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ABSTRACT

Are children in poor agrarian households withdrawn from school in periods of low income? Using panel data from rural India, we test whether households adjust child school attendance in each agricultural season in response to both anticipated and unanticipated changes in income, conditional on changes in child wages. We find that while school attendance responds strongly and as predicted to seasonal income and wage movements, village-wide shocks have larger effects than idiosyncratic shocks. We also find evidence of limitations on ex-ante insurance against idiosyncratic risk for small farmers, but no empirical support for borrowing constraints across seasons.

I. INTRODUCTION

High income variability is a pervasive feature of agrarian economies. In the rain-fed agriculture of India, for example, not only are crop yields subject to substantial risk due to the erratic timing and intensity of the monsoon, but cropping patterns are also highly seasonal. The question we address in this paper is whether children in Indian villages are pulled in and out of school in response to these income fluctuations. A number of studies, most notably Townsend (1991), examine whether rural financial markets or other institutions are capable absorbing income risk. While Townsend tests the implications of complete markets for consumption behavior,[1] other researchers focus on specific forms of self-insurance, such as the selling off of durable production assets in bad times (e.g., Rosenzweig and Wolpin, 1989). This paper pursues the latter line of inquiry.

Human capital investment requires a child time input that is costly to the household in terms of forgone earnings or home production. Thus, when a household lacking other forms of insurance experiences an income shortfall (holding child productivity constant), it may withdraw its child from school. Sensitivity of school attendance to changes in household income raises important policy issues, suggesting that programs designed to stabilize rural incomes, and financial development in general, might indirectly enhance school attainment. But previous studies of school attendance or enrollment in developing countries, all of which analyze cross-sectional data, are incapable of shedding light on this question.[2] This paper is the first to use

[1]Tests of the complete markets hypothesis using U.S. consumption data have been conducted by Altonji, Hayashi and Kotlikoff (1991), Altug and Miller (1990), Cochrane (1991), and Mace (1991).

[2]These studies typically view the schooling decision as part of a static time

longitudinal data, the ICRISAT survey from South India, to investigate the dynamics of school attendance.

This paper also contributes to the debate on the structure of rural financial markets. The ability to trade consumption claims over time (borrow and lend) and across states (insure) obviates the need for child labor as a buffer against income fluctuations. Comovements of school attendance and income can thus reveal market incompleteness. For example, in their analyses of the ICRISAT data, Townsend (1991) and Lim (1991) find that village markets or informal institutions serve to mitigate, though do not eliminate, the impact of idiosyncratic (household specific) shocks on consumption, but that households are less well insured against aggregate village risk (e.g., regional droughts). If this is indeed the case, then school attendance should be less responsive to idiosyncratic shocks than to aggregate village shocks.

Our analysis also distinguishes between credit market and insurance market limitations. The credit market allows households to smooth consumption and child school attendance across anticipated income fluctuations, and can also provide ex-post insurance against unanticipated income shortfalls, in the form of emergency loans. However, unless households are insured ex-ante, they will be worse off if income falls unexpectedly, and they may respond by withdrawing children from school.[3] Lim's study suggests that Indian villagers are at least partially insured against idiosyncratic risk ex-ante, but Morduch (1990), using the same yearly ICRISAT consumption data, finds evidence of borrowing constraints (limitations on ex-post insurance) for poor households.

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allocation problem (see, e.g., Rosenzweig and Evenson, 1977).

[3]The distinction between credit and ex-ante insurance is not necessarily clean in practice. Udry (1991) shows that loan repayments in rural Nigeria are made contingent on crop outputs, thus serving an insurance function.

This paper focuses on fluctuations in income across agricultural seasons, which in our data are larger in magnitude than year to year fluctuations. Much has been written on seasonality in Third World agriculture (see Sahn, 1989), but the role of credit markets in smoothing consumption--and, needless to say, school attendance--has been neglected (Paxson 1991 is an exception).[4] Because seasonal income fluctuations are largely anticipated by households, the response of school attendance provides a test for perfect credit markets. Meanwhile, the response to unanticipated fluctuations gauges the effectiveness of insurance. We are able to distinguish anticipated from unanticipated changes in income by using information on village level rainfall "surprises".

The plan of the paper is as follows. Section II examines a simple dynamic human capital investment model with uncertainty. The model is used to predict the response of school attendance to income fluctuations, under market structures ranging from complete to autarkic. Section III discusses the ICRISAT data and section IV develops the econometric specifications used to test the hypotheses. The empirical results are reported in section V. Our findings are striking and generally supportive of Townsend and Lim. While school attendance responds strongly and as predicted to seasonal income and wage movements, village-wide shocks have larger effects than idiosyncratic shocks. We also find evidence of limitations on ex-ante insurance against idiosyncratic risk for small farmers, but no empirical support for borrowing constraints across seasons. Section VI concludes the paper.

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[4]The most common form of credit for landless households and small farmers in village India is the seasonal loan, given at the start of the growing season and repaid at harvest time (see Walker and Ryan, 1990).

II. CREDIT, INSURANCE AND SCHOOL ATTENDANCE

A model of human capital investment under uncertainty

The objective of this section is to construct a human capital investment model, based on a plausible set of assumptions, that yields empirically tractable relationships between changes in school attendance and changes in income and prices, under different market structures. By design these relationships mirror those for consumption and leisure derived by Townsend (1991), Cochrane (1991), Mace (1991), Altug and Miller (1990), and others.

Consider a household over the time interval $[\tau,T]$ in which its single child is eligible for school. Each year is divided into two agricultural seasons, so that t indexes season-years. Each state of nature ε_t represents the publicly known history of the economy up to time t, and has conditional probability $\pi(\varepsilon_t|\varepsilon_\tau)$. Household i chooses total consumption $C_i(\varepsilon_t)$ and the time its child spends in school, $S_i(\varepsilon_t) \in [0,\Omega_i^C(\varepsilon_t)]$, where $\Omega_i^C(\varepsilon_t)$ is the child time endowment, to maximize expected utility

$$(2.1)\quad \left\{\sum_{t=\tau}^{T}\sum_{\varepsilon_t} \pi(\varepsilon_t|\varepsilon_\tau)\beta^{t-\tau}U(C_i(\varepsilon_t))\right\} + \sum_{\varepsilon_T} \pi(\varepsilon_T|\varepsilon_\tau)\phi(H_{iT+1}(\varepsilon_T)).$$

β denotes the household's discount factor, and the increasing concave function ϕ represents the "salvage" value parents place on the final stock of child human capital, H_{iT+1}. Educational attainment may be valued because it raises adult farm management ability, increases the returns to rural-urban migration, or enhances marriage market prospects. The main point is that these gains are realized after schooling has been completed.[5]

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[5]With perhaps less motivation, we could dispense with ϕ and human capital altogether and assume that school attendance provides direct utility. If U is additively separable in consumption and child school attendance, the results

School attendance is assumed to augment the beginning of period stock of human capital, H_{it}, according to

$$(2.2)\quad H_{it+1}(\varepsilon_t) = H_{it}(\varepsilon_{t-1}) + g(S_i(\varepsilon_t),\theta_i(\varepsilon_t)),$$

where g is an increasing concave function, so that school attendance exhibits diminishing returns, and $\theta_i(\varepsilon_t)$ indexes education "productivity" shifts (e.g., arbitrary cross-sectional and intertemporal variation in school quality or child motivation).[6] A simplifying assumption embedded in (2.2) is that past schooling does not enhance the productivity of current school attendance; i.e., the technology is additive (we can write $H_{iT+1}(\varepsilon_T) = H_{i\tau} + \sum_{t=\tau}^{T} g(S_i(\varepsilon_t),\theta_i(\varepsilon_t))$). Even under this assumption, which can be relaxed in part of the analysis, erratic school attendance is costly. The concavity of g implies that stable school attendance over time will produce more human capital than a variable attendance rate with the same mean.[7]

We assume that the child works at home or in the casual labor market when not in school, and that foregone production or earnings are the only cost of attending school (there are no school fees in India). The "spot" real wage for child labor, $w(\varepsilon_t)$, is taken to be the price of child time. Thus, in contrast to the literature emanating from Ben-Porath (1967), a child's wage is assumed independent of his stock of human capital (at least prior to time T).

