Parental Preferences and Inequality Within the Family: Evidence From Mexican Siblings

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Abstract

This paper develops and tests a simple model of parental allocations which stresses the efficiency-equity tradeoff they face when investing in children of varying ability. The empirical component of the paper focuses on inequalities in schooling and child labour. Using sibling-based fixed effect models, I show that large differences in IQ test scores between siblings do not translate into large differences in schooling. The evidence also suggests that richer households compensate more than poorer ones. On child labour participation, the data show that there is a great deal of variation amongst brothers along this margin. If parents take compensating action they may be able to attenuate any harmful effects arising from these differences. I show that in fact there are substantial adverse effects: participation in paid employment during elementary school leads to rise of 8 percentage points in the probability of being affected by one of seven acute morbidity conditions. These results suggest that even if parents act to compensate along some dimensions, in the end they may be forced to pick and choose amongst their children for other allocations, leading to large inequalities.

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1 Introduction

Understanding the role of the family in transmitting inequality is a question of public policy concern. A better appreciation of parental behaviour has the potential to improve the design of poverty alleviation programmes.[1] A handful of empirical studies in the economics literature, focusing on outcomes such as education, earnings and health status, have attempted to determine whether parents reinforce or compensate for endowment differences amongst their offspring. But there does not appear to be a firm consensus, with some studies finding evidence of reinforcing behaviour (Behrman, Rosenzweig and Taubman 1994) whilst others find that parents act in a compensating manner (Griliches 1979, Ashenfelter and Rouse 1998 and Ermisch and Francesconi 2000).

In this paper I develop a simple theoretical model of human capital investment with multiple offspring, in the spirit of Becker and Tomes (1976) and Behrman, Pollak and Taubman (1982). The model predicts that rich parents invest to maximise returns, so that the marginal return to investment equals the cost of capital, i.e. the market rate of return. Any income inequalities amongst adult children are offset via transfers. Less rich, credit constrained parents face an efficiency-equity tradeoff when allocating investments amongst children of different abilities. If parents invest efficiently then the marginal product of human capital is again equalised across siblings, leading to adult income inequality amongst siblings.[2] Parents can offset some of this inequality by investing more in the less able child. Thus the simplest benchmark economic model predicts that the ability-investment relationship is strongest (or at least no less strong) in richer households than in poorer ones. In other words, richer parents reinforce more for endowment differences amongst their children than do poorer parents.[3]

However, this prediction is sensitive to the exact formulation of parents' preferences. In particular, if parents care about children's education, rather than their income, so that schooling enters directly parents' utility function - as in the paternalistic preferences view discussed by Pollak (1998) - and if it is costly to take compensating action, then the relationship between ability and schooling will be *weaker* for richer households than for poorer ones. In sum, theory alone cannot guide whether the rich will reinforce more than the poor.[4]

I use Mexican data to address two key empirical issues. The first is to ask whether parents

[1] Becker (1991, Chapter 6), for example, has argued that education programmes which target individual children may be undone by parents who in effect react in a compensating manner by devoting greater resources to non-participating children and to themselves.

[2] It is assumed that the return on human capital for each child in credit constrained households is above the market rate of return.

[3] Another interpretation of this result is that role of ability changes with opportunity. Richer parents invest to fulfil their child's potential. In poorer families it does not matter whether a child is able or not - given the limited resources available, all children will end with the same low level of schooling. In a different context, but consistent with the role of opportunity, Card (2001) shows that the relationship between ability (IQ) and schooling is stronger for men in the US who grew up near a college than for those who did not.

[4] If in poor families the more able sibling can commit, or parents can compel her, to compensate the less able sibling in adult life, then this will be another reason why poor households may in fact engage in strongly reinforcing behaviour.

reinforce or compensate for IQ differences amongst their children when making schooling decisions. I investigate whether this relationship between ability and schooling changes with family income. The evidence does not support strong reinforcing behaviour. In particular, richer parents appear to compensate more than poorer ones.

Despite these findings rejecting strong reinforcing behaviour, large inequalities in parental allocations remain. In particular, evidence presented below shows that there is a great deal of variation within the family on the child labour participation margin. When deciding to engage a child in work, there may be good reasons why parents do not allocate some child labour to each child, equalising marginal disutility of work across children. Two possible reasons are fixed costs associated with child labour participation and the timing of adverse income shocks (if increased child labour participation is a response to such shocks then only children of a given age at that point in time will be 'eligible' for participation). Under these circumstances, even though parents may be averse to inequalities between their children, they may nevertheless be forced to pick and choose amongst their young children for income generating work.

This leads into the second key empirical question addressed in this paper: Do differences in child labour participation within the family matter in the long run? More specifically, I ask: What are the long-term health effects of child labour? If parents can take compensating action then it may be that any adverse consequences of working as a child are mitigated. The empirical evidence suggests that the long-term health consequences for the working child are in fact quite severe. Even if parents act in a compensating manner on the ability-schooling dimension, it would appear that they are unable to undo the harmful effects on the sibling who participated in child labour.[5]

The data employed in this study are from the Mexican Family Life Survey (MxFLS), which has a number of very attractive features for the design of this study. Each individual in the family over the age of 15 is interviewed and provides, amongst many other things, detailed information on current and past schooling. This includes not just grade attainment, but also information on grade repetition, the age of quitting school and whether the individual worked for pay during elementary school. This level of detail permits the use of potentially superior measures of parental investments than has been the case in previous studies, which have typically used attainment and wages of adult children.

Interviewees also take an IQ test. Using this test score as a direct measure of the individual's endowment, I estimate pooled and family fixed effect models for siblings in order to determine whether parents reinforce or compensate for ability differences.[6] A potential disadvantage with this approach is that the IQ test score may itself be determined by schooling.[7] Fortunately, the IQ measure used, the Raven's Progressive Matrices test (Raven 1956), is believed to be less contami-

[5] Child labour is defined in this study as participation in an income generating activity during elementary school.

[6] At the end of his celebrated paper, Griliches (1979, Table 2), uses exactly this idea to speculate that parents compensate for ability differences amongst their children.

[7] One further potential concern with this empirical startegy is the possibility of attenuation bias in fixed effect methods when classical measurement error is present (see, for example, Griliches 1977, 1979 and Ashenfelter and Krueger 1995). In fact the results show that some of the within estimates are close to or even larger in magnitude than the pooled estimates, suggesting that this issue may not be of first order importance.

nated by schooling than is the case for most verbal reasoning tests.[8] The Raven's test (which has been extensively used in industrial and developing countries - see, for example, Boissiere, Knight and Sabot 1985 and Raven 2000) involves matching pictorial patterns and does not require the subject to be literate.[9]

The identification strategy for the analysis of the health consequences of child labour relies on variation in participation for brothers. The retrospective question in the survey on child labour participation, combined with a detailed set of health measures, permit the use of sibling-based family fixed effect methods to identify the long-term effects of child labour on health. This strategy allows me to control for time-invariant unobserved family characteristics which may be correlated with the child labour decision. Economists have used such models in a variety of settings.[10] I am also able to test the robustness of the fixed effect results to the inclusion of controls for important individual-level characteristics. It should be noted that child labour is a significant problem in Mexico: children in the 7 to 14 age group make up 30% of day labourers in the agricultural sector (US Department of Labor, 1998) and it is estimated that around 10% of children between 10 and 14 years are economically active (ILO, 2001).

Summarising the key results, I find that there are large differences in IQ within families (the mean of the difference between high and low ability sibling pairs is close to one whole standard deviation) but these do not appear to translate into large schooling differences between siblings. The within family estimates suggest that a one standard deviation rise in own ability relative to siblings is associated with higher attainment of 0.37 of a grade (mean grade attainment is around 8 grades). The relationship between ability and child labour participation also appears to be quite weak.

When I investigate the ability-schooling relationship by family income, I find that richer families appear to compensate more (reinforce less) than poorer ones: for families where the head has no schooling, the within family estimates suggest that a one standard deviation rise in own ability relative to siblings is associated with higher attainment of 0.4 of a grade; for families where the head has at least some secondary schooling, the corresponding estimate is 0.2. These findings are even stronger for other measures of parental schooling investments, such as the age at which

[8] See, for example, the quasi-experimental evidence presented in Cahan and Cohen (1989) on the effects of schoolimng on various IQ tests, including Raven's. On the impact of schooling on an ability test popular in the economics literature, namely the Armed Forces Qualification Test, or AFQT, in the National Longitudinal Survey of Youth, see Hansen, Heckman and Mullen (2005).

[9] Nevertheless, the suspicion remains that this measure reflects schooling and so estimates of the impact of ability will be upward biased. If the coefficient estimates suggest reinforcing behaviour, i.e. where the correlations between schooling outcomes and ability within families are strong and positive, then we must be cautious in interpreting this as evidence favouring the reinforcing hypothesis. But if the coefficients suggest compensating behaviour (zero or negative correlation between schooling and ability within families) then we may be on firmer ground in claiming that this provides evidence that parents invest to attenuate endowment differences amongst their children.

[10] Other than the papers cited earlier, examples of studies using sibling and twin methods - which aim to control for genetic characteristics as well as family fixed effects - include evaluations of the returns to education (Griliches 1977, 1979, Ashenfelter and Krueger 1994 and Ashenfelter and Zimmerman 1997), the returns to school quality (Altonji and Dunn 1996), the impact of Head Start (Currie and Thomas, 1995) and the effects of teenage childbearing (Geronimus and Korenman 1992 and Rosenzweig and Wolpin 1995).

the individual quits school. These results suggest that the simplest version of the Becker-Tomes model, which predicts that the rich should reinforce for endowment differences more than the poor, is rejected.

On the long-term health effects of child labour, I find that it is critical to control for unobserved family fixed effects: for example, for one key health outcome, whether as an adolescent or adult the individual suffers from one of seven acute morbidity conditions, the pooled estimates of the effects of child labour participation for a brothers sample are half the size of those estimated using family fixed effect methods. The within estimates suggest that participation in income generating work during childhood leads to an 8 percentage point greater likelihood of suffering from acute morbidity (the mean prevalence of acute morbidity for the sample is 9 per cent). These results are robust to controls for individual ability as well as for current occupation and sector of employment.

The contribution of this paper is twofold. First, it sheds new light on whether parents compensate or reinforce for children's endowment differences. In addition, as far as I am aware, this is the first study to distinguish rich and poor family investment strategies in the compensating-reinforcing literature. One limitation of the current setup is that it takes a unidimensional view of children's endowments - there may well be other endowments on which parents take action. Nevertheless, given the lack of consensus on this issue, it is valuable to explore a study design little explored to date.

The second contribution relates to the issue of the long-term health effects of child labour. There is very limited hard evidence on this topic (see for example the survey by Edmonds, 2007) and this is one of the first studies to identify the causal effects of child labour on long-term health.

The rest of this paper is organised as follows. Sections 2 and 3 present the theoretical model and empirical strategy, respectively. Section 4 describes the data and sample selection. Section 5 presents empirical evidence on parental preferences using ability endowments and schooling outcomes. Section 6 lays out the evidence on inequalities arising from within family variation in child labour participation, focusing particularly on the health effects of child labour. Section 7 concludes.

