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## THE EDUCATION, LABOUR MARKET AND HEALTH CONSEQUENCES OF CHILD LABOUR


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# THE EDUCATION, LABOUR MARKET AND HEALTH CONSEQUENCES OF CHILD LABOUR

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# ABSTRACT

## The Education, Labour Market and Health Consequences of Child Labour*

Though there is a large literature on the determinants of child labour and many initiatives aimed at combating this phenomenon, there is limited evidence on the consequences of child labour for socioeconomic outcomes such as education, occupational choice, wages, and health. Using panel data from Vietnam and an instrumental variables strategy, we evaluate the effect of child labour participation on outcomes over a five-year horizon. We find significant negative impacts of child labour on subsequent school participation and educational attainment. On the other hand, we find that those who worked as children are likely to earn a higher wage as young adults. This effect more than fully offsets the foregone earnings due to reduced schooling, particularly for girls. We find no significant effects of child labour on several indicators of health. This evidence in part accounts for why child labour is such a pervasive phenomenon and suggests that the case against child labour requires both future increases in the returns to schooling (i.e., beyond the five year horizon encompassed by our dataset) and that parents are able to borrow (or internally fund) the investment in schooling.

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## 1. Introduction

This paper studies the effect of child labor on schooling, labor market, and health outcomes. In particular, we examine whether having worked as a child reduces subsequent school attendance, educational attainment, and affects occupational choice, wages, and health. We find that children who worked when they were young are, five years later, significantly less likely to be attending school, have a significantly lower level of educational attainment, and are more likely to lag behind in terms of grade-per-year measures. However, we find evidence that child labor leads to a greater probability of wage-employment and to an increased wage rate that more than fully offsets the foregone earnings due to reduced schooling. Child labor does not appear to have significant effects on health.

The question we examine is important for many reasons. The assumption that child labor is harmful to children's development underpins both the theoretical literature and the policy debate. For example, from the policy perspective, there is a general perception that the worldwide returns to eliminating child labor are very large (see International Labour Organization [ILO], 2003). However, the evidence that rigorously quantifies the consequences of child labor is scant. Both theoretically and empirically it is not clear whether child labor substantially displaces schooling. In rural settings in developing countries (and more than 70 percent of child labor in developing countries is rural; ILO 2002), both school and child labor tend to be low-intensity activities, in contrast to the sweatshops and full-time schooling that characterize child labor in the popular imagination and historically in some urban settings in North America and Europe (see Basu, 1999). Furthermore, even if child labor disrupts schooling, it presumably also provides the child with labor market experience that could subsequently lead to increased wages. Which effect dominates is an empirical matter.

A growing empirical literature analyses the relationship between child labor and school attainment (reviewed in Section 2.1) but, with a few exceptions, this literature examines the correlation, not the causal relationship, between these variables. There are many reasons to doubt a causal interpretation of the correlation between child labor and education. Households that resort to child labor presumably differ along an array of dimensions, both observable (education, wealth, occupation) and unobservable (social networks, concern for children, etc.), from those that do not. Second, within households, children's ability is unobserved to the econometricians but is observable to parents. To the extent that parents send their least (most) motivated children to work, this would generate a negative (positive) correlation between child labor and school attainment simply because of selection.

In this paper, we estimate the causal relationship between child labor and school attainment, and examine whether child labor has an effect on the subsequent wages and health of children. While there is some literature examining the effect of child labor on schooling, on subsequent labor market outcomes, and on health, this is, to our knowledge, the first paper to simultaneously tackle all of these questions.

We employ an instrumental variables strategy that addresses some of the limitations of previous work. Using data from Vietnam, we instrument for participation in child labor using crop shocks and rice prices, variables that influence child labor but that are plausibly exogenous with respect to household choices (we provide a detailed discussion of our empirical strategy in Section 4).

We find that, over the 5-year period spanned by our panel, a one standard deviation increase in hours worked leads to a 48% lower chance of being in school, a 35% decrease in educational attainment, and a 66% percent increase in the number of years off-track in terms of grade-per-year. Our indicators of health are generally not affected by child labor status. However, children who have experienced child labor are more likely to be working for wages five years later, and are also more likely to be earning a higher daily wage. The estimates are significant at standard levels, and suggest that the returns to work experience are higher than the returns to schooling and that, overall, child labor might amount to a net benefit for children. We do not interpret these results as evidence in favor of child labor, but instead as evidence for why child labor is difficult to eliminate in the short term.

The paper is organized as follows. Section 2 provides a review of the literature, and Section 3 describes the data. Section 4 outlines our empirical strategy. Section 5 presents our results on the consequences of child labor. Section 6 compares the magnitude of the loss from educational attainment with the gain in terms of wages. Section 7 concludes.

## 2. Literature Review

### *2.1 The Child Labor–Schooling Tradeoff*

There is a large literature that examines the tradeoff between child labor and schooling. In this section, we highlight a few of the existing results. Patrinos and Psacharopoulos (1995) show that factors that predict an increase in child labor also predict reduced attendance and an increased chance of grade repetition. In a subsequent paper, the authors estimate this relationship directly, and show that child work is a significant predictor of age-grade distortion (see Patrinos and Psacharopoulos, 1997). Akabayashi and Psacharopoulos (1999) show that in addition to school attainment, children's reading competence (as assessed by parents) decreases with child labor hours. Finally, Heady (2003) uses objective measures of reading and mathematics ability and finds a negative relationship between child labor and educational attainment in Ghana.

The papers reviewed thus far examine the correlation between child labor and schooling, rather than the causal relationship. As we discuss in detail below, there are many reasons to doubt that these two coincide. A few recent papers address this issue. Using data from Ghana, Boozer and Suri (2001) exploit regional variation in the pattern of rainfall as a source of exogenous variation in child labor. They find that a one hour increase in child labor leads to a 0.38 hour decrease in contemporaneous schooling. Cavalieri (2002) uses propensity score matching and finds a significantly negative effect of child labor on educational performance. Ray and Lancaster (2003) instrument child labor with household measures of income, assets, and infrastructure (water, telephone, and electricity) to analyze its impact on several school outcome variables in seven countries. Their findings generally indicate a negative impact of child labor on school outcomes.[1] However, their two-stage strategy is questionable, as its relies on the strong assumption that household income, assets, and infrastructure satisfy the exclusion restriction in the schooling equations. Finally, Ravallion and Wodon (2000) indirectly assess this relationship in their study of a food-for-school program in Bangladesh that exploits between-village variation in program participation. They find that the program led to a significant increase in schooling, but that only one eighth to one quarter of the increased schooling hours seem to have come from decreased child labor. This suggests that child labor does not lead to a one-for-one reduction in schooling.

