Early Childhood during Indonesia's Wildfires: Health Outcomes and Long-Run Schooling Achievements

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I. Introduction

In the past 20 years, a growing number of studies have documented the existence of positive complementarities between schooling outcomes and health and nutrition conditions experienced in early childhood. There are two main channels that by interacting with each other relate these two important aspects of human development. First, because nutritional status in the earliest years of life contributes to determining the health capital of an individual, poorly nourished children are likely to be more vulnerable to disease or simply physically weaker. This, in turn, will affect the development of the children's cognitive skills and their ability to learn and to attend classes regularly, hence impacting their educational performance. Given that cognitive development and school achievement are two important components of human capital, the long-term consequences of poor nutritional status are also likely be reflected in worse labor productivity and lower lifetime earnings.

Second, children with poor nutritional status are exposed to higher risks of morbidity and may therefore enroll in school later. This is especially the case in a developing country, where the enforcement of rules on compulsory school attendance may be relatively weak and where the economic returns to investments in health capital are relatively large. Although delayed enrollment decisions are rational caregivers' responses to early childhood malnutrition (Glewwe and Jacoby 1995), late entry is never optimal as it will result in fewer years of earnings. This is because in order to complete the total years of compulsory schooling after delayed enrollment, an individual will have to enter the job market later.[1]

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[1] This implies that delayed enrollment does not necessarily lead to fewer years of education. However, this may be an expected outcome if one supposes that the opportunity cost of schooling increases with age.

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In this paper, I conduct a microeconometric analysis on a long-term panel data set collected in 13 Indonesian provinces over the period 1997–2007, my purpose being to gauge the impact of child health status (given by height-for-age *z*-scores) on subsequent educational attainments. The econometric strategy adopted is based on an estimator of instrumental variable plus mother fixed effects where differences in height among siblings are identified by exposure to an exogenous shock, that is, the Indonesian forest fires of late 1997.

This study thereby seeks to contribute to the literature in a number of ways. First, it extends the literature on human capital formation. By relying on longitudinal data and applying an estimation methodology that takes the unobservable behavioral choices underlying child health into account (Alderman et al. 2001b), this study seeks to analyze the relationship between preschool health and subsequent education and cognitive outcomes.

Many empirical works based on cross-sectional data have documented positive associations between these two important aspects of children's human capital. Yet the question of causality is still open.[2] A number of studies based on randomized evaluations (see, e.g., Miguel and Kremer 2004; Bobonis, Miguel, and Puri-Sharma 2006), which typically address endogeneity problems, have provided robust evidence on the causal effect of early childhood nutrition on school attendance or on cognitive skills. In these cases, the focus has largely been on specific health and nutrition interventions, such as deworming, preschool feeding programs, or iron supplementation (see, e.g., Dickson et al. 2000; Vermeersch and Kremer 2004; Bobonis et al. 2006).

Second, this paper aims at improving current knowledge on the impact of shocks at the individual level. These kinds of adverse events can drastically affect households' welfare by generating substantial reductions in their levels of income and consumption (Morduch 1995; Townsend 1995), but the magnitude and the duration of such shocks may vary substantially among the households' members. By considering the effect of exposure to the wildfires on children's health outputs and—through this—on future educational achieve-

[2] In order to estimate the impact of child health on education outcomes, the minimum requirement for cross-sectional data is that they contain at least one (retrospective) measure of health status at some point during preschool age and at least one variable that measures current education achievement. Cross-sectional data, including retrospective variables based on parents' self-reports on their children's past health status, are likely to lead to a substantial amount of recall error and therefore lead to attenuation bias, i.e., underestimation of the impact of the poorly measured variables on education outcomes (e.g., Bound, Brown, and Mathiowetz 2001; Wooldridge 2002). As noted by Glewwe and Miguel (2008), the "estimates based on them are likely to suffer from bias toward zero (if measurement error is classical) or bias in an unknown direction (if measurement is nonclassical, which is plausible in the context of retrospective health and education reports)" (3585).

ments, this paper contributes to current knowledge on both short- and long-term consequences of exposure to transitory environmental shocks.

Third, this analysis will also extend current understanding on the consequences of the Indonesian wildfires of 1997. To date, only two studies have investigated the effects of this massive environmental shock on the Indonesian population. They have focused on the impact of haze on adult respiratory problems (Frankenberg, McKee, and Thomas 2005) and of air pollution on under-3 mortality rates (Jayachandran 2009). In this study, I consider the short-term effect of this shock on children's height for age and as an output of health more broadly and—through this effect—its long-term consequences on cognitive and schooling outcomes. The assumption I test is that the health status of very young children exposed to this shock may have been affected by many problems typically caused by inhalation of polycyclic aromatic hydrocarbons (PAH), ingestion of PAH-contaminated raw food (including the breast milk of mothers exposed to high levels of smoke and haze), or even to the temporary lack of adequate care given by unhealthy adult family members. These shocks were moreover likely to be exacerbated by problems (e.g., water scarcity, destruction of crops, or temporary disruption of food supply) that were related more directly to the combination of these wildfires with the prior drought.

Last, this study contributes to knowledge on the strength of the health-learning nexus in Indonesia: a country that has been growing remarkably in the past 20 years and that has recently experienced large reductions in poverty rates. The Indonesian education system has benefited from massive supply-side interventions that have boosted school enrollment rates (Duflo 2001). Yet, despite these gains, there are still some challenges that the country needs to address in terms of disparities within and among provinces and regions on many quantitative and qualitative indicators of school achievement (World Bank 2011).

The remainder of this paper is organized as follows: I present the analytic framework and its econometric implications in Section II and then describe the data in Section III. Section IV deals with the empirical approach and instrumental validity and illustrates the main findings and robustness checks. Section V concludes.

II. Analytic Framework and Econometric Implications

Following Alderman et al. (2001b), Glewwe, Jacoby, and King (2001), Alderman, Hoddinott, and Kinsey (2006), Cunha et al. (2006), and Yamauchi (2008), this analysis is based on a simple achievement production function that relates health status in early childhood to the educational performance realized in late childhood or adolescence.

It is assumed that both child health and education, beyond being determined by genetic endowment, are influenced by a set of household and community characteristics (e.g., the availability of schools and learning facilities, teachers' and parents' levels of education, and household wealth). Moreover, investment in a child's human capital reflects parental tastes and attitudes toward the child's education and health, subject to the constraints imposed by family resources and by the options available in the community.

An important assumption underlying this achievement function is that there is a dynamic household behavior that contributes to shaping the simple input-output relationship between health and school and that possibly interacts with the degree of complementarity or substitutability between health capital and schooling inputs (Yamauchi 2008).[3]

In light of these background assumptions, I consider the following schooling equation, which can be empirically estimated:

$$E_{it} = \beta_0 + \beta_1 H_{it-1} + \beta_2 C_{it} + \varepsilon_i, \tag{1}$$

where H_{it-1} represents the health status of the child in the preceding period (e.g., proxied by her height-for-age *z*-scores), and C_{it} is a vector including household and community characteristics as well as a set of school fees and prices of consumption goods and schooling materials that determine the household budget constraint.

Last, ε_i is a disturbance term that represents the sum of a child-specific unobserved component (i.e., child genetic potential, innate ability, and motivation), home-invariant factors (i.e., parental tastes and attitudes toward the child's education and health), and a white-noise error term component. The main interest of this paper is, of course, to assess the magnitude and significance of the coefficient β_1, but there are a number of econometric problems involved in the estimation of (1) that should first be addressed.[4]

[3] The optimal level of schooling investment is also affected by whether health capital substitutes for schooling inputs or increases their productivity. Assuming perfect substitutability implies that parents will make more schooling investments in unhealthier children. On the other hand, if health capital and schooling inputs are complementary, only healthier children will attract more schooling investment (Yamauchi 2008).

[4] Clearly, one might expect that current health status (H_{it}) influences current education achievements as well, so that if this assumption holds true, estimates of the impact of health on subsequent schooling outcomes may be biased to the extent that past and current health conditions are correlated. However, here I assume that the effect of H_{it} is negligible, i.e., the strongest effects of child health on schooling outcomes appear in early childhood. This assumption is supported by Glewwe et al. (2001), the vast nutritional literature, and the studies on the health-learning nexus I review in this paper, all of which rule out the presence of any effect of current health status on schooling outcomes. Moreover, a recent study (Mani 2012) on the issue of catch-up growth in Indonesia (also using the IFLS data) finds that poor nutrition at young ages will cause some but not severe retardation in the

First, there may be an omitted variable bias problem; that is, there may be other factors that relate to both height and education driving the association between these two variables.

