THE EDUCATION, LABOR MARKET, AND HEALTH CONSEQUENCES OF CHILD LABOR*

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Although there is an extensive literature on the determinants of child labor and many initiatives aimed at combating it, there is limited evidence of the consequences of child labor on socio-economic outcomes such as education, wages, and health. We evaluate the causal effect of child labor participation on these outcomes using panel data from Vietnam and an instrumental variables strategy. Five years subsequent to child labor, we find significant negative impacts on school participation and educational attainment, but also find that those who worked as children have substantially higher earnings as (young) adults. We find no significant effects on health. Over a longer horizon, we estimate that from age 27 onward the forgone earnings attributable to lost schooling exceed any earnings gain; the net present discounted value of an incremental increase in child labor is positive for discount rates of 12 percent or higher. We provide evidence that child labor is prevalent among households that are likely to have higher borrowing costs, that are farther from schools, and whose adults experienced negative returns to their own education. This evidence suggests that reducing child labor will require both facilitating access to credit and convincing parents that their children will experience positive returns to education.

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1. Introduction

In this paper, we study the effect of child labor on schooling, labor market, and health outcomes. In particular, we ask whether having worked as a child reduces later school attendance and educational attainment, and affects occupational choice, wages, and health. We find that children who worked when they were young are significantly less likely to be attending school five years later, 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 that child labor leads to a greater probability of wage employment and to higher daily labor earnings which more than fully offset the foregone earnings attributable 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 which have existed historically in some urban settings in North America and Europe (see Basu, 1999). Furthermore, even if child labor does disrupt

schooling, it presumably also provides the child with labor market experience that subsequently could lead to increased wages. Which effect dominates is an empirical matter.

A growing empirical literature analyzes 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. Even within households, children's ability is unobserved to the econometrician but 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 based on selection. Furthermore, to our knowledge, this is the first paper simultaneously to examine education, labor market, and health outcomes within a causal framework.

We use an instrumental variables strategy that addresses some of the limitations of previous work. Using data from Vietnam, we instrument for participation in child labor by using community shocks and rice prices, two variables that influence child labor but 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 percent lower chance of being in school, a 35 percent 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 also have higher daily earnings from wage work. These 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, at least until early adulthood. Over a longer horizon, we find that the returns to education increase with age, whereas the returns to child labor experience decline monotonically; the net present discounted value of child labor is positive for households with a discount rate of 12 percent or higher.

The paper is organized as follows. Section 2 provides a review of the literature. 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 earnings from wage work. Section 7 concludes.

2. Literature Review

2.1 The Child Labor–Schooling Tradeoff

There is an extensive literature that examines the tradeoff between child labor and schooling. In this section, we highlight a few existing results.

Patrinos and Psacharopoulos (1995) show that factors that predict an increase in child labor also predict reduced school attendance and an increased chance of grade repetition. In a subsequent paper, the same 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.

All of these papers 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 the 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 effect 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, because it 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 only one eighth to one quarter of the

[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.

increased hours of schooling 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, but no significant effect emerges for adolescents (which is closer to the age range we examine). However, the authors do not address the endogenous choice to enter into child labor; thus, their findings cannot be interpreted causally.

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 selection biases that emerge in child labor studies. Although no identification strategy is perfect in an observational study, 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: whether the net effect of child labor is positive or negative.

2.2 The Returns to Schooling

In order to compare the effect of child labor on schooling with the effect on labor market outcomes, we require an estimate of the returns to schooling. A vast literature exists. Psacharopoulos and Patrinos (2002) summarize a range of studies that focus on individual wage earnings (i.e. excluding returns to education in self-employment or returns associated with labor contributions to family business and farms). They find, overall, 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 and 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.

For Vietnam, a recent paper by Moock et al. (2003) finds that an additional year of schooling is associated with a 5 percent increase in earnings. We will discuss how these findings relate to our evidence in Section 6.

2.3 Existing Research 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. O'Donnell *et al*. (2003) investigate the impact of child labor on health outcomes for children in Vietnam. Using instrumental variables estimation, they

find that work during childhood has a negative impact on health outcomes for as long as five years later. We discuss their results further in Section 5.6.