The link between child labor and subsequent labor market outcomes is examined by Emerson and Souza (2002). They show that, controlling for family background and cohort, early exposure to child labor significantly reduces earnings while no significant effect emerges for adolescents (closer to the age range we examine). However, the authors do not address the endogenous choice to enter into child labor and, thus, their findings cannot be interpreted as causal.

In this paper, we make two contributions beyond these studies. First, we use instrumental variables and household fixed effects to try to correct for the discussed selection biases that emerge in these type of child labor studies. Though in an observational study no identification strategy is perfect, we believe our use of these two methods produces a plausible range of estimates. Second, we examine both educational and labor market outcomes, which allows us to address the key question of this paper, namely whether the net effect of child labor is negative.

### *2.2 The Returns to Schooling*

In order to compare the effects of child labor on schooling and on labor market outcomes, we need estimates of returns to schooling. The literature on returns to schooling is vast. Psacharopoulos and Patrinos (2002) summarize a wide range of studies that focus on individual wage earnings (i.e. excluding returns to education in self-employment activities or returns associated with labor contributions to family business and farms). Overall they find that the returns to education are higher in developing countries than in developed countries. For Asian countries, they estimate a 10 percent rate of return to a year in school, compared to 7.5 percent for OECD countries or 12 percent for Latin America and the Caribbean.

Of course, it is also useful to compare these estimates to those from the standard studies for the United States that use quasi-experimental data (e.g., Angrist, 1990; Ashenfelter and Krueger, 1994; and Ashenfelter and Rouse, 1998). These studies produce estimates on the order of a 10 percent return to a year of schooling.

A recent paper by Moock et al. (2003) uses the 1992-93 LSMS to estimate returns to education in Vietnam. They find that an additional year of schooling is associated with a 5% increase in earnings. We will discuss how these findings relate to our evidence in section 6.

[1] Note that in some cases they find the marginal impact of child labor to be positive. In particular, for Sri Lanka, the impact is positive for all schooling outcomes.

### *2.3 Existing work on Vietnam*

The rapid economic growth in Vietnam in the 1990s has been characterized by a decline in both the incidence and intensity of child labor (see Rosati and Tzannatos, 2004, for a description of these trends). Edmonds and Turk (2003) document the sharp decline in child labor in the 1990s, and they link this decline to significantly improved living standards. In particular Edmonds (2003) and Edmonds and Pavcnik (2003) examine the effect that the integration of Vietnam's rice market had on the child and adult labor markets. They find that the increase in rice prices between 1992-93 and 1997-98 was associated with reduced child labor. This result motivates the first stage of our two-stage least squares procedure.

A recent paper by O'Donnell *et al*. (2003) investigates the impact of child labor on health outcomes for children in Vietnam. Using instrumental variable estimation, the authors find that child labor has a negative impact on health outcomes in the long run. We will discuss their work further in section 5.4.

Finally, in terms of the rural labor market and returns to schooling, Glewwe and Jacoby (1998) note that it may not be efficient to keep productive family members in school since evidence suggests that primary schooling raises productivity in agriculture whereas secondary schooling does not provide additional productivity gains.[2]

## 3. Data Description

We use data from the Vietnam Living Standards Survey household surveys (VLSS) that were conducted in 1992-93 and 1997-98. Both surveys were conducted by Vietnam's General Statistics Office (see www.worldbank.org/lsms). Of the 4,800 household interviewed in 1992-93, about 4,300 of these were among the 6,000 households interviewed in 1997-98. The surveys contain information on household composition, time use for children, educational attainment, and labor market activities of household members. In conjunction with the household survey, a community survey was conducted in rural communes to gather information such as presence of schools, roads, electricity, local rice prices, and occurrence of crop disasters. For this paper, our analysis will utilize information on the panel of rural households with children between the ages of 8 and 13 at the time of the 1992-93 survey.

How to best capture the accumulation of human capital in this context is a debated issue. Ideally one would want to use differential measures of IQ or of comparable test scores before and after the child worked. In practice, controls for initial ability are usually not available, especially for developing countries where the problem of child labor is more diffused. In our work, we use a range of measures to capture different aspects of a child's human capital. First we use a dichotomous variable for school attendance. School attendance is an input in the process of formation of human capital and, as such, only a distant proxy for the outcome of interest, the accumulation of knowledge. However, existing evidence (see for example King *et al*., 1999) suggests that indicators of attendance co-vary quite substantially with child labor (working children attend less regularly than non working children) and appear to be better measures of time in school

[2] At the same time, the tradeoff to reduced schooling would be increased experience in working on the family farm which may have significant benefits (see, for example, Rosenzweig and Wolpin, 1985).

than, say, enrollment. Next, we use highest grade attained and grade-for-age (in our case, the number of years a child is off track) as outcome variables. These are two output measures of the schooling process. In particular, by tracking whether a child attains sufficiently within a grade, grade-for-age is close to a measure of the value added of schooling and, therefore, particularly suitable as a proxy for the accumulation of human capital.

Table 1 summarizes the variables we use. Of the 2,108 children between the ages of eight and thirteen in our sample, 637 are observed to work in the first round of the survey. We measure total labor hours as the total hours the child was engaged in income-generating work, including work on the family business or farm. The majority of children working in either the first survey (1992-93) or the follow-up survey (1997-98) were working as unpaid family labor in agriculture or non-agricultural businesses run by the household.[3] The average work intensity is 7 hours a week, but among children who work is 24 hours a week. The gender distribution of working children is balanced. Interestingly, there are no significant differences in the education of parents of working and non working children. Households where children do not work have a slightly higher level of per capita expenditure than those where children work.

The middle section of the table summarizes the two instruments we will use to identify the decision to send a child to work: community-level rice prices and crop shocks in 1992-93. There is substantial variation in both rice prices and crop disasters in 1992-93. As noted in Benjamin and Brandt (2003) and Edmonds and Pavcnik (2003), the variation in rice prices in 1992-93 is due to the fact that the sale of rice across communities was prohibited prior to 1997. Neither rice prices nor incidence of crop disasters appear to be unconditionally correlated with child labor. However, these are highly significant predictors of child labor in a regression framework (see below).

Finally, table 1 summarizes the outcomes of interest. In the second survey round, 64 percent of children are in school overall, but the rate of school attendance is 8 percentage points higher among non-working children than among those who work. Though it is also true that there tend to be more schools in villages where children do not work, we will find that the schooling-child labor relationship is significant even after controlling for this difference. The level of educational attainment is higher and the number of years behind in school significantly lower among non-working children compared to working children. Finally, we note that children who work in the first round are not more likely to be working for a wage by 1997-98. However, we will find that once additional regression controls are accounted for, children who worked in round one earn a significantly higher wage in round two.[4]

[3]The concept of child labor (by ILO standards) does not necessarily refer to simply any work done by a child, but, rather, work that stunts or limits the child's development or puts the child at risk. However, in survey data it is difficult (perhaps impossible) to appropriately isolate the portion of time spent working on the farm that qualifies under this very nuanced definition.

