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This paper analyzes the strategies employed by households in rural China to allocate educational expenditure to children of different endowment, examining whether parents use educational funding to reinforce or compensate for variation in endowment. Employing climatic shocks as an instrument for children's endowment yields results indicating that parental expenditure is preferentially directed to children of lower endowment. This result appears robust to the potentially confounding effects of gender and grade level and holds across a number of measures of expenditure. This analysis is consistent with a hypothesis that parents use the allocation of household resources to compensate for differences in endowment among their children.

1 Introduction

For decades, social scientists have analyzed the decisions households make about human capital accumulation and the implications of such decisions for individual outcomes. The majority of an individual's education occurs in childhood and it is particularly important to understand the decisions that parents make about education on behalf of their children. In multichild families, this decision entails not only identifying resources for education in the form of money or parental time, but allocating those resources among multiple children. The process by which these decisions are made remains poorly understood.

This paper provides evidence about the parental allocation of resources for education among children of varying endowments. The research question of interest is whether parents employ a compensatory or a reinforcing strategy in responding to variations in endowment among their children. Direct estimation of the relationship between allocations of education and endowment poses serious challenges, as any measurement of relative endowment of children will include a component of endogenous parental nurturing.

The principal methodological contribution of this paper is to address the endogeneity of children's measured ability by employing as an instrument a measure of exogenous variability of resources correlated with nutritional status and physical endowment. The instrument used is rainfall and grain yield in infancy for each child, an index of nutritional availability during a critical period of childhood development that substantially determines physical endowment. There is a broad consensus in the existing medical literature that malnutrition in the first years of life, particularly the prenatal period and between birth and age three, has a substantial negative impact on physical and cognitive development (Mendez & Adair, 1999; Mallucio *et al.*, 2005; Grantham-McGregor *et al.*, 1991; Husaini *et al.*, 1991; Pollitt, 1988; Pollitt *et al.*, 1999; Waber *et al.*, 1981; Pollitt & Oh, 1994; Glewwe & King, 2001; Grantham-McGregor & Ani, 2001; Levitsky & Strupp, 1995; Brockman & Ricciuti, 1971; Grantham-McGregor, 1995; Freeman *et al.*, 1980; Scrimshaw, 1998; Wachs, 1995; Emond *et al.*, 2007). Shocks to a child's nutritional intake in this period are correlated with endowment, but exogenous to other intrahousehold decision-making processes given household fixed effects that absorb shocks to the household's overall budget constraint.

The results show a clear pattern of spending allocations favoring the child with lower endowment, consistent with a parental preference for equality that seeks to compensate for variation in endowment induced by early childhood environmental shocks. This pattern of preferential allocations holds across multiple measures of expenditure, and is robust to the inclusion of gender and sibling parity. In this geographic context, educational spending seems to be employed as a tool to neutralize differences in endowment at birth that would a priori be correlated with differences in expected income between children.

Previous literature examining intrahousehold allocation of resources to offspring has largely focused on the question of differential allocation to male versus female children, with a substantial literature establishing a pattern of preferential allocations to male children in both south and east Asia (Chen *et al.*, 1981; Hazarika, 2000; Pakrasi & Halder, 1971; Sen, 1988; Sen & Sengupta, 1983; Waldron, 1987; Bardhan, 1974; Behrman & Deolalikar, 1990; Ono, 2004; Rosenzweig & Schultz, 1982). Other studies have examined the impact of the sex ratio of siblings on a child's education, finding that a child with more sisters has better health and education outcomes than one with more brothers (Garg & Morduch, 1998; Morduch, 2000), though the inverse relationship appears to hold in the United States (Butcher & Case, 1994). A separate literature has focused on the relationship between birth order and the intrafamily distribution of resources (Lindert, 1977; Horton, 1988; Parish & Willis, 1993; Tenikue & Verheyden, 2007; Bommiere & Lambert, 2004).