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. The 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 (VLSS), a household survey that was conducted in 1992-93 and again 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 were re-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 the presence of schools, roads, electricity, local rice prices, and the occurrence of disasters in the community. For this paper, we use 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 best to 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. In practice, controls for initial ability are usually not available, especially for

[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).

developing countries where child labor is more prevalent. In our work, we use a range of measures to capture various aspects of a child's human capital. First, we use a dichotomous variable for school attendance. School attendance is an input in the 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 (that is, working children attend school less regularly than non-working children) and appear to be better measures of time in school 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 two are output measures of the schooling process. In particular, because it tracks whether a child achieves appropriately within a grade, grade-for-age is close to a measure of the value added of schooling and, therefore, is particularly well suited 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 8 and 13 in our sample, 637 worked 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 per week, but among children who work is 24 hours per week. The gender distribution of working children is balanced. Interestingly,

[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.

there are no significant differences in the education of parents of working and non-working children. Households whose children do not work have a slightly higher level of per capita expenditure than those whose children do 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 community disasters (storms, floods, drought, pest attacks) in 1992-93. There is substantial variation in both rice prices and shocks in 1992-93. As noted in Benjamin and Brandt (2003) and Edmonds and Pavcnik (2003), the variation in rice prices in 1992-93 stems from the prohibition on the sale of rice across communities prior to 1997. Neither rice prices nor incidence of community 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 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 do not appear to be more likely to be working for a wage by 1997-98; however, with additional controls, we will find the difference to be significant.[4]

[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

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 defined as 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:

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

where X_i are household and community-level controls. We impose several restrictions on the sample that we examine. First, we consider children between the ages of 8 and13. The prevalence of labor among younger children is low. Likewise, by some definitions, labor at age 14 and above 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 also would have to include the in-school variable in equation (1) above, which then would 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).

Two potential sources of selection bias exist in estimating equation (1) using OLS: between-household selection (that is, which types of households opt into child

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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, since our child labor

labor) and within-household selection (that is, which of their children parents select to work more or less). To address the first, we control for a range of household characteristics including parental education and household expenditure in round one; of course, omitted household characteristics that determine participation in child labor and that affect educational choices remain a concern.

It is inherently more difficult to control for within-household differences among children, since our dataset does not include child-level ability measures. We are able to address both sources of bias using an instrumental variables strategy. Our instrumental variables specification is:

$$T_{i,t} = a + bZ_{i,t} + \nu_{i,t} \quad (3)$$

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

where in equation (4) we make the necessary two-stage least squares adjustments. We expect shocks to have differential impacts in poorer and richer households. To capture this effect, we add the interaction of our community-shock instrument with log per capita household expenditure in 1992-93 to our list of instruments. This 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.

The ideal instrument induces variation in child labor that only affects the outcome of interest (e.g., schooling) through the child labor participation decision. We consider two instruments: community disasters and rice prices (both measured at the community level in the first survey round). The relevance of community shocks to child labor is

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treatment occurs in the first survey round this is not a concern in our framework.

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 hence increase the demand for child labor. Higher rice prices also would 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 to affect it only through the child labor decision in round one. Since both instruments are measured at the community level, they are both exogenous with respect to household decisions. Regarding the exclusion restriction, community shocks are not significantly correlated across survey rounds (a correlation of 0.01, not significant at standard levels); 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, then 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.

[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.

However, there are mechanisms other than child labor (essentially omitted variables) through which our set of instruments could affect the outcomes of interest, for example nutrition (see, for example, Strauss, 1986), wealth, or (unobserved) household concern for children's welfare. We have two strategies for addressing this concern. First, if our instruments proxy for omitted variables, then we should be able to find an effect of these instruments on other child-related outcomes, such as health; we examine these effects below. Second, we use a fixed-effects estimator as a robustness check, because it is not subject to this misspecification bias:

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

When comparing the two estimators, in principle, instrumental variables address both potential sources of bias (between- and within-household selection), but also are 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 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. Although 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 enrollment, an increased likelihood of wage work, and higher daily earnings in wage work (where those not working for wages are coded with zero earnings). Both mother's and father's education are positively and significantly associated with enrollment, 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: Relevance and Exclusion

In Table 3 we present the first stage of our instrumental variables regression. Column (1) reports our basic specification, with community disasters, rice prices, and community 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 community disaster is associated with an increased use of child labor, and rice prices are associated 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 9.07 (a p-value of less than 0.00005).

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 either magnitude or significance. Finally, we include additional community controls – distance to roads and the presence of a secondary school in the community – because 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 9.23 (a p-value of less than 0.00005).