[4] Two features of the data are worth noting. First, one might be concerned that children more (or less) likely to be working in the second round are more likely to drop out of the sample. However, Edmonds and Turk (2003) find this problem not to be severe. Secondly, as noted in Edmonds and Pavcnik (2003), the form of the child labor question changed between the two surveys. However, this is not a concern for us since our child labor treatment occurs in the first survey round.

## 4. Empirical Framework

In this section we outline the framework we use to identify the effect of child labor on a range of subsequent child outcomes.

The treatment in our analysis is having participated in child labor in the first round of the survey, $T_i$. The outcomes ($Y_i$) in which we are interested (school enrollment, highest grade completed, age-to-grade, occupation, wage, and health) are measured five years later. Thus our basic specification is of the form:

$$Y_{i,\,t+5} = \alpha + \beta T_{i,t} + \gamma X_{i,t+5} + \varepsilon_{i,t+5}, \tag{1}$$

where $X_i$ are household and community-level controls. There are several restrictions we impose on the sample that we examine. First, we consider children between the ages of eight and thirteen. The prevalence of labor among younger children is low. Likewise, by some definitions, labor at age fourteen and older would not be viewed as a particularly serious form of child labor. Second, we restrict the sample to those children who were in school during the first round of interviews. If we included children who were not in school during round one, we would also have to include the in-school variable in equation (1) above, which would then create additional problems of identification (namely identifying the separate effects of schooling and child labor in round one on outcomes in round two). Instead, we choose to identify the effect of child labor among those children who were in school in round one (1992-93).

There are two potential sources of selection bias in estimating equation (1) using OLS: between-household selection (which types of households opt into child labor) and within-household selection (which children parents select to work more or less). To address the first, we are able to control for a range of household characteristics such as parental education and household expenditure in round one; omitted household characteristics that determine participation in child labor and that affect educational choices remain a concern. We can also include household fixed effects to control for non-time-varying household unobservables:

$$Y_{i,\,t+5} = \alpha_h + \beta T_{i,t} + \gamma X_{i,t+5} + \varepsilon_{i,t+5}. \tag{2}$$

It is inherently more difficult to control for within-household differences among children, since our dataset does not include child-level ability measures. However, we are able to address both sources of bias using an instrumental variables strategy. The ideal instrument induces variation in child labor that affects the outcome of interest (e.g., schooling) only through the child labor participation decision. We consider two instruments: crop disasters and rice prices (both measured in the first survey round). The relevance of crop shocks to child labor is clear: a shock to production technology affects the demand for child labor.[5] Rice prices potentially affect both the demand for and supply of child labor.[6] Higher rice prices could lead to the decision to cultivate more rice, and

[5] In a related paper, we find that crop shocks are important determinants of child labor decisions. See Beegle et al. (2003).

[6] See the discussion in Edmonds and Pavcnik (2003) and Kruger (2002). For example, Kruger (2002) finds a positive effect of coffee prices on child labor in Nicaragua.

hence increase the demand for child labor. Higher rice prices would also have an income effect on rice producing households, leading households to reduce the supply of child labor. For our purposes, which effect dominates does not matter, as long as rice prices are relevant for determining child labor decisions.

In order to be valid, the instruments need to be exogenous to the schooling decision in round two of the survey and affect it only through the child labor decision in round one. We will show that, over a five year horizon, crop shocks are transitory; thus there is no reason to believe that the occurrence of a disaster in the first round, 1992-93, would be directly relevant to schooling decisions five years later. Likewise, rice prices reflect demand and supply conditions in the rice market over a relatively short horizon. In that sense, if rice prices were to affect schooling decisions in 1997-98 through channels other than child labor in 1992-93, it would presumably be through rice prices in 1997-98. By controlling for rice prices in 1997-98, we can ensure that rice prices in 1992-93 should be excluded from the outcome equation.

However, one can think of mechanisms other than child labor through which our set of instruments can affect the outcomes of interest. Two possible examples are nutrition (see, for example, Strauss, 1986) and wealth. If higher rice prices are associated with lower nutrition for children, and lower nutrition in round one affects schooling in round two, then part of our schooling effect could be due to nutrition. Likewise, if crop shocks permanently reduce wealth, then the negative effect of child labor on schooling (and increased work) could simply be due to reduced wealth.

There are two responses to this critique. First, since we have two instruments and a range of outcomes, it is unlikely that either nutrition or wealth could explain away all of our results. Although a nutrition effect would bias our education results in the direction of finding an effect, it would bias our labor market results toward zero (since poor nutrition is presumably bad for labor market outcomes). Moreover, it is unlikely that a wealth effect would arise when using rice prices as an instrument, since the effect of rice prices on agricultural households' wealth is either positive or ambiguous.

Second, our fixed effects estimator will provide a robustness check since it is not subject to this misspecification bias.

Our instrumental variables specification is:

$$T_{i,t} = \mathrm{a} + \mathrm{b}Z_{i,t} + v_{i,t} \tag{3}$$

$$Y_{i,\,t+5} = \alpha_h + \beta\hat{T}_{i,t} + \gamma X_{i,t+5} + \varepsilon_{i,t+5}, \tag{4}$$

where in equation (4) we make the necessary two-stage least squares adjustments. We expect crop shocks to have differential impacts in poorer and richer households. To capture this effect we add to our list of instruments the interaction of our crop-shock instrument with log per capita household expenditure in 1992-93. This addition also improves the power of the instruments in the first stage. Since we are using more than one instrument, we will be able to use over-identifying restrictions to test the validity of the instrument set.

When comparing the two estimators, instrumental variables in principle address both potential sources of bias (between- and within-household selection), but are also potentially exposed to misspecification error if the instruments are invalid. Household fixed effects instead correct only for the first source of bias, but are less exposed to misspecification. We will present both sets of results below.

## 5. Results

### *5.1 OLS*

We begin by briefly discussing the OLS relationship between child labor and our outcomes. Though we do not believe that these estimates are causal, they are a useful reference point for our subsequent instrumental variables results. In looking at the first row of Table 2, we note that child labor in the first round is not significantly associated with any of the outcomes we examine. Nonetheless, with the exception of highest completed grade, the OLS results run in the direction we expect: more child labor is associated with lower enrolment, an increased likelihood of wage work, and a higher wage for those who in fact work. Both mother's and father's education are positively and significantly associated with enrolment, educational attainment, and lagging behind in terms of grade-for-age. A higher level of per capita household expenditure is associated with a higher enrollment probability, higher grade completed, and smaller lag in terms of grade-for-age. Somewhat surprisingly it is negatively associated with the probability of being engaged in wage work and earnings per day in wage work. Given the many selection problems with these results, we do not attempt to interpret them further.