Second, child health and subsequent education are both influenced by parents' preferences toward sibling inequality in human capital, which in turn are reflected in their decisions regarding the allocation of resources among their children.[5]

Third, as suggested by the medical and biology literature, not only does the individual genetic endowment correlate closely with health status (see, e.g., Preece 1996; Weedon et al. 2008), but there is also evidence of a shared genetic architecture between height and intelligence (Posthuma et al. 2000; Sundet et al. 2005; Van Dam et al. 2005; Silventoinen et al. 2006; Keller et al. 2013; Marioni et al. 2014). Therefore, over subsequent time periods, children with higher genetic potential will be healthier than their peers, while less-endowed children will be more likely to experience worse health conditions and may even die before the educational outcome is realized, leaving one with a biased sample of selected healthier individuals (Alderman et al. 2006; Yamauchi 2008).

These considerations imply that simple ordinary least squares (OLS) estimates of β_1 are likely to be either upward or downward biased because the main independent variable may not be orthogonal to the error term. In other words, there is an endogeneity problem due to the possible correlation existing between child health, home-invariant factors, and child-specific unobserved characteristics (Behrman 1996; Alderman et al. 2006).

As suggested by Glewwe et al. (2001), the econometric approach best able to sweep out these two forms of correlation combines a sibling difference model with instrumental variable techniques: maternal fixed effects will indeed remove the bias caused by the correlation between the endogenous variable and the siblings-invariant error term component, while the use of a relevant and exogenous instrument will purge the remaining correlation with the child-specific error term component.

Last, the estimation of the schooling equation requires data measured at different points in an individual's life.

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growth of future height, indicating partial catch-up effects. In particular, Mani (2012) finds that "(i) stunted children exhibit larger catch-up effects compared to children who do not suffer from growth faltering at an early age [and] (ii) younger children have larger catch-up potential than older children" (693). These findings contribute to alleviating any concerns about the bias due to the correlation between past and current nutritional status.

[5] As noted by Yamauchi (2008), "if parents are averse to the inequality among their children, they may increase investment in schooling of their less endowed children to equalize future incomes among their children" (658).

To date, the literature on this research field is made up of a few qualified studies (i.e., Alderman et al. 2001b, 2006; Glewwe et al. 2001; Yamauchi 2008; Alderman, Hoogeveen, and Rossi 2009; Duc 2011) that have used longitudinal data to estimate the impact of child health on later education achievements.[6]

Table 1 provides an overview of these peer-reviewed published studies by summarizing the main information concerning the country on which the research was based, the variables used for educational achievement, the type of estimation approach employed, the endogenous health status variable, and the variable chosen to instrument for it.

While all of these studies have relied on an instrumental variable approach to wipe out the bias due to unobservable child-specific characteristics, inference has been based in most of the cases on variations among children living in the same community (with community fixed effects) or in the same household (with the use of household fixed effects). Household fixed effects, nevertheless, are not exactly the same as differencing across siblings of the same mother, especially in a developing country context where more than one family unit shares the same house.

Except for the Vietnam study, which finds a significant effect of height for age (at age 1) on cognitive achievements (at age 5) only for children born preterm, all these studies have found a strong and often statistically significant effect of child nutritional status on later academic achievement.[7]

By using age and mother fixed effects, this paper takes a similar approach to Alderman et al. (2006) and to Yamauchi (2008). Differently from previous studies, I exploit variations in exposure to the forest fires among children of different ages and living in different islands to shed some light on the direct and indirect effects of such a shock on child development.

III. Data and Sample

A. The Indonesian Family Life Survey

My main source of data is the Indonesian Family Life Survey (IFLS), which is an ongoing longitudinal survey of individuals, households, communities, and facilities conducted in 13 Indonesian provinces extending across the islands of Sumatra, Java, Kalimantan, Sulawesi, Bali, and West Nusa Tenggara. The first

[6] I have updated the literature already surveyed in Glewwe and Miguel (2008) by conducting in February 2016 a search on the EconLit database and a manual search in Google Scholar using the words “childhood health” or “nutrition,” “schooling” or “education,” or learning” and “longitudinal data.”

[7] The most recent study surveyed (Duc 2011) finds that there is no impact of nutrition on subsequent cognitive achievement if the effect of being born preterm is taken into account. However, as also pointed out by the author, this finding cannot rule out that undernutrition in early childhood is important for subsequent cognitive outcomes. Instead, it suggests the importance of controlling or checking for conditions in utero in order to avoid bias in the estimated effect.

TABLE 1

REVIEW OF PREVIOUS PANEL DATA–BASED STUDIES ON THE HEALTH-LEARNING NEXUS

Source	Country	Educational Outcome Variable(s)	Estimation Strategy	Endogenous Health Status Variable	Instrument(s)
Alderman et al. 2001b	Pakistan	Dummy = 1 if enrolled in time	IV	Height-for-age *z*-scores	Food price shocks
Glewwe et al. 2001	Philippines	Test scores	HHFE-IV	Height-for-age *z*-scores	Height of older sibling at 24 months
Alderman et al. 2006	Zimbabwe	Adolescent height; grades attained; age on starting school	MFE-IV	Height-for-age *z*-scores	Exposure to civil war and drought
Yamauchi 2008	South Africa	Age on starting school; grades completed; mathematics test scores	HHFE-AFE-IV	Height-for-age *z*-scores	Community health facilities; weight-for-age *z*-scores
Alderman et al. 2009	Tanzania	Age on starting school; grades completed	CFE-IV	Height as percentage of median of reference population	Crop loss interacted with age and gender; flood or drought interacted with age and gender
Duc 2011	Vietnam	Cognitive test score	CFE-IV	Height-for-age *z*-scores	Birth weight and mother's height

Note. IV = instrumental variable; HHFE = household fixed effects; MFE = mother fixed effects; AFE = age fixed effects; CFE = community fixed effects.

wave (IFLS1) was conducted in late 1993 and surveyed 7,224 households and 22,000 individuals in 321 enumeration areas. Between August and December 1997, the second full sample wave (IFLS2) successfully managed to reinterview more than 94% of the IFLS1 households (Thomas, Frankenberg, and Smith 2001).

Two further follow-up surveys were conducted in 2000 (IFLS3) and 2007 (IFLS4). Among the IFLS1 households, 90.3% were either interviewed in all four waves of the survey or had died, and 87.6% were actually interviewed in all four waves (Thomas et al. 2012).

There are interesting features in the IFLS that make these data particularly suited to my research purposes. First, these high recontact rates contribute significantly to data quality by reducing bias due to nonrandom attrition. Second, besides respondents' basic demographic and socioeconomic characteristics, the IFLS collected detailed information on various aspects of their education (e.g., current schooling grade, age at which the child first enrolled in school, number of correct answers given in a cognitive test) as well as on the anthropometric measures necessary to derive child nutritional status variables.

B. Description of Key Variables

I considered the panel of individuals surveyed in IFLS2, IFLS3, and IFLS4, and I shrank the initial IFLS2's size by keeping only eight cohorts of individuals born between 1990 and 1997.[8] These children were then tracked after 3 years (i.e., in IFLS3) and/or after 10 years (i.e., in IFLS4) in order to obtain information on their current educational achievement. The data showed that in 2000 and/or in 2007, 936 observations were traced from an initial sample of 2,163 children for which there was complete information available on basic demographic and socioeconomic characteristics and on anthropometric measures such as weight and height that were used to construct sex- and age-standardized *z*-scores for height and weight based on the standards provided in 2006 by the World Health Organization (WHO) in the Multicentre Growth Reference Study (MGRS).[9]

In order to consider different facets of learning achievements, education outcomes were measured by three distinct variables: (a) completed years of

[8] Despite the availability of one more wave of data from the IFLS administered in 1993, this was not used as the baseline survey, given that in my identification strategy, I needed only observations where child nutritional status was measured at one point in time during preschool age, and I needed to include also children born after 1993, since the instrument used in this analysis identified children aged 12–36 months in September 1997 (see Sec. IV.A for more).