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would be similar to those derived below.

[6]Child illness might depress educational productivity, but also effectively reduces the child's time endowment and hence household income, though such income effects are not likely to be large.

[7]Human capital depreciation is straightforward to incorporate in (2.2), but does not affect the analysis.

This assumption is important for technical reasons, but is also plausible in the rural South Indian context. Estimates using the ICRISAT data reported in Skoufias (1992) show no significant effect of education on the wages of adult male and female casual laborers, and the same is undoubtedly true for children.[8] It is also unlikely that schooling greatly enhances the productivity of children in home activities.

Leisure of the child and other family members is fixed at zero without affecting the main conclusions. In each period, the total value of household resources prior to any consumption transfers is given by real full income,

$$(2.3)\quad F_i(\varepsilon_t) = W(\varepsilon_t)'\Omega_i(\varepsilon_t) + \sum_K \Pi_{ik}(\varepsilon_t),$$

where $W(\varepsilon_t)$ and $\Omega_i(\varepsilon_t)$ are $N_i \times 1$ vectors of spot real wages and time endowments for the N_i worker types in household i (e.g., men, women and children), and $\Pi_{ik}(\varepsilon_t)$ is real profit from activity k (e.g., farming or handicrafts). Changes in $F_i(\varepsilon_t)$ reflect truly "exogenous" income fluctuations, uncontaminated by ex-post responses such as inter-household transfers or adjustments in family members' labor supply.

Complete markets

In a complete markets equilibrium, households can be viewed as trading time-state contingent consumption claims at date τ so as to maximize ex-ante expected utility (2.1). This market (or set of informal institutions that mimic it) may be confined to the village, or may involve a whole constellation

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[8]Estimating the effect of education on child wages is problematic because schooling, often in progress, is determined in part by current wages.

of villages; we let the data speak on this issue below. In addition to (2.2), households face a date τ budget constraint of the form

$$(2.4) \quad \sum_{t=\tau}^{T} \sum_{\varepsilon_t} P(\varepsilon_t)\Big[C_i(\varepsilon_t) + w(\varepsilon_t)S_i(\varepsilon_t) - F_i(\varepsilon_t)\Big] \le B_i,$$

where B_i is an exogenously determined "bequest" (net of initial wealth) to the post-schooling era. Here $P(\varepsilon_t)$ is the date τ Arrow-Debreu price of consumption deliverable in season-year t, contingent on state ε_t occurring. Denoting the Lagrange multiplier for household i's budget constraint by η_i, the first-order conditions, assuming $0 < S_i(\varepsilon_t) < \Omega_i^C(\varepsilon_t)$, are

$$(2.5) \quad \pi(\varepsilon_t|\varepsilon_\tau)\beta^{t-\tau}U'(C_i(\varepsilon_t)) = \eta_i P(\varepsilon_t),$$

$$(2.6) \quad \pi(\varepsilon_T|\varepsilon_\tau)\phi'(H_{iT+1}(\varepsilon_T))g_s(S_i(\varepsilon_t),\theta_i(\varepsilon_t)) = \eta_i w(\varepsilon_t)P(\varepsilon_t).$$

Once full income is insured in the date τ contingent claims market, condition (2.6) can be met by trading child time in the spot labor market.

Suppressing the state notation for expositional convenience, equation (2.6) produces the efficiency condition

$$(2.7) \quad g_s(S_{it+1},\theta_{it+1})/g_s(S_{it},\theta_{it}) = w_{t+1}P_{t+1}/w_tP_t,$$

which says that households equate the relative marginal productivity of school investments in each period to the relative price of child time. If, for example, $g(S_{it},\theta_{it}) = \sigma - \exp[-(\theta_{it} + \gamma S_{it})]$, with $\sigma \ge \exp(-\theta_{it}) > 0$ and $\gamma > 0$, then (2.7) implies

$$(2.8)\quad \Delta S_{it+1} = -\frac{1}{\gamma}\left\{\log(w_{t+1}/w_t) + \log(P_{t+1}/P_t) - \Delta\theta_{it+1}\right\},$$

where $\Delta S_{it+1} = S_{it+1} - S_{it}$ and $\Delta\theta_{it+1} = \theta_{it+1} - \theta_{it}$. The remarkable implication of complete markets is that, conditional on the undiversifiable aggregate shock captured by the Arrow-Debreu prices, school attendance is unaffected by changes in household full income; i.e., by idiosyncratic income shocks. However, school attendance does respond negatively to increases in child wages, and to adverse school productivity shocks (negative values of $\Delta\theta_{it+1}$).[9]

The same arguments apply in a multi-child household. We must consider the possibility that parents could specialize their children, withdrawing a "marginal" child from school during bad times while leaving the others to attend full-time. Nevertheless, with complete markets, even the attendance of these marginal children does not respond to idiosyncratic income fluctuations.

Incomplete markets: Credit but no ex-ante insurance

We now close down the ex-ante insurance market, but allow households to borrow and lend by trading a riskless financial asset. The single household budget constraint, (2.4), is thus replaced by the sequence of constraints

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[9]With a multiplicative human capital production technology, $H_{it+1} = H_{it}[1 + g(S_{it},\theta_{it})]$, the analog to equation (2.7) is $g_{sit+1}[1 + g_{it}]/g_{sit}[1 + g_{it+1}] = w_{t+1}P_{t+1}/w_tP_t$. Although the theoretical implications of this alternative specification are no different than before, it is operationally inconvenient when the θ_{it} parameter is allowed to vary over time. In fact, if $\theta_{it} = \theta_i$ is constant, taking logarithms and a first-order Taylor expansion produces an equation identical in form to (2.8).

$$(2.9) \quad A_{it+1} = (1+r_t)\{ A_{it} + F_{it} - w_t S_{it} - C_{it} \},$$

where A_{it} is net assets of household i at the beginning of period t and r_t is the risk-free interest rate across seasons t and t+1.

The necessary conditions for the maximization of (2.1) subject to (2.2) and (2.9) can be developed by switching over to dynamic programming notation and writing the value function as

$$(2.10) \quad V(A_{it},H_{it}) = \max \left\{ U(C_{it}) + \beta\, \mathbb{E}_t\, V(A_{it+1},H_{it+1}) \right\},$$

where $\mathbb{E}_t$ is the expectations operator conditional on information available at time t.

Differentiating (2.10) yields the following first-order conditions, under the assumption of an interior solution for S_{it},

$$(2.11) \quad U'(C_{it}) = \lambda_{it},$$

$$(2.12) \quad \beta\, \mathbb{E}_t[\mu_{it+1} g_s(S_{it},\theta_{it})] = w_t \lambda_{it},$$

$$(2.13) \quad \mu_{it} = \beta\, \mathbb{E}_t \mu_{it+1},$$

$$(2.14) \quad \lambda_{it} = \beta \mathbb{E}_t[(1+r_t)\lambda_{it+1}],$$

where μ_{it} and λ_{it} are the multiplier functions associated with constraints (2.2) and (2.9), respectively.[10]

Combining (2.12) and (2.13) gives

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[10] By the Envelope Theorem, we have $\mu_{it} = \partial V(A_{it},H_{it})/\partial H_{it}$ and $\lambda_{it} = \partial V(A_{it},H_{it})/\partial A_{it}$. Also, $\mu_{iT} = \beta \mathbb{E}_T \partial V(A_{iT+1},H_{iT+1})/\partial H_{iT+1} = \beta \mathbb{E}_T \phi'(H_{iT+1})$. The bequest constraint, $A_{iT+1} = B_i$, pins down λ_{iT}.

$$(2.15) \quad g_s(S_{it+1},\theta_{it+1})/g_s(S_{it},\theta_{it}) = w_{t+1}\nu_{it}/w_t\nu_{it+1},$$

where $\nu_{it} = \mu_{it}/\lambda_{it}$ is the period t shadow price of human capital relative to nonhuman capital. Comparing (2.7) and (2.15), the difference between complete and incomplete markets becomes apparent; ν_{it} is not a market price like P_t, but is determined within each household (c.f., Altug and Miller, 1990). To understand the operational implications of an absent insurance market, we need to examine how ν_{it} evolves over time.