Having established that the instruments we use have power in the first-stage, we next consider whether they satisfy the exclusion restriction. In particular, our concern is that the instruments may be correlated with an omitted variable. For example, community shocks could reduce household wealth and consequently also belong in the second-stage regression. Likewise, rice prices are related to agricultural production, which in turn could be correlated with community attitudes toward child labor. Although it is not possible to test directly whether the instruments are correlated with attitudes toward child labor, we can examine their correlation with a range of related variables that are observed.

In Table 4 column (1), we examine whether rice prices and community disasters in the first survey round predict the future occurrence of shocks; Morduch (1994) has suggested that vulnerable households might be more likely to be shocked. Neither instrument is significant. In column (2) we examine whether the instruments are correlated to the presence of secondary schools within communities – which may reflect a preference for education – and find no significant effect. In column (3), there is no evidence that the value of durable assets (a measure of wealth) in the second survey round is correlated with occurrence of community disasters (and rice prices) in the first

survey round. This suggests that, for example, correlation between community disasters and household wealth should not explain away our results regarding the effect of child labor on schooling. In columns (4) and (5) we confirm that the instruments are not correlated with the incidence of illness among children in the last month or last12 months. In particular, if rice prices were correlated with community-level attitudes toward children's welfare then we might expect to find not only a greater use of child labor but also worse health. We do not find evidence of this. Overall, these results lend credence to the use of rice prices and community disasters as instruments for child labor.

5.3 Instruments: 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 5 column (1) we present the results obtained using community shocks, community 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 (13 hour) increase in child work 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 community disasters, and finally just prices and community 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 5 that our specification passes the test with a p-value of 0.24.

5.4 Main Results

In Table 6, column (1), we again present 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 5 percent level, and in column (3) it is positive and significant at the 5 percent level. Children who worked in the baseline survey 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]

[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 10 percent level: a one standard deviation increase in child labor hours in the baseline survey doubles the likelihood of being a wage worker in the second survey round. The effect of child labor on earnings is also positive and significant at the 10 percent level (column 5). A concern with this result is that some of the children in the second survey round are still in school. In that sense, the earnings result captures both the reduction in school attendance and an increased wage rate. In column (6) we address this by focusing on individuals age 17 and older, who are unlikely still to be attending school. Even among this group, we find a large and significant effect of child labor. 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.

It is interesting to note that the IV estimates are larger than the OLS estimates. To the extent that families send the less academically gifted children to work (and child ability is unobservable), OLS should overestimate the impact of child labor on schooling relative to the causal effect (as estimated by IV). Our results instead lend support to the view that families send their more academically gifted children to work (possibly because they are also more productive).

In Table 7 we examine the heterogeneity of the treatment effect at different levels of work intensity and by gender. In the upper panel of Table 7, the treatment is an

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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

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 75th percentile, and at the 90th percentile. We see that the effect of having worked more than the median number of hours (namely zero hours) is not statistically significant, although substantial in magnitude: the 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 of having worked more than the 75th percentile (more than 12 hours per week) on educational outcomes 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 90th 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.

In the lower panel of Table 7, we split the sample by gender. For educational outcomes, the effect of child labor is significant for girls but not for boys, but the magnitude of the effects 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 have higher earnings from wage employment in 1997-98. For earnings 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 earnings for girls. For boys, the effect is on the order of 20 percent.

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both economic work (dominated by working on household farms) and in chores.

5.5 Robustness of the Results and Instruments

The causal interpretation of the results presented in the previous section relies on the validity of the instruments. In this section, we explore – and try to rule out – a range of arguments against our instruments.

One concern with rice prices as an instrument is that villages with higher rice prices in 1992-93 might simply have a higher overall price level, which would automatically lead to higher child earnings from wage work. We confirm in Table 8, column (1), that children who have worked in 1992-93 have higher earnings in 1997-98, even when earnings are normalized by rice prices. The effect is significant at the 10 percent level and comparable in magnitude to previous results.

Another problem is that one source of variation in rice prices is that southern Vietnam is a rice-growing (and rice surplus) region, whereas Northern Vietnam is a rice deficit region. In 1992-93, there were severe restrictions to trade across regions, which led to lower rice prices in the south than the north. This leads to two concerns. First, if low rice price (high child labor) areas experienced more rapid development of their labor markets, then this could explain away our results for wage increases among children who were working at the time of the baseline survey. To test for this, we use our base specification to estimate the effect of adult work in the baseline survey on adult earnings in the follow-up survey five years later. If the child wage result were simply due to a labor market effect, then we would expect to find a similar result for adults. However, we do not find any such effect (column (2)). Second, north and south could differ in their average levels of education and use of child labor. We test for this by restricting our sample to communities in the north. These results are presented in columns (3) and (4)

for selected outcomes, and are similar in sign, magnitude, and significance to our base results.