2 Intrahousehold Investment in Children: Theory

This section illustrates how ideas of reinforcing and compensating parental behaviour can be incorporated into standard economic models of human capital investment.

The key ingredients of the model are: parents are altruistic, their preferences being defined over the distribution of their children's earnings. Rich and poor parents have the same preferences. Rich families are not credit constrained but poor families are. There are complementarities between ability and education investments in the schooling and earnings production functions.[11]

[11] Although altruistic preferneces are commonplace in the economic literature, other possibilities should not be precluded. For example, Parsons and Goldin (1989) argue that nonaltruistic parental behaviour was pervasive amongst late nineteenth century families in the US. Models of exchange (Bernheim et al 1985, Cox 1987 and Light and McGarry 2004), where parental transfers are payments for child services such as regular contact and care,

2.1 The Basic Setup

This simple model assumes that there are two periods and two children in the household. Children's consumption when adults is c_1 and c_2, and is composed of own income and parental transfers. Parents' utility function is CES in their children's adult consumption. There are no transfers between siblings. x represents total resources allocated by parents to children and is assumed to be separable from other expenditures. Of the total x each child i receives h_i in the form of education investment and a monetary transfer, t_i, when adult. Child i earnings (y_i) next period, depend on own endowment (or ability), θ_i, and human capital investment, h_i. Furthermore, the earnings production function is assumed to be Cobb-Douglas: $y_i = A\theta_i h_i^d$. Prices for education expenditure, h_i, are the same for all children; assumed to be unity.

Case 1: Rich parents, leave bequests

In this case x is 'large' and parents leave transfers (bequests) t_i. Parents' problem is as follows:

$$\max_{h_i,t_i} \quad u(c_1, c_2) = (c_1^\rho + c_2^\rho)^{\frac{1}{\rho}} \tag{1}$$

$$\begin{aligned} &\text{s.t.} \\ x &= h_1 + t_1 + h_2 + t_2 \\ y_1 &= A\theta_1 h_1^d \quad , \quad y_2 = A\theta_2^b h_2^d \qquad (d < 1) \\ \frac{c_1}{1+r} &= \frac{y_1}{1+r} + t_1 \\ \frac{c_2}{1+r} &= \frac{y_2}{1+r} + t_2 \end{aligned}$$

The solution to this maximisation problem is such that parents invest in children's education until the marginal product of human capital, $\frac{\partial y_i}{\partial h_i}$, falls to the market rate of return, $1 + r$, so that education investment:

$$h_1 = \left(\frac{Ad\theta_1}{1+r}\right)^{\frac{1}{1-d}}, \qquad h_2 = \left(\frac{Ad\theta_2}{1+r}\right)^{\frac{1}{1-d}} \tag{2}$$

Any inequalities in investments and earnings are offset by parental transfers so that parents do not trade off efficiency and equity: the solutions for transfers are $t_1 = \frac{1}{2}(x - h_1 - h_2) + \frac{1}{2}\left(\frac{y_2 - y_1}{1+r}\right)$ and $t_2 = \frac{1}{2}(x - h_1 - h_2) + \frac{1}{2}\left(\frac{y_2 - y_1}{1+r}\right)$ and so c_1 and c_2 are equal.

Two clear, unambiguous predictions emerge from this model.

- First, inequality in human capital investment is determined by differences in endowments,

have been discussed in the literature on inter vivos transfers and bequests. Pollak (1989) postulates 'paternalitic' prefernces where parents care directly about the child's consumption set.

the higher ability child receiving greater investments[12]: $\frac{h_1}{h_2} = \left(\frac{\theta_1}{\theta_2}\right)^{\frac{1}{1-d}}$, so that

$$\ln h_1 - \ln h_2 = \frac{1}{1-d}\left(\ln \theta_1 - \ln \theta_2\right). \tag{3}$$

- Second, as equation (2) shows, own investment does not depend on sibling's endowment, so that $\frac{\partial h_j}{\partial \theta_k} = 0$.

Case 2: Poorer, credit constrained parents

The maintained assumption now is that x is small, such that the returns on education investment remain above the market rate of return, $1 + r$. In this case the total budget allocated to children, x, is not large enough to leave bequests, so that t_1 and t_2 are set to 0 in the parents' optimization problem. The solution for education investment is then:

$$h_1 = \frac{\alpha}{1+\alpha}x, \quad h_2 = \frac{1}{1+\alpha}x \qquad \text{where} \quad \alpha = \left(\frac{\theta_1}{\theta_2}\right)^{\frac{\rho}{1-d\rho}}. \tag{4}$$

So that the human capital investment ratio for the two siblings is $\frac{h_1}{h_2} = \left(\frac{\theta_1}{\theta_2}\right)^{\frac{\rho}{1-d\rho}}$, and in logs:[13]

$$\ln h_1 - \ln h_2 = \frac{\rho}{1-d\rho}\left(\ln \theta_1 - \ln \theta_2\right). \tag{5}$$

In this version of the model parents face an equity-efficiency tradeoff when investing in their children. The predictions now depend on the inequality aversion parameter and consequently are less clear cut than in the with-bequests version of the model (case 1 above). First, equation (4) demonstrates that sibling ability *does* influence own investment, though not if parental preferences are Cobb-Douglas (i.e. $\rho = 0$).

Second, whether the higher ability child receives more or less investment relative to the less able child depends on the inequality aversion parameter as follows:

1. If $\rho > 0$ then the higher ability child receives relatively more investment. For the case $0 < \rho < 1$ the more able child is allocated greater education investment, but not as much as in the maximum returns case ($\rho = 1$). When $\rho = 1$ the utility function becomes linear in children's consumption and parents act to maximise the return on their investment, regardless of distributional consequences - the marginal product of human capital is equalised for the two children. In this case investment inequality, as measured by $\frac{h_1}{h_2}$, is identical to that in Model 1.

[12] This translates into inequality in earnings as follows: $\frac{y_1}{y_2} = \left(\frac{\theta_1}{\theta_2}\right)^{\frac{1}{1-d}}$.

[13] These investments translate into the following earnings ratio: $\frac{y_1}{y_2} = \left(\frac{\theta_1}{\theta_2}\right)^{\frac{1}{1-d\rho}}$.

2. $\rho = 0$: Cobb-Douglas utility function. This leads to equal sharing of x amongst the siblings, i.e. $\frac{h_1}{h_2} = 1$.

3. $\rho < 0$: the higher ability child receives relatively *less* investment, so parents are said to compensate the less able child for his or her lower endowment. As $\rho \longrightarrow -\infty$, the Leontief case, earnings inequality is eliminated, so that $\frac{y_1}{y_2} \longrightarrow 1$ and $\frac{h_1}{h_2} \longrightarrow \left(\frac{\theta_1}{\theta_2}\right)^{\frac{-1}{d}}$.

The various solutions in the two models are illustrated in Figures 1a and 1b. These depict the marginal return to human capital curves and the equilibrium investment outcomes for high and low ability children. For the case where parents have sufficient resources to offset any inequalities in investments via transfers, parents invest in their children until the marginal return equals the market rate of return, $1 + r_0$; in Figure 1a the return-investment solutions are at points M and N for the low and high ability child respectively. For parents facing credit constraints, for the case $\rho = 1$, i.e. parents maximise returns, the optimal strategy is to invest until the marginal returns are equalised across siblings - the solution points are P and Q. For ρ less than 1, on the other hand, investment in the high ability child shifts leftwards on his own marginal returns curve and rightwards for the low ability child as ρ declines, as depicted by the arrows in Figure 1b. When $\rho = 0$ the two solution points are directly above each other, i.e. investments are shared equally by the high and low ability children. As ρ declines further the respective leftward and rightward shift continue, so that investments in the low ability child exceed those in the high ability one.

With the above model in the background it is useful to formally define compensating and reinforcing behaviour, before going on to summarise the model's empirical predictions.

Definition

Parents are said to *reinforce* for endowment differences if the marginal effect of a rise in own ability is bigger for own investment than it is for sibling's investment: $\frac{\partial h_j}{\partial \theta_j} - \frac{\partial h_k}{\partial \theta_j} > 0$. Further, if $\frac{\partial h_j}{\partial \theta_j} > 0$ and $\frac{\partial h_k}{\partial \theta_j} = 0$ then investments are said to be fully efficient.

Conversely, parents are said to *compensate* for endowment differences if the marginal effect of a rise in own ability on own investment is equal to or smaller than the effect on sibling's investment: $\frac{\partial h_j}{\partial \theta_j} - \frac{\partial h_k}{\partial \theta_j} \leq 0$.

The analysis above shows that rich households, who are not credit constrained, will invest in each of their children until it is no longer profitable. Any inequalities in education, and hence wage earnings, which arise as a consequence of this investment strategy are smoothed out by higher transfers to those who receive relatively low investments. Credit constrained 'poor' families, on the other hand, will devote all their resources to education investment, with none left over for transfers, assuming that at these relatively low levels of human capital investment returns remain high. In this sense education investment may be considered a necessity and transfers a luxury. How poor households allocate their investment expenditure will depend on their aversion to inequality. For rich households, although aversion to inequality influences transfers, it does not enter into the education investment decision.

3 Empirical Strategy

In order to identify reinforcing and compensating behaviour, the following linearised, two-sibling version of the education investment relationship (equation 4) can be estimated:

$$h_{ij} = X_{ij}\alpha_1 + X_i\alpha_2 + \beta_1 A_{ij} + \beta_2 A_{ik} + u_{ij} \tag{6}$$

where h_{ij} is family i's investment in child j; X_{ij} and X_i are a detailed set of family and child-specific covariates, such as parents' education and income, own sex and birth order; A_{ij} is the child's own ability and A_{ik} is sibling j's ability or IQ, measured by the Raven's test instrument. u_{ij} consists of all unobserved factors affecting outcome h_{ij}. A variety of outcomes are considered in the empirical section, including the highest grade attained (the traditional measure of schooling), the age at which the individual quit school and whether the child participated in income generating work during elementary school.

The parameters of interest here are the causal effects of own and sibling ability on h_{ij}. Under the zero conditional mean assumption, $E(u_{ij} \mid X_{ij}, X_i, A_{ij}, A_{ik}) = 0$, these are identified: ordinary least square estimation of equation (6) delivers consistent estimates of β_1 and β_2. A concern here is that u_{ij} and A_{ij} (as well as u_{ij} and A_{ik}) are correlated, and so OLS estimates of β_1 and β_2 are biased. This will be the case if unobserved family characteristics driving schooling outcomes are also correlated with ability. For example, if higher ability parents, producing higher ability children, also happen to be richer, have better access to funds for education or inculcate a greater desire for education, thereby reducing psychic costs of studying, then these other factors may be driving education outcomes, rather than the child's ability per se. In this case it is important to take account of such unobserved family background factors.