Since ν_{it+1}/ν_{it} is the ratio of two random variables, assume as an approximation that $\lambda_{it+1}/\lambda_{it}$ and μ_{it+1}/μ_{it} are jointly stationary and lognormally distributed, and use the form for g posited above to obtain[11]

$$(2.16) \quad \Delta S_{it+1} = \frac{1}{\gamma}\left\{K_i + \log(1+r_t) - \log(w_{t+1}/w_t) + \Delta\theta_{it+1} + \xi_{it+1}\right\},$$

where K_i is a constant and ξ_{it+1} represents household i's error in forecasting ν_{it+1}. When a household without ex-ante insurance experiences an

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[11]See, e.g., Hansen and Singleton (1983). Dropping the i subscript, let $x = \lambda_{t+1}/\lambda_t$ and $z = \mu_{t+1}/\mu_t$, then (2.14) gives

$$\log \beta(1+r_t) + \log \mathbb{E}_t x = \log \beta(1+r_t) + \mathbb{E}_t \log x + \frac{1}{2}\operatorname{Var}[\log(x)] = 0$$

and (2.13) yields

$$\log \beta + \log \mathbb{E}_t z = \log \beta + \mathbb{E}_t \log z + \frac{1}{2}\operatorname{Var}[\log(z)] = 0.$$

Combining these two equations, we have $\mathbb{E}_t\,[\log z - \log x] = K + \log(1 + r_t)$, where $K = \frac{1}{2}\operatorname{Var}[\log(x)] - \frac{1}{2}\operatorname{Var}[\log(z)]$ is a constant by assumption. Using the fact that $z/x = w_{t+1}g_{st}/w_t g_{st+1}$ gives us our result.

unanticipated income shortfall at date t+1, it revises ν_{it+1} downward (nonhuman wealth becomes relatively more valuable). The negative forecast error, ξ_{it+1}, implies a fall in school attendance between t and t+1. Purely forecastable income fluctuations still have no effect on attendance in (2.16).

Finally, as in (2.8), undiversifiable aggregate risk is reflected in market prices, in this case the interest rate r_t. Thus, if the village is cut off from regional credit markets, and if, for example, in the dry season all households in the village anticipate higher incomes in the subsequent rainy season, then the village interest rate on seasonal loans will be high and, according to (2.16), there will be a large increase in school attendance in the rainy season, *ceteris paribus*.

Autarky

In the absence of even a credit market, and without the ability to store goods across seasons, the household's consumption expenditures and schooling costs in each season are constrained by full income, i.e.,

$$(2.17)\quad C_{it} + w_t S_{it} = F_{it}.$$

As a result, condition (2.14) no longer holds, and, using the exponential form for g, (2.15) can be written as

$$(2.18)\quad \Delta S_{it+1} = \frac{1}{\gamma}\left\{-\log(\beta) - \log(w_{t+1}/w_t) - \log(\lambda_{it+1}/\lambda_{it}) + \Delta\theta_{it+1} + \omega_{it+1}\right\},$$

where ω_{it+1} is (approximately) the forecast error in the shadow price of human capital, μ_{it+1}. Any change in F_{it}, whether anticipated or not, now affects school attendance through changes in λ_{it}. As income falls, the shadow value of child time ($w_t \lambda_{it}$) rises, and school attendance falls.

Borrowing Constraints

Borrowing constraints can be viewed as an intermediate case between autarky and unfettered credit markets. Suppose that the household can lend or store goods across seasons, but it cannot take on debt (or only a limited quantity); i.e., $A_{t+1} \geq 0$. Without going into a formal derivation, we can see the consequences of the asymmetry of the borrowing constraint. Child school attendance is unaffected by anticipated declines income (because savings is possible), but attendance responds positively to anticipated increases in income (as in the autarky case).[12]

A taxonomy of income effects

Table 1 summarizes the theoretical response of school attendance to different types of income shocks, under market structures ranging from complete to autarkic. As mentioned earlier, analogous responses are expected in consumption and leisure. The first row indicates that when households <u>and</u> villages are connected by a complete set of markets, school attendance decisions are entirely decoupled from income fluctuations (except those that affect the whole country). Moving down the table, as markets are closed down the restrictions on the relationship between changes in school attendance and changes in income are successively relaxed. The goal of the empirical analysis is to test this sequence of restrictions, until one is found that cannot be rejected. But before proceeding, we discuss the data.

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[12] See also the discussion of Zeldes (1989) in the context of consumption.

III. DATA

The Village Level Studies (VLS) survey, conducted by the International Crops Research Institute for the Semi-Arid Tropics (ICRISAT), selected ten villages to represent four broad agroclimatic zones of semi-arid India (Singh, Binswanger and Jodha, 1985; Walker and Ryan, 1990). In each village, a stratified random sample of forty households was chosen, consisting of equal numbers of landless, small, medium, and large scale farm households. We use data on the time allocation of children ages 5 to 18 from six of these villages; Aurepalle and Dokur in the Mahbubnagar District of Andhra Pradesh, Shirapur and Kalman in the Sholapur District of Maharashtra, and Kanzara and Kinkheda in the Akola District of Maharashtra.

It is important to understand the nature of agricultural production and risk in these villages. Land in Aurepalle and Dokur has red soil with limited water storage capacity, and the level of rainfall and its timing are quite uncertain from season to season. Castor, paddy and sorghum are the main crops, cultivated mainly during the rainy season. Shirapur and Kalman have black soils, which retain more moisture (i.e., rainfall has more persistent effects), and the main crop, sorghum, is cultivated during the post-rainy season. The quantity and timing of rainfall are also erratic in these two villages. Farmers in Kanzara and Kinkheda cultivate mainly cotton, mung beans and sorghum. They have black soils as well, but more reliable rainfall. Overall, irrigation is relatively uncommon (except for Dokur where irrigated land is about 30 per cent of gross cropped area).

Time allocated to major activities[13] by each present household member for

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[13] Activities include crop production, animal husbandry, building and other construction, repairs and maintenance, trade marketing and transport, domestic work, other activities (such as handicrafts, religious services and social functions, shop keeping, wine making and toddy tapping and other miscellaneous) food and fuel gathering, school attendance (including travel

the day preceding the interview was recorded on a monthly basis during the years 1975 to 1978.[14] Since there is hardly any cultivation during the summer season in any of the villages,[15] we divide each agricultural year into two crop seasons; the rainy (Kharif) season and the post-rainy (Rabi) season. Typically, the third quarter of each calendar year coincides with the planting and sowing operations of the rainy season crops, the fourth quarter with the harvesting of the Kharif crop, and the first and second quarter of each calendar year coincide with the planting and harvesting operations for the post-rainy season (Rabi) crops.

Monthly observations of each child's schooling hours are aggregated into two alternative seasonal averages. The first, $S^{(1)}$, averages all monthly observations in a given season, including those of zero reported hours, and thus takes into account missed school days. However, to the extent that zero reported hours in school reflects the day of interview (i.e., a Monday or a day following a holiday), $S^{(1)}$ may be subject to measurement error.[16] The second measure, $S^{(2)}$, averages only the positive monthly observations, capturing solely the adjustments in the length of the school day.

From our initial sample of 475 children, we exclude those who never

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time to and from school), and regular job. If any activity was performed for others (outside the household) the total wage received was also recorded.

[14]Beginning in 1979 the labor utilization survey (VLS-K) was redesigned and detailed time allocation information was not collected any more. Note that the time coverage of the VLS-K is not uniform across villages. The survey was initiated during June-July of 1975 in all six village, but it was stopped earlier in Dokur, Kalman and Kinkheda.

[15]It is noteworthy that the main school vacation is taken during the summer season, as is customary under the British system.

[16]Households were supposedly interviewed on regular work days, but it is not clear that the previous day, the day of reference, was always a work day.

attended school during the survey period.[17] After also deleting observations to eliminate nonconsecutive seasons (32 cases) and children with only a single observation (19 cases), we are left with 1256 season-year observations on 258 children; 161 boys and 97 girls. Of these observations, there are 212 cases of zero school hours for a whole season, 84 of which occur between two seasons of positive school attendance; in other words, they are transitory withdrawals from school. The remaining 128 zeros occur at the beginning or the end of the sampling period for the child in question, and thus are more likely to reflect either a delay in starting school or a permanent drop out.