More generally, we are concerned that the instruments may not be excluded from the outcome equations. We address this concern in several ways. For rice prices, the concern is that if rice prices affect child labor decisions in 1992-93 and are correlated over time, they could affect schooling and occupational decisions in 1997-98 directly, not only through child labor. We address this concern by controlling for rice prices in 1997-98 in columns (5) and (6); our results are stable both in magnitude and significance. For agricultural shocks, it is worth noting that these shocks occur in the 12 months prior to the first interview. Thus, when we control for household consumption in the baseline survey, we are controlling for the wealth effects that could result from the shocks. Furthermore, in Table 4, we demonstrated that household wealth in the second survey round (as measured by durable assets) is not significantly affected by the occurrence of shocks in the first round. This increases the plausibility of the view that the shocks are excluded from the outcome equation in the second round.

As a final robustness check, in Table 9, we present household fixed effects results. Although these results do not correct for within-household selection, they do correct for non-time-varying between-household selection and are not exposed to potential misspecification of the instruments. In Table 9, we see that the results are qualitatively similar to those in Tables 5 and 6. Child labor has a negative and significant effect on school attendance, although the effect is smaller in magnitude than in Table 6. The educational attainment and year-to-grade results point in the same direction as Table 6, but again are smaller in magnitude. The results for wage work and wages are positive and

significant, although smaller in magnitude than Table 6. But as we discuss in Section 6, our main finding – that the returns to experience are higher than the returns to education – still stand.

5.6 Health Effects

Taking a broader perspective of human capital, we extend the analysis by examining health outcomes. Beyond the intrinsic notion of the importance of health for well-being, improved health status is widely recognized to lead 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, worse health could offset some of the gains from increased labor market earnings that were noted in Section 5.3. In this section, we examine the effect of child labor on subsequent health outcomes.

Health is a multidimensional concept. As with schooling, there is no single indicator of health. We use two self-reported measures and a physical assessment. For the former, we first examine an indicator of whether the individual had any illness in the last 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.

We present our estimates in Table 10. Column (1) shows that the probability of illness is not significantly associated with previous work. In column (2) the depth of illness (as indicated by the number of days ill) is not significantly associated with previous work either. Finally, column (3) shows that growth is not significantly associated with work.

Our findings differ from those reported by O' Donnell *et al.* (2003). In particular, in a bivariate probit setup, they find that child labor is associated, five years later, with a higher likelihood of a recent illness. A number of factors can explain this difference. First, we measure the intensity of child labor (hours a child worked in the seven days preceding the interview). O' Donnell *et al.* (2003) instead use an indicator for any child labor in the previous 12 months. However, this difference is unlikely to account for the contrast in our results, because the two variables are highly correlated, with an overall correlation of 0.63. More importantly, our results identify the effect of child labor on health only among children who were in school in 1993. As discussed above, this allows us to abstract from the issue that child labor can affect contemporaneous schooling decisions. In turn, schooling might affect health in the following survey round, in which case O' Donnell *et al.* (2003) are estimating a child labor-cum-education effect while we identify a pure child labor effect.

It should be noted that the range of health outcomes we observe is limited. Nonetheless, because the evidence is not significant, 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 and Extensions

6.1 The Net Cost of Child Labor: Static Analysis

In this section we present a highly simplified calculation of the net economic cost of child labor. We compare the cost of child labor via foregone schooling with the direct benefits in terms of earnings five years subsequent to the child labor in question.

Several caveats should be emphasized. First, parents (and children) presumably are forward-looking in their schooling and child-labor decisions. But our returns to schooling estimates are based on the contemporaneous experience of parents, which may not be relevant in evaluating the value of education for children in a transition economy. Second, both the returns to education and the direct benefits of child labor experience may vary over the lifecycle. In this section, we present a static comparison at the five year horizon. In Section 6.2, we use adults' labor market experience to extrapolate the costs and benefits of child labor over a longer horizon. Third, although we found no systematic evidence of any health costs of child labor in Section 5.6, such costs may exist along other dimensions.