A potential solution is offered if the error term can be decomposed into a family component and a white noise component, so that $u_{ij} = f_i + v_{ij}$. Here f_i captures unobserved time-invariant family factors and it is assumed that $E(v_{ij} \mid X_{ij}, X_i, A_{ij}, A_{ik}, f_i) = 0$. Under this setup, taking differences across siblings and rearranging terms delivers the following model:

$$h_{ij} - h_{ik} = (X_{ij} - X_{ik})\alpha_1 + (\beta_1 - \beta_2)(A_{ij} - A_{ik}) + (v_{ij} - v_{ik}). \tag{7}$$

So now although β_1 and β_2 are not individually identified in the fixed effect model, we can determine $\beta_1 - \beta_2$. Focusing on $\frac{\partial h_{ij}}{\partial A_{ij}} - \frac{\partial h_{ik}}{\partial A_{ij}}$, the difference in the marginal effects of own ability on own and sibling investments, reinforcing and compensating behaviour is defined as follows: parents are said to reinforce when $\beta_1 - \beta_2 > 0$ and compensate when $\beta_1 - \beta_2 < 0$. Alternatively, this definition states that a marginal increase in the gap in children's endowments leads to a change in the gap in education outcomes favouring the more (less, respectively) able child when $\beta_1 - \beta_2 > 0$ ($\beta_1 - \beta_2 < 0$, respectively) and parental investments reinforce (compensate, respectively) for sibling differences.

So far the discussion has focused on the two siblings case, but in practice there may be three or more siblings in the estimation sample. When there are n ($\geqslant 2$) siblings in total, model (6) may be generalised for sibling k as follows:

$$h_{ik} = \beta_1 A_{ik} + \beta_2(A_{i1} + ... + A_{ik-1} + A_{ik+1} + ... + A_{in}) + u_{ik} \tag{8}$$

where the X's have been dropped to simplify the notation. This equation states that investment depends on own ability and on the sum of all siblings' abilities; the restriction here is that siblings' abilities are given equal weight, so that children are treated symmetrically. The within family deviations version of the model is as follows:[14]

$$h_{ik} - \overline{h_{i.}} = (\beta_1 - \beta_2)(A_{ik} - \overline{A_{i.}}) + v_{ik} - \overline{v_{i.}}. \tag{9}$$

Thus, the coefficient on A_{ik} in a fixed effect regression of h_{ik} on A_{ik} will yield an estimate of $\beta_1 - \beta_2$. As before, parents reinforce when $\beta_1 - \beta_2 > 0$ and compensate when $\beta_1 - \beta_2 < 0$.

Econometric Concerns

There are a number of potential objections to the strategy outlined above. First, the measure of ability, the Raven's test score, may be contaminated by schooling. In this case estimates for $\beta_1 - \beta_2$ may be upward biased. This suggests that confidence in claims of reinforcing behaviour on the basis of large, positive estimates for $\beta_1 - \beta_2$ may be justifiably weak. If on the other hand, estimates are close to zero or negative then, given the possible upward bias, there may be greater justification in claiming that this evidence supports the compensating behaviour hypothesis.

A second concern is that if ability is measured with error then the within family estimates will be subject to potentially large attenuation biases (on the assumption that measurement error is classical). As will be seen in the estimates presented below, the within estimates are frequently larger, in absolute terms, than the pooled OLS estimates of equation (6) under the restriction $\beta_2 = 0$, suggesting that measurement error may not be a first order concern. (This is the case for some of the estimates for samples stratified by household income, as well as for estimates of the health consequences of child labour.)

Finally, many of the outcomes are discrete and so discrete choice models may be preferred to the linear probability model adopted above.

4 Data and Initial Evidence

4.1 The Mexican Family Life Survey and Sample Selection

The Mexican Family Life Survey (MxFLS) is a nationally representative household survey, covering over 8,400 households in 150 communities across the whole of Mexico. The survey, undertaken

[14] To see this, note that $h_{ik} - \overline{h_{i.}} = \beta_1(A_{ik} - \overline{A_{i.}}) + \beta_2[A_{i1} + ... + A_{ik-1} + A_{ik+1} + ... + A_{in} - (n-1)\overline{A_{i.}}] + u_{ik} - \overline{u_{i.}}$ $= \beta_1(A_{ik} - \overline{A_{i.}}) + \beta_2[n\overline{A_{i.}} - A_{ik} - (n-1)\overline{A_{i.}}] + u_{ik} - \overline{u_{i.}}$

in 2002, brings together extremely detailed information on a wide range of social, economic, demographic and health behaviour of individuals and their families. Each member aged 15 and above is interviewed and, importantly for this study, provides information on current and retrospective schooling and employment. A detailed health instrument is also administered, as well as the Raven's Progressive Matrices test. For further details see Rubalcava and Teruel (2004).

In order to implement the sibling-based fixed effect models described in the previous section, information is required for multiple siblings on completed schooling outcomes (such as final grade attainment) and whether the individual participated in income generating work during childhood. For these reasons sample selection is on children aged 15 to 30 still residing in the household, and on households where there are at least two such children.[15]

Table 1 summarises the data for three sets of sub-samples employed in the empirical analysis. The first three columns report summary statistics for children aged 15 to 30 from households where there are at least two children in this age range residing in the household; the next three columns report statistics for children aged 15 to 30 *who have quit school* from households where there are at least two such children; the final three columns report statistics for male children aged 15 to 30 from households where there are at least two brothers in this age range. The first two sub-samples are used in the analysis on the role of ability in schooling. The last sub-sample is employed in the analysis on the health effects of child labour.[16]

The Raven's test score is standardised so that the mean is zero and variance unity within each year cohort for *all* individuals in the survey aged 15 to 21 and for the group aged 22 to 30. The summary statistics suggest that the sample selection rules generate sub-samples fairly representative of the population surveyed in the MxFLS: test scores range between 10% of a standard deviation below (column 5) and 9% of a standard deviation above (column 2) the survey average.

For the analysis of the effect of within family variation in ability on schooling and child labour outcomes, six outcomes, reported in Table 1, are considered: the highest grade attained; whether or not an elementary grade was repeated; age of enrolment into elementary school; age quit school; whether or not attended secondary school; and whether or not participated in income generating work during elementary school. Arguably a measure such as age of quitting school better captures parental investments than final grade attainment since the latter captures both parental investments and ability even if parents invest equally in high and low ability children.[17]

'Child labour' in this study refers to whether the individual worked or developed an activity to help with household expenditure whilst attending elementary school. I use the terms 'child labour'

[15] Such a wide age range is used to increase the sample size. It might be argued that children up to the age of 30 still residing with their parents are not a representative sample and so this selection rule may lead to biased results. In order to address this crticism, in the empirical analysis I also report results for the sample of siblings age 15 to 21.

[16] The reason for this last selection rule is that boys are much more likley than girls to be engaged in income generating work at a young age.

[17] For example, if parents use a rule such that each child quits at the same age and also treat children equally on other dimensions such as time use, the higher ability child will still have higher attainment becuase he or she is likley to repeat a grade.

and 'income generating work during elementary school' interchangeably. It should be noted that one limitation of the data is that information on work during childhood is available only for this variety of child labour. Non-market work, such as in household production, may also have long-term consequences on health. In addition, the exact nature of the income generating work during childhood is not reported.

The table also reports summary statistics for the seven health related outcomes used for the analysis on the long-term effects of child labour: whether the individual was ill in the last four weeks, coded 0 or 1; whether the individual suffers from one of seven acute morbidity conditions, coded 0 or 1; a rating, from 1 (very good) to 5 (very bad), of own health; rating of relative health, where 1 indicates that own health is much better than others and 5 indicates that it is much worse than others; whether the individual has had serious health problems during their life; an emotional wellbeing variable which is the mean response to 21 questions where a response of 0 represents never feeling sad, pessimistic, insecure, losing appetite, etc. in the last four weeks and 3 represents always feeling this way; finally, height, standardised to have mean zero and unit variance within each year-sex cohort for ages 15 to 21 and by sex for 22 to 30 year olds. Further details on exact definitions are provided in Appendix A.

4.2 Differences in Mean Outcomes For Sibling Pairs

This section presents simple statistics on the differences in various outcomes for high versus low ability siblings, providing some preliminary evidence on the reinforcing-compensating issue. The focus is on sibling pairs - this yields simple pairwise comparisons and so the gain in transparency relative to an analysis using multiple siblings ought to be worth the tradeoff in terms of the fall in the sample size (the regression results in the sub-section below employ the full sample).

Panel A in Table 2 presents for each outcome y, the mean value of the following family level differences: $y_{high\ ability\ child} - y_{low\ ability\ child}$. The results show that the average high-low ability gap in test scores is 0.88 of a standard deviation. This may seem very large but is in line with findings in the psychology literature. For example, Jensen (1982) notes that in the United Sates the gap in IQ test scores between siblings is 0.8 of a standard deviation. Panel A also shows that this test score difference is associated with a 7 percentage point lower probability of repeating elementary grade for the higher ability child and a gap of 0.35 of a grade in attainment. For other outcomes the mean difference does not appear to be statistically significant.

Panel B provides further evidence on this issue by stratifying the sample by education of the head. The mean of the difference between siblings in IQ test scores are broadly similar across the three types of household. The results for households where the head has no education show large and significant differences between the more and less able siblings for two key outcomes: the higher ability sibling attains a higher level of schooling (0.83 of a grade) and attends school for longer (0.67 of a year). In contrast, for less poor and richer households (head has primary or secondary schooling) the differences in attainment and quit age between the more and less able child are much smaller and insignificant.

These initial findings suggest that, large average differences in ability within households do not translate into higher attainment for the high ability child, except in the poorest families. In other words, most parents appear to be acting in a compensating manner. In the poorest households there does appear to be a positive relationship between within family ability differences and schooling outcomes.

5 Evidence on Parental Preferences: Ability Endowments and Schooling Outcomes

Before presenting the main findings in the next section, a useful starting point is the school and work participation model common in the literature, presented in Table 3. This reports pooled OLS estimates of equation (8) with the restriction that the coefficient on siblings' ability, β_2, is zero. As before, the outcomes are: the highest grade attained; the total number of years in school; whether or not the child undertook paid work whilst in elementary school; age at which the child enrolled into elementary school; age at which the child quit school; and whether or not the child attended secondary school. The sample consists of children aged 15-30 who have quit school from households where there are at least two such children.

The basic regressions in column (1) for each outcome exclude parental education and IQ test scores. The first result of note is that for each of these regressions the coefficients on the Raven's test score are statistically significant and economically meaningful, *except* in the paid work participation equation. These cross section results suggest that a one standard deviation rise in the test score is associated with: 0.62 of a grade higher attainment; a 9 percentage point reduction in the probability of repeating an elementary grade; 0.35 extra years in school; and a 9 percentage point rise in the probability of attending secondary school.

There may be countervailing forces at play in the paid work equation. If parents are forced to send a child out to work it may make sense to send the brighter one in order to maximise earnings. On the other hand, the human capital investment model, described above, suggests that the optimal strategy is to keep the brightest children in school.

Parental education dummies are added as controls in column (2) of Table 3 for each outcome. As expected, these generally have very large effects on outcomes: head education is important in nearly all of the outcomes whilst the impact of spouse education appears to be strong in the grade repetition and paid work equations, although these latter estimates are quite noisy.[18] Despite these strong effects, adding parental education dummies leads to relatively small declines in the absolute size of the test score coefficient estimates, suggesting that test scores do not simply capture unobserved family background characteristics. Adding head and spouse test scores, as well as parental education dummies in column (3) leads to a decline in the own test score coefficient

[18] Compare with previous findings - e.g. Attanasio et al (2006)..

estimates of 10 to 20 per cent, relative to the estimates in column (1).