Figure 1 plots time series of the village means of the two schooling measures, $S^{(1)}$ (excluding the 128 zero observations just mentioned) and $S^{(2)}$. The different intertemporal patterns of the alternative measures points to the need for a sensitivity analysis. In figure 2, we compare the plot of $S^{(1)}$ with one of hours per day in work activities by these same children. In all villages the plots are virtual mirror images of one another, illustrating the stark trade-off between the production of human capital and current income.

Full income is defined as

$$(3.1)\quad F = (w_m N_m + w_f N_f + w_c N_c)\,\Omega + \Pi + V,$$

where the N_i, $i = m,f,c$, are, respectively, the numbers of adult males, adult females, and children ages 5 to 15 residing in the household in any given season; the w_i are the respective village average daily wage rates; Ω is the total time endowment, assumed to equal 156 days each season; Π is seasonal profit from crop cultivation, defined as value of output (including

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[17] We address the issue of potential sample selectivity bias below.

by-products, such as fodder) minus expenses on variable inputs inclusive of family labor;[18] and V is net seasonal revenue from sales of livestock products (e.g., milk and bullock rental), handicrafts produced within the household, and land rental. Note that V is not a pure profit measure, since the allocation of familial time to livestock and handicrafts activities is unknown. Thus, in addition to measurement error, F may be slightly contaminated by ex-post labor supply adjustments.[19,20]

We also examine a conventional income measure, likely to reflect the ex-post adjustments of the household,

$$(3.2)\quad Y = w_m L_m + w_f L_f + w_c L_c + \Pi + V,$$

where the L_i are the total labor supply of males, females and children. In calculating Y, we use reported labor earnings (cash and in kind) received by family members employed either as regular or casual workers.[21]

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[18]Expense items are seeds, fertilizers, manure, pesticides, machine services, owned and hired bullock labor, hired and family male, female and child labor (all valued at the gender/age specific village average wage rates). Some of these inputs may have been purchased in prior seasons, but this is difficult to tell from the data.

[19]Rosenzweig (1988) bases his full income measure on the value of the male time endowment and farm profits alone. He excludes the value of the female time endowment because, over the nine years of the ICRISAT survey he uses, the number of women in the household fluctuates considerably due to marriage, which may reflect intra-family insurance arrangements. Since we look at a much shorter panel, and because demographic information is only reported on a yearly basis and so does not contribute to within year changes in full income, we include the value of the female time endowment.

[20]Forty-seven observations are dropped because of missing full income, and four are dropped because full income is negative (since we later take logs).

[21]These various sources of income, including labor earnings, are extracted from the original household transaction files (VLS-L) of ICRISAT. As a check, we sum the two seasonal observations of each income category for each household in each agricultural year. These yearly sums match very closely to the

Figure 3 plots the village means of both income measures, deflated by the district level price index with base year in 1975. As with schooling hours, Figure 3 suggests high intertemporal variability within any given village as well as considerable inter-village variability. The plot for Kanzara shows the most distinct seasonal income pattern, consistent with rainy season cultivation of its main crop. Income seasonality is reversed in Shirapur and Kalman because of sorghum cultivation during the Rabi season.

Average hourly wage rates for adult males, adult females and children in each village for each season-year are calculated from observations on daily wages and hours of individuals working outside the household.[22] It should be noted that child participation in the paid labor force is only about 20 percent overall (based on the initial sample of 475 children). We attempt to address this issue in the next section. Figure 4 plots real child wages along with total seasonal rainfall (in millimeters). In the empirical work we break down seasonal rainfall into quarterly totals to capture the all important timing of rainfall. In addition, we construct deviations of rainfall from the average in each three month period, calculated for each village from daily rainfall data collected between 1975 and 1984.

IV. ESTIMATION STRATEGY

Testing for complete markets within and across villages

Our first task is to see whether there is a positive relationship in the data between changes in school attendance and changes in household income over

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corresponding numbers calculated independently by ICRISAT.

[22] Wages for regular jobs (e.g. school teachers and permanent servants) are not reported in the survey, so the average wage refers to casual labor.

time, conditional on the change in child productivity. One can view this as a test for complete inter- and intra-village markets, in the absence of country-wide aggregate shocks (see row (1) of table 1). The regression is

$$(4.1) \quad \Delta S_{ivt} = \alpha_0 + \alpha_1 \Delta\log F_{ivt} + \alpha_2 \Delta\log w_{vt} + e_{ivt},$$

where ΔS_{ivt} is the change in school attendance of child i, in village v, from season-year t-1 to season-year t; $\Delta\log F_{ivt}$ is the change in the log of household real full income; $\Delta\log w_{vt}$ is the change in the log average child wage in village v; and e_{ivt} is a random, mean zero, disturbance term capturing measurement error and school productivity shocks, $\Delta\theta_{it}$. We assume these shocks are uncorrelated with changes in household income.

If full income is measured with error or is not entirely uncontaminated by ex-post adjustments, then ordinary least squares estimates of α_1 will be biased. In the case of white noise measurement error, the bias will be toward zero. In the second case, α_1 will also tend to be underestimated because ex-post adjustments will attenuate measured income changes. Endogeneity of income, if true, will lead us to reject complete markets too infrequently. We use lagged farm characteristics and rainfall variables as instruments to obtain consistent estimates of (4.1) under the hypothesis that $\Delta\log F_{ivt}$ is correlated with e_{ivt}.

Testing for complete markets within villages

If we find that α_1 is significantly different from zero in (4.1), then we can proceed to test the weaker restrictions summarized in table 1. Complete intra-village markets imply that, once we control for a village-time effect ($\log(P_{t+1}/P_t)$ in equation (2.8)), the change in school attendance should be uncorrelated with income changes (row (3) of table 1). So, we can estimate

$$(4.2)\quad \Delta S_{ivt} = \gamma_{vt} + \alpha_1 \Delta \log F_{ivt} + e_{ivt},$$

where γ_{vt} is a village-season intercept that incorporates the child wage variable as well as aggregate shocks, and test the null hypothesis $\alpha_1 = 0$.[23]

The complement of (4.2) is the village means regression

$$(4.3)\quad \Delta \bar{S}_{vt} = \alpha_0 + \alpha_1 \Delta \overline{\log F}_{vt} + \alpha_2 \Delta \log w_{vt} + e_{vt},$$

which isolates the impact of aggregate village shocks. A significantly positive value of α_1 here indicates that the village lacks access to regional financial markets.

Distinguishing between credit and insurance market failure

Equation (4.2) provides a joint test against the failure of credit markets and ex-ante insurance. Our next step is to test for perfect credit markets, but allow for the possibility that insurance markets fail (row (4) of table 1). To do so, we decompose income changes into anticipated and unanticipated components using information on the deviations of rainfall from long run seasonal averages. Consider the following regression

$$(4.4)\quad \Delta \log F_{ivt} = X'_{ivt-1}\beta_1 + (X_{ivt-1} \otimes R^u_{vt})'\beta_2 + u_{ivt},$$

where X_{ivt-1} is a vector of farm characteristics lagged one period (including a constant), R^u_{vt} is a vector of measures of rainfall deviations in season-year

[23] Townsend (1991) and Mace (1991) carry out similar tests using consumption data. However, they use conventional income measures, not full income.

t (and perhaps functions of these variables), and u_{ivt} is a random component consisting of unobserved factors affecting income changes and measurement error. The Kronecker product (⊗) generates a vector of interaction terms.

We take as the anticipated component of the change in income the projection of the actual change on information available to the household at time t-1, namely

$$(4.5)\quad \Delta\log\hat{F}^{a}_{ivt} = X'_{ivt-1}\hat{\beta}_1.$$

On the other hand, we estimate the unanticipated component of the change in income as

$$(4.6)\quad \Delta\log\hat{F}^{u}_{ivt} = (X_{ivt-1}\otimes R^{u}_{vt})'\hat{\beta}_2,$$

since $X_{ivt-1}\otimes R^{u}_{vt}$ is unknown to the household at date t-1. Note that the estimated residual from (4.4), $\hat{u}_{ivt}$, will generally contain both unanticipated and anticipated components.[24]

With this income decomposition in hand, we can re-estimate equation (4.2) allowing $\Delta\log\hat{F}^{a}_{ivt}$, $\Delta\log\hat{F}^{u}_{ivt}$ and $\hat{u}_{ivt}$ to have separate coefficients, i.e.,

$$(4.7)\quad \Delta S_{ivt} = \gamma_{vt} + \alpha_{1a}\Delta\log\hat{F}^{a}_{ivt} + \alpha_{1u}\Delta\log\hat{F}^{u}_{ivt} + \alpha_{1R}\hat{u}_{ivt} + e_{ivt}.$$

The key to identifying α_{1u} in (4.7) is the set of interaction terms between farm characteristics and rainfall deviations in (4.4), since by itself R^{u}_{vt}

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[24]Altonji and Siow (1987), in their study of U.S. consumption behavior, estimate unanticipated income changes as residuals from a regression of actual income changes on lagged exogenous variables, but they have no direct instruments for the unanticipated components.

would be swept out with the village-season dummies.