Finally, there are several challenges in identifying the returns to schooling. In Section 6.2 we will use data on adults in the baseline survey to estimate dynamic profiles of returns to schooling and experience. In this section we instead use estimates for Vietnam from the literature, namely Moock *et al.* (2003). 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. 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 the results in Table 6, a one standard deviation (13 hour) increase in hours worked per week will lead to a decrease in educational attainment of slightly more than two years. This reduction in schooling in turn implies future earnings that are 11 percent lower relative to the mean. On the other hand, an increase in hours worked in 1992-93 is associated with an increase in earnings that is more than double relative to the mean. Thus, an increase in child labor leads five years later to a substantial net increase in earnings per day. We reach the same conclusion based on our fixed effects estimates in Table 9, although the magnitudes are smaller. A 13 hour increase in child labor implies a one-fifth of a year reduction in schooling. The corresponding wage decrease is one percent relative to mean earnings, 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 child labor leads to a 2.1 year decrease in schooling and a 7 percent decrease in earnings associated with this reduced schooling. Earnings instead increase on the order of 20 percent with 13 additional hours of child labor. Thus the net effect is still positive, but much smaller than the average. For girls, the loss in earnings from a 13 hour increase in hours worked is 18 percent from decreased schooling, whereas the returns to these hours through higher wages correspond to a three-fold increase in earnings. The positive net earnings for girls

are driven by both participation and intensity: a higher probability of wage work and greater working hours for girls in 1997-98 among those who worked in 1992-93.

It is notable that our key finding would not be overturned if we assume a much higher return to education, even returns on the order of 10 to 15 percent or higher for example. However, if the private costs of education are factored in, then the private returns to schooling are lower than the returns 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.

6.2 The Dynamic Returns to Schooling and Experience

In the previous section we examined the costs and benefits of child labor at a five year horizon. In this section, we estimate the age profile of the returns to schooling and experience, and examine the net benefit of child labor over a longer time horizon. To do this, we assume: either that as the children in the sample age they will face an age profile of returns to schooling and experience similar to the current adults in the sample; or that children use their parents' experience in the labor market as a guide in making their schooling and child labor decisions.

We begin by estimating the age profile of the returns to schooling and experience. Table 11 presents the estimates. We regress earnings per day on education, age and age squared, and the interaction of age and education. In column (1), we present our OLS results. We find that the returns to education start out negative; for example, at age 20, an additional year of schooling leads to earnings that are 15 percent lower relative to the

mean. At age 29, returns are close to zero. They reach a maximum of 15 percent at age 50. The effect of experience (age) instead decreases with age, reaching a minimum at age 64. In column (2), we present estimates that control for household fixed effects. The results are qualitatively similar. The returns to education reach a maximum at age 47, when they are on the order of 20 percent, and the effects of experience reach a minimum at age 60.

We use these estimates to generate an age profile of the returns to child labor as follows. We assume that the age profile is the same for those currently 18 and younger as for those 18 and older. For education, we use the returns to education in column (2) to compute the percentage earnings return of one less year of education (which is the loss in schooling from seven additional hours of child labor per week). We then apply this age profile to the children in our sample. Similarly, we apply the age profile of the percentage returns to experience to children's returns to child labor experience later in life.[8] Figure 1 plots the results. Initially, the returns to education are negative, so earnings in fact increase for a year or two following the loss in education. But after age 20, there is an earnings loss that reaches a maximum at age 44. The returns to experience start out positive – corresponding to the returns to child labor experience discussed in Section 6.1 – but decline steeply with age. We see that the net returns to child labor are negative from age 26 on. In present discount value terms, the net benefits of child labor are positive for discount rates of 11.8 percent or higher.

In terms of the range of interest rates available to rural households in Vietnam, 11.8 percent is on the low end of the spectrum. Microcredit organizations seem to offer

[8] Note that this assumes that the age profile of the returns to child labor experience are the same, in percentage terms, as the returns to standard labor market experience.

interest rates between 12 and 14 percent per annum, which are below market compared to other sources of informal credit.[9]

Figure 1 illustrates one of the key tradeoffs of child labor. Although the returns to education eventually dominate any gains from child labor experience, the payoff to education is realized only 10 or more years after the earnings benefits of child labor.

6.3 Why Do We Observe Child Labor?

In this section we try to explain features of the selection process that lead some households to choose to send their children to work but not others. In particular, in the previous two sections we argued that the returns to child labor are substantially positive at a five-year horizon. This naturally leads to the question: why do some households choose to send their children to work and not others? We consider a range of theories.