5.1 Basic Results

Table 4 presents pooled OLS (panel A) and family fixed effect (panel B) estimates of the impact of the Raven's IQ test score on schooling and child labour participation outcomes. The pooled regressions estimate the own ability coefficient β_1 for the restricted version ($\beta_2 = 0$) of equation (8). Meanwhile, the within family regression recovers $\beta_1 - \beta_2$, the parameter describing reinforcing or compensating behaviour (see equation 9).[19] If the restriction $\beta_2 = 0$ holds, then the within family estimator yields β_1, as is usually assumed in sibling-based fixed effect models.

The pooled estimates in Table 4 tend to suggest that across the population, parents reinforce for ability. Thus, higher IQ of one standard deviation leads to higher attainment of 0.72 of a grade and a rise in the school quit age of 0.53 of a year.

The within estimates in panel B of Table 4 suggest that reinforcement is quite mild. For example, the point estimate suggests that a one standard deviation relative rise in own test score leads to higher attainment of 0.37 of a grade relative to siblings. This is quite modest when compared to the average level of attainment for this population (just over eight grades). Similar effects are obtained for schooling outcomes such as age quit school and the probability of attending secondary school. Although it is estimated with large error, the within family estimates also suggest a modest negative effect of ability on the childhood work participation outcome.

The pooled estimates are likely upward biased due to unobserved family-level heterogeneity, whilst the within estimates may be subject to downward bias if measurement error in ability is a significant issue. The true estimates may lie somewhere in between the two sets of results. Furthermore, both sets of results may be subject to upward bias if the IQ measure is contaminated by schooling. Overall, the message from Table 4 appears to be that large within family variation in IQ leads to relatively small differences in attainment: if parents do reinforce for endowment differences amongst their offspring, such reinforcing behaviour is fairly modest.

The Ability-Schooling Relationship by Family Income

Table 5 reports pooled estimates without (panel A) and with (panel B) siblings' test scores, as well as within family estimates (panel C), both for the full sample as well as for the sample stratified by education of the head. In the discussion below education of the head is adopted as a marker of the household's permanent income - the first group, where the household is headed by a parent with no education, is referred to as the poorest group and the third group, with head schooled to secondary or higher level, is referred to as the wealthiest set of households.

The results in Table 5 show stark variation in the role of IQ in determining outcomes across the different household types. For example, for the age quit school outcome the within estimates in

[19] The pooled regressions control for own age, gender, whether sibling is oldest, family characteristics such as parents' education and IQ test scores, as well as community dummies. The within family regressions include female, sibling is oldest and age dummies as controls.

panel C show that for the households where the head has secondary schooling or more, the impact of ability is very small and statistically insignificant, suggesting compensating behaviour. For the poorest households, on the other hand, own ability has a positive effect on the age at which the individual quits school.[20] For these households the within estimate of 0.487 years is comparable to the pooled estimate in panel A of 0.610 years. This last result suggests that the small within estimates for the wealthiest set of households is not simply driven by measurement error.

Similar patterns across the different types of households are observed for the probability of attending secondary school. On the other hand, the pattern is reversed for elementary grade repetition; but this result for the richer set of households does establish that relative ability clearly matters for some outcomes in this set of families.[21] For the outcome which encapsulates all of these measures, highest grade attained, it is clear that there is greater compensation (less reinforcement) for endowment differences in the richer households than in the poorer ones.

Griliches (1979) is the only other study I am aware of which carries out an analysis similar to the one undertaken here. His analysis is limited to the outcome years of attainment and he does not report whether the coefficients change with family characteristics. He finds that in the pooled sample of sibling pairs the impact of a one standard deviation rise in IQ raises schooling by between 1 and 1.6 years, depending on the data set used.[22] The within estimates suggest a much smaller association: between 0.4 and 0.9 years. Griliches (p.S62) states that: "The fact that the within-families coefficient is lower I interpret as evidence of attenuation. Families do not go all the way with the IQ differences among their children as far as their investment in human capital decisions is concerned." He also speculates (p.S62): "I would expect that within-family variance in socioeconomic achievement would decline at higher income levels." This is not borne out by the Mexican evidence presented here, at least as far as siblings' schooling attainment is concerned.

One other piece of evidence comes from Altonji and Dunn (1996). They find that higher IQ is not associated with higher quality of schooling within families in the US.

5.2 Robustness Tests

Ideally the data set for this study would include test scores and schooling information for all children, whether they reside in the household or not. Although such data will eventually be available with the second and third waves of the Mexican Family Life Survey, until that time sample selection rules such as the one employed above (restricting estimates to those households

[20] One might ask whether there is any evidence that the poorest parents invest so as to maximise returns. In order to answer such questions we may turn to the pooled regression estimates for β_1 and β_2 in model (8), reported in panel B of Table 4. The theoretical discussion above showed that parental preferences may be efficient when the marginal effect of a rise in own ability on own investment is positive and the effect is zero for sibling investment, i.e. $\beta_1 > 0$ and $\beta_2 = 0$. Panel B provides estimates for $\beta_1 > 0$ and β_2, but the rider is that the coefficinet on siblings' ability, β_2, may in fact capture the family fixed effect, as in Chamberlain's (1982) formulation of fixed effects model. The results in panel B suggest that the effect of a rise in own ability on own investment is positive, but the effect on siblings' investment is small and statistically insignificant from zero.

[21] The various schooling outcomes may be combined in a dynamic analysis - to be condsidered for future work - such as the Markov schooling transition model employed by Behrman et al (2005).

[22] Griliches (1979) employs three sibling pairs and two twins data sets, all from the US.

where there are at least two children residing in the household within a given age range) are necessary in order to perform within family estimations. In order to obtain a relatively large sample size a wide age range (15 to 30) is used in the preceding analysis. A possible criticism of this sample selection strategy is that children up to the age of 30 still residing with their parents are not necessarily representative of the population. Furthermore, if parents treat boys and girls differently on margins other than ability, then mixed sex siblings may not provide a good testing ground for reinforcing and compensating mechanisms.

In order to probe the robustness of the above results to these two issues, this sub-section reports results using alternative sample selection rules. The first experiment is to narrow the age range and select children where there are at least two children between the ages of 15 and 21 who have quit school from households where there are at least two such children. The results in Appendix Table A1 suggest that the previous results are qualitatively unchanged under the new sample. Indeed, the conclusion that richer parents compensate to a greater degree than poorer ones now appears to be even stronger: the pooled coefficient estimates for the IQ test score for the grade attainment outcome for the poorest and richest households are 0.82 and 0.21 respectively; the within estimates are 0.39 and -0.11 (the estimates are now much noisier due to the reduced sample size).

Table A2 reports estimates for the brothers only and sisters only samples (the age range is 15 to 30). The sample size is now much diminished, but it is clear from the results that estimates for brothers and sisters are broadly similar.

6 Evidence on Inequalities Arising From Within Family Variation in Child Labour Participation

The results in the previous section suggest that the tendency amongst parents to reinforce for endowment differences amongst their children is not strong. But still, inequalities in parental allocations will remain. In particular, there are substantial inequalities within the family on the child labour participation margin. For example, in the brothers sample used below, of the families where at least one brother worked as a child, in 80% of cases there is some variation within the family on this margin.[23] Even though parents may be averse to inequalities between their children, these inequalities may nevertheless be a natural by-product of, for example, fixed costs in the child labour participation decision. Parents may then be forced to pick and choose amongst their young children for child labour. The question then is do these differences in labour participation matter in the long run? If parents can take compensating action then it may be that any adverse consequences of working as a child are mitigated.

The evidence below suggests that there are large negative long-term health consequences for

[23] For this sample, in 4% of families all brothers worked to generate income for the household during elementary school; in 18% of families at least one brother worked whilst a second brother did not; and in the remaing 78% of families, none of the brothers worked.

the working child. Even if most parents are acting in a compensating manner along the IQ or ability dimension they are unable to undo the harmful effects of one of their children working in an income generating activity at a young age.

Using the retrospective question asked of all individuals of fifteen and above on whether they participated in an income generating activity during elementary school, this section investigates the long-term effects (at ages 15 to 30) of this type of work on health outcomes.[24] It also considers the association between child labour and schooling.

The strategy once again relies on sibling methods, this time to control for time-invariant unobserved family characteristics which may be correlated with the child labour decision. Before proceeding further, it should be noted that the sample selection for the analysis below is male siblings, since young daughters are much less likely to undertake paid work than sons.[25] Thus, in the within family estimates, the comparison is between brothers who participated in paid work during their childhood and those who did not.

6.1 The Long-Term Effects of Child Labour on Health Outcomes

For the brothers aged 15 to 30 sample, Table 6 reports pooled and within family estimates of the effect of undertaking income generating work during childhood for seven current health-related outcomes: whether the individual was ill in the last four weeks; whether the individual suffers from an acute morbidity condition; rating of own health; health relative to others; whether the individual has had serious health problems during their life; an emotional wellbeing variable; and height, standardised to have mean zero and unit variance. Further details of these variables are provided in Appendix A.

The first two measures may be considered 'hard' measures of health, since they ask about specific illnesses and conditions, whilst the remaining four (excluding height) are 'soft' measures since they are more subjective and there may be concerns about anchoring for these measures.

The key finding in Table 6 is apparent from each of the first columns of the within family regressions for 'acute morbidity' and 'ill last four weeks': the coefficients for participation in income generating work in both cases are large and statistically significant. These suggest that child labour leads to an 8.3 percentage point greater likelihood of having one of seven acute morbidity conditions and a 16.0 percentage point greater likelihood of having had one of fifteen illnesses in the recent past. These represent rises of 93 per cent and 28 per cent, respectively, relative to the means for these two outcomes reported in the last three columns of Table 1. Comparing the pooled and within family estimates for acute morbidity reveals the importance of controlling for unobserved family characteristics: the within estimate is over twice as large as the pooled estimate.

[24] As noted earlier, the Mexican Family Life Survey only collects this type of child work for individuals aged 15 and above.

[25] Only 4% of daughters aged 15-30 in the household undertook such work whilst the number is 11% for sons. The results for females (not reported) are in fact qualitatively similar to those for males, though the parameters are estimated with greater noise.

For the four subjective health measures, the coefficient on paid work is statistically significant in only one case: the sibling who undertook income generating work during elementary school scores worse on emotional wellbeing. For height, the within estimates, though noisy, suggest that siblings who worked during their childhood are about 5 per cent of a standard deviation taller in adolescence and adulthood than those who did not. Given the negative effects of health suggested by the other health outcomes, one explanation for this finding is that at the time the decision is made, parents select their *healthier* or fitter children for child labour.[26]

One econometric concern is that although the common family-level characteristics are controlled for in the within family regressions, the possibility remains that omitted individual factors correlated with child labour participation may in fact be driving the estimated effects on health outcomes. In order to account for some of this individual-level heterogeneity the regressions in the second column for each outcome in Table 6 include individual IQ test scores. The results in the previous section suggested that there is some, perhaps weak, relationship between ability and participation in child labour. If ability also determines health, then omitting it from the within family regressions may lead to upward biased estimates of the impact of child labour. In fact the results in Table 6 suggest that there is little change in the estimates of the impact of child work on health when ability is taken into account.