### *5.2 Instruments: First Stage*

In Table 3 we present the first stage of our instrumental variables regression. Column (1) reports our basic specification, with crop disasters, rice prices, and crop disasters interacted with log per capita household expenditure as our instrument set. These instruments are, individually, highly significant (see also Edmonds and Pavcnik, 2003). A crop disaster is associated with an increased use of child labor, and rice prices with reduced child labor. Moreover, the increased use of child labor is significantly smaller among households with higher per capita expenditure. The instruments are jointly significant, with an F-statistic of 6.12 (a p-value of 0.0005).

In columns (2) and (3) we present two alternative specifications which we also use below. In column (2) we control for rice prices in 1997-98, because this increases the plausibility that rice prices in 1992-93 satisfy the exclusion restriction. The effect of the instruments is virtually unchanged in terms of magnitude and significance. Finally, we include additional community controls – distance to roads and the presence of a secondary school in the community – since these are potentially relevant for selection into education. The coefficients on the instruments are virtually unchanged, and the instrument set remains jointly significant with an F-statistic of 5.36 (p-value 0.001).

### *5.3 Instruments: Second Stage and Robustness*

In this section we present several versions of our basic instrumental variables estimator applied to the currently-enrolled-in-school outcome in 1997-98. In subsequent sections, we will examine a range of outcomes, while here we are interested in examining the robustness of our estimator to alternative specifications.

In Table 4 column (1) we present the results obtained using crop shocks, crop shocks interacted with household expenditure, and rice prices as instruments. The effect of child labor is negative, significant at the 1 percent level, and large in magnitude: relative to a mean level of attendance of 64 percent, a one standard deviation increase in child work hours (13 hours) leads to a nearly 50 percent decrease in attendance. In columns (2) to (4), we rotate the instruments, first using only rice prices, then only crop disasters, and finally just prices and crop disasters (dropping the interaction term). Overall our key result of interest is robust in magnitude across these specifications, except in column (3) where it is much larger. We do, however, lose precision in the estimates without the full set of instruments.

Given that we have more than one instrument, we can subject our set of instruments to a test of over-identifying restrictions. We see in the final row of Table 4 that our specification passes the test with a p-value of 0.24.

Another concern with crop disasters and rice prices as instruments is that, even though we measure these variables in 1992-93, they may capture long-run features of the communities (and hence not satisfy the exclusion restriction). We address this concern in two ways. First, in our regressions (below) we will also include a range of community controls. Second, in columns (5) and (6) we directly examine whether these instruments predict two community-level variables in 1997-98, namely crop disasters and the presence of schools. In both cases we see that the instruments do not significantly predict either variable. In the case of crop disasters this suggests that crop disasters are largely transitory over a five-year horizon. In the case of schools, it provides evidence that the instruments in 1992-93 do not directly predict an important community-level characteristic in 1997-98.

### *5.3 Main Results*

In Table 5, column (1), we present again our benchmark result for school attendance. Working as a child during the first survey round leads to a significantly lower level of school attendance five years later. A one standard deviation increase in hours worked leads to a nearly 50 percent reduction in the proportion of children attending school. Of course, school attendance in and of itself is not a complete measure of educational attainment: children may be in school but lag behind, or may not be in school because they have graduated. In columns (2) and (3) we consider highest grade completed (controlling for age) and a measure of years off track from grade-for-age. In column (2) we see that the effect is negative and significant at the five percent level, and in column (3) positive and significant at the five percent level. Children who worked in round one have a significantly lower level of educational attainment, and lag behind in terms of grade-for-age. The magnitudes are significant as well: a one standard deviation increase in child labor leads to a 35 percent decrease, relative to the mean, in educational attainment, and an even larger increase in years behind in terms of grade-for-age (66 percent).[7]

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[7] Results are similar when instead of working hours, total hours in both economic work and household chores are the measure of child labor in the regression. In the sample, children average six hours of chores

In columns (4) and (5) we examine the impact of child labor on occupational choice and earnings. Column (4) measures the proportion of respondents who are wage workers in the second round of the survey. The effect of child labor is positive and significant at the ten percent level: a one standard deviation increase in child labor hours in round one doubles the likelihood of being a wage worker in round two. The effect of child labor on earnings is also positive and significant at the 10 percent level (column 5). The magnitude of the coefficient is substantial: a one standard deviation increase in child labor is associated with a 180 percent increase in the daily earnings. This result is robust to controlling for age, both linearly and non-linearly.

Overall our results clearly point to a negative effect of child labor in terms of educational attainment, with an effect on the order of 50 percent. The effect is large. A child who works 15 hours a week instead of the average 7 will have a highest grade completed that is 1.1 years lower than the mean of 6.3 years. Since these are IV estimates, we interpret these effects causally.

It is interesting to note that IV estimates are larger than OLS. To the extent that families send to work the less academically gifted children, OLS would attribute to child labor a stronger impact on schooling whenever child ability is unobservable. In this case, we would expect IV coefficients to be smaller than OLS. However, when productivity in working in the field and academic ability are positively correlated, families might send to work the more productive children, who would also be those who are more likely to be successful in school. Myopic or financially constrained households would also follow this behavior when in need. In this case, OLS would underestimate the negative impact of child labor on schooling. Our results lend support to this second hypothesis.

We anticipate that children who engage in child labor might also benefit from increased work experience. When we measure this through the proportion of children who work and their wage, we find significant, positive effects five years later. Thus, the overall negative effect of child labor is offset in part by a positive effect on the probability of working for wages and on wage levels.

In Table 6 we examine the heterogeneity of the treatment effect at different levels of work intensity and by gender. In the upper panel of Table 6 the treatment is an indicator for having worked more than a given percentile of the child labor work hours distribution. In particular, we examine the effect at the median, at the 75$^{th}$ percentile, and at the 90$^{th}$ percentile. We see that the effect of having worked more than the median number of hours (namely zero hours) is not statistically significant, though still substantial in magnitude: highest grade attained is more than 6 years lower, and the effect on wage per day amounts to a 30 percent increase among those with positive earnings. The impact on educational outcomes of having worked more than the 75th percentile (more than 12 hours per week) is somewhat larger in magnitude, and is significant at the 10 percent level. Finally, when the treatment is defined as having worked more than the 90$^{th}$ percentile (28 hours per week), all but one of the treatment effects are significant. The magnitude of the effects is also larger. This suggests that much of the precision of our estimates comes from the upper end of the child labor work distribution.