[9] For further discussion of tracking issues, see Sec. IV.C. The MGRS was based on a sample of 8,500 children from widely different ethnic backgrounds and cultural settings (Brazil, Ghana, India, Norway, Oman, and the United States). These children were breastfed during infancy, appropriately fed later on in life, and raised in optimal conditions (WHO 2006).

schooling, (b) score obtained in a cognitive test (to consider the development of cognitive and learning skills), and (c) age at which primary school was started (to proxy for readiness to enter school; see table A1). The first variable was observed in 2007 (IFLS4), and it was measured by summing the number of grades completed at each level of school.[10] The score on the cognitive test was measured either in 2000 or in 2007, depending on child age. This test, in fact, was administered to children aged 8–14 years, and the variable on its outcome was constructed as the ratio of correct answers to total questions.[11] Last, the information on the age at which the child started school was taken directly from the answers provided by the mothers either in IFLS3 or in IFLS4.

Child height was measured in 1997, when these children were at preschool age. To be noted is that, on average, the children sampled had poor height and weight for age relative to the MGRS sample of adequately nourished children. For the whole sample, which included both only children and children with siblings, the figures reported in table 2 show that, given age and sex, child height (weight) was −1.74 (−1.44) standard deviations below the median child in that age group.

It can also be observed that the subsample of children who were in the age range 12–36 months and living in Sumatra or Kalimantan during the spread of the forest fires had, on average, lower grades of schooling, lower scores in the cognitive tests, and started school slightly later than children who were not exposed to the shock.

Last, as illustrated in table 3, children suffering from moderate to severe stunting conditions are more likely to experience worse educational outcomes in later stages of their lives if compared to their healthier peers.

While some clear correlations between preschool health conditions and subsequent educational achievements already emerge from these statistics, the presence of a causal relationship and of possible transmission channels needs to be formally established. This, in fact, is the central subject of investigation in this paper.

IV. Findings

A. Estimation Approach and Instrumental Validity

The empirical approach employed was based on estimation of the afore-specified schooling equation in which three alternatives measures for E_{it} are

[10] The Indonesian school system consists of 6 years of primary education, 3 years of junior secondary education, and a further 3 years of senior general or vocational education. Primary school starts by law at age 6 or 7.

[11] The test consisted of a set of 17 questions, of which 12 were cognitive and five were based on simple mathematics.

TABLE 2
DESCRIPTIVE STATISTICS FOR THE FULL SAMPLE AND FOR THE SUBSAMPLE OF CHILDREN WITH MULTIPLE SIBLINGS, BY TREATMENT STATUS

	Full Sample (936)		Subsample: Children with Multiple Siblings					
			All Children (424)		Children Exposed to Shock (75)		Children Not Exposed to Shock (349)	
	Mean (SD)	Min to Max	Mean (SD)	Min to Max	Mean (SD)	Min to Max	Mean (SD)	Min to Max
Gender (male)	.52 (.50)	0 to 1	.53 (.49)	0 to 1	.57 (.50)	0 to 1	.53 (.49)	0 to 1
Age (first period)	3.31 (1.98)	0 to 6	3.28 (2.05)	0 to 6	1.73 (.63)	1 to 3	3.41 (2.07)	0 to 6
Height-for-age *z*-score	−1.74 (1.34)	−5.8 to 4.2	−1.77 (1.27)	−5.6 to 1.8	−2.26 (1.61)	−5.6 to 0.9	−1.73 (1.23)	−5.3 to 1.8
Weight-for-age *z*-score	−1.44 (1.19)	−5.7 to 4.1	−1.46 (1.13)	−4.8 to 2.8	−1.72 (1.15)	−4.8 to 0.39	−1.44 (1.12)	−4.6 to 2.8
Age (second period)	13.5 (2.0)	9 to 17	13.47 (2.07)	10 to 17	11.85 (.79)	10 to 13	13.61 (2.08)	10 to 17
Age on starting school	6.3 (.65)	5 to 11	6.30 (.65)	5 to 10	6.46 (.67)	5 to 8	6.28 (.65)	5 to 10
Cognitive test scores	.76 (.18)	0 to 1	.75 (.18)	0 to 1	.66 (.26)	0 to 1	.76 (.17)	0 to 1
Age cognitive test scores	10.93 (2.07)	8 to 14	10.86 (2.04)	8 to 14	11.83 (0.80)	10 to 13	10.76 (2.10)	8 to 14
Years of schooling	6.70 (2.22)	0 to 12	6.69 (2.32)	0 to 12	5.24 (1.5)	1 to 8	6.81 (2.33)	0 to 12
Mother's education	7.08 (3.5)	0 to 12	7.45 (3.4)	0 to 12	8.61 (3.2)	0 to 12	7.35 (3.38)	0 to 12
Mother's age	30.13 (5.3)	15 to 50	29.9 (4.7)	21 to 44	29.4 (3.43)	24 to 38	29.9 (4.8)	21 to 44
Rural	.49 (.50)	0 to 1	.48 (.50)	0 to 1	.48 (.51)	0 to 1	.48 (.50)	0 to 1
Fires shock	.0161 (.24)	0 to 1	.177 (.26)	0 to 1	1 (0)	1 to 1	0 (0)	0 to 0

Source. Author's calculations based on data from Indonesian Family Life Surveys 2–4.

used: completed years of schooling, the score obtained in the cognitive test, and the age at which primary school was started. The effect of H_{it-1} (measured in terms of height-for-age z-scores, as a general proxy for health status) was estimated by mainly relying on a mother fixed effects–instrumental variable (MFE-IV) model, which—as argued above—addresses endogeneity in the relationship of interest.

TABLE 3

MEAN EDUCATIONAL ACHIEVEMENTS OF CHILDREN WITH MULTIPLE SIBLINGS ABOVE AND BELOW MODERATE STUNTING AND UNDERWEIGHT THRESHOLDS

	Children with Moderate to Severe Stunting	Nonmalnourished	Children Moderately to Severely Underweight	Nonunderweight
Completed years of schooling	6.39	6.69	6.64	6.67
Age on starting school	6.39	6.18	6.35	6.17
Cognitive test score	0.73	0.76	0.72	0.73

Source. Author's calculations based on data from Indonesian Family Life Surveys 2–4.

It is important to note that the MFE-IV estimation procedure entails the choice of an instrument that should significantly affect a child's health status, be adequately variable across children born to the same mother, and be sufficiently transitory not to exert any direct effect on E_{it}. The instrument that I used was the shock resulting from individual exposure to the Indonesian forest fires of late 1997.

In Sumatra and Kalimantan, small and controlled fires have been traditionally used by small-scale farmers to clear land for the planting of new crops.[12] But these fires went rapidly out of control in early September 1997 because of the extraordinary dry weather conditions caused by the El Niño Southern Oscillation (Jim 1999) phenomenon. The drought associated with El Niño that exacerbated the intensity of the fires became particularly severe and prolonged. Only in mid-to-late November, when fires were quenched by the first rains, did land and environmental conditions in these two islands begin to recover (see fig. A1).

The damage inflicted by these wildfires and the resulting haze was massive: the lives of the majority of the population living and working in rural areas were adversely affected by the destruction of farms and plantations, the interruption of transport systems, and the severe respiratory and physical problems that resulted from months of breathing heavy smoke and haze (Frankenberg et al. 2005; Jayachandran 2009).

The smoke generated by burning wood and vegetation typically contains polycyclic aromatic hydrocarbons, which contaminate air, soil, and food and which are known mutagens or animal carcinogens.

Young children exposed to this shock may have suffered from nutritional deficiencies related to many health problems typically caused by PAH inhalation and/or ingestion of PAH-contaminated raw food (including the breast

[12] Although this slash-and-burn technique plays an important ecological role in the local ecosystem, it was increasingly used in more extended areas during the 1990s because of the expansion of the timber and palm oil industries in Indonesia.

milk of mothers exposed to high levels of smoke and haze).[13] The wildfires may have also negatively affected children's nutrition and health conditions as a result of the temporary lack of adequate care given by unhealthy adult family members.[14]

The instrumental variable was therefore constructed as a dichotomous variable that equaled one if the child was living in Sumatra or Kalimantan and was aged 12–36 months when the forest fires began (i.e., on September 5, 1997). Hence, variation in the exposure to such shock mainly derives from two sources: place of residence and age.