It is worth mentioning how our specification differs from Paxson (1992), who uses rainfall data to predict "transitory" (unanticipated) income and estimate its effect on savings in Thailand. First, Paxson ignores region specific aggregate shocks in her savings equation (she does not include interactions between region and time dummies).[25] By implicitly assuming that interest rates in different villages or regions move together, she effectively imposes inter-village credit markets. Second, Paxson adopts a restrictive specification of the income equation, in which the effect of unexpected rainfall on income does not depend on farm characteristics; i.e., differences in transitory income across households within a region are purely random. Equation (4.4) above contains such interactions, and in fact they are necessary to properly account for village-wide aggregate shocks (see the preceding paragraph). Finally, because Paxson does not have panel data, she must predict levels as opposed to changes in income, which raises the specter of omitted variable bias. In particular, the presence of unobserved household fixed effects that are correlated with included household characteristics would bias her estimates of permanent income.

Testing against the alternative of borrowing constraints

In equation (4.7), we test for perfect credit markets ($\alpha_{1a} = 0$) against the autarky alternative ($\alpha_{1a} > 0$). Borrowing constraints present a more restrictive alternative than autarky, because they imply asymmetric effects of <u>anticipated</u> income changes. However, not all households in a village will be borrowing constrained. We follow Morduch (1990) and split our sample by farm

[25]Paxson does include region dummies to control for regional fixed effects and time dummies to control for country-wide shocks.

size, a reasonably good proxy for household wealth.

To test for asymmetric income effects, we construct the dummy variable

$$(4.8)\quad D^{+}_{ivt} = 1 \quad \text{if} \quad \Delta\log \hat{F}^{a}_{ivt} > 0,$$
$$= 0 \text{ otherwise.}$$

Equation (4.7) is then modified as follows,

$$(4.9)\quad \Delta S_{ivt} = \gamma_{vt} + \alpha^{+}_{1a} D^{+}_{ivt} \Delta\log \hat{F}^{a}_{ivt} + \alpha^{-}_{1a}(1-D^{+}_{ivt})\, \Delta\log \hat{F}^{a}_{ivt}$$
$$+ \alpha_{1u}\Delta\log \hat{F}^{u}_{ivt} + \alpha_{1R}\hat{u}_{ivt} + e_{ivt}.$$

The null hypothesis of perfect credit markets is $\alpha^{+}_{1a} = \alpha^{-}_{1a} = 0$ and the alternative hypothesis of borrowing constraints is $\alpha^{+}_{1a} > \alpha^{-}_{1a} = 0$;[26] i.e., constrained households should adjust child school attendance only in response to anticipated income growth.[27]

V. ESTIMATION RESULTS

Rows (1) and (2) of table 2 report OLS estimates of equation (4.1) on two different samples. The first sample ("+ zeros") includes all observations of $S^{(1)}$, the unconditional seasonal mean of school attendance, while the second

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[26]A similar test for symmetry in the response of consumption to income changes is proposed by Altonji and Siow (1987) using U.S. data.

[27]An important caveat in interpreting this test is that some children may be constrained by the upper bound on time in school. If a child is already attending school full-time, then an increase in income cannot lead to increased attendance even if borrowing is constrained. In other words, it may appear as though borrowing is unconstrained even though it is constrained, thus diminishing the power of the test.

sample trims consecutive zeros from the beginning and end of each child's time series as previously discussed. In both rows, the effect of changes in child wages and real full income are in the expected direction and are remarkably significant (standard errors are corrected for arbitrary forms of heteroskedasticity using White's method).[28] Rows (1) and (2) give almost identical results, and from now on we use the trimmed sample.

Row (3) corrects for possible sample selection bias due to the exclusion of the 195 children who never attended school during the sampling period.[29] Selectivity may lead to downward biased estimates of income and wage effects, because the children who would have been more responsive to income and wage changes end up quitting school early or never enroll in the first place. In fact, though the inverse Mill's ratio correction term is highly significant, the selectivity corrected estimates in row (3) are indistinguishable from their uncorrected counterparts in row (2).

Row (4) replicates specification (2) using the <u>conditional</u> seasonal mean of school attendance, $S^{(2)}$.[30] The resulting wage effect is substantially weaker and the income effect is now insignificant, suggesting that most of the economically meaningful variation in school attendance is in days attended

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[28] Due to the short, unbalanced panel, the standard errors are not adjusted for possible autocorrelation generated by first differencing.

[29] We use a two-step estimation method. First we construct an indicator variable for whether or not the child ever reports positive school attendance, and then we estimate a probit regression on the full sample of 475 children. Explanatory variables include child age, age squared, sex, household caste, demographics and farm characteristics, and village wage rates, all observed in the initial period. Each child's inverse Mill's ratio is thus constant across seasons. We do not adjust the standard errors, except that the White standard errors correct for any heteroskedasticity induced by this procedure.

[30] The somewhat smaller sample is due to the elimination of the remaining observations with zero school attendance for a season.

rather than in the length of the school day. That most of this variation in days attended is not merely measurement error due to interview timing (see section III) is evident from the low standard errors in rows (1) and (2).

In row (5), we replace full income with the conventional measure of income, Y, discussed in the previous section (sample size is lower due to missing earnings data). The estimate of α_1 is smaller in magnitude than in row (2), confirming an expected downward bias when the income measure is contaminated by ex-post adjustments. Adding gross or net intra-household transfer receipts to Y (results not reported here) does not significantly affect these results.

Because of the relatively thin child labor market, one could argue that the average child wage is unrepresentative of the shadow value of child time, and reflects more the productivity of adult males. However, when we include changes in the log of the village average adult male wage in row (6), we still find a strongly negative child wage effect, and our estimate of the income effect is hardly changed from specification (2). It is also interesting to add interaction terms between the income and wage changes and the age and sex of the child. Only the wage change-sex interaction approaches significance, and it reveals that boys' school attendance falls by about half as much as that of girls for a given child wage increase.[31]

Rows (7) and (8) report two-stage least squares estimates using two sets of instruments. In row (7) the instrument set includes lagged farm characteristics, lagged quarterly rainfall totals, and current deviations of quarterly rainfall from long run averages, as well as interactions between

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[31] This finding is consistent with Rosenzweig and Evenson (1977), who regress sex specific school enrollment rates in India on district level child wages and finds a significantly negative effect only for girls. However, their estimates are based on cross-sectional data.

these rainfall variables and lagged farm characteristics (the full set of instruments is listed in the appendix). All of these instruments should be orthogonal to the measurement error in the change in full income. However, if the problem is that our full income measure is contaminated by ex-post adjustments, then the rainfall deviation variables are not valid instruments because they are realized ex-post. Consequently, our second set of instruments (IV2) excludes all rainfall deviation variables, and their interactions, from IV1. In either case, we find evidence of attenuation bias; the 2SLS estimates of α_1 are larger than the OLS estimate in row (2), and the Wu-Hausman tests are significant at the five percent level.

Rows (1)-(8) of table 2 allow us to reject complete markets across South India (barring country-wide aggregate seasonal shocks). We now proceed to estimate equation (4.2) and test the much weaker restrictions implied by complete intra-village markets. The OLS estimate of α_1 in row (9) indicates that idiosyncratic changes in full income are significantly positively associated with changes in school attendance. However, the estimated income effect is considerably smaller and weaker than in row (2), where income changes include an aggregate component. A conventional income measure in place of full income in row (10) again leads to an underestimate of the income effect.