A much smaller literature has analyzed whether or not parents have a general preference for equality among their children. An early paper by Griliche presented evidence that parents attempted to limit intrafamily equality and attenuate preexisting endowment differences, noting that the effect of IQ on schooling is significantly lower within families (Griliche, 1979). Similarly, Sheshinski and Weiss argue that interactions between a child's schooling and his or her sibling's endowment via the channel of differential parental allocations could render attempts to use sibling data to estimate returns to education invalid, but did not extend this insight to estimate the relationship between allocations of education and endowment (Sheshinski & Weiss, 1982). Behrman, Pollak and Taubman examine familial allocations using twin data from the U.S. and reject the pure investment model in which parents care only about the total return to educational expenditure, though only by imposing extensive functional form assumptions on the parental welfare function (Behrman *et al.*, 1982). Using developing country data, Rosenzweig and Wolpin find that parents in Colombia attempt to compensate for the disadvantages suffered by children with lower weight at birth by a longer interval prior to the birth of the next child, though there is contravening evidence that healthier children are more often breastfed (Rosenzweig & Wolpin, 1988). Behrman finds evidence in India of a pro-male bias as well as parental inequality aversion, though such aversion declines in the lean season (Behrman, 1988).

This paper contributes to the existing literature on parental intrahousehold allocation while making several new contributions. It is the first study to estimate the response of parental allocations to endowment as well as gender differentials among children, and the first to employ exogenous climatic variation in an identification strategy to address the endogeneity of measured endowment. This paper proceeds as follows. Section 2 outlines the theoretical framework for analyzing intrahousehold allocation. Section 3 presents the data. Sections 4 and 5 present the main empirical results and robustness checks. Section 6 analyzes the relative effectiveness of educational expenditure for children of different endowment, testing the hypothesis that parental allocation decisions are driven by the desire to maximize returns to educational spending. The final section concludes.

2 Modeling intrahousehold allocation of education

Assume that parents have two children and obtain utility from the welfare of each child as measured by his or her expected lifetime income. The parental utility function is a weighted sum of total income earned by both children and income earned by the poorer child, with the latter term capturing a parental preference for equality. The parameters θ_α and θ_β index the relative importance to parental utility of total income accruing to their family and the income accruing to the worst-off child, respectively.

$$U^i = \theta_\alpha(Y_1 + Y_2) + \theta_\beta \min \ \{Y_1, Y_2\} \tag{1}$$

Parents determine their children's expected lifetime earnings by allocating funds for education, provided at a price P_e presumed to be equal across children. Income is assumed to be an additively separable function of both education and endowment (denoted W) increasing in both arguments, such that $\frac{\partial Y}{\partial E \partial W} = 0$ and $\frac{\partial Y}{\partial E} > 0$ and $\frac{\partial Y}{\partial W} > 0$.

$$Y_i = f(E_i) + g(W_i) \tag{2}$$

In this framework, the parents' optimization problem conditional on previous investments can be written as follows:

$$max\ U(Y_1(E_1, W_1), Y_2(E_2, W_2)) \quad s.t. \tag{3}$$

$$(E_1 + E_2)P_e \leq W \tag{4}$$

$$Y_i = f(E_i, W_i) \tag{5}$$

$$E_i \geq 0 \tag{6}$$

2.1 Parental allocations in an investment model

Assume that $\theta_\beta = 0$ and thus parents seek to maximize only total income earned by both children. The solution is described by the following first-order conditions.

$$\frac{\partial Y_1}{\partial E_1} - \lambda + \mu_1 = 0 \tag{7}$$

$$\frac{\partial Y_2}{\partial E_2} - \lambda + \mu_2 = 0 \tag{8}$$

$$W - E_1 - E_2 \geq 0, \lambda \geq 0 \tag{9}$$

$$E_1 \geq 0, \mu_1 \geq 0 \tag{10}$$

$$E_2 \geq 0, \mu_2 \geq 0 \tag{11}$$

Given the assumption that Y_i is strictly increasing in E_i, the budget constraint will always be binding; the non-negativity constraints may, however, be binding. If $\mu_i > 0$ and $\mu_j = 0$, then $E_i = 0$. From (7) and (8), $\frac{\partial Y_j}{\partial E_j} > \frac{\partial Y_i}{\partial E_i}$, with $E_j > E_i = 0$. Thus corner solutions obtain when returns to education are convex, and one randomly chosen child receives all the education.

If the condition for a corner solution is not satisfied, the interior solution is defined by the first-order condition $\frac{\partial Y_1}{\partial E_1} = \frac{\partial Y_2}{\partial E_2}$, and this holds only when returns to education are concave. Given the assumption that returns to education are equal across children, optimization given concave returns entails equal allocations of education. If returns to education are linear, any allocation of education equalizes returns and satisfies the condition for optimality.