The choice of a specific age range was motivated by the large number of studies by nutritionists, physiologists, and social scientists positing the existence of a critical period in human life when brain development is most sensitive to poor nutrition (Stein et al. 1975; Dobbing 1976; Waber et al. 1981; Villar et al. 1984; Glewwe and King 2001). There have been mixed findings, however, concerning the exact age range in which this critical period can be identified, although the bulk of the literature agrees that the impact of shocks on children older than 36 months is zero (Glewwe and King 2001; Hoddinott and Kinsey 2001; Shrimpton et al. 2001).

I therefore conducted a preliminary analysis where I tested for a different age range as well as for fetal exposure and found that the shock experienced during the second and third years of life had the largest negative impact on child health status.[15]

Table 4 reports the subdistrict and the mother fixed effects first-stage estimates of the effect of the exposure to the forest fires on two anthropometric measures: height-for-age *z*-scores (cols. 1, 2) and weight-for-age *z*-scores (cols. 3, 4).[16] In this first stage, therefore, these anthropometric measures in the children

[13] The ingestion of PAH-contaminated food can negatively influence a child's health status—specifically, her growth potential—in several ways. It has been shown in the medical literature that PAH ingestion causes several acute or short-term health effects (e.g., nausea, vomiting, diarrhea, confusion) as well as more harmful effects, e.g., kidney, liver, and gastrointestinal damage and oxidative stress and lipid peroxidation that alter the immunological system (Leonard et al. 2000; ATSDR 2003; Unwin et al. 2006; Bølling et al. 2009; Choi et al. 2010; Jeng et al. 2011; Kim et al. 2013). This implies that there can be consequences on the risk of catching infections and on the body's ability to absorb food nutrient intakes, which in turn will compromise child growth.

[14] As shown in Frankenberg et al. (2005), adults exposed to haze significantly experience greater increases in difficulty with everyday life activities.

[15] These tests are shown in the robustness checks discussed in Sec. IV.C.

[16] This analysis, indeed, follows a consolidated literature in this field that has extensively used height or weight for age as a proxy for health status. According to the WHO (1997), the height-for-age *z*-score is a measure of the nutritional status of the child to the extent that it defines whether child growth reflects a "process of failure to reach linear growth potential as a result of suboptimal health and/or nutritional conditions" (46). I use weight for age, instead, as a robustness check. This is indeed

TABLE 4
EXPOSURE TO FOREST FIRES AND CHILD HEALTH (FIRST-STAGE ESTIMATES)

	Height-for-Age *z*-Scores		Weight-for-Age *z*-Scores	
	SDFE (1)	MFE (2)	SDFE (3)	MFE (4)
Exposure to forest fires	−.522***	−.924***	−.764***	−.714***
	(.234)	(.251)	(.188)	(.200)
Boy	. . .	−.127	−.054	−.034
	. . .	(.119)	(.103)	(.108)
Constant	−1.567***	−1.502***	−1.294***	−1.305***
	(.142)	(.101)	(.114)	(.142)
F-statistics on significance of fires' shock	4.98**	13.52***	16.46***	12.47***
Observations	424	421	417	416
R^2	.078	.159	.141	.152
Number of fixed effects	131	199	129	195

Source. Author's calculations based on Indonesian Family Life Survey 2.
Note. Age fixed effects are included in all specifications. Robust standard errors clustered at the subdistrict level are reported in parentheses. SDFE = subdistrict fixed effects; MFE = mother fixed effects.
** Significant at 5%.
*** Significant at 1%.

affected by the shock are compared to a counterfactual formed by children of the same age (i.e., 12–36 months) living outside Sumatra and Kalimantan and older and younger children from the same islands. As shown in figure A2, height-for-age *z*-scores were, on average, lower for the treated group of children. This is confirmed in the regression results, which indicate that, across all the specifications, there is a negative and significant effect at the 1% level on the endogenous variables, with the magnitude of the coefficient being relatively larger in the height-for-age *z*-score regressions.[17] Moreover, as suggested by the *F*-test statistic, the instrument's validity, at least with respect to the strong correlation with the endogenous variable, is above the thresholds recommended in Staiger and Stock (1997) and Bound, Jaeger, and Baker (1995).[18]

Exposure to forest fires is associated with a decline of about 90% of a standard deviation in height for age and 70% of a standard deviation in weight for age. Given that the normal growing rate for an individual in the age range 12–36 months in the MGRS sample of adequately nourished children is about 1 cm

an indicator that is influenced by both a child's height and weight, and so it has a more complex interpretation. However, it is commonly used to reflect short-term nutritional deficiencies since "short-term changes, esp. reduction in weight-for-age, reveals change in weight-for-height" (WHO 1997:47).

[17] The sibling difference model produces, moreover, larger estimates than those that only exploit within-subdistrict variation, suggesting that parents' behavior and discount rates definitely play an important role in child health.

[18] Additional tests, such as the Kleinbergen-Paap rk LM statistics for underidentification and the Kleinbergen-Paap rk Wald *F*-statistic for weak identification (Kleibergen and Paap 2006), were implemented. Their outcomes further support the validity and exogeneity of the instrument (see table 5).

per month, this effect is translated into a slower growth rate of about 0.10 cm per month within the 3-month period that, on average, occurred between exposure and observation.[19] Exposed children were, therefore, growing but only very little compared to the standards. This effect might therefore be plausible given the young age of these children, which explains their vulnerability to shocks that compromise their body's ability to absorb food nutrient intakes or even to get access to food.[20]

A last important point concerns the second condition for instrumental validity, that is, the exclusion restriction assumption that implies that the only way in which the instrument affects the dependent variable is via its impact on the endogenous variable. Given that there is no statistical test that can be performed to check whether this assumption is violated (at least when using one instrument only), the instrument validity can never be known with complete certainty and can only by checked indirectly or falsified by the data. Therefore, I investigated some auxiliary hypotheses or implications that could add plausibility to the exclusion restriction.

One can think of different situations that can violate the exclusion restriction in this context. The forest fires, for example, may have exerted their effect on children's educational achievements through two other possible channels: (1) on the supply side, they may have destroyed books and schools and harmed teachers; (2) on the demand side, they may also have negatively affected household incomes and thereby probably depleted parental resources devoted to education.

With respect to the first point, this channel does not seem to have mattered to any great extent: the damage reported by the press and by the literature basically consisted in the burning of millions of hectares of wild forest and the spread of smoke and haze. The state of emergency declared by the government of Indonesia, which required the temporary closing of schools, government offices, businesses, airports, and harbors, lasted only 10 days (Dauvergne 1998). Since in this paper I am concerned with children hit by the fires in their earliest months of life and who therefore went to school several years later, this supply-side channel is probably not relevant.

[19] Note that according to the WHO growth charts, a standard deviation corresponds, on average, to 3 cm for children in the age range 12–36 months. Weight and height were measured from late September 1997 to March 1998: about 82% of the sample was measured between late October and December 1997, about 8% was measured between January and March 1998, and the remaining 10% was measured in late September 1997. The few observations (about 2.7% of the sample) measured before September 1997 were dropped.

[20] A similar effect is found by Alderman et al. (2006), who estimated a 73% decline of a standard deviation in height for age in children of the same age range who were exposed to the drought.

Similarly, it can be argued that although the forest fires may have hit household incomes as well, as long as these economic shortages were temporary, it is likely that parents' investments in education were not affected by these income losses.

In order to determine whether this argument is confirmed by the data, I tested (see table A2) whether there were significant differences in the effect of exposure to forest fires on household per capita expenditure in the time period considered and whether exposure to forest fires significantly changed the share of education expenditure. The results attenuate any further concern about instrumental validity because they clearly indicate that the instrument neither significantly affected household income in any of the years considered nor had any impact on education expenditure.

B. Empirical Estimates

Before discussing the main findings based on the preferred MFE-IV approach, I report in columns 1–3 of table 5 the estimates resulting from three alternative and less accurate econometric approaches that, despite being affected by some bias, are still interesting insofar as they provide a first picture of the strength of the health-learning nexus and—compared with the core model—inform about the magnitude and the direction of the endogeneity bias in the relationship under investigation. These approaches were (1) the subdistrict (i.e., *kecamantan*) fixed effects (SDFE), which essentially control for unobserved heterogeneity within each administrative unit; (2) the subdistrict fixed effects combined with the instrumental variable (SDFE-IV), which also concern the correlation between child height and child-specific characteristics; and (3) the mother fixed effects (MFE), which only sweep out the bias due to aspects common across siblings. Last, in column 4, I report the main findings, which, based on the MFE-IV estimator, can be interpreted as the effect of the health shock that affected child height and, through this, long-run education achievement, both through direct effects and parental responses to the shock.