First, we consider the most obvious theory based on the results above: that households who face a lower discount rate are less likely to resort to child labor. We use household wealth (log per capita durable assets and household expenditures) as a proxy for a household's ability to borrow (for example, see Beegle, Dehejia, and Gatti (2003), Jacoby (1994), and Zeldes (1989)). Second, we consider a behavioral theory: that parents use their own experience (i.e., returns to education[10]) in the labor market to decide

[9] For example, the Vietnam Bank for the Poor charges an interest rate of 1.2 percent per month (see http://www.gdrc.org/icm/country/viet-vbp.html). The Vietnam Bank for Agriculture charges an interest rate of 1.75 percent per month (see http://www.gdrc.org/icm/viet-dat_tran.html). The World Bank's Mobile Banking Program charges an interest rate of 12 percent per annum (see http://www.imf.org/external/pubs/ft/fandd/2004/06/pdf/hung.pdf). The Vietnames-Beligian Credit Project provides loans at 12 percent per annum (see http://www2.btcctb.org/vietnam/micro-finance.htm).

[10] We estimate the returns to schooling for adult members of households who have children in wave one. These estimates measure the correlation between adults' education and earnings, not necessarily the causal effect. This is appropriate if adults use their own experience of the returns to education, without disentangling what portion of their returns to schooling is causal and what portion is due to factors such as ability.

whether to send their children to work.[11] Third, we examine the effect of landowning, on the view that landowning households could have a greater demand for child labor. Fourth, we examine whether households for whom it is more costly to send their children to school are more likely to use child labor; we use distance from the nearest school as a proxy for this cost.

The results are presented in Table 12. In column (1), we show that households in which the adults experienced negative returns to education are significantly (8.9 percent) more likely to use child labor; this effect is significant at the one percent level. We find that child labor increases significantly with household size. Household wealth has a negative effect of on child labor, significant at the one percent level. A one standard deviation increase in per capita household wealth leads to a 3.5 percentage point reduction in the likelihood of child labor. In column (2), we introduce an additional measure of household wealth, log per capita expenditure. Since the two measures of household wealth are highly correlated (a correlation coefficient of 0.7), it is not surprising that the coefficient on durables is no longer significant and that the other coefficients remain robust in magnitude and significance. In subsequent specifications, we control for only the first of these two wealth variables. In column (3) we add an indicator for landowning households. Landownership is related to wealth, but is also potentially correlated with the demand for child labor among agricultural households (see Bhalotra and Heady, 2001). The latter seems to dominate: landowning households are 15

[11] Ray (1998) makes a similar point in the context of mortality and fertility. It may take several generations before the sharp and unprecedented decline in mortality is incorporated into optimal fertility decisions. In a similar sense, though rates of returns to education may be increasing in as a country develops, parents may still use their own experience in making schooling decisions for their children. Picketty (1995) shows – in a different setting – that it can be rational to use one's own experience as a basis to form expectations of the future.

percent more likely to use child labor. Finally, while distance to primary school is not associated with child labor, households that are further from secondary schools are more likely to use child labor. In column (5), a one standard deviation (5 km) increase in the distance to the nearest secondary school makes child labor 2 percentage points more likely.

Overall, our results in Table 12 suggest that behavioral factors – adults' own experience in the labor market – and landownership are the most important factors in accounting for child labor. But household wealth and the cost of sending children to school also play significant roles.

To the extent these factors account for selection into child labor, we should control for them in our previous regressions. However, because our instrumental variables strategy is based on community rice prices and agricultural shocks, and the variables we have examined in this section do not to vary systematically with our instruments, our previous results remain valid.[12]

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 important to determine empirically whether child labor in fact has harmful consequences for children later in life. We view our work as a step in this direction.

[12] We confirm this by including the variables from this section as additional controls in our instrumental variables specifications. Results available upon request.

We find that child labor significantly reduces school attainment. However, this negative effect is offset by 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 (13 hour) increase in child labor leads to a significant net increase in earnings five years later. We find no significant offsetting effects on health.

The conclusion we draw from these results is *not* that child labor is beneficial. Indeed, given the many qualifications and caveats to our results, we believe this would be inappropriate. Rather, we believe that our results show that in the medium run (i.e. over a five to ten year horizon) there are important economic benefits to child labor that offset its opportunity cost (lower school attainment). However, over a longer horizon the returns to education increase, with more educated individuals experiencing increased wage growth and the returns to experience declining. Thus, our findings provide a rationale for why child labor exists and illustrate the fundamental difficulty in reducing its prevalence.