2.2 Parental allocations in an equality-preferring model

Now assume that $\theta_\alpha = 0$ and parents seek only to maximize income earned by the poorer child. Utility is maximized where $Y_1 = Y_2$. If the children are equally able, $E_1 = E_2$. If child 1 is assumed to be more able, satisfaction of the condition of equality in incomes requires $E_2 > E_1$ in order to counterbalance the higher expected income of the more able child.

2.3 Parental allocations in a hybrid model

Now assume that $\theta_\alpha > 0$ and $\theta_\beta > 0$. In this case, the outcome is indeterminate. The interior solution, if it exists, is defined by the following first-order condition.

$$\theta_\alpha f'(E_1) = (\theta_\alpha + \theta_\beta) f'(E_2) \tag{12}$$

Here, E_1 is the allocation of education to the child with the greater endowment. There is also a corner solution in which all education is provided to a single child. The optimality of either solution is determined by the degree of convexity in returns to education and the relative magnitudes of the θ parameters indexing the preference for maximizing aggregate income versus equalizing income between the two children.

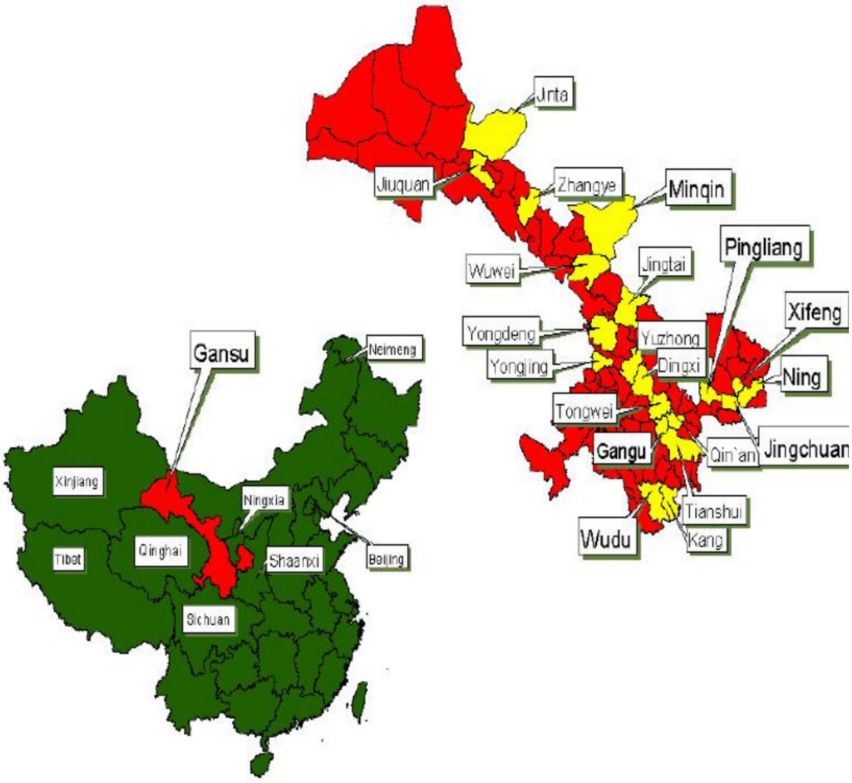
In the stylized modeling framework used here, a parental utility function that maximizes total income is consistent with full specialization in one child or equal allocations to both, while a pure parental preference for equality entails allocating more education to the child with the lower endowment. Intermediate preferences between the two extreme cases are compatible with a range of different allocations.

3 Data: Gansu Survey of Children and Families

The data set employed in this analysis is the Gansu Survey of Children and families (GSCF), a panel, multi-level study of rural children's welfare outcomes conducted in Gansu province, China. The first wave, conducted in 2000, surveyed a representative sample of 2000 children in 20 rural counties aged 9-12 as well as their mothers, household heads, teachers, principals, and village leaders. The second wave, implemented in 2004, supplemented the first wave with a sample of the younger siblings and fathers of the target children. Map 1 shows Gansu province and the survey counties.

Gansu, located in northwest China, is one of the poorest and most rural provinces in China. In 2005, per capita income in rural areas was 1979.88 yuan, or less than \$250; this was the second-lowest level of rural per capital income in China. 70% of the provincial population lived in rural areas (National Bureau of Statistics of the PRC, 2006). Fertility rates are relatively low, with the average size of a family household 3.97 persons (UN-ESCAP, 2002). The decline in fertility has generated an extremely high male-to-female sex ratio as traditional preferences for a son have led to increased sex-selective abortion, female abandonment and the underreporting of girls. The sex ratio in Gansu in 2000 was 111.2, close to the national average of 113.6 (Banister, 2004).