Since most of the children had not completed their schooling in 2007, all the specifications include age dummies that can better standardize the years of schooling (see Yamauchi 2008). Moreover, it is important to consider the possibility that a random shock (other than the main shock of interest) affected all the children living in a certain geographical area. In this case, the use of robust standard errors would not solve issues of contemporaneous and serial correlation of the error term (Bertrand, Duflo, and Mullainathan 2004).

For this reason, in all the specifications, estimates of the impact were produced by clustering the robust standard errors at the *kecamantan* level, which is the smallest administrative unit in Indonesia.

TABLE 5
CHILD HEIGHT FOR AGE AND SUBSEQUENT EDUCATIONAL ACHIEVEMENTS

	SDFE (1)	SDFE-IV (2)	MFE (3)	MFE-IV (4)
	A. Years of Schooling			
Height for age (*z*-scores)	.115***	1.167*	.0130	.513**
	(.0369)	(.633)	(.0716)	(.261)
Boy	−.161	−.0238	−.292*	−.201
	(.101)	(.167)	(.160)	(.189)
Mother's education	.0955***	.0285		
	(.0160)	(.0474)		
Mother's age	−.0125	−.00617		
	(.0118)	(.0174)		
Observations	421	421	421	421
Number of fixed effects	131	131	199	199
R^2 (within)	.697	.383	.792	.756
Kleinbergen-Paap LR.stat (*p*-value)		4.84		9.28
		(.027)		(.002)
Kleinbergen-Paap *F*-statistic		4.98		13.52
	B. Cognitive Test Scores			
Height for age (*z*-scores)	.0173	.0484	.0228*	.0665
	(.0120)	(.0592)	(.0129)	(.0628)
Boy	.0150	.0185	−.00312	.000543
	(.0213)	(.0231)	(.0236)	(.0257)
Mother's education	.0182**	.0152*		
	(.00732)	(.00862)		
Mother's age	.0141**	.0160**		
	(.00550)	(.00688)		
Observations	317	315	317	317
Number of fixed effects	110	108	153	153
R^2 (within)	.099	.066	.059	−.009
Anderson canon.corr.LR.stat		9.24		10.49
		(.002)		(.001)
Cragg-Donald *F*-statistic		14.37		18.36
	C. Age on Starting School			
Height for age (*z*-scores)	−.0813***	−.207	−.0914***	−.261**
	(.0261)	(.136)	(.0329)	(.124)
Boy	.106	.0889	.114	.0896
	(.0683)	(.0697)	(.0765)	(.0805)
Mother's education	−.0433***	−.0304		
	(.0149)	(.0237)		
Mother's age	.00269	.00429		
	(.0102)	(.0109)		

TABLE 5 (*Continued*)

	SDFE (1)	SDFE-IV (2)	MFE (3)	MFE-IV (4)
	C. Age on Starting School			
Observations	400	394	400	385
Number of fixed effects	129	123	196	181
R^2 (within)	.090	.037	.061	−.033
Anderson canon.corr.LR.stat		10.67		11.22
		(.001)		(.000)
Cragg-Donald *F*-statistic		20.12		20.33

Note. The sample consisted of children with a height-for-age *z*-score in the range of −6 to 6 who were aged 0–6 in 1997. These children were subsequently aged 5–11 when they started school, 8–14 when they took the cognitive test (either in 2000 or 2007), and 9–17 when completed years of schooling were observed (i.e., in 2007). Age fixed effects are included in all specifications. Robust standard errors clustered at the subdistrict level are reported in parentheses. SDFE = subdistrict fixed effects; SDFE-IV = subdistrict fixed effects combined with instrumental variable; MFE = mother fixed effects. MFE-IV = mother fixed effects combined with instrumental variable.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

The estimates from the two naïve SDFE and SDFE-IV regressions suggest that there are large positive associations between child height and educational achievements. More specifically, healthier children tended to complete more grades and enter school at younger ages. Moreover, estimates reported in column 3 suggest that the size of the SDFE coefficients is slightly increased on controlling for the endogeneity bias related to the correlation between child height for age and the sibling-invariant error term component (i.e., on assuming a certain degree of substitutability between health and school inputs, parents can engage in compensatory actions in order to equalize learning outcomes among their children). Yet the simple within-sibling estimator does not account for the remaining correlation between the endogenous variable and the specific error term component.

In accordance with our initial expectations, the main findings from the MFE-IV regressions reported in column 4 show that greater child height significantly contributes to improved educational performance, in terms of both completed years of schooling and readiness to enter school.

The results indicate that if the children sampled in this analysis had had the height for age of the well-nourished population of reference, they would have gained, on average, 0.8 additional grades of school.[21] Interestingly, this estimated impact is close to the one found by Alderman et al. (2006).

[21] It can be assumed, however, that part of the effect of child health status on future schooling achievement was transmitted by the age at which the child entered school. In order to detect this transmission channel, I conducted a preliminary analysis where I tested this hypothesis by adding a dummy indicating whether the child entered school on time (i.e., by the age of 7). The results show that the inclusion of this variable indeed mitigated the magnitude of the impact exerted by height for age on the dependent variable but left the statistical significance of the coefficient unaltered.

Inspection of the estimates on the scores achieved on the cognitive test (col. 4, panel B) shows that increased height for age is associated with better performance on the test. For these results, however, we fail to reject the null of no change in the coefficients of interest.

Last, part of the positive effect exerted by child health on schooling achievement may be transmitted by the optimal timing of entering primary school. Indeed, a negative and significant direct relationship is found between height-for-age *z*-score and age on starting school. Less healthy children start school about 6 months later than the well-nourished population of reference (see panel C).

Consistently with the findings on years of schooling, the inclusion of the instrumental variable estimator increases the magnitude of the impact, suggesting the presence of a downward bias in the within-sibling estimator, which can be partly attributed to measurement error bias and partly to the correlation between health status and child innate ability or motivation.[22]

C. *Robustness Checks*

This section presents various checks conducted in order to test for the validity of the findings of the present analysis. Hence, I will deal with issues related to selection bias in the data and address other concerns related to the robustness of the main results.

1. Potential Attrition Bias

One of the main concerns arising from the use of longitudinal data sets relates to the presence of selection bias that may be caused by deaths, by missing data in fundamental variables, or by the screening out of multiple observations recorded in the same wave and of observations with discrepant information provided across the survey's waves.

Since the main analysis reported by this paper was based on variations among siblings, any attrition stemming from maternal, household, and community characteristics was removed by the inclusion of mother fixed effects (Ziliak and Kneiser 1998). Nevertheless, there remained some attrition at the individual level that should be tested for.

[22] As also argued by Imbens and Angrist (1994), Card (2001), and Alderman et al. (2006), the larger size of the estimate in the mother fixed effects–instrumental variable estimator is related to the heterogeneity in returns to early childhood health conditions. Specifically, as demonstrated in Card (2001), it informs us that we are identifying those children with larger initial costs of improving their health conditions and hence with higher marginal returns to an additional gain in their preschool health status. It therefore means that we are identifying children with above-average educational achievements (Alderman et al. 2006).

Table 6 reports the determinants of attrition from the 1997–2007 waves. This test, which followed the methods set out in Fitzgerald, Gottschalk, and Moffitt (1998a, 1998b) and Alderman et al. (2001a), was based on a linear probability model where the dependent variable equalled one if any of the educational outcomes were observed in the second period and zero otherwise. As explanatory variables, I used the same main variables included in the schooling equation: height-for-age *z*-scores, weight for age *z*-scores, child sex, child age, and mother's years of schooling. If these were not significantly correlated with attrition, I could assume that there was no bias in my estimates stemming from attrition on the observables.