A further, and arguably more important, test of the importance of within family heterogeneity is provided by including controls in the regressions for the individual's occupational status and sector of employment. Apart from incorporating the effects of current work, these should also capture less easily measured factors such as greater parental care for one child over another, assuming these also determine adult labour market outcomes. The results are shown in the third and final column for each outcome in Table 6. These regressions now include eight current occupational status dummies as well as dummies for Mexican industrial classification codes for the individual's sector of employment. The work status dummies include whether the individual is in employment, whether he or she is a student and whether he or she is currently too sick to work. The results in Table 6 show that the previous conclusions are robust to the inclusion of these variables. Indeed, for the acute morbidity outcome, the coefficient on income generating work during childhood *rises* by over 10 per cent.

Further Robustness Tests

Table A3 in the Appendix reports estimates for the narrower age range of 15 to 21 year old brothers. Due to the lower cut-off point for age, the sample size is now halved and the standard errors are larger. The within family estimates for the coefficient on income generating work during childhood, both for the 'ill last four weeks' and 'acute morbidity' outcomes, appear to be in line with those reported in Table 6, although the former is now measured with substantially bigger

[26] Stratifying the sample by education of the head (results are not reported) suggests that there may be an upward gradient with respect to head's education in the adverse effects of child labour, though due to the small sample size, none of the coefficients are statistically different from zero.

error. For the other four health outcomes, child work appears to lead to worse health outcomes, but these estimates are not statistically significant. Overall, the results do not appear to be sensitive to the age at which the cut-off point is set for the sample selection rule.[27]

6.2 Child Labour and Education

The next set of results report estimates from regressions of education outcomes on child labour participation. Unlike the analysis of the effects on health outcomes, this exercise does not provide causal evidence on the effects of child labour, but rather it is a descriptive exercise which sheds light on the association between child labour and schooling.[28] Put another way, the question answered here is: What is the schooling outcome conditional on the individual participating in income generating work during childhood? As will be shown, the family fixed effect model once again proves itself useful in uncovering estimates substantially different from those where family-level unobserved heterogeneity is not accounted for.

Table 7 shows pooled and within family estimates for five schooling outcomes for the sample of brothers aged 15 to 30, where the brothers have all quit school. Though the child labour coefficients are estimated with large error, perhaps due to the small sample size, the results in Table 7 appear to tell a consistent story across the various schooling outcomes: first, participation in income generating work as a child is associated with relatively large (when compared with the effect of IQ test scores) losses in schooling; and second, it is important to take into account unobserved time invariant family characteristics – for three of the five outcomes (attainment, age quit school and whether or not the individual attended secondary school) the association is substantially larger for the within estimates than for the pooled estimates.

7 Conclusion

This paper develops a model of parental investment in children of varying ability. The model stresses the different tradeoffs faced by credit constrained and non-credit constrained households. The model predicts that richer parents can invest efficiently and offset inequalities in children's adult incomes via transfers. If poorer parents invest efficiently then the resulting income inequalities between children cannot be offset since parental transfers are limited or non-existent. Thus, if parents are averse to inequality amongst their children then poorer households will allocate more resources to the less able child, trading off some efficiency for greater equity. This simple model predicts that the ability-investment association is stronger in better off households, i.e. richer households probably reinforce more (and certainly no less) than poorer households.[29]

[27] Qualitatively similar results are obtained when the cut-off age is set to 18, although this time the standard errors are even larger.

[28] More so than adult health and child labour, schooling and child labour decisions are likely to be jointly determined.

[29] Another interpreation of this result is that rich parents are able to fulfil their children's full potential, whilst the lack of opportunity limits able children in poorer households.

These predictions are tested using Mexican household data. Using IQ test score measures as a direct measure of individuals' endowments, I find that even though there appear to be large differences in ability between siblings, these do not lead to large differences in schooling. There is no evidence of strong reinforcing behaviour. Furthermore, richer parents appear to engage in greater compensating behaviour than poorer ones.

I then investigate whether the large observed variation in child labour participation leads to any significant adverse effects for the sibling who works. If parents take compensating action, then it may be that any adverse effects are mitigated. More specifically, the question addressed is: What is the long-term health effect of participating in income generating work at a young age? The results show that there are large negative effects on health. Taken together, these results suggest that even though parents may compensate for endowment differences along some dimensions, at the end of the day they nay be forced to pick and choose amongst their offspring for some allocations, leading to large inequalities.

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Appendix A: Definitions of Health and Child Labour Variables

'Ill last four weeks' is a zero one dummy, coded one if the individual reports having any one of the following 15 conditions in the last four weeks: flu; cough; difficulty breathing; stomach ache; nausea or vomited; diarrhea; swollen joints; rash; irritated eyes; tooth ache; headache; fever; body ache; chest pain; and awoke with headaches.

'Acute morbidity' is a zero one dummy, coded one if the individual has any one of the following conditions: diabetes; hypertension; heart disease; cancer; rheumatoid arthiritis; gastric ulcer; migraine; or another, self-reported chronic illness.

'Rate own health' is the response to the question "Currently, would you say that you health is . . . ?" Options run from 1 (very good) to 5 (very bad).

'Relative health' is the response to the question: "If you compare yourself with people the same age and gender, would you say your health is . . . ?" Options vary between 1 (Much better than others) to 5 (Much worse than others).

'Had serious health problems' -is a zero one dummy, coded one if the response is Yes to the question: "Have you ever had any serious health problems during your life?"

'Emotional wellbeing' is the mean response to 21 questions where a response of 0 represents never feeling sad, pessimistic, insecure, losing appetite, etc. in the last four weeks and 3 represents always feeling this way.

The variable 'Income generating work during elementary school' is a yes / no response to the question: "Whilst attending elementary school, did you work or develop an activity to help with household expenditure?"

Figure 1a: Marginal return to human capital investment: equilibrium investments in high and low ability siblings