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per week (ten for children who do chores). Interestingly, girls' chores average 1.5 hours more per week than boys - a statistically significant difference. Overall, children in the sample work 13 hours per week in both economic work (dominated by working on household farms) and in chores.

In the lower panel of Table 6 we split the sample by gender. For educational outcomes, the child labor effects are significant for girls but not for boys. However, it is worth noting that the magnitude of the effect for boys and girls is similar. Instead, for labor market outcomes we find that the effects are far greater in significance and magnitude for girls. Girls who worked when they were young (in 1992-93) are more likely to be wage workers and to be paid a higher wage in 1997-98. For wages per day, a one standard deviation increase in child labor hours (13 hour) is associated with earnings per day that are almost triple the mean level of wages for girls. For boys, the effect is on the order of 20 percent.

In Tables 7 and 8 we subject our main results to additional robustness checks. We first include controls for rice prices in 1997-98 (Table 7). This is potentially important because rice price in 1992-93 is one of our instruments. If rice prices affect child labor decisions in 1992-93, and if rice prices between regions are correlated over time, they could affect schooling and occupational decisions in 1997-98 directly, not only through child labor. By controlling for rice prices in 1997-98 we address this concern. In columns (1) to (5), we find that our results are stable both in magnitude and significance.

In column (6) we consider an alternative exercise. Rather than using wages in 1997-98 as the dependent variable (as in column (5)), we use wages in 1997-98 relative to rice prices in 1997-98. The concern with the specification in column (5) is that villages with higher rice prices in 1992-93 might just have a higher overall price level, which would automatically lead to a higher child wage rate. When we normalizing the child wage rate with contemporaneous rice prices, we confirm that children who have worked have higher wages in 1997-98, even relative to a measure of the overall price level. The effect is significant at the 10 percent level and comparable in magnitude to the effect in column (5). A final concern is that low rice price (high child labor) areas might have experienced a more rapid development of labor markets. If true, this could explain away our results for wage increases among children who worked in round one. To test for this, we use our base specification to estimate the effect of adult work in round one on adult earnings in round two. If the child wage result were simply due to a labor market effect, we would expect to find a similar result for adults. However, we do not find any such effect (column (7)).

In Table 8, we present household fixed effects results. Though these results do not correct for within-household selection, they correct for non-time-varying between-household selection and are not exposed to potential misspecification of the instruments. In Table 8 we see that the results are qualitatively similar to Tables 5 and 6. Child labor in round one has a negative and significant effect on school attendance, though the effect is much smaller in magnitude than Table 5. The educational attainment and year-to-grade results point in the same direction as Table 5, but again are smaller in magnitude and not significant. The results for wage work and wages are positive and significant, though smaller in magnitude than Table 5. As we discuss in Section 6, our main finding – that the returns to experience are higher than the returns to education – stand.

*5.4 Health Effects*

Taking a broader perspective of human capital, we extend the analysis by examining health outcomes. Beyond the intrinsic notion of the value of health for well-being, improved health status is widely recognized as leading to greater economic productivity (Strauss and Thomas, 1995). Moreover, health status can interact with school performance (see, for example, Glewwe *et al.*, 2001, and Alderman *et al*., 2001). The existence of a significant health effect could offset (or reinforce) a tradeoff between child labor and subsequent well-being; in particular, worsening health could offset some of the gains from increased labor market earnings that we have noted in Section 5.3. In this section we use the specifications from Section 5.3 (instrumental variables and household fixed effects) to examine the effect of child labor on subsequent health outcomes.

Health is a multidimensional concept and so, as with schooling, there is no single indicator of health. Here we employ two self-reported measures of health and a physical assessment (body-mass-index). For the former, we first examine an indicator for whether the individual had any illness in the previous four weeks, ranging from headaches and cough to fever, diarrhea, and infection. Adding depth to this indicator, the second health measure is the number of days the individual suffered from any of these illness in the last four weeks. Body mass index (BMI), an indicator of current nutritional status, is computed as weight in kilograms divided by squared height in meters. This measure has been found to be associated with physical functioning and to be positively related to productivity and earnings.

Our estimates are presented in Table 9. In the first row, we examine our instrumental variables estimators. Probability of illness is not associated with previous work, although the depth of illness (as indicated by the number of days ill) is increasing with more work. Growth is not statistically associated with work. In the second row, we see that the fixed effects estimates for self-reported illness are very small in magnitude and not statistically significant. Growth, as measured by change in log BMI, is increasing in prior work intensity, a finding contrary to expectation, assuming that work is detrimental for health.

Our findings differ from those reported by O' Donnell *et al.* (2003). In particular, the authors find that, in a bivariate probit setup, child labor is associated with higher chance of illness. A number of reasons can explain this difference. First, we measure child labor as the hours a child worked in the 7 days preceding the interview. O' Donnell *et al.* (2003) instead use an indicator taking value of 1 if the child worked at all in the previous 12 months. However, this difference is unlikely to fully account for the contrast in our results, as the two variables are highly correlated, with an overall correlation of 0.63. More importantly, we identify the effect of child labor on health only among the children who were in school in 1993. As discussed in other sections, this allows us to abstract from the issue that child labor affects schooling decision contemporaneously. Since schooling in survey round one might affect health in the following round, O' Donnell *et al.* (2003) might be estimating a child labor-cum-education effect, while we identify a pure child labor effect. Finally, consistently with the previous regressions, we use a more parsimonious set of regressors.

It should be noted that the range of health outcomes we observe is limited. Nonetheless, since the evidence is not significant and, where significant, mixed, we will set aside the health consequences of child labor in the next section, when we compare the economic costs and benefits of child labor.

## 6. Discussion

In this section we present a highly simplified calculation of the net economic cost of child labor. At the outset of this exercise, several caveats must be emphasized.

First, the estimates of the returns to education that we present in this section are fraught with identification issues. There are several challenges in identifying the returns to schooling. First, estimation is restricted to the wage labor force and excludes the large self-employed sector (for which earnings information is unavailable in the VLSS). Second, parents (and children) are presumably forward-looking in their schooling and child-labor decisions, whereas in our computations parents assume that their children's return to schooling will be similar to what they observe contemporaneously (i.e. in 1992-93). Third, the two dimensions of cost and benefit we measure – earnings foregone due to a reduced level of education and increased earnings due to labor market experience – are only two facets of a much more complicated issue. Both child labor and education can have other costs and benefits that our calculation does not take into account. Fourth, our comparison is confined to the five-year time window of our data set; in principle, the returns to schooling relative to child labor could increase over time. We proceed with these qualifications.