Both the subdistrict and the mother fixed effects estimates indicate that more likely to be observed are individuals with higher initial age (due to the higher

TABLE 6
DETERMINANTS OF ATTRITION

	Observed Outcome					
	Years of Schooling		Cognitive Test Scores		Age Starting School	
	SDFE (1)	MFE (2)	SDFE (3)	MFE (4)	SDFE (5)	MFE (6)
Height for age (*z*-scores)	−.0127	−.0103	−.0117	−.0022	−.0122	−.0063
	(.0094)	(.0087)	(.0100)	(.0087)	(.0095)	(.0101)
Weight for age (*z*-scores)	.0099	−.0104	.00581	−.0147	.0089	−.0174
	(.0115)	(.0123)	(.0112)	(.0104)	(.0117)	(.0135)
Boy	.0182	−.0081	.0190	−.00491	.0128	−.0122
	(.0206)	(.0169)	(.0205)	(.0147)	(.0205)	(.0184)
Age in 1997	.0101*	−.0029	.0108*	.00178	.0054	−.0078
	(.0054)	(.00532)	(.00552)	(.00445)	(.0056)	(.0058)
Mother's education	.0112**		.00943*		.0108**	
	(.0047)		(.0051)		(.0045)	
Mother's age	−.0078***		−.0087***		−.0074***	
	(.0021)		(.0021)		(.0021)	
Constant	.435***	.291***	.507***	.319***	.427***	.294***
	(.0773)	(.0211)	(.0817)	(.0222)	(.0759)	(.0239)
Subdistrict fixed effects	Yes		Yes		Yes	
Mother fixed effects		Yes		Yes		Yes
Observations	2163	2163	2163	2163	2163	2163
Number of subdistrict.id	324		324		324	
Number of mother.id		1,698		1,698		1,698
R^2 (within)	.021	.012	.021	.013	.019	.016

Note. Dependent variable = 1 if educational outcome was observed, 0 otherwise. The initial sample was 2,163 children born from 1990 to 1997 whose height-for-age *z*-scores or weight-for-age *z*-scores, or both, were observed in 1997 and lying in the range −6 to 6. From this sample, 936 children had their educational outcome observed in subsequent waves. The estimation method used was the linear probability model. Robust standard errors clustered at the subdistrict level are reported in parentheses. SDFE = subdistrict fixed effects; MFE = mother fixed effects.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

rates of mortality during infancy) born to younger and more educated mothers (probably because of their better accuracy in answering the questionnaire).

However, controlling for mother fixed effects, none of the variables has a significant effect on the probability of being observed, thereby attenuating any concern about selection bias due to attrition.

2. Other Checks

I address here three main concerns that might cast doubt on the validity of the main analysis: robustness of the estimates to the inclusion of additional covariates in the MFE-IV regressions, sensitivity of the results to slight changes in the timing of exposure to the shock, and adequacy of the indicator used for child health status. Tables A3–A5 report the results on the three different measures of educational attainments used in this analysis.

It can be observed that boys tend to have worse educational attainments than girls, especially in school completion and readiness for school. This result is consistent with the evidence provided in many World Bank and Asian Development Bank reports (see, e.g., Asian Development Bank 2006) and may be partly explained by the higher returns to schooling (at later stages of education) for women with respect to men (Deolalikar 1993).

Column 2 in tables A3 and A4 includes a dummy variable for children already at school when their height was measured. It can be argued that parents alter the amount of nutrition and education inputs for their children on observing their school performance. Running this check slightly decreased the magnitude of the height-for-age coefficient but—in the model of years of schooling—increases its statistical significance.

I then included birth order and interaction terms between this and sex. In fact, there may be concern that there is a competition for resources among siblings or that there is a gender bias in parental preferences that is mediated by birth order (Das Gupta 1987). The results suggested that higher-order children tend to perform relatively worse and that—for years of schooling—the effect is stronger among boys.

A second set of robustness checks related to the robustness of the 12- to 36-months age range is used to identify exposure to the shock. As argued in Section IV.A, the majority of empirical studies have excluded that ages above 36 months matter, but some of them find a stronger impact at the age range of 12–24 months. Moreover, a recent body of research in both the epidemiological and economic literature that has tested Barker's fetal-origin hypothesis (Barker 1998) provides evidence that fetal health conditions matter for many adult socioeconomic outcomes (Behrman and Rosenzweig 2004; Almond

2006; Jayachandran 2009; Maccini and Yang 2009; Royer 2009). However, some criticisms have been made (Glewwe and King 2001; Lancet 2001; Rasmussen 2001; Maccini and Yang 2009) on the methodological shortcomings of part of this research.

In a preliminary analysis that I conducted, I found zero impact below the second year of life and above the third year of life, but similar results emerged when using both the 12- to 36-months and 12- to 24-months age ranges (see table A6). The coefficients for the model of years of schooling and age starting school are highly significant and larger in magnitude with respect to those of the main model. This is in line with the trend depicted in figure A2, where a large gap between the treated and nontreated groups is visible at age 24 months. Nevertheless, it should also be noted that the power of the tests for instrumental validity and exogeneity was relatively lower with respect to the main model.

These results are in accordance with the findings reported in the study by Glewwe and King (2001), which used longitudinal data from the Philippines to examine the issue of the timing of malnutrition in early childhood and subsequent school performance. Glewwe and King could neither prove that the most critical period is during the first 6 months of life nor find evidence to support Dobbing's (1976) hypothesis, that is, that health conditions in utero are more important than postnatal nutritional deficiencies. Moreover, Maccini and Yang's (2009) analysis on the consequences of early life rainfall on Indonesians gives no indication that prenatal shocks are relevant, compared with shocks experienced in the postweaning period.

Likewise, this study does not find any significant impact of health conditions experienced during the first year of life and in utero on subsequent school performance. The tests set out in tables A7 and A8 show that—across the different specifications—the estimated coefficients of the height-for-age *z*-scores in most of the cases do not significantly differ from zero.

Nevertheless, it is important to note that—given that the lack of significant results for exposure in utero may also be related to measurement and sample-size issues—this study cannot indisputably conclude that poor health conditions before birth are not important for subsequent education and cognitive outcomes.[23] Rather, consistently with Maccini and Yang (2009) and with Glewwe and King (2001), I provide stronger evidence that child health status in the postweaning period matters.

[23] Given the very small number of observations for which information on birth weight was available, I used as the measure of nutritional status the height-for-age *z*-scores measured in 2000, i.e., when children were 24- to 36-months old.

A last concern relates to the adequacy of the health measure considered in the main analysis: height for age. This variable, beyond capturing nutrition, is an output of health more broadly. Children's height was measured a few months after the shock took place. Given this short time interval, it can be argued that the use of an indicator of chronic nutritional deprivation, such as height-for-age *z*-scores, may not be adequate for correct identification of the effect of the shock on health status. The first-stage regressions shown in table 4 already confirm the validity of the instrument on height-for-age *z*-scores and exhibit a smaller impact (in terms of both magnitude and statistical validity) of the shock on the weight-for-age *z*-scores, which is an indicator of previous and current nutritional deficiencies.

Table A9 summarizes the results for the MFE-IV regressions using weight-for-age *z*-scores as the endogenous variable. The estimated coefficients indicate that the magnitude of the effect is relatively larger than the one in table 6, although the statistical significance is, on average, lower for years of schooling. On the other hand, the estimated impact on age on starting school is always significant at the 5% level, suggesting that short-term nutritional deficiencies may play an important role in influencing readiness for school.

V. Conclusions

The analysis conducted in this study represents a new step in the debate on the role of health in education outcomes, which is of even greater importance and urgency in a developing country like Indonesia, where malnutrition rates are high and inequalities in education opportunities are widening (Ferreira and Gignoux 2014; World Bank 2014). The use of longitudinal data provides a clear advantage in the analysis of the causal relationship between child health and subsequent educational attainments because it makes it possible to obtain observations at different points in a long time period and thus avoid problems of omitted variable bias and attenuation bias that may affect the validity of the estimates (Glewwe and Miguel 2008). Moreover, because child health and schooling reflect parental decisions concerning investments in children's human capital (Alderman et al. 2001b; Glewwe and Miguel 2008), I have taken into account the behavioral choices underlying child health by applying a sibling difference model combined with instrumental variable estimation, since these address endogeneity biases. There are three relevant remarks that emerge from this study. First, the results suggest that poor health conditions in childhood significantly and negatively influence school attainments, both in terms of years of education and readiness to enter school. On the other hand, the strength of the relationship with cognitive test scores has weak statistical support. The main findings, which are confirmed by robustness checks, imply that education and health objectives should not be seen as competing goals but as closely interlinked. Many countries—including Indonesia—have been struggling to achieve the Millennium Development Goals. Although remarkable progress has been made, there is still substantial room for improvement, and it is necessary to continue on stable and durable paths of development. Financial resources devoted, for example, to child nutrition policies do not necessarily compete with those for education; instead, as implied by this study, they can be regarded as more cost-effective means to enhance present and future socioeconomic development.