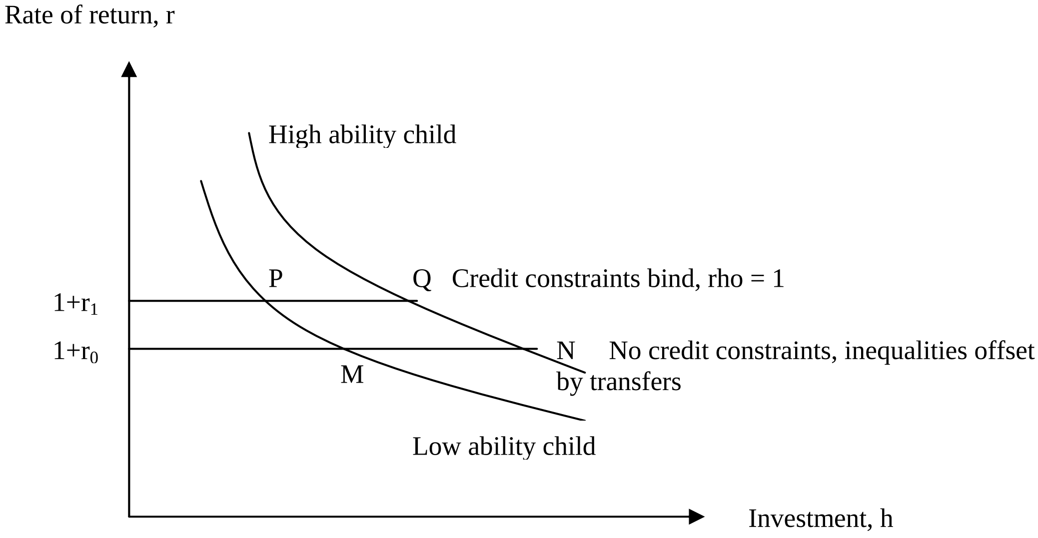

Figure 1b: The effect of increasing rho when credit constraints bind

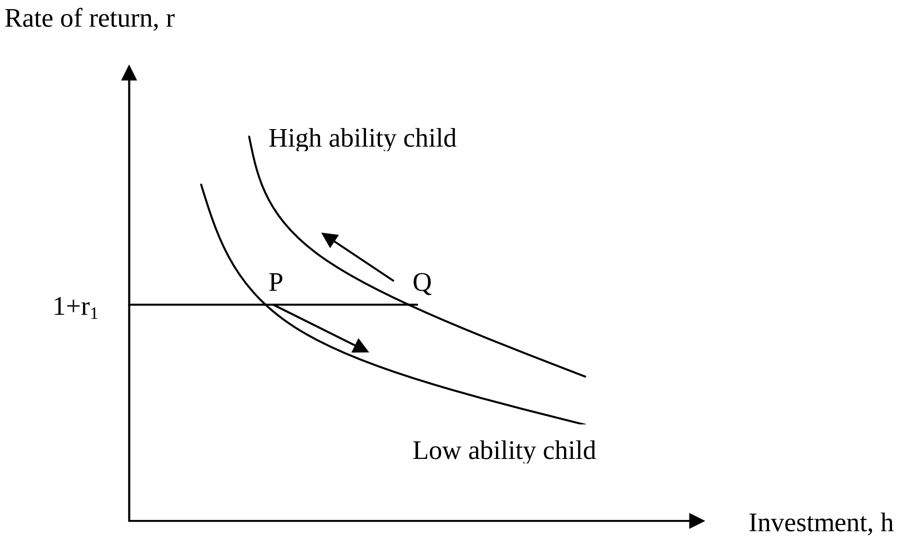

Table 1: Summary Statistics: Children Aged 15-30, Two or More Siblings Sample

	2+ children, age 15-30			2+ children, age 15-30 AND quit school			Brothers: 2+ male children, age 15-30		
	Observations (1)	Mean (2)	Standard error (3)	Observations (4)	Mean (5)	Standard error (6)	Observations (7)	Mean (8)	Standard error (9)
Age	2958	19.95	0.071	1389	21.14	0.1046	883	19.99	0.1266
Female	2958	0.503	0.0092	1389	0.478	0.0134	883	0.000	0
Schooling and work									
Currently enroled at school	2958	0.367	0.0089	1389	0.000	0	883	0.302	0.0155
Highest grade attained	2956	9.045	0.0496	1389	8.054	0.0729	883	8.558	0.0915
Repeated elementary grade	2958	0.296	0.0084	1389	0.354	0.0128	883	0.359	0.0162
Age at enrolment	2958	6.18	0.0166	1389	6.26	0.0266	883	6.22	0.0271
Age quit school	1820	15.71	0.0792	1389	15.42	0.0881	617	15.51	0.1315
Ever attended secondary school	2958	0.819	0.0071	1389	0.680	0.0125	883	0.776	0.014
Income generating work during elementary school	2958	0.070	0.0047	1389	0.090	0.0077	883	0.123	0.0111
Health									
Ill last four weeks (= 1 if had one of 15 illnesses)	2934	0.628	0.0089	1381	0.617	0.0131	877	0.580	0.0167
Acute morbidity (= 1 if had one of 7 conditions)	2596	0.121	0.0064	1217	0.116	0.0092	772	0.089	0.0103
Rate own health (1=very good; 5=very bad)	2934	2.277	0.0116	1381	2.368	0.0164	877	2.310	0.021
Relative health (1= Better than others; 5=Worse)	2934	2.627	0.0115	1381	2.657	0.0161	877	2.645	0.0204
Had serious health problems (0 / 1)	2934	0.125	0.0061	1381	0.103	0.0082	877	0.096	0.0099
Emotional wellbeing (0=Never gloomy; 3=Always)	2934	0.280	0.0056	1381	0.281	0.0086	877	0.213	0.0086
Height, IQ and family background									
Height (standardised)	2476	0.054	0.0196	1185	-0.046	0.0293	706	0.044	0.0349
Own Raven's test score (standardised)	2958	0.090	0.0184	1389	-0.100	0.0271	883	0.059	0.0343
Education of head = none	2958	0.146	0.0065	1389	0.199	0.0107	883	0.163	0.0124
Education of head = primary	2958	0.578	0.0091	1389	0.647	0.0128	883	0.605	0.0165
Education of head = secondary	2958	0.197	0.0073	1389	0.141	0.0093	883	0.232	0.0142
Education of head = college	2958	0.078	0.0049	1389	0.013	0.003	883	0.000	0
Education of spouse = none	2424	0.157	0.0074	1128	0.226	0.0125	734	0.183	0.0143
Education of spouse = primary	2424	0.615	0.0099	1128	0.676	0.0139	734	0.644	0.0177
Education of spouse = secondary	2424	0.180	0.0078	1128	0.090	0.0085	734	0.158	0.0135
Education of spouse = college	2424	0.049	0.0044	1128	0.007	0.0025	734	0.015	0.0045
Head standardised Raven's test score	2475	-0.053	0.02	1150	-0.191	0.0283	756	-0.143	0.0359
Spouse standardised Raven's test score	2284	-0.101	0.0198	1078	-0.275	0.0268	695	-0.227	0.0337
No. children in household age 0 to 14	2958	0.938	0.0236	1389	0.952	0.0368	883	1.014	0.0457
No. of households	1225			586			398		

Note: For columns 1 to 3 selection is on children aged 15-30 from households where there are at least two children in this age range residing in the household. For columns 4 to 6 selection is on children aged 15-30 who have **quit school** from households where there are at least two such children. In columns 7 to 9 selection is on **male children** aged 15-30 from households where there are at least two brothers in this age range. Before performing these selections, and in order to have a consistent sample for later regressions, for columns 1 to 9 observations are dropped if any of the following variables are missing: own Raven's test score, age at enrolment, paid work whilst in elementary school, education of head. In addition, observations for which school quit age is missing are also dropped for columns 4 to 6.

Table 2: Differences in Outcomes for High and Low Ability Siblings

Panel A: All Sibling Pairs

Outcome	Mean	Std. Error
Standardised test score	0.88	0.03
Highest grade attained	0.35	0.14
Repeated elementary grade	-0.07	0.03
Age at enrolment	-0.07	0.06
Age quit school	0.23	0.19
Attended secondary school	0.03	0.03
Income generating work during elementary school	0.003	0.020
Age	0.15	0.24
Female = 1	-0.02	0.04
(No. sibling pairs = 349)		

Panel B: By Education of Head

Outcome	Education of head (no. sibling pairs)	Mean	Std. Error
Standardised test score	None (69)	0.84	0.08
Standardised test score	Primary (214)	0.90	0.04
Standardised test score	Secondary (66)	0.86	0.08
Highest grade attained	None (69)	0.83	0.33
Highest grade attained	Primary (214)	0.26	0.17
Highest grade attained	Secondary (66)	-0.06	0.35
Repeated elementary grade	None (69)	0.06	0.08
Repeated elementary grade	Primary (214)	-0.12	0.04
Repeated elementary grade	Secondary (66)	-0.11	0.07
Age at enrolment	None (69)	-0.06	0.13
Age at enrolment	Primary (214)	-0.13	0.06
Age at enrolment	Secondary (66)	0.09	0.19
Age quit school	None (69)	0.67	0.43
Age quit school	Primary (214)	0.10	0.25
Age quit school	Secondary (66)	-0.03	0.46
Attended secondary school	None (69)	0.10	0.07
Attended secondary school	Primary (214)	0.00	0.04
Attended secondary school	Secondary (66)	0.00	0.04
Income generating work during elementary school	None (69)	0.00	0.05
Income generating work during elementary school	Primary (214)	-0.01	0.03
Income generating work during elementary school	Secondary (66)	0.06	0.04
Age	None (69)	0.45	0.59
Age	Primary (214)	0.20	0.30
Age	Secondary (66)	-0.29	0.46
Female = 1	None (69)	-0.03	0.08
Female = 1	Primary (214)	-0.01	0.05
Female = 1	Secondary (66)	-0.05	0.08

Note: This table shows the differences in Raven's test scores and other outcomes for the high versus low ability sibling. Sample selection is on children aged 15-30 who have quit school from households where there are exactly **two** such children. Before performing this selection, child observations are dropped if any of the following variables are missing: Raven's test score, age at enrolment, paid work whilst in elementary school, school quit age and education of head.

Table 3: Determinants of Schooling and Child Labour Participation Outcomes

Regressors	Grade Attainment			Elementary Grade Repetition			Age at Enrolment			Age Quit School		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Test score	0.851	0.769	0.724	-0.099	-0.095	-0.084	-0.103	-0.097	-0.091	0.632	0.544	0.532
	(0.071)	(0.070)	(0.070)	(0.013)	(0.013)	(0.014)	(0.025)	(0.026)	(0.025)	(0.083)	(0.083)	(0.087)
Female	0.420	0.453	0.447	-0.101	-0.104	-0.099	0.023	0.016	0.018	0.065	0.089	0.088
	(0.134)	(0.131)	(0.131)	(0.026)	(0.025)	(0.025)	(0.055)	(0.056)	(0.057)	(0.156)	(0.154)	(0.154)
Sibling is oldest = 1	-0.133	-0.201	-0.224	0.089	0.093	0.096	-0.038	-0.034	-0.024	-0.316	-0.387	-0.413
	(0.155)	(0.151)	(0.154)	(0.028)	(0.028)	(0.028)	(0.059)	(0.058)	(0.058)	(0.186)	(0.181)	(0.182)
No. children age 1-14	-0.222	-0.205	-0.212	-0.005	-0.008	-0.008	0.027	0.026	0.026	-0.069	-0.060	-0.065
	(0.067)	(0.065)	(0.066)	(0.012)	(0.012)	(0.012)	(0.027)	(0.026)	(0.026)	(0.073)	(0.073)	(0.074)
Education of head = primary		0.639	0.635		0.008	0.008		-0.050	-0.058		0.846	0.855
		(0.227)	(0.231)		(0.037)	(0.037)		(0.072)	(0.073)		(0.240)	(0.242)
Education of head = secondary		1.429	1.385		-0.043	-0.033		-0.024	-0.024		1.772	1.773
		(0.269)	(0.273)		(0.051)	(0.051)		(0.144)	(0.146)		(0.295)	(0.298)
Education of head = college		2.495	2.288		-0.107	-0.045		-0.245	-0.229		2.611	2.583
		(0.458)	(0.468)		(0.091)	(0.090)		(0.168)	(0.173)		(0.660)	(0.674)
Education of spouse = primary		0.375	0.339		-0.076	-0.065		-0.229	-0.225		-0.222	-0.249
		(0.233)	(0.238)		(0.041)	(0.040)		(0.087)	(0.085)		(0.272)	(0.275)
Education of spouse = secondary		0.786	0.727		-0.095	-0.063		-0.350	-0.340		0.104	0.084
		(0.344)	(0.351)		(0.063)	(0.062)		(0.134)	(0.130)		(0.390)	(0.394)
Education of spouse = college		2.516	2.496		-0.208	-0.191		-0.358	-0.349		2.243	2.222
		(0.452)	(0.461)		(0.136)	(0.141)		(0.404)	(0.394)		(0.744)	(0.733)
Head test score			0.165			-0.022			-0.027			0.032
			(0.092)			(0.017)			(0.026)			(0.110)
Spouse test score			0.020			-0.038			0.016			-0.020
			(0.103)			(0.018)			(0.033)			(0.122)

(table continues overleaf)

Table 3 (cont.)

	Attended Secondary School			Income generating work during elementary school		
	(1)	(2)	(3)	(1)	(2)	(3)
Test score	0.104	0.093	0.086	-0.002	-0.002	-0.001
	(0.013)	(0.013)	(0.013)	(0.008)	(0.008)	(0.008)
Female	0.040	0.046	0.045	-0.113	-0.113	-0.114
	(0.024)	(0.024)	(0.024)	(0.017)	(0.017)	(0.017)
Sibling is oldest = 1	-0.041	-0.050	-0.053	0.001	0.001	0.004
	(0.027)	(0.027)	(0.027)	(0.017)	(0.017)	(0.016)
No. children age 1-14	-0.054	-0.053	-0.053	0.008	0.009	0.009
	(0.012)	(0.012)	(0.012)	(0.007)	(0.007)	(0.007)
Education of head = primary		0.130	0.130		-0.022	-0.025
		(0.040)	(0.040)		(0.026)	(0.026)
Education of head = secondary		0.240	0.233		-0.004	-0.004
		(0.044)	(0.045)		(0.038)	(0.038)
Education of head = college		0.225	0.190		-0.002	-0.006
		(0.062)	(0.066)		(0.066)	(0.064)
Education of spouse = primary		0.045	0.041		-0.002	-0.008
		(0.041)	(0.042)		(0.028)	(0.028)
Education of spouse = secondary		0.080	0.070		-0.053	-0.065
		(0.057)	(0.059)		(0.035)	(0.036)
Education of spouse = college		0.086	0.083		-0.089	-0.097
		(0.060)	(0.062)		(0.054)	(0.056)
Head test score			0.028			-0.026
			(0.016)			(0.009)
Spouse test score			0.005			0.033
			(0.020)			(0.013)

Note: Standard errors, clustered at the family level, in brackets. Each regression also includes age, state and four community size dummies. Missing spouse education dummies are included in specifications (2) and (3). Missing head and spouse test scores are set to zero and missing dummies included in specification (3). Raven's test scores are standardized to have mean zero and unit variance. 'Grade attainment' refers to highest grade attained; 'elementary grade repetition', 'income generating work during elementary school' and 'attended secondary school' are all binary variables. The 'Sibling is oldest' dummy is turned on if the child is the eldest of the siblings included in the sample. Sample selection is on children aged 15-30 who have quit school and where there are **at least two such children** residing in the household. Before performing this selection, child observations are dropped if any of the following variables are missing: own test score, age at enrolment, paid work whilst in elementary school, education of head and school quit age. Sample size for all five regressions: 1,389.