Reducing child labor will require parents both to be farsighted (i.e., to recognize the future returns to schooling) and to be able to engage in such costly investments (i.e., either through internal funding or borrowing). This conclusion underscores one of the key aspects of the ILO (2003) child labor 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)
Community disaster in last 12 months 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	0.0002	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 and are clustered at the community level. *** 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.

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)
Community disaster 1992-93	30.35***	30.86***	30.70***
	(9.65)	(9.77)	(9.71)
Community disaster 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	9.07	9.23	8.57

Notes: N=2,108. Standard errors are in parentheses and are clustered at the community level. *** 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.

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

Dependent variable:	(1) Community disaster 1997-98	(2) Upper secondary school in village 1997-98	(3) Value of durable assets 1997-98	(4) Ill in the last month 1992-93	(5) Ill in the last 12 months 1992-93
Rice price 1992-93	0.057	0.044	-200	-0.073	-0.049
	(0.11)	(0.16)	(228)	(0.054)	(0.068)
Community disaster 1992-93	0.49	0.73	15974	0.48	0.13
	(0.52)	(0.73)	(12514)	(0.34)	(0.43)

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

Table 5: Robustness of the Instruments: Labor Hours in 1997-98

	(1)	(2)	(3)	(4)
Specification:	IV	IV	IV	IV
Instrument set:	Rice price, community disaster, interaction	Rice price	Community disaster	Rice price, community disaster
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***
	(0.025)	(0.026)	(0.100)	(0.028)
Father's education	0.007	0.006	-0.018	0.005
	(0.005)	(0.005)	(0.098)	(0.006)
Mother's education	0.018***	0.017***	-0.002	0.016**
	(0.006)	(0.006)	(0.075)	(0.007)
LN Per Cap Exp. 1992-93	0.095**	0.089**	-0.117	0.081*
	(0.037)	(0.044)	(0.798)	(0.046)
P-value of OIR test	0.24	--	--	0.29

Notes: N=2,108. Standard errors are in parentheses and are clustered at the community level. *** 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.

Table 6: Outcomes in 1997-98, conditional on being in school in 1992-93: IV

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	(6) Earnings per day, ages 17-19 in 1997-98
Labor hours 1992-93	-0.024**	-0.175**	0.156**	0.006*	0.124*	0.22*
	(0.010)	(0.072)	(0.073)	(0.0036)	(0.07)	(0.13)
Male	0.094***	0.406**	-0.385**	-0.003	0.098	0.28
	(0.025)	(0.163)	(0.162)	(0.011)	(0.208)	(0.54)
Father's education	0.007	0.049	-0.055*	-0.003*	-0.048	-0.061
	(0.005)	(0.032)	(0.031)	(0.002)	(0.032)	(0.084)
Mother's education	0.018***	0.162***	-0.163***	0.001	0.015	-0.049
	(0.006)	(0.042)	(0.042)	(0.002)	(0.039)	(0.10)
LN Per Cap Exp. 1992-93	0.095**	0.604**	-0.575**	-0.026*	-0.262	0.83
	(0.037)	(0.261)	(0.262)	(0.015)	(0.285)	(0.88)
one sd of work relative to mean	*-48%*	*-35%*	*66%*	*127%*	*183%*	*191%*
P-value of OIR test	0.24	0.89	0.05	0.32	0.26	

Notes: N=2,108 for columns 1-5 and 535 for column 6. Standard errors are in parentheses and are clustered at the community level. *** 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.

Table 7: Heterogeneous Treatment Effect of Child Labor on Outcomes

Dependent 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 in last 7 days	(5) Earnings per day
Full sample					
Worked more than median (0 hours per week)	-0.641	-7.331	6.112	0.259	5.837
	(0.582)	(5.180)	(4.964)	(0.286)	(5.677)
Worked more than 75th percentile (12 hours per week)	-0.961*	-7.121*	6.589*	0.173	4.162
	(0.550)	(3.970)	(3.921)	(0.200)	(3.818)
Worked more than 90th percentile (28 hours per week)	-1.001***	-6.703***	6.056***	0.206	4.540*
	(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 community 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 8: Outcomes in 1997-98, conditional on being in school in 1992-93: IV with Rice Price Controls