Second, in line with the growing body of scientific studies on the long-run impact of childhood shocks (see, e.g., Bhalotra 2010; Almond and Currie 2011a, 2011b; Akresh et al. 2012), exposure to environmental disasters may have long-lasting effects on individuals, despite any compensatory actions that they or their caregivers may undertake to alleviate the impact of the shock.

Third, like Glewwe and King (2001), I do not find any strong support for the hypothesis that prenatal health conditions and those of the first 6 months of life have long-term effects on cognitive and education outcomes.

One has to be careful, however, to note that these findings are subject to one important caveat related to the lack of variation attributable to forest fires in the limited data available. In this paper, I considered only a coarse, between-islands variation. Of course, within those islands, there could have been variation in the intensity of fires or smoke, and some areas could have been affected more than others. This study considers only an average effect across these different intensities of smoke, my assumption being that, independently of the intensity of the smoke, the shock (a conjunction of fires, smoke, and drought) was already large enough to compromise child health.

Appendix

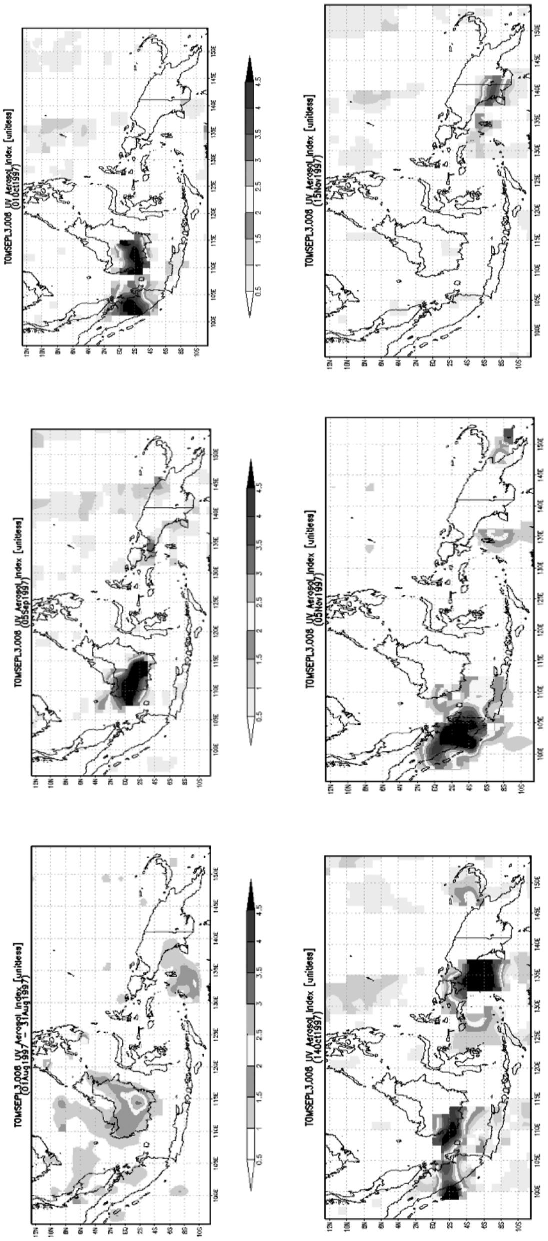

Figure A1. Location and timing of the Indonesian wildfires. *Clockwise from top left*, levels of haze and smoke in August 1997 (monthly average); on September 5, 1997; on October 1, 1997; on October 14, 1997; on November 5, 1997; and on November 15, 1997. Haze is measured using the UV aerosol index. Data source: NASA Total Ozone Mapping Spectrometer (TOMS). A color version of this figure is available online.

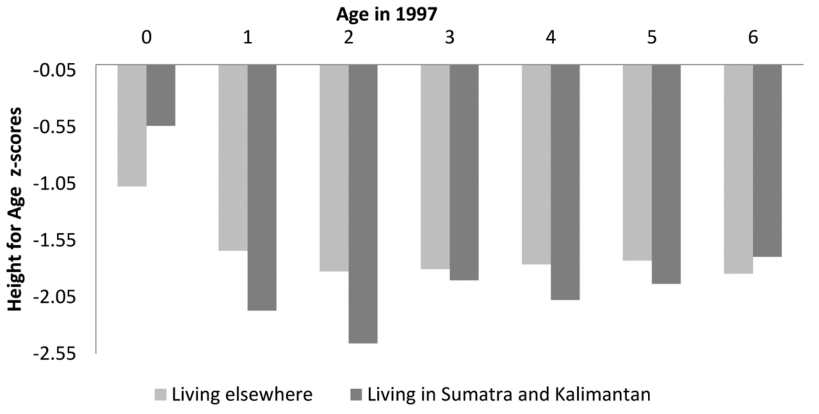

Figure A2. Height-for-age *z*-scores in children exposed and not exposed to the shock. Shown are average height-for-age *z*-scores against age in September 1997 for subjects living in Sumatra and Kalimantan (the treatment areas) and for subjects living elsewhere. Data source: Author's elaboration on Indonesian Family Life Survey 2.

TABLE A1

DESCRIPTION OF THE VARIABLES USED

Variable	Definition
Gender (male)	1 = child is boy; 0 = child is girl
Age (first period)	Child's age (in years)
Height-for-age *z*-score	Height-for-age (first period) *z*-score statistics
Weight-for-age *z*-score	Weight-for-age (first period) *z*-score statistics
Age (second period)	Child's age (years)
Age start school	Age (years) at which child entered school
Cognitive test score	Score obtained for cognitive test (range 0–1)
Age cognitive test score	Estimated age (years) at which child took cognitive test
Years of schooling	Years of education completed in second period
Mother's education	Years of education completed by mother
Mother's age	Mother's age in years
Rural	1 = household located in rural area; 0 = household located in urban area
Fires shock	1 = child living in Sumatra or Kalimantan and aged 12–36 months on September 9, 1997

TABLE A2

EFFECT OF FOREST FIRES ON INCOME AND EDUCATION EXPENDITURE: A TEST FOR THE ASSUMPTION OF EXCLUSION RESTRICTION

	Dependant Variable				
	Log of Household Per Capita Expenditure			Change in Share of Education Expenditure	
	In 1997 (1)	In 2000 (2)	In 2007 (3)	Between 1997 and 2000 (4)	Between 1997 and 2007 (5)
Exposure to forest fires	−.0788	.116	−.0772	−.802	.829
	(.103)	(.0978)	(.0983)	(.849)	(1.419)
Constant	11.28***	11.90***	12.85***	2.456***	7.556***
	(.0226)	(.0216)	(.0216)	(.208)	(.344)
Observations	934	932	919	930	917
R^2	.001	.002	.001	.001	.000

Note. Household per capita expenditure is measured in nominal terms. Provincial dummies are included in regressions 1–3 in order to control for differences in price levels among provinces. Standard errors are in parentheses.
*** Significant at 1%.

TABLE A3

ROBUSTNESS CHECK 1: MFE-AFE-IV ESTIMATES OF HEIGHT FOR AGE ON YEARS OF SCHOOLING, WITH ADDITIONAL COVARIATES ADDED

	(1)	(2)	(3)	(4)	(5)
Height for age (*z*-scores)	.677*	.528**	.398*	.408*	.401*
	(.388)	(.261)	(.238)	(.246)	(.241)
Boy	−1.069*	−.192	−.240	−.631	−.066
	(.550)	(.188)	(.183)	(.395)	(.286)
ZHFA × boy	−.456				
	(.298)				
At school in 1997		.199			
		(.276)			
Birth order			−.421*	−.560**	−.430*
			(.248)	(.245)	(.245)
Birth order × boy				.209	
				(.176)	
Age × boy					.047
					(.062)
Observations	418	418	399	399	399
Number of mother.id	196	196	187	187	187
R^2 (within)	.752	.754	.770	.770	.770
Kleinbergen-Paap LR.stat (*p*-value)	6.725	10.23	8.558	8.616	8.846
	(.0095)	(.0014)	(.0034)	(.0033)	(.0033)
Kleinbergen-Paap *F*-statistic	10.25	15.83	13.10	13.36	13.25

Note. The sample consisted of children aged 0–6 in 1997 and 9–17 in 2007, with height-for-age *z*-scores in the range −6 to 6 in 1997 and aged 5–11 when they started school. Robust standard errors clustered at the subdistrict level are reported in parentheses. MFE = mother fixed effects; AFE = age fixed effects; IV = instrumental variable; ZHFA = height-for-age *z*-scores.
* Significant at 10%.
** Significant at 5%.