Table 4: The Impact of Ability on Schooling and Child Labour Participation: Pooled and Within Family Estimates
(Sample: all children aged 15-30 who have quit school from families with two or more such children)

	Grade Attainment	Elementary Grade Repetition	Age at Enrolment	Age Quit School	Attended Secondary School	Income generating work during elementary school
Panel A: Pooled Estimates						
Test score	0.724	-0.084	-0.091	0.531	0.086	-0.001
	(0.070)	(0.014)	(0.025)	(0.087)	(0.013)	(0.008)
Number of children	1389	1389	1389	1389	1389	1389
Panel B: Within Family Estimates						
Test score	0.371	-0.067	-0.029	0.350	0.033	-0.008
	(0.081)	(0.019)	(0.032)	(0.115)	(0.018)	(0.011)
Number of children	1389	1389	1389	1389	1389	1389
Number of families	586	586	586	586	586	586

Note: Standard errors, clustered at the family level, in brackets. Each regression in Panel A includes female, sibling is oldest dummies, number of children age 0-14 in the household, head and spouse education dummies, head and spouse test scores as well as age, state and four community size dummies; missing head and spouse test scores are set to zero and missing dummies included; missing spouse education dummies are also included. The within family regression in Panel B include female, sibling is oldest and age dummies. Test scores are standardized to have mean zero and unit variance. 'Grade attainment' refers to highest grade attained; 'elementary grade repetition', 'attended secondary school' and 'Income generating work during elementary school' are all binary variables. Sample selection is as in Table 3.

Table 5: The Impact of Ability on Schooling and Child Labour Participation: Pooled and Within Family Estimates Stratified by Household Type

(Sample: all children aged 15-30 who have quit school from families with two or more such children)

	Grade Attainment				Elementary Grade Repetition				Age at Enrolment				Age Quit School			
		Head education				Head education				Head education				Head education		
	Full sample	None	Primary	Secondary and higher	Full sample	None	Primary	Secondary and higher	Full sample	None	Primary	Secondary and higher	Full sample	None	Primary	Secondary and higher
Panel A: Pooled																
Test score	0.724	0.856	0.710	0.453	-0.084	-0.034	-0.099	-0.077	-0.091	-0.073	-0.083	-0.179	0.531	0.610	0.492	0.486
	(0.070)	(0.168)	(0.087)	(0.186)	(0.014)	(0.033)	(0.017)	(0.036)	(0.025)	(0.065)	(0.026)	(0.097)	(0.087)	(0.189)	(0.115)	(0.227)
Panel B: Pooled																
Test score	0.654	0.800	0.648	0.464	-0.082	-0.038	-0.094	-0.076	-0.081	-0.049	-0.080	-0.182	0.500	0.595	0.471	0.501
	(0.065)	(0.153)	(0.083)	(0.181)	(0.014)	(0.035)	(0.017)	(0.035)	(0.024)	(0.065)	(0.026)	(0.099)	(0.087)	(0.197)	(0.113)	(0.215)
Siblings' test score	0.225	0.158	0.224	0.361	-0.006	0.012	-0.017	0.036	-0.036	-0.065	-0.011	-0.091	0.099	0.043	0.074	0.469
	(0.069)	(0.152)	(0.080)	(0.198)	(0.011)	(0.024)	(0.013)	(0.032)	(0.019)	(0.051)	(0.019)	(0.114)	(0.072)	(0.147)	(0.091)	(0.171)
Panel C: Within Family Estimates																
Test score	0.371	0.402	0.372	0.193	-0.067	-0.028	-0.068	-0.125	-0.029	0.117	-0.069	-0.086	0.350	0.487	0.323	0.085
	(0.081)	(0.184)	(0.101)	(0.227)	(0.019)	(0.052)	(0.024)	(0.049)	(0.032)	(0.095)	(0.036)	(0.087)	(0.115)	(0.251)	(0.144)	(0.253)
Number of children	1389	276	899	214	1389	276	899	214	1389	276	899	214	1389	276	899	214
Number of families	586	114	372	100	586	114	372	100	586	114	372	100	586	114	372	100

(table continues overleaf)

Table 5 (cont.)

	Attended Secondary School				Income generating work during elementary school			
		Head education				Head education		
	Full sample	None	Primary	Secondary and higher	Full sample	None	Primary	Secondary and higher
Panel A: Pooled								
Test score	0.086	0.133	0.076	0.027	-0.001	0.008	-0.006	0.012
	(0.013)	(0.032)	(0.017)	(0.035)	(0.008)	(0.021)	(0.010)	(0.029)
Panel B: Pooled								
Test score	0.072	0.118	0.065	0.028	-0.001	0.015	-0.007	0.013
	(0.013)	(0.032)	(0.017)	(0.034)	(0.009)	(0.022)	(0.010)	(0.028)
Siblings' test score	0.045	0.041	0.042	0.024	0.000	-0.022	0.004	0.019
	(0.011)	(0.026)	(0.014)	(0.027)	(0.006)	(0.018)	(0.006)	(0.017)
Panel C: Within Family Estimates								
Test score	0.033	0.052	0.031	-0.023	-0.008	0.026	-0.014	-0.017
	(0.018)	(0.045)	(0.022)	(0.037)	(0.011)	(0.029)	(0.013)	(0.032)
Number of children	1389	276	899	214	1389	276	899	214
Number of families	586	114	372	100	586	114	372	100

Note: Standard errors, clustered at the family level, in brackets. Each regression in Panel A and Panel B includes female, sibling is oldest dummies, number of children age 0-14 in the household, head and spouse education dummies, head and spouse test scores, age, state and four community size dummies; missing head and spouse test scores are set to zero and missing dummies included; missing spouse education dummies are also included. Regressions in Panel A include own Raven's test score but not (the sum of) sibling's test score; regressions in Panel B include both. The within family regression in Panel C include female, sibling is oldest and age dummies. Test scores are standardized to have mean zero and unit variance. 'Grade attainment' refers to highest grade attained; 'elementary grade repetition', 'attended secondary school' and 'Income generating work during elementary school' are all binary variables. Sample selection is as in Table 3

Table 6: The Effect of Child Labour on Long-term Health Outcomes: Pooled and Within Family Estimates

(Sample: male children – still at school as well as those who have quit school – aged 15-30 from families with two or more brothers in this age range)

	Ill last four weeks (=1 had if one of 15 illnesses)			Acute morbidity (= 1 if had one of 7 conditions)			Rate own health (1=very good; 5=very bad)			Relative health (1= Better than others; 5=Worse)		
Pooled estimates												
Income generating work during	0.138	0.135	0.144	0.038	0.036	0.048	0.169	0.165	0.173	0.122	0.120	0.124
elementary school	(0.050)	(0.050)	(0.052)	(0.036)	(0.035)	(0.036)	(0.062)	(0.062)	(0.065)	(0.067)	(0.067)	(0.069)
Test score		-0.032	-0.037		-0.017	-0.025		-0.052	-0.043		-0.023	-0.021
		(0.018)	(0.019)		(0.012)	(0.013)		(0.023)	(0.024)		(0.023)	(0.024)
Occupation and sector controls			YES			YES			YES			YES
Within estimates												
Income generating work during	0.160	0.163	0.164	0.083	0.079	0.089	0.009	0.005	0.024	0.077	0.074	0.069
elementary school	(0.078)	(0.078)	(0.082)	(0.041)	(0.041)	(0.041)	(0.087)	(0.088)	(0.091)	(0.097)	(0.098)	(0.098)
Test score		0.016	0.019		-0.018	-0.020		-0.022	-0.020		-0.012	-0.008
		(0.025)	(0.025)		(0.017)	(0.016)		(0.031)	(0.031)		(0.030)	(0.031)
Occupation and sector controls			YES			YES			YES			YES
Observations	878	878	878	771	771	771	878	878	878	878	878	878
Number of families	396	396	396	347	347	347	396	396	396	396	396	396

(table continues overleaf)

Table 6 (cont.)

	Had serious health problems (yes / no)			Emotional wellbeing (0=Never gloomy; 3=Always)			Height (standardised)		
Pooled estimates									
Income generating work during	0.058	0.058	0.057	0.134	0.132	0.135	0.027	0.031	0.005
elementary school	(0.035)	(0.035)	(0.035)	(0.030)	(0.030)	(0.031)	(0.106)	(0.104)	(0.107)
Test score		-0.005	-0.011		-0.025	-0.029		0.096	0.085
		(0.011)	(0.011)		(0.009)	(0.010)		(0.038)	(0.040)
Occupation and sector controls			YES			YES			YES
Within estimates									
Income generating work during	0.004	0.004	-0.005	0.103	0.102	0.089	0.053	0.056	0.027
elementary school	(0.047)	(0.047)	(0.045)	(0.038)	(0.039)	(0.039)	(0.108)	(0.108)	(0.114)
Test score		-0.001	-0.003		-0.007	-0.013		0.013	0.007
		(0.015)	(0.014)		(0.011)	(0.012)		(0.036)	(0.041)
Occupation and sector controls			YES			YES			YES
Observations	878	878	878	878	878	878	655	655	655
Number of families	396	396	396	396	396	396	298	298	298

Note: For each of the seven health outcomes this table shows results from three regressions each for the pooled and within family estimates. The first set of regressions, reported in the first column for each outcome, exclude test scores and the adult/adolescent's occupation and sector of employment dummies; the second regression adds test scores; the third regression adds test scores as well as occupation and sector dummies. Standard errors, clustered at the family level, in brackets. The pooled estimates include sibling is oldest dummy, number of children age 0-14 in the household, head and spouse education dummies, head and spouse test scores, age, state and four community size dummies; missing head and spouse test scores are set to zero and missing dummies included; missing spouse education dummies are also included. The within family regressions includes age and sibling is oldest dummies. Test scores are standardized to have mean zero and unit variance. For definitions of the health outcomes, see Appendix A. See notes to columns 7 to 9 in Table 1 for full details on the sample selection.

Table 7: Child Labour and Education Outcomes: Pooled and Within Family Estimates

(Sample: male children aged 15-30 who have quit school from families with two or more such children)

	Grade Attainment		Elementary Grade Repetition		Age at Enrolment		Age Quit School		Attended Secondary School	
Pooled estimates										
Income generating work during	-0.19	-0.27	0.09	0.10	0.11	0.12	0.09	0.03	-0.01	-0.02
elementary school	(0.37)	(0.35)	(0.06)	(0.06)	(0.15)	(0.15)	(0.37)	(0.36)	(0.07)	(0.06)
Test score		0.63		-0.10		-0.08		0.47		0.11
		(0.12)		(0.02)		(0.04)		(0.15)		(0.02)
Within estimates										
Income generating work during	-0.56	-0.54	0.04	0.03	0.16	0.16	-0.31	-0.28	-0.11	-0.11
elementary school	(0.36)	(0.35)	(0.09)	(0.09)	(0.22)	(0.21)	(0.51)	(0.52)	(0.08)	(0.08)
Test score		0.28		-0.08		-0.11		0.38		0.06
		(0.11)		(0.03)		(0.05)		(0.20)		(0.03)
Observations	478	478	478	478	478	478	478	478	478	478
Number of families	216	216	216	216	216	216	216	216	216	216

Note: Standard errors, clustered at the family level, in brackets. The pooled estimates include sibling is oldest dummy, number of children age 0-14 in the household, head and spouse education dummies, head and spouse test scores, age, state and four community size dummies; missing head and spouse test scores are set to zero and missing dummies included; missing spouse education dummies are also included. The within family regressions includes age and sibling is oldest dummies. Test scores are standardized to have mean zero and unit variance. All females are dropped before performing the sample selection as in Table 3.