We take returns to schooling in Vietnam from the analysis by Moock *et al*. (2003) who estimate returns using the VLSS for 1992-93. They note that their analysis focuses on the early stages on transition when limited market reforms may not have resulted in increasing returns to schooling. They find that private returns to education are relatively low, which is consistent with the fact that salary reforms had yet to be introduced. An additional year of schooling is associated with a 5 percent increase in earnings. Males have a lower return (3 percent) than females (7 percent). These returns are lower than those observed in low-income economies but consistent with transition economies.

Based on Table 5 results, a one standard deviation (13 hour) increase in hours worked per week will lead to a decrease of slightly more than two years in educational attainment from the mean level of schooling attainment. This reduction in schooling in turn implies future earnings that are 11 percent lower. On the other hand, an increase in hours worked in 1992-93 is associated with an increase in earnings of more than double at the mean levels. Thus, a 13 hour increase in child labor leads to a substantial net increase in earnings per day five years later. Our fixed effects estimates from Table 8 indicate instead that the effect of hours of work on grade attainment by 1997-98 is not significant and is negligible (implying a reduction of one-fifth of a year of schooling). The corresponding wage decrease from this lower schooling is one percent whereas child labor is associated with a 41 percent increase in earnings. Again, the net effect is a very large increase in earnings relative to the mean.

Our basic finding is not overturned if we consider boys and girls separately, but it is much stronger for girls than for boys. Among boys, a 13 hour increase in earnings leads to a 2.1 year decrease in schooling and a 7 percent decrease in earnings. Earnings instead increase on the order of 20 percent with additional hours of child labor. Thus the net effect is still positive, but much smaller than the average. For girls, a 13 hour increase in hours worked amounts to an earning loss of 18 percent due to decreased schooling, whereas the returns to these hours through increased wages are a three-fold increase in earnings.[8] The positive net earnings for girls is driven both by participation and intensity: the higher probability of wage work and greater working hours for girls by 1997-98 among girls who worked in 1992-93.

It is also worth noting that our basic finding is not overturned even if we assume a much higher return to education, even for example returns on the order of 10 to 15 percent or higher. However, if the private costs of education are factored in, private returns to schooling might be even lower than the magnitudes used above. Moock *et al.* (2003) conclude that secondary education did not appear to be a good investment in terms of future wage earnings in 1992-93. Glewwe and Jacoby (1998) draw a similar conclusion with regards to agriculture where secondary schooling does not provide additional productivity gains.

## 7. Conclusion

Much attention has been devoted recently to the problem of child labor. While the moral distaste for child labor is beyond question, we feel – particularly in developing countries where most child labor is rural and is a relatively low-intensity activity – that it is an important empirical matter to determine whether in fact child labor has harmful consequences for children later in life. We view our work as a step in this direction.

We find that child labor significantly reduces school attainment. However, this negative effect is offset by an increased prevalence of wage work and increased earnings from wage work among those who worked as children. Indeed our results suggest that purely in terms of earnings, the loss due to reduced education is more than fully offset by increased labor market experience as a child: a one standard deviation increase in child labor (13 hour) leads to significant net increases in earnings in round two. We find no significant offsetting effects on health.

The conclusion we draw from these results is *not* that child labor is not a concern. Indeed, just given the many qualifications and caveats to our results, we believe this would be the wrong conclusion. Instead, we believe that our results show that in the medium run (i.e. over a five-year horizon as we have examined) there are important economic benefits to child labor that could offset its opportunity cost – lower school attainment. We anticipate that over a longer horizon the returns to education would increase, with more educated children having greater opportunities and experiencing increased wage growth, though this is an open question empirically (and the subject of future research).

Our findings provide a rationale for why we observe child labor and illustrate the fundamental difficulty in reducing its prevalence. The results also suggest potentially lower global returns to eliminating child labor than those found in the ILO report. Sending children to work has a positive return in the medium- and short-run, especially given the low returns to schooling in rural areas. Thus, reducing child labor will require parents both to be farsighted (i.e., to recognize that in the future there may be increased returns to schooling) and to be able to fund their children's education (either internally or

[8] One of the potential limitations of this calculation is that we assume a constant age profile of the returns to experience. In principle, if these age profiles are not flat, then over a longer horizon the returns to schooling could overtake the returns to experience. In fact, when we allow for a polynomial age profile in our specification, our results are similar.

with borrowing). This conclusion underscores one of the key aspects of the ILO (2003) report, namely that some kind of household-level transfers will be needed in order to lead to the voluntary elimination of child labor.

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**Table 1: Descriptive Statistics**

| | Full sample (children 8-13) | Children not working in 1992-93 | Children working in 1992-93 |
|---|---|---|---|
| Labor hours (wage + non-wage), 1992-93 | 7.19 | | 23.79 |
| | (12.71) | | (11.81) |
| Age, 1992-93 | 10.16 | 9.92 | 10.72 |
| | (1.62) | (1.57) | (1.57) |
| Male | 0.53 | 0.52 | 0.53 |
| | (0.50) | (0.50) | (0.50) |
| Father's education | 6.94 | 6.81 | 7.24 |
| | (3.92) | (4.00) | (3.73) |
| Mother's education | 5.47 | 5.32 | 5.84 |
| | (3.54) | (3.58) | (3.42) |
| LN per capita expenditure,1992-93 | 7.32 | 7.37 | 7.23 |
| | (0.44) | (0.45) | (0.41) |
| Rice price 1992-93 (/1000) | 1.76 | 1.75 | 1.78 |
| | (0.24) | (0.22) | (0.28) |
| Crop disaster 1992-93 | 0.78 | 0.78 | 0.77 |
| | (0.42) | (0.41) | (0.42) |
| In school, 1997-98 | 0.64 | 0.66 | 0.58 |
| | (0.48) | (0.47) | (0.49) |
| Highest grade attained, 1997-98 | 6.30 | 6.36 | 6.15 |
| | (3.52) | (3.38) | (3.83) |
| Years off-track from grade-for-age, 1997-98 | 3.01 | 2.70 | 3.70 |
| | (4.08) | (3.88) | (4.43) |
| Wage worker in last 7 days, 1997-98 | 0.06 | 0.06 | 0.06 |
| | (0.24) | (0.23) | (0.24) |
| Earnings per day, 1997-98 | 0.86 | 0.85 | 0.88 |
| | (4.11) | (4.04) | (4.26) |
| Any illness in last 4 weeks, 1998 | 0.276 | 0.273 | .281 |
| | (0.447) | (0.446) | (0.450) |
| Number of days ill in last 4 weeks, 1998 | 1.528 | 1.429 | 1.772 |
| | (3.476) | (3.535) | (4.185) |
| BMI, 1998 | 17.847 | 17.722 | 18.132 |
| | (1.961) | (1.983) | (1.881) |
| Number of observations | 2108 | 1471 | 637 |

Notes: Number of observations for BMI = 1,930, 1,347, and 592.