When idiosyncratic income changes are instrumented in row (11), the effect of income seems to disappear completely. However, these 2SLS estimates are rather uninformative; the standard errors are so large that it impossible to reject either the exogeneity of income or a zero income effect.[32] Since our

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[32]In an attempt to reduce the standard errors, we tried adding farm characteristics lagged two seasons back and their interactions with current rainfall deviations to the instrument set. Doing so reduces the sample size considerably, and still leads to insignificant income effects.

strong prior was that the OLS estimate of the income effect would, if biased, be biased downward and that 2SLS would thereby make it <u>more</u> likely to reject the complete intra-village markets hypothesis, we must continue to reject that hypothesis.

The final regression in table 2 estimates equation (4.3). Note the small sample size; each observation is a village mean in a season-year and there are only six villages and no more than seven season-years. Nevertheless, the estimate of α_1 is significantly positive and much larger than its counterpart in row (9). We again conclude that school attendance responds more readily to aggregate than to idiosyncratic shocks.

To shed further light on the results in rows (9) and (11) of table 2, we split the sample by landholding category. If access to village financial markets is positively related to household wealth (see Morduch, 1990, and Townsend, 1991), then we expect idiosyncratic income shocks to have more of an impact on children from smaller farms and landless households than on those from large and medium size farms. However, the first two rows of table 3 appear to show just the opposite result. Based on OLS estimates, only the school attendance of children from the larger farm households responds significantly to idiosyncratic income changes.[33]

In rows (3) and (4) of table 3 we reestimate rows (1) and (2) using a two-step instrumental variables procedure based on equation (4.4).[34] After

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[33]Note that landless households receive minimal farm profits, so most of their income is from wages. Since we use village average wages, the landless experience only small idiosyncratic income changes (many report income from nonfarm self-employment activities). When we exclude landless children from the small farm subsample, the results are not appreciably affected.

[34]As discussed in section III, the same two-step procedure is used below to estimate anticipated and unanticipated income effects. The total idiosyncratic income effects are reported in rows (3) and (4) for purposes of comparison with these later results.

predicting full income changes with instrument set IV1 (no village-season dummies), we again split the sample according to landholdings. Now income effects become insignificant for the large farm group, but become large and significant for the children of smaller farmers.[35] Attenuation bias of the estimated income effect thus appears to be a problem in the small farm subsample. These households no longer seem better insured than their larger neighbors, as was indicated in rows (1) and (2). Unfortunately, we must also conclude that our instruments do not perform especially well in the large farm subsample.

In rows (5)-(7) of table 3, separate OLS regressions are run for each of the three villages that have all seven seasons of data: Aurepalle, Shirapur and Kanzara. Results for the first two villages conform to those of the full sample, leading us to reject complete markets. In Kanzara, however, the effect of idiosyncratic income fluctuations is not statistically significant. This finding could be due to either the fact that rainfall in Kanzara is the highest and least erratic of the three villages (see figure 4), and hence child labor plays less of a self-insurance role there, or because of the fact that formal credit institutions are the most developed in Kanzara.

We now proceed to decompose income changes into anticipated and unanticipated components, based on the first-stage regression already discussed. As mentioned, village-season dummies are not included in the first stage (equation (4.4)), because they can represent either anticipated or unanticipated effects. Row (1) of table 4 estimates equation (4.7), but

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[35] Since income changes are predicted from a first-stage regression, the standard errors should be adjusted. Rather than correct for the specific form of heteroskedasticity induced by the generated regressor, we again use White's correction for general forms of heteroskedasticity (a similar approach has been taken by Altonji and Siow, 1987).

excludes village-season dummies. The estimated coefficients on the anticipated and unanticipated components, which confound aggregate and idiosyncratic effects, are highly significant, pointing to both credit and ex-ante insurance market imperfections. When we include the village-season dummies in row (2) of table 4 to isolate the idiosyncratic effects, the significance of anticipated and unanticipated income shocks evaporates; now only the unexplained portion of income changes has a significant impact on school attendance. Based on the full sample results in table 2, the high standard errors here probably reflect a lack of good instruments for the idiosyncratic component of income changes.

Splitting the sample by landholdings in rows (3) and (4) of table 4, yields more clear cut results. In row (3), the unanticipated component of idiosyncratic income changes has a positive coefficient that verges on significance, indicating that smaller farms are not well insured ex-ante, although access to seasonal credit does not appear to be a problem, as evidenced by the insignificance of the anticipated component. Neither anticipated nor unanticipated income shocks have statistically important effects on school attendance for children from larger farms, but this may simply be due to the inadequacy of our instruments for this subsample (c.f., table 3).[36]

In table 5, we test for symmetry in the effects of anticipated income changes on school attendance. The null hypothesis, that anticipated increases in income have the same zero impact on attendance as do anticipated declines

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[36]In an attempt to increase precision, we expanded our instrument set by interacting the Kharif dummy (see appendix) with a set of village dummies and then interacting these variables with lagged farm characteristics, using them to predict $\Delta \log F^{a}_{ivt}$. But this procedure did not yield much sharper estimates.

in income, cannot be rejected in the full sample. But, as before, this test suffers from lack of power. The estimates in row (2) indicate an asymmetry, as predicted under borrowing constraints, among children from smaller farms and landless households, but it is not significant ($\alpha^{+}_{1a} = \alpha^{-}_{1a} = 0$ cannot be rejected). Likewise, no asymmetry is detected for children from large and medium size farms. Thus, although these tests are only as good as the available instruments, we find no evidence for constraints on the ability of households to borrow across seasons. This finding is consistent with the active seasonal loan markets observed in ICRISAT villages.

VI. SUMMARY AND CONCLUSIONS

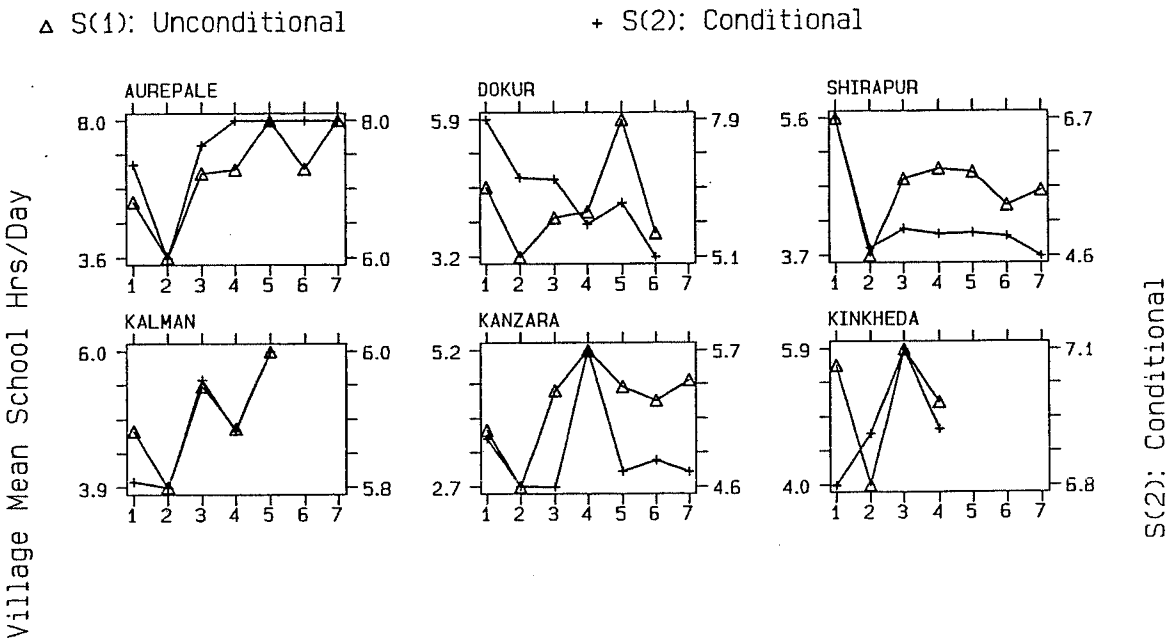
The idea that child labor, and hence school attendance, plays a role in the self-insurance strategy of poor households has been a topic of speculation but never of rigorous empirical investigation. This paper presents strong evidence that children in agrarian households in rural India are pulled in and out of school as household income rises and falls across seasons.