Dependent variable:	(1) Earnings per day / rice price	(2)‡ Adult earnings per day	(3) Highest grade attain- ed, Northern VN	(4) Earnings per day, Northern VN	(5) Highest grade attained	(6) Earnings per day
Labor hours 1992-93	0.039*	-0.006	-0.13**	0.10*	-0.168**	0.120
	(0.023)	(0.006)	(0.064)	(0.061)	(0.072)	(0.078)
Male	0.024	0.132***	0.47**	0.008	0.407**	0.098
	(0.062)	(0.027)	(0.22)	(0.17)	(0.161)	(0.208)
Father's education	-0.012	0.001	0.066*	-0.019	0.051	-0.049
	(0.009)	(0.002)	(0.034)	(0.033)	(0.032)	(0.031)
Mother's education	0.004	-0.002	0.14***	0.042	0.162***	0.015
	(0.012)	(0.007)	(0.048)	(0.041)	(0.041)	(0.039)
LN Per Cap Exp. 1992-93	-0.073	0.004	0.87**	0.095	0.614**	-0.265
	(0.085)	(0.014)	(0.39)	(0.068)	(0.255)	(0.288)
Rice price 1997-98					0.146	-0.085
					(0.271)	(0.304)
Number of observations	2,108	7,139	1,121	1,121	2,108	2,108

Notes: Standard errors are in parentheses and are clustered at the community level. *** 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. Results are robust to controlling for availability of schools and roads at the village level.‡ Own labor hours from the sample of adults are used as regressors.

Table 9: Outcomes in 1997-98, conditional on being in school in 1992-93: Household Fixed Effects

Dependent variable:	(1) In school	(2) Highest grade attained	(3) Years off-track from grade-for-age	(4) Wage worker	(5) Earnings per day
Labor hours 1992-93	-0.004**	-0.014	0.019*	0.001*	0.028**
	(0.001)	(0.01)	(0.012)	(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 10: Health Outcomes in 1997-98, conditional on being in school in 1992-93: IV

Dependent variable:	(1) Any illness	(2) Days ill	(3) Growth
Labor hours 1992-93	0.010	0.122	0.160
	(0.009)	(0.073)	(0.158)
Observations	2108	2108	1939

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

Table 11: Returns to Education and Experience Among Adults in 1992-93

Dependent variable:	(1) Earnings per day	(2) Earnings per day
Education	-1.52***	-1.09*
	(0.47)	(0.67)
Education · Age	0.074***	0.070**
	(0.022)	(0.033)
Education · Age^2 (/1000)	-0.741	-0.791
	(0.255)	(0.384)
Age	-0.32**	-0.24
	(0.15)	(0.20)
Age^2	0.002*	0.002
	(0.001)	(0.002)
Household fixed effects	No	Yes
Observations	8639	8639

Notes: OLS estimates. Standard errors are in parentheses. *** indicates significance at 1%; ** at 5%; and, * at 10%.

Table 12: Probability of Child Labor in the Household in 1992-93: Impact of Adult Characteristics

	(1)	(2)	(3)	(4)	(5)
Returns to education negative‡	0.089***	0.081***	0.071***	0.114***	0.068***
	(0.016)	(0.016)	(0.018)	(0.022)	(0.020)
Household size	0.039***	0.037***	0.048***	0.035***	0.048***
	(0.004)	(0.004)	(0.004)	(0.006)	(0.005)
Log per capita value of durable goods	-0.025***	-0.006	-0.012*	-0.023***	-0.009
	(0.006)	(0.007)	(0.007)	(0.008)	(0.007)
Log per capita expenditure		-0.086***			
		(0.022)			
Household owns land			0.150***	0.218***	0.137***
			(0.026)	(0.032)	(0.041)
Distance to primary school attended (kms)				0.001	
				(0.002)	
Distance to secondary school nearest community (kms)					0.004**
					(0.002)
Observations	2793	2793	2421	1802	2196

Notes: Probit estimates for households with any children under 14 in 1992-93. Standard errors are in parentheses. *** indicates significance at 1%; ** at 5%; and, * at 10%.

‡ This variable is an indicator for households in which the average adult returns to education are negative. Individual returns to education are estimated using the sample of adults over 20 years by regressing earnings per day on completed education, age, age interacted with education, community dummies, and community dummies interacted with education.

Figure 1: Age Profile of Returns to Education and Returns to Experience