This analysis will focus on a subsample of the families in the survey: those with two



Map 1. Gansu Province, GSCF Counties Marked

observed children. Here, the child aged 9-12 identified in the first round of the survey is referred to as the index child; in families where the index child had a younger sibling of school age, that child was surveyed in the second round. If these two children are the only children in the household, this constitutes a complete survey of parental allocations and child endowment, and these households are included in the analysis. Such complete data is available on 579 families, and they constitute the relevant subsample. Comparing the subsample to the overall sample in table 1, no statistically significant difference is apparent in net income, per capita income, or parental education. The only significant difference between the two samples is in parental age: parents in the subsample are younger, reflecting the exclusion of households with larger numbers of children who will generally be headed by older parents.

The dependent variable of interest is educational expenditure per child per semester, reported by the mother in six categories: tuition, educational supplies, food consumed in school, transportation and housing, tutoring and other fees. In the Chinese educational context, supplies, tutoring, and other fees correspond to discretionary expenditure employed to raise a child's performance. Expenses for transportation and housing and food, on the other hand, are more likely to vary in accordance with the choice of school, and particularly the choice to have a child board at school or not. Discretionary expenditure

Table 1: Summary statistics

Demographic data			Educational expenditure per child			
	Sample	Subsample		Mean	Std. Dev.	Max.
Net income	7297.35	7459.49	Total	291.78	384.00	7760
Income per capita	1825.92	1850.27	Discretionary	112.32	205.50	2240
Father educ.	6.84	6.84	Tuition	179.47	228.56	6000
Mother educ.	4.17	4.17	Supplies	39.43	43.39	600
Father age	38.6	35.1	Transport / Housing	13.16	48.86	600
Mother age	35.2	33.0	Food	42.3	127.99	1700
Index child age	11.0	11.1	Tutoring	7.20	35.17	1000
Height-for-age	-1.18	-1.20	Other fees	10.21	30.78	420
Num. of obs.	1918	596		596		

is defined as the sum of all expenditure excluding tuition. Summary statistics for average expenditure per child for the subsample of families analyzed can be found in table 1. Total educational expenditure averages slightly less than 300 yuan per child per semester, suggesting an average of 1160 yuan for two children over a year. This indicates that an average of 15% of mean household income is allocated to educational expenses.

The measurement of the child's endowment is height-for-age, normalized to a Z-score using the World Health Organization growth charts for children of ages 2-18. Height-for-age is widely used in the literature as a measure of endowment and a summary indicator of physical robustness, and it is correlated with a range of physical and cognitive indicators (Mosley & Chen, 1984; World Health Organization, 1995; Grantham-McGregor *et al.*, 2007; Moock & Leslie, 1986; Glewwe & Jacoby, 1995; Glewwe *et al.*, 2001; Mendez & Adair, 1999). At the same time, evidence suggests it largely reflects the history of nutrition or health prior to age 3, as after this age catch-up for a child stunted in infancy is limited. (Martorell, 1995; Martorell, 1999; Alderman *et al.*, 2006; Hoddinott & Kinsey, 2001). Accordingly, a robust relationship between height-for-age and early childhood shocks is expected. Summary statistics on height-for-age in the sample and the subsample are shown in table **??**.

The primary data is supplemented by climatic data for Gansu. Grain yield data pre-1996 is from data generated by the Ministry of Agriculture; grain yield post-1996 is drawn from annual editions of the Rural Chinese Statistical Yearbook. Grain yield is measured annually at the county level in tons per hectare. Rainfall data is from the data collected by the the Carbon Dioxide Information Analysis Center and is measured in mean millimeters per month. Data at the station level is interpolated to generate county-level measures using the inverse distance weighting measurement. Each county's measurement is calculated as the weighted average of measurements at stations within 100 kilometers of distance; the weights are equal to the distance between the station and the county divided by the sum of distances.