**Table 2: Outcomes in 1997-98, conditional on being in school in 1992-93: OLS**

| *Dependent variable:* | (1) In school | (2) Highest grade attained | (3) Years off-track from grade-for-age | (4) Wage worker in last 7 days | (5) Earnings per day |
|---|---|---|---|---|---|
| Labor hours 1992-93 | -0.001 | 0.001 | 0.000 | 0.001 | 0.014 |
| | (0.001) | (0.007) | (0.007) | (0.001) | (0.010) |
| Male | 0.093*** | 0.396*** | -0.376** | -0.003 | 0.105 |
| | (0.021) | (0.144) | (0.148) | (0.011) | (0.199) |
| Father's education | 0.011*** | 0.078*** | -0.080*** | -0.004** | -0.066** |
| | (0.003) | (0.025) | (0.025) | (0.002) | (0.032) |
| Mother's education | 0.021*** | 0.184*** | -0.183*** | 0.001 | 0.001 |
| | (0.004) | (0.029) | (0.030) | (0.002) | (0.034) |
| LN Per Cap Exp. 1992-93 | 0.127*** | 0.849*** | -0.791*** | -0.033** | -0.415* |
| | (0.030) | (0.206) | (0.208) | (0.013) | (0.245) |
| R-squared | 0.23 | 0.11 | 0.31 | 0.06 | 0.05 |

Notes: N=2,108. Standard errors are in parentheses. *** indicates significance at 1%; ** at 5%; and, * at 10%. Other regressors included, but omitted from the table, are age and indicator variables for missing parental education and region. Standard errors are clustered at the community level.

**Table 3: First Stage Estimates, Dependent variable: Labor Hours in 1992-93**

| | (1) | (2) | (3) |
|---|---|---|---|
| Rice price 1992-93 | -4.80** | -4.94** | -4.71** |
| | (1.94) | (2.03) | (2.07) |
| Disaster in last 12 months 1992-93 | 30.35*** | 30.86*** | 30.70*** |
| | (9.65) | (9.77) | (9.71) |
| Disaster in last 12 months x log per capita expenditure 1992-93 | -3.98*** | -4.06*** | -4.02*** |
| | (1.27) | (1.29) | (1.28) |
| Male | 0.05 | 0.06 | 0.07 |
| | (0.53) | (0.53) | (0.53) |
| Father's education | -0.18* | -0.18 | -0.18* |
| | (0.11) | (0.11) | (0.11) |
| Mother's education | -0.13 | -0.13 | -0.14 |
| | (0.14) | (0.14) | (0.14) |
| LN Per Cap Exp. 1992-93 | 1.60 | 1.64 | 1.67 |
| | (1.03) | (1.03) | (1.02) |
| Rice price 1997-98 | | 0.46 | 0.60 |
| | | (1.02) | (1.05) |
| Distance to major road 1997-98 | | | -0.12 |
| | | | (0.12) |
| Any upper secondary school in community 1997-98 | | | 0.34 |
| | | | (0.81) |
| F-test on instruments | 6.12 | 5.72 | 5.36 |

Notes: N=2,108. Standard errors are in parentheses. *** indicates significance at 1%; ** at 5%; and, * at 10%. Other regressors included, but omitted from the table, are age and indicator variables for missing parental education and region. Standard errors are clustered at the community level.

**Table 4: Checking the Robustness of the Instruments: Outcomes in 1997-98**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| *Dependent variable:* | In school | In school | In school | In school | Crop disaster | Upper secondary school in village |
| *Specification:* | IV | IV | IV | IV | OLS | OLS |
| *Instrument set:* | Rice price, crop shock, interaction | Rice price | Crop disaster | Rice price, crop shock | -- | -- |
| Labor hours 1992-93 | -0.024** | -0.028* | -0.176 | -0.034** | | |
| | (0.010) | (0.016) | (0.558) | (0.017) | | |
| Male | 0.094*** | 0.094*** | 0.103 | 0.094*** | -0.011 | -0.057** |
| | (0.025) | (0.026) | (0.100) | (0.028) | (0.014) | (0.023) |
| Father's education | 0.007 | 0.006 | -0.018 | 0.005 | 0.000 | 0.007 |
| | (0.005) | (0.005) | (0.098) | (0.006) | (0.003) | (0.005) |
| Mother's education | 0.018*** | 0.017*** | -0.002 | 0.016** | -0.005 | 0.002 |
| | (0.006) | (0.006) | (0.075) | (0.007) | (0.004) | (0.006) |
| LN Per Cap Exp. 1992-93 | 0.095** | 0.089** | -0.117 | 0.081* | -0.008 | -0.031 |
| | (0.037) | (0.044) | (0.798) | (0.046) | (0.028) | (0.045) |
| Rice price 1992-93 | | | | | 0.056 | 0.043 |
| | | | | | (0.107) | (0.159) |
| Crop disaster 1992-93 | | | | | -0.012 | -0.117 |
| | | | | | (0.056) | (0.090) |
| P-value of OIR test | 0.24 | -- | -- | 0.29 | -- | -- |

Notes: N=2,108. Standard errors are in parentheses. *** indicates significance at 1%; ** at 5%; and, * at 10%. Other regressors included, but omitted from the table, are age and indicator variables for missing parental education and region fixed effects. Standard errors are clustered at the community level.

**Table 5: Outcomes in 1997-98, conditional on being in school in 1992-93: IV**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| *Dependent variable:* | In school | Highest grade attained | Years off-track from grade-for-age | Wage worker in last 7 days | Earnings per day |
| Labor hours 1992-93 | -0.024** | -0.175** | 0.156** | 0.006* | 0.124* |
| | (0.010) | (0.072) | (0.073) | (0.0036) | (0.07) |
| Male | 0.094*** | 0.406** | -0.385** | -0.003 | 0.098 |
| | (0.025) | (0.163) | (0.162) | (0.011) | (0.208) |
| Father's education | 0.007 | 0.049 | -0.055* | -0.003* | -0.048 |
| | (0.005) | (0.032) | (0.031) | (0.002) | (0.032) |
| Mother's education | 0.018*** | 0.162*** | -0.163*** | 0.001 | 0.015 |
| | (0.006) | (0.042) | (0.042) | (0.002) | (0.039) |
| LN Per Cap Exp. 1992-93 | 0.095** | 0.604** | -0.575** | -0.026* | -0.262 |
| | (0.037) | (0.261) | (0.262) | (0.015) | (0.285) |
| *one sd of work relative to mean* | *-48%* | *-35%* | *66%* | *127%* | *183%* |
| P-value of OIR test | 0.24 | 0.89 | 0.05 | 0.32 | 0.26 |

Notes: N=2,108. Standard errors are in parentheses. *** indicates significance at 1%; ** at 5%; and, * at 10%. Other regressors included, but omitted from the table, are age and indicator variables for missing parental education and region fixed effects. Standard errors are clustered at the community level.