Appendix Table A1: The Impact of Ability on Schooling and Child Labour Participation
Sensitivity Analaysis: Age 15-21 Sample
(All children aged 15-21 who have quit school from families with two or more such children)

	Grade Attainment				Elementary Grade Repetition				Age at Enrolment				Age Quit School			
		Head education				Head education				Head education				Head education		
	Full sample	None	Primary	Secondary and higher	Full sample	None	Primary	Secondary and higher	Full sample	None	Primary	Secondary and higher	Full sample	None	Primary	Secondary and higher
Panel A: OLS																
Test score	0.50	0.82	0.42	0.21	-0.09	-0.01	-0.11	-0.12	-0.09	-0.05	-0.11	-0.09	0.15	0.78	-0.03	-0.17
	(0.11)	(0.19)	(0.16)	(0.37)	(0.02)	(0.06)	(0.03)	(0.06)	(0.04)	(0.12)	(0.05)	(0.09)	(0.11)	(0.27)	(0.15)	(0.30)
Panel B: OLS																
Test score	0.46	0.84	0.38	0.41	-0.09	-0.01	-0.11	-0.09	-0.09	-0.07	-0.11	-0.14	0.14	0.77	-0.07	-0.09
	(0.11)	(0.16)	(0.15)	(0.35)	(0.02)	(0.06)	(0.03)	(0.06)	(0.04)	(0.11)	(0.05)	(0.10)	(0.11)	(0.27)	(0.15)	(0.30)
Siblings' test score	0.27	0.41	0.27	0.50	0.01	-0.00	-0.01	0.07	-0.04	-0.29	0.03	-0.11	0.09	-0.19	0.19	0.18
	(0.09)	(0.16)	(0.12)	(0.34)	(0.02)	(0.05)	(0.02)	(0.06)	(0.04)	(0.10)	(0.04)	(0.08)	(0.10)	(0.24)	(0.13)	(0.25)
Panel C: Within Family Estimates																
Test score	0.19	0.39	0.17	-0.11	-0.084	0.002	-0.090	-0.193	-0.03	0.23	-0.15	-0.01	0.16	0.98	-0.10	-0.33
	(0.13)	(0.23)	(0.17)	(0.35)	(0.032)	(0.075)	(0.040)	(0.067)	(0.05)	(0.12)	(0.07)	(0.07)	(0.15)	(0.29)	(0.20)	(0.27)
Number of children	593	126	370	97	593	126	370	97	593	126	370	97	593	126	370	97
Number of families	270	57	166	47	270	57	166	47	270	57	166	47	270	57	166	47

(table continues overleaf)

Table A1 (cont.)

	Attended Secondary School				Paid Work Whilst at Elementary School			
		Head education				Head education		
	Full sample	None	Primary	Secondary and higher	Full sample	None	Primary	Secondary and higher
Panel A: OLS								
Test score	0.07	0.14	0.06	0.01	0.01	0.01	0.00	0.02
	(0.02)	(0.05)	(0.03)	(0.07)	(0.01)	(0.03)	(0.02)	(0.03)
Panel B: OLS								
Test score	0.06	0.14	0.05	0.03	0.01	0.00	0.00	0.02
	(0.02)	(0.05)	(0.03)	(0.07)	(0.01)	(0.03)	(0.02)	(0.03)
Siblings' test score	0.06	0.10	0.06	0.06	0.00	-0.02	0.01	0.02
	(0.02)	(0.04)	(0.03)	(0.05)	(0.01)	(0.03)	(0.01)	(0.02)
Panel C: Within Family Estimates								
Test score	0.02	0.05	0.02	-0.03	0.00	0.02	-0.01	0.00
	(0.03)	(0.07)	(0.04)	(0.06)	(0.02)	(0.03)	(0.02)	(0.03)
Number of children	593	126	370	97	593	126	370	97
Number of families	270	57	166	47	270	57	166	47

Note: Standard errors, clustered at the family level, in brackets. Each regression in Panel A and Panel B includes female, sibling is oldest dummies, number of children age 0-14 in the household, head and spouse education dummies, head and spouse test scores, age, state and four community size dummies; missing head and spouse test scores are set to zero and missing dummies included; missing spouse education dummies are also included. Regressions in Panel A include own Raven's test score but not (the sum of) sibling's test score; regressions in Panel B include both. The within family regression in Panel C includes age, female and sibling is oldest dummies. Test scores are standardized to have mean zero and unit variance. 'Grade attainment' refers to highest grade attained; 'elementary grade repetition', 'attended secondary school' and 'paid work whilst at elementary school' are all binary variables. Sample selection is as in Table 3, with the exception that selection is on siblings aged 15-21 in this table.

Appendix Table A2: The Impact of Ability on Schooling and Child Labour Participation
Sensitivity Analysis: Brothers Only and Sisters Only Samples
(Sample: all children aged 15-30 who have quit school from families with two or more such children)

	Grade Attainment	Elementary Grade Repetition	Age at Enrolment	Age Quit School	Attended Secondary School	Income generating work during elementary school
Panel A: Brothers						
Pooled Estimates						
Test score	0.627	-0.093	-0.081	0.467	0.107	0.019
	(0.118)	(0.024)	(0.039)	(0.146)	(0.024)	(0.018)
Number of children	482	482	482	482	482	482
Panel B: Brothers						
Within Family Estimates						
Test score	0.291	-0.075	-0.111	0.388	0.057	-0.009
	(0.109)	(0.029)	(0.051)	(0.202)	(0.029)	(0.022)
Number of children	482	482	482	482	482	482
Number of families	218	218	218	218	218	218
Panel C: Sisters						
Pooled Estimates						
Test score	0.630	-0.068	-0.076	0.501	0.058	-0.004
	(0.123)	(0.028)	(0.054)	(0.167)	(0.023)	(0.014)
Number of children	415	415	415	415	415	415
Panel D: Sisters						
Within Family Estimates						
Test score	0.397	-0.115	0.054	0.428	0.038	-0.005
	(0.179)	(0.037)	(0.076)	(0.210)	(0.034)	(0.020)
Number of children	415	415	415	415	415	415
Number of families	188	188	188	188	188	188

Note: Standard errors, clustered at the family level, in brackets. Each regression in Panel A includes sibling is oldest dummies, number of children age 0-14 in the household, head and spouse education dummies, head and spouse test scores as well as age, state and four community size dummies; missing head and spouse test scores are set to zero and missing dummies included; missing spouse education dummies are also included. The within family regression in Panel B include sibling is oldest and age dummies. Test scores are standardized to have mean zero and unit variance. 'Grade attainment' refers to highest grade attained; 'elementary grade repetition', 'attended secondary school' and 'Income generating work during elementary school' are all binary variables. Sample selection is as in Table 3, with the exception that only one sex is kept before performing the selection.

Appendix Table A3: The Effect of Child Labour on Long-term Health Outcomes
Sensitivity Analysis: Age 15-21 Sample
(Sample: male children – still at school as well as those who have quit school – aged 15-21 from families with two or more brothers in this age range)

	Ill last four weeks (=1 had if one of 15 illnesses)			Acute morbidity (= 1 if had one of 7 conditions)			Rate own health (1=very good; 5=very bad)			Relative health (1= Better than others; 5=Worse)		
Pooled estimates												
Income generating work during	0.084	0.082	0.099	0.044	0.041	0.066	0.119	0.117	0.173	0.117	0.115	0.096
elementary school	(0.076)	(0.077)	(0.079)	(0.046)	(0.045)	(0.047)	(0.088)	(0.088)	(0.097)	(0.088)	(0.089)	(0.088)
Test score		-0.043	-0.052		-0.025	-0.031		-0.024	-0.009		-0.034	-0.045
		(0.026)	(0.029)		(0.015)	(0.017)		(0.035)	(0.035)		(0.037)	(0.037)
Occupation and sector controls			YES			YES			YES			YES
Within estimates												
Income generating work during	0.171	0.176	0.178	0.095	0.091	0.115	0.088	0.092	0.120	0.180	0.180	0.132
elementary school	(0.130)	(0.132)	(0.136)	(0.057)	(0.058)	(0.063)	(0.134)	(0.134)	(0.139)	(0.119)	(0.119)	(0.127)
Test score		0.030	0.027		-0.028	-0.019		0.019	0.032		-0.002	-0.008
		(0.034)	(0.037)		(0.020)	(0.022)		(0.045)	(0.047)		(0.044)	(0.040)
Occupation and sector controls			YES			YES			YES			YES
Observations	457	457	457	404	404	404	457	457	457	457	457	457
Number of families	217	217	217	191	191	191	217	217	217	217	217	217

(table continues overleaf)

Table A3 (cont.)

	Had serious health problems (yes / no)			Emotional wellbeing (0=Never gloomy; 3=Always)			Height (standardised)		
Pooled estimates									
Income generating work during	0.048	0.048	0.043	0.086	0.086	0.085	0.285	0.282	0.252
elementary school	(0.048)	(0.048)	(0.049)	(0.036)	(0.036)	(0.038)	(0.125)	(0.121)	(0.139)
Test score		0.002	-0.013		-0.005	-0.010		0.107	0.108
		(0.013)	(0.013)		(0.011)	(0.012)		(0.048)	(0.053)
Occupation and sector controls			YES			YES			YES
Within estimates									
Income generating work during	0.104	0.104	0.077	0.034	0.035	0.015	0.157	0.160	0.137
elementary school	(0.066)	(0.066)	(0.068)	(0.048)	(0.049)	(0.046)	(0.150)	(0.151)	(0.169)
Test score		0.003	-0.013		0.002	-0.004		0.015	0.005
		(0.016)	(0.016)		(0.014)	(0.015)		(0.048)	(0.057)
Occupation and sector controls			YES			YES			YES
Observations	457	457	457	457	457	457	355	355	355
Number of families	217	217	217	217	217	217	169	169	169

Note: Standard errors, clustered at the family level, in brackets. For each of the seven health outcomes this table shows results from three regression each for the pooled and within family estimates. The first regression in each case excludes test scores and the adult/adolescent's occupation and sector of employment dummies; the second regression adds test scores; the third regression adds test scores as well as occupation and sector dummies. The pooled estimates include sibling is oldest dummy, number of children age 0-14 in the household, head and spouse education dummies, head and spouse test scores, age, state and four community size dummies; missing head and spouse test scores are set to zero and missing dummies included; missing spouse education dummies are also included. The within family regressions includes age and sibling is oldest dummies. Test scores are standardized to have mean zero and unit variance. For definitions of the health outcomes, see Appendix A. See note to columns 7 to 9 in Table 1 for full details on the sample selection, the only difference being that selection is on brothers aged 15 to 21 in this table.