Based on the proposition that fully insured households would never withdraw children from school in response to income shortfalls, we have argued that school attendance patterns can reveal the structure of formal or informal financial markets. Income seasonality and weather related risk have a large village-wide component, and our evidence indicates that aggregate fluctuations are harder to insure against than idiosyncratic fluctuations. Although we reject the implications of the complete intra-village markets hypothesis, the rejection is not overwhelming and the response of school attendance to idiosyncratic income shocks is substantially weaker than the response to aggregate shocks. These findings are consistent with those of Townsend (1991) and Lim (1991) for the ICRISAT consumption data. We also find that idiosyncratic risk has the smallest impact on school attendance in Kanzara, the village with the most assured rainfall and most developed financial markets.

Distinguishing between credit and insurance market failure is a delicate task. To our knowledge, this is the first paper to decompose <u>changes</u> in full income into anticipated and unanticipated components using rainfall data. In doing so, we have uncovered evidence that small farmers and landless households are inadequately insured ex-ante; unanticipated income shocks significantly affect their children's school attendance. On the other hand, even for these poorer households, we find no empirical support for borrowing constraints across seasons. However, these statistical tests must be viewed with caution, as they may not be of sufficient power to detect modest departures from perfect credit or insurance markets.

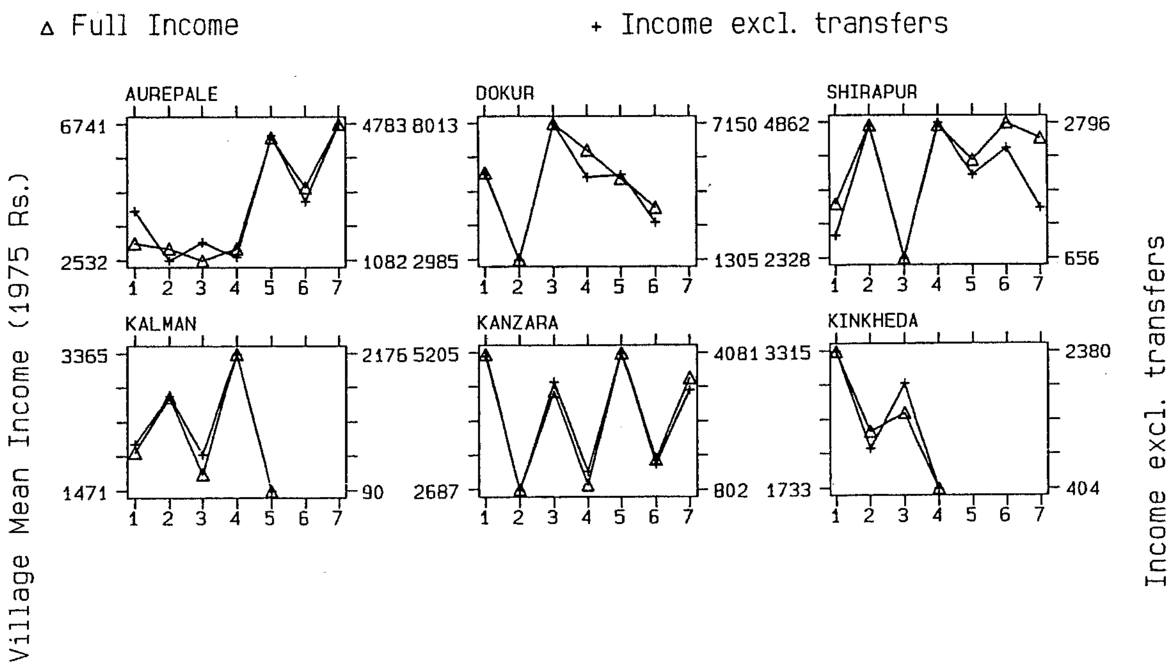
Overall, our findings highlight the potentially important link between what are widely viewed as two engines of economic growth, financial market development and the accumulation of human capital. Further detailed studies of village financial markets and their interaction with household choices are necessary to increase our understanding of developing economies and to inform development policy.