4 Empirical evidence

4.1 Ordinary Least Squares

The relationship of interest is the following, where the dependent variable is the reported familial expenditure on the education of child i for previous semester, denoted E_i, and the independent variable is endowment as measured by height-for-age, denoted H_i. ϵ_i is a child-specific shock to educational expenditure. All specifications will include household fixed effects, thus eliminating any household-level shocks. Because the subsample is composed of two-child families, a household fixed effects specification is equivalent to estimation of the equation in first differences across the two children.

$$E_i = \delta H_i + \epsilon_i \tag{13}$$

One potential confounding factor of concern is variation by grade or age on the "demand" side of expenditure: older children may use more school supplies than a primary school child, or may attend a secondary school that is more likely to be further away and thus incur greater costs in transportation and housing. In order to address these effects, equation (13) will be estimated both with and without a full set of grade fixed effects. In each case, the model is estimated for each of the six categories of educational expenditure, as well as for total expenditure and discretionary expenditure and a dummy dependent variable for enrollment. All specifications include cohort fixed effects for the elder child and standard errors clustered at the county-year level.

The results, shown in table 2, are largely insignificant with the exception of enrollment, which yields a negative coefficient of very small magnitude. However, there is the potential for bias in these results if endowment measured at the age of primary school already embodies a significant component of prior parental investment. The child who has already been the target of greater parental investment will appear more able, and, if there is some serial correlation in parental behavior, is likely to continue to receive more substantial investment. This will generate an upward bias in the estimated coefficients. Eliminating the bias is the goal of the identification strategy.

Table 2: OLS

	Enrollment	Discretionary	Tuition	Supplies	Transport/Housing	Food	Tutoring	Other fees
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Height-for-age	.021	-2.984	6.990	-.516	.151	-4.177	1.136	.126
	(.005)***	(6.601)	(6.686)	(.631)	(1.885)	(6.570)	(.705)	(.637)
Grade dummies	-2.60e-18	-6.873	-.168	-.991	-2.232	-4.696	.882	.165
	(3.43e-17)	(7.299)	(4.034)	(.746)	(1.645)	(5.148)	(.593)	(.533)
Num. of obs.	423	423	377	377	377	377	377	377

Note: asterisks indicate significance at 1, 5 and 10 percent levels. All regressions include clustering at the county-year level.

4.2 First stage

The key to identification in this case is the use of a climatic, and thus nutritional, shock that is correlated with the relative endowment of the two children. The climatic measures of interest are grain yield and rainfall. Table 3 presents the coefficients for the first stage, regressing height-for-age on climatic shocks in utero (defined as the year prior to birth) and in infancy (defined as the year from birth to age one). The first panel displays results using grain yield, and the second panel rainfall. The first and second columns show the results of regressing height-for-age in the entire sample of children measured, index children and their younger siblings. Columns 2 through 6 show parallel regressions for a sample limited to the older child (child 1) and the younger child (child 2) in two-child households, to confirm that the relationship holds in a restricted sample. All specifications are estimated both with and without cohort fixed effects and employing clustering at the county-year level. Though the instrument employed in subsequent analyses will be climatic shock in infancy as this relationship is more robust, the first stage for both set of shocks is shown to demonstrate the consistent pattern of such effects over the critical period of cognitive and physical development.

The results show coefficients that are generally positive and significant on grain yield, as expected: higher grain yield corresponds to greater nutritional availability in infancy, resulting in increased height. The coefficients on rainfall are negative, an initially surprising pattern that can be attributed to the uniformly negative impact of rainfall on grain yield and agricultural outcomes in general. Due to soil erosion and a pattern of heavy rainfall during the harvest months (June and July) that is highly damaging, rainfall in this geographic area has an effect on nutrition and height opposite of that of grain yield. The left panel of figure 1 shows results from regressing grain yield on rainfall in all counties, in a subset of counties that are less arid, and in the five counties that are highly arid located in the northwest of the province. The relationship is highly negative and significant in the arid counties, characterized by high soil erosion and agriculture that is not rain-fed, and positive and of low-magnitude in the less arid regions. For the overall sample, this yields a negative relationship at low levels of rainfall, where the effect observed in the arid counties dominates. A local polynomial regression of grain yield on rainfall is shown in the right panel.

