, and controls for individuals' religion and ethnicity. Data are unweighted. Standard errors clustered at the rainfall neighborhood level in parentheses. The F-statistics test the hypothesis that the shocks coefficients are jointly zero.

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