FIGURE 1



ALTERNATIVE MEASURES OF SCHOOL HRS/DAY BY VILLAGE

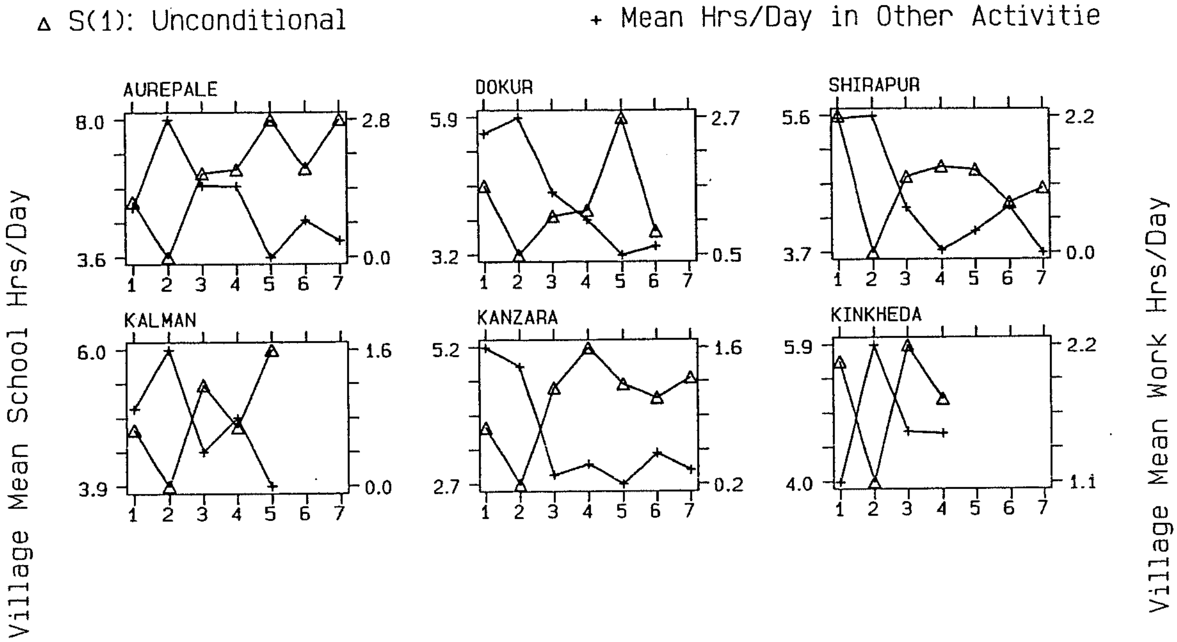
FIGURE 3



1=75:Kharif, 2=75:Rabi, 3=76:Kharif, etc

ALTERNATIVE INCOME MEASURES BY VILLAGE

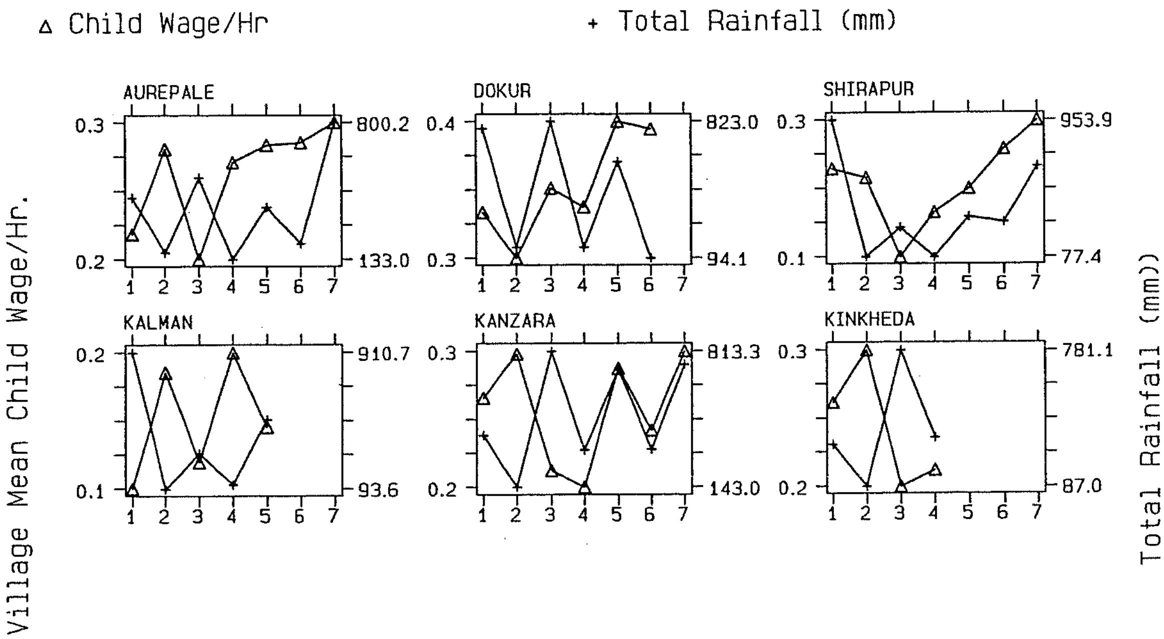
FIGURE 2



1=75:Kharif, 2=75:Rabi, 3=76:Kharif, etc

MEAN SCHOOL & WORK HRS/DAY BY VILLAGE

FIGURE 4



1=75:Kharif, 2=75:Rabi, 3=76:Kharif, etc

MEAN CHILD WAGE/HR AND RAINFALL BY VILLAGE

TABLE 1

A TAXONOMY OF INCOME EFFECTS ON SCHOOL ATTENDANCE

Market Structure	Idiosyncratic		Aggregate (Village)	
	Anticipated	Unanticipated	Anticipated	Unanticipated
Complete Intra-Village Markets and:				
Complete inter-village markets	NONE	NONE	NONE	NONE
Inter-village credit only	NONE	NONE	NONE	YES
Village Autarky and:				
Complete intra-village markets	NONE	NONE	YES	YES
Intra-village credit only	NONE	YES	YES	YES
Household autarky	YES	YES	YES	YES

TABLE 2

EFFECTS OF CHANGES IN INCOME AND CHILD WAGES ON SCHOOL ATTENDANCE

Specification	ΔIncome α_1	ΔWage α_2	Vill.-season dummies	R^2	Nobs
(1) $\Delta S^{(1)}_{ivt}$, + zeros, OLS	0.534 (4.05)	-1.908 (6.27)	NO	0.057	995
(2) $\Delta S^{(1)}_{ivt}$, OLS	0.565 (4.02)	-2.110 (6.32)	NO	0.062	867
(3) $\Delta S^{(1)}_{ivt}$, selectivity corrected OLS	0.586 (4.17)	-2.263 (6.95)	NO	0.089	867
(4) $\Delta S^{(2)}_{ivt}$, OLS	0.069 (0.94)	-0.751 (3.83)	NO	0.028	767
(5) $\Delta S^{(1)}_{ivt}$, $\Delta \ln Y_{ivt}$, OLS	0.222 (3.52)	-1.789 (5.01)	NO	0.046	788
(6) $\Delta S^{(1)}_{ivt}$, with adult male wage, OLS	0.597 (4.36)	-1.409 (3.62)	NO	0.080	867
(7) $\Delta S^{(1)}_{ivt}$, 2SLS (IV1)	0.948 (4.65)	-2.463 (6.19)	NO	[2.377]	867
(8) $\Delta S^{(1)}_{ivt}$, 2SLS (IV2)	1.092 (4.61)	-2.596 (6.37)	NO	[2.642]	867
(9) $\Delta S^{(1)}_{ivt}$, OLS	0.320 (1.95)	...	YES	0.261	867
(10) $\Delta S^{(1)}_{ivt}$, $\Delta \ln Y_{ivt}$, OLS	0.126 (1.78)	...	YES	0.273	788
(11) $\Delta S^{(1)}_{ivt}$, 2SLS (IV1)	0.183 (0.66)	...	YES	[0.543]	867
(12) $\Delta \bar{S}^{(1)}_{vt}$, OLS	1.390 (2.68)	-3.337 (3.55)	NO	0.335	30

NOTE.— Absolute values of White t-statistics in parentheses. All specifications include a constant term and use $\Delta \ln F_{ivt}$ unless otherwise stated. In row **(3)** coefficient on inverse Mill's ratio is -3.915 (4.62). In row **(6)** the coefficient on change in adult male wage is -2.593 (4.03). In rows **(7)**, **(8)**, **(11)** and **(12)** the number in [.] is the absolute t-value for Wu-Hausman exogeneity test. IV1 includes one season lagged farm characteristics, current rainfall deviations and interactions between the two. IV2 drops current rainfall deviations and their interactions from IV1 (for more details see Appendix)

TABLE 3

EFFECTS OF INCOME CHANGES ON SCHOOL ATTENDANCE BY FARM SIZE AND VILLAGE

Specification	ΔIncome α_1	Vill.-season dummies	R^2	Nobs
(1) Landless & Small scale farms, OLS	-0.025 (0.07)	YES	0.333	235
(2) Medium & Large scale farms, OLS	0.395 (2.20)	YES	0.316	632
(3) Landless & Small scale farms, IV1	1.899 (2.15)	YES	[2.361]	235
(4) Medium & Large scale farms, IV1	-0.109 (0.41)	YES	[1.780]	632
(5) Aurepalle	0.723 (2.37)	YES	0.280	155
(6) Shirapur	0.712 (1.97)	YES	0.219	247
(7) Kanzara	0.228 (1.05)	YES	0.306	204

NOTE.— Absolute values of White t-statistics in parentheses. All specifications include a constant term and use $\Delta \ln F_{ivt}$. IV1 regressions use an instrumental variables procedure described in the text. Number in [.] is the absolute t-value for Wu-Hausman exogeneity test. Village regressions include only season dummies.

TABLE 4

EFFECTS OF ANTICIPATED AND UNANTICIPATED INCOME CHANGES ON SCHOOL ATTENDANCE

Equation (4.7) Sample:	α_{1a}	α_{1u}	α_{1R}	Vill.-season dummies	R^2	Nobs
(1) Full Sample	0.887 (4.65)	0.536 (2.78)	0.288 (1.64)	NO	0.123	867
(2) Full sample	0.175 (0.62)	0.168 (0.69)	0.368 (1.94)	YES	0.261	867
(3) Landless & Small scale farms	1.093 (0.96)	1.747 (1.83)	-0.426 (1.07)	YES	0.349	235
(4) Medium & Large scale farms	0.068 (0.23)	-0.028 (0.11)	0.509 (2.55)	YES	0.320	632

NOTE.— Absolute values of White t-statistics in parentheses.

TABLE 5

TESTING FOR SYMMETRIC EFFECTS OF INCOME CHANGES ON SCHOOL ATTENDANCE

Equation (4.9) Sample:	α^{+}_{1a}	α^{-}_{1a}	α_{1u}	α_{1R}	Vill.-season dummies	R^2	Nobs
(1) Full sample	0.121 (0.40)	0.342 (0.50)	0.137 (0.56)	0.368 (1.94)	YES	0.261	867
(2) Landless & Small scale farms	1.468 (1.29)	-1.520 (0.59)	1.950 (1.94)	-0.494 (1.16)	YES	0.351	235
(3) Medium & Large scale farms	-0.102 (0.32)	0.598 (0.84)	-0.133 (0.48)	0.509 (2.54)	YES	0.320	632

NOTE.— Absolute values of White t-statistics in parentheses.

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APPENDIX

LIST OF INSTRUMENTAL VARIABLES

I. One season lagged farm/village characteristics: village average male, female and child hourly wage rates; the number of adult male, female and child household members; a binary variable equal to 1 if the current season is the Kharif (rainy) season; village rainfall (in mm) during the first and second 3 month periods of the previous season; total cropped area, irrigated area, value of livestock, value of implements, as well as interactions of the preceding four variables with the two lagged rainfall variables.

II. Current rainfall variables: Deviations and squared deviations of rainfall in the first and second 3 month periods of the current season from the corresponding period mean rainfall (calculated using the daily rainfall data collected in each village for the years 1975 to 1984); the date of onset of the monsoon interacted with the Kharif season dummy variable (see above); interactions between these five variables and lagged total cropped area, irrigated area, value of livestock and value of implements.

NOTE.— In the text and tables IV1 refers to the union of instrument sets I and II, while IV2 refers to instrument set I.