Returning to the first stage results in table 3, the first stage employing grain yield is positive and narrowly insignificant for the first child, and then much larger and highly significant for the second child. This reflects a sharp climatic shift that occurred in a subset of the sample counties lying in the Heihe river basin. Though these counties were consistently characterized by much higher mean grain yield over this period, they also experienced very sharp increases in yield post-1990 following the reversal of a previous process of desertification. This generates a much more robust first stage among children born post-1990. In the case of rainfall, the hypothesis of differing coefficients between

Regressing grain yield on rainfall

	All counties (1)	Non-desert (2)	Desert (3)
Rainfall	-.012	.011	-.045
	(.006)**	(.005)**	(.015)***
Cons.	3.380	1.568	7.229
	(.174)***	(.168)***	(.277)***
e(N)	298	224	74

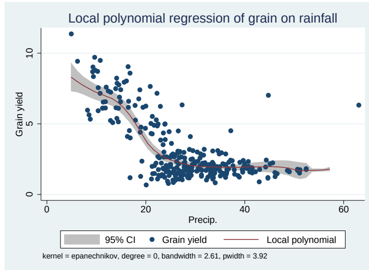


Figure 1: Grain yield and rainfall in Gansu

the older and younger siblings can be rejected when a full set of cohort fixed effects are included. The hypothesis of different coefficients in the first stage for male and female children among either the elder or younger siblings can be rejected for both instruments.

The nature of the climatic shift in the river basin and its implications for the magnitude of the coefficients on grain yield are evident in panel C of table 3. The first two columns show the first stage regressions including a suppressed dummy for those counties lying in the river basin and an interaction between the dummy and grain yield. Among the full sample and the first born children, the interaction is insignificant. However, among the younger children, the interaction is significant and of extremely large magnitude: the slope is nearly four times higher in the three river basin counties, though the coefficient is less precisely estimated given the lower number of observations. When this subgroup is not dummied in panel A, the estimated coefficient is lower than both the main and interaction effects in this specification. This is attributable to the fact that the cluster of river county observations, characterized by a level shift to the right along the axis of grain yield and a much steeper slope, generates a flatter relationship between grain yield and height-for-age when the effect is estimated for the pooled group of counties.

Similarly, the third and fourth columns display the first stage including county fixed effects rather than year fixed effects. For the elder sibling, eliminating the geographic variation renders the first stage insignificant; however, for the second sibling, it remains significant and of extremely large magnitude, reflecting the dramatic inter-county differences emerging during this period.

Shifting the focus to a fixed-household, sibling-difference framework, the first column in table 4 shows a positive and significant relationship between the difference in grain yield in county of birth in the prenatal year for the two children and the observed difference in height-for-age. Similarly, there is a negative and significant relationship between the difference in rainfall and the difference in height-for-age. These specifications are estimated with fixed effects for the elder sibling; a full set of cohort fixed effects eliminates virtually all the variation that is the source of this first stage relationship, namely variation in the rate of change of climatic shocks over time in different counties.

In both cases, the coefficient on the difference is larger in magnitude than the estimated coefficients in the cross-section. For grain yield, the dramatically higher coefficient of .547 reflects primarily the trend break in the river basin counties that is visible in the panel C specifications. The normalization of the county-specific grain yield intercept to zero, by virtue of the differencing across siblings, eliminates the level shift in grain yield that attenuated the coefficient in the pooled regressions across younger siblings. Accordingly, the resulting coefficient is intermediate between the main effect and interaction effect in the specification including the subregional dummy.

The final point of interest is whether there is cross-dependence of shocks: controlling for his or her own shocks in infancy, the exclusion restriction demands that there is no dependence of one sibling's height-for-age on grain yield during the infancy of the other sibling. Columns 2 and 3 of table 4 show tests of cross-dependence employing fixed effects for the elder sibling, demonstrating that there is no dependence of one child's outcome on the other child's shock in infancy.

This indicates that the primary channel for the impact of the shock in infancy on height-for-age is the effect on a child's own physical development, not a household-level mechanism that would induce cross-dependence of one sibling's outcome on another's shock. A major potential violation of the exclusion restriction would be reallocation of resources by households in response to an early childhood shock: for example, following the birth of the second child, households could preferentially direct resources to either the first or the second child in the event of a negative shock. This would be evident either in a significant relationship between the older child's height and the shock to the younger child, or a difference in the coefficient on climatic shock for the younger child. Given the absence of any such effect, the exclusion restriction remains plausible.

The magnitude of the coefficients can be interpreted by noting that the mean within-county standard deviation in grain yield is .28 tons per hectare; for rainfall, it is 5.52 centimeters per month of rainfall. The mean difference in height-for-age between the elder and younger siblings is -.07. Thus a one standard deviation increase in grain yield in the county