TABLE A4

ROBUSTNESS CHECK 1: MFE-AFE-IV ESTIMATES OF HEIGHT FOR AGE ON COGNITIVE TEST SCORE, WITH ADDITIONAL COVARIATES ADDED

	(1)	(2)	(3)	(4)	(5)
Height for age (*z*-scores)	.0739	.0661	.0656	.0669	.0633
	(.0798)	(.0634)	(.0604)	(.0601)	(.0590)
Boy	−.0514	−.00135	−.000006	.0317	−.0445
	(.112)	(.0259)	(.0259)	(.0673)	(.142)
ZHFA × boy	−.0304				
	(.0668)				
At school in 1997		.0656			
		(.0519)			
Birth order			−.00601	.00493	−.00577
			(.0321)	(.0325)	(.0318)
Birth order × boy				−.0178	
				(.0341)	
Age × boy					.00407
					(.0126)
Observations	312	312	312	312	312
Number of mother.id	148	148	148	148	148
R^2 (within)	−.011	.007	−.006	−.008	.002
Kleinbergen-Paap LR.stat					
(*p*-value)	10.47	10.49	10.62	10.50	11.46
	(.0012)	(.0012)	(.0011)	(.0012)	(.0007)
Kleinbergen-Paap *F*-statistic	22.84	18.30	17.51	17.32	18.60

Note. The sample consisted of children aged 0–6 in 1997 and 8–14 when they took the cognitive test (either in 2000 or in 2007), with height-for-age *z*-scores in the range −6 to 6 in 1997 and a school enrollment age of 5–11. Robust standard errors clustered at the subdistrict level are reported in parentheses. MFE = mother fixed effects; AFE = age fixed effects; IV = instrumental variable; ZHFA = height-for-age *z*-scores.

TABLE A5

ROBUSTNESS CHECK 1: MFE-AFE-IV ESTIMATES OF HEIGHT FOR AGE ON AGE WHEN STARTING SCHOOL, WITH ADDITIONAL COVARIATES ADDED

	(1)	(2)	(3)	(4)
Height for age (*z*-scores)	−.370**	−.262**	−.263**	−.268**
	(.180)	(.124)	(.130)	(.128)
Boy	.635**	.105	.338	−.0525
	(.265)	(.0780)	(.209)	(.132)
ZHFA × boy	.286**			
	(.141)			
Birth order		−.0803	.00385	−.0740
		(.131)	(.143)	(.133)
Birth order × boy			−.123	
			(.0982)	
Age × boy				.0431
				(.0328)
Observations	385	366	366	366
Number of mother.id	181	172	172	172
R^2 (within)	.015	−.027	−.019	−.025
Kleinbergen-Paap LR.stat (*p*-value)	7.628	10.46	10.38	10.50
	(.0058)	(.0012)	(.0013)	(.0012)
Kleinbergen-Paap *F*-statistic	12.49	19.30	19.19	19.23

Note. The sample consisted of children that in 1997 were aged 0–6 and had height-for-age *z*-scores in the range −6 to 6 in 1997 and were subsequently aged 5–11 when they started school. The difference between age when starting school and age at which height was measured is larger than or equal to zero. Robust standard errors clustered at the subdistrict level are reported in parentheses. MFE = mother fixed effects; AFE = age fixed effects; IV = instrumental variable; ZHFA = height-for-age *z*-scores.
** Significant at 5%.

TABLE A6

ROBUSTNESS CHECK 2: MFE-IV ESTIMATES OF HEIGHT FOR AGE INSTRUMENTED WITH EXPOSURE TO FOREST FIRES DURING THE FIRST 12–24 MONTHS OF LIFE IN BASELINE AND ALTERNATIVE SPECIFICATIONS

	Educational Outcome		
	Years of Schooling	Cognitive Test Score	Age Starting School
Baseline	.796**	.108	−.256**
	(.315)	(.090)	(.125)
ZHFA × boy included	1.209**	.141	−.429**
	(.538)	(.138)	(.173)
At school in 1997 included	.800***	.107	. . .
	(.308)	(.091)	. . .
Birth order included	.680***	.105	−.308**
	(.325)	(.082)	(.146)
Birth order × boy included	.712**	.099	−.319**
	(.331)	(.079)	(.146)
Birth order and age × boy included	.685**	.104	−.312**
	(.326)	(.082)	(.148)

Note. Age fixed effects are included in all specifications. Robust standard errors clustered at the subdistrict level are reported in parentheses. MFE = mother fixed effects; IV = instrumental variable; ZHFA = height-for-age *z*-scores.
** Significant at 5%.
*** Significant at 1%.

TABLE A7

ROBUSTNESS CHECK 2: MFE-IV ESTIMATES OF HEIGHT FOR AGE INSTRUMENTED WITH EXPOSURE TO FOREST FIRES DURING THE FIRST 0–12 MONTHS OF LIFE IN BASELINE AND ALTERNATIVE SPECIFICATIONS

	Educational Outcome		
	Years of Schooling	Cognitive Test Score	Age Starting School
Baseline	.968	−.014	−.042
	(.682)	(.069)	(.227)
ZHFA × boy included	1.056	1.250	−.203
	(2.232)	(10.21)	(.725)
At school in 1997 included	.970	−.006	. . .
	(.749)	(.069)	. . .
Birth order included	.930	−.011	−.049
	(.655)	(.069)	(.216)
Birth order × boy included	.903	−.008	−.032
	(.650)	(.070)	(.208)
Birth order and age × boy included	.905	−.011	−.042
	(.666)	(.070)	(.214)

Note. Age fixed effects are included in all specifications. Robust standard errors clustered at the subdistrict level are reported in parentheses. MFE = mother fixed effects; IV = instrumental variable; ZHFA = height-for-age *z*-scores.

TABLE A8

ROBUSTNESS CHECK 2: MFE-IV ESTIMATES OF HEIGHT FOR AGE INSTRUMENTED WITH PRENATAL EXPOSURE TO FOREST FIRES IN BASELINE AND ALTERNATIVE SPECIFICATIONS

	Educational Outcome	
	Years of Schooling	Age Starting School
Baseline	1.758*	.515*
	(.988)	(.305)
ZHFA × boy included	1.799	.521*
	(1.115)	(.273)
At school in 1997 included	1.798*	. . .
	(1.048)	. . .
Birth order included	2.087	.540
	(1.592)	(.445)
Birth order × boy included	2.129	.511
	(1.639)	(.449)
Birth order and age × boy included	1.865	.542
	(1.569)	(.417)

Note. Age fixed effects are included in all specifications. Height for age for children exposed in utero is measured in Indonesian Family Life Survey 2000. Robust standard errors clustered at the subdistrict level are reported in parentheses. MFE = mother fixed effects; IV = instrumental variable; ZHFA = height-for-age *z*-scores.
* Significant at 10%.

TABLE A9
ROBUSTNESS CHECK 3: MFE-IV ESTIMATES OF WEIGHT FOR AGE IN BASELINE AND ALTERNATIVE SPECIFICATIONS

	Educational Outcome		
	Years of Schooling	Cognitive Test Score	Age Starting School
Baseline	.667**	.046	−.388**
	(.327)	(.065)	(.186)
ZWFA × boy included	.458	.064	−.447**
	(.351)	(.090)	(.224)
At school in 1997 included	.701**	.048*	. . .
	(.337)	(.066)	. . .
Birth order included	.552*	.048	−.426**
	(.330)	(.061)	(.212)
Birth order × boy included	.577	.051	−.428**
	(.353)	(.061)	(.227)
Birth order and age × boy included	.575*	.044	−.447**
	(.345)	(.060)	(.227)

Note. Age fixed effects are included in all specifications. Robust standard errors clustered at the subdistrict level are reported in parentheses. MFE = mother fixed effects; IV = instrumental variable; ZWFA = weight-for-age *z*-scores.
* Significant at 10%.
** Significant at 5%.

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