**Table 6: Heterogeneous Treatment Effect of Child Labor on Outcomes**

| *Treatment variable*: [each cell is a separate regression] | (1) In school | (2) Highest grade attained | (3) Years off-track from grade-for-age | (4) Wage worker | (5) Earnings per day |
|---|---|---|---|---|---|
| Full sample | | | | | |
| Worked more than median | -0.641 | -7.331 | 6.112 | 0.259 | 5.837 |
| (0 hours per week) | (0.582) | (5.180) | (4.964) | (0.286) | (5.677) |
| Worked more than 75$^{th}$ | -0.961* | -7.121* | 6.589* | 0.173 | 4.162 |
| percentile (12 hours per week) | (0.550) | (3.970) | (3.921) | (0.200) | (3.818) |
| Worked more than 90$^{th}$ | -1.001*** | -6.703*** | 6.056*** | 0.206 | 4.540* |
| percentile (28 hours per week) | (0.362) | (2.203) | (2.271) | (0.142) | (2.563) |
| Male sample | | | | | |
| Labor hours 1992-93 | -0.027 | -0.167 | 0.161 | 0.000 | 0.014 |
| | (0.018) | (0.131) | (0.135) | (0.007) | (0.115) |
| Female sample | | | | | |
| Labor hours 1992-93 | -0.023** | -0.206** | 0.178** | 0.010** | 0.199** |
| | (0.011) | (0.082) | (0.080) | (0.005) | (0.090) |

Notes: Each cell represents a separate regression of the outcome identified in the column on the treatment and subsample as defined in each row. Each regression also controls for age, mother's education, father's education, region fixed effects, and instruments for child labor using rice prices and crop shocks. Standard errors are in parentheses and are clustered at the community level. *** indicates significance at 1%; ** at 5%; and, * at 10%. Results are robust to controlling for availability of schools and roads at the village level. The magnitudes of the estimates are similar, though we lose precision in some cases.

**Table 7: Outcomes in 1997-98, conditional on being in school in 1992-93: IV with Rice Price Controls**

| *Dependent variable:* | (1) In school | (2) Highest grade attained | (3) Years off-track from grade-for-age | (4) Wage worker | (5) Earnings per day | (6) Wages per day / rice price 1997-98 | (7)[‡] Adult wages per day |
|---|---|---|---|---|---|---|---|
| Labor hours 1992-93 | -0.022** | -0.168** | 0.159** | 0.005 | 0.120 | 0.039* | -0.006 |
| | (0.010) | (0.072) | (0.074) | (0.004) | (0.078) | (0.023) | (0.006) |
| Male | 0.094*** | 0.407** | -0.384** | -0.003 | 0.098 | 0.024 | 0.132*** |
| | (0.024) | (0.161) | (0.163) | (0.011) | (0.208) | (0.062) | (0.027) |
| Father's education | 0.007 | 0.051 | -0.054* | -0.003* | -0.049 | -0.012 | 0.001 |
| | (0.005) | (0.032) | (0.032) | (0.002) | (0.031) | (0.009) | (0.002) |
| Mother's education | 0.018*** | 0.162*** | -0.163*** | 0.001 | 0.015 | 0.004 | -0.002 |
| | (0.006) | (0.041) | (0.042) | (0.002) | (0.039) | (0.012) | (0.007) |
| LN Per Cap Exp. 1992-93 | 0.097*** | 0.614** | -0.571** | -0.027* | -0.265 | -0.073 | 0.004 |
| | (0.036) | (0.255) | (0.261) | (0.015) | (0.288) | (0.085) | (0.014) |
| Rice price 1997-98 | 0.024 | 0.146 | 0.071 | -0.016 | -0.085 | | |
| | (0.043) | (0.271) | (0.284) | (0.019) | (0.304) | | |

Notes: N=2,108 for columns 1-6. N=7,139 for column 7. Standard errors are in parentheses. *** indicates significance at 1%; ** at 5%; and, * at 10%. Other regressors included, but omitted from the table, are age and indicator variables for missing parental education and region fixed effects. Standard errors are clustered at the community level. Results are robust to controlling for availability of schools and roads at the village level.[‡] Adult labor hours are used as regressors.

**Table 8: Outcomes in 1997-98, conditional on being in school in 1992-93: Household Fixed Effects**

| *Dependent variable:* | (1)<br>In school | (2)<br>Highest grade attained | (3)<br>Years off-track from grade-for-age | (4)<br>Wage worker | (5)<br>Wage worker |
|---|---|---|---|---|---|
| Labor hours 1992-93 | -0.004** | -0.014 | 0.019 | 0.001* | 0.028** |
| | (0.001) | (0.01) | (0.013) | (0.001) | (0.012) |
| Male | 0.117*** | 0.762*** | -0.689*** | 0.004 | 0.088 |
| | (0.028) | (0.240) | (0.244) | (0.014) | (0.227) |

Notes: N=2,108. Standard errors are in parentheses. *** indicates significance at 1%; ** at 5%; and, * at 10%. Age is included in the regressions but is omitted from the table.

**Table 9: Health Outcomes in 1997-98, conditional on being in school in 1992-93**

| | (1) | (2) | (3) |
|---|---|---|---|
| *Dependent variable:* | Any illness | Days ill | Growth |
| Instrumental variables | | | |
| Labor hours 1992-93 | 0.010 | 0.122* | 0.160 |
| | (0.009) | (0.073) | (0.158) |
| Household fixed effects | | | |
| Labor hours 1992-93 | 0.0004 | 0.006 | 0.086*** |
| | (0.001) | (0.013) | (0.029) |
| Observations | 2108 | 2108 | 1939 |

Standard errors are in parentheses. *** indicates significance at 1%; ** at 5%; and, * at 10%. in IV specifications: other regressors included, but omitted from the table, are age, gender, and indicator variables for missing parental education and region fixed effects; standard errors are clustered at the community level. FE specifications: other regressors included, but omitted from the table, are age and gender. In column 3, growth is measured as the change in natural logarithm of body mass index (BMI) controlling for lagged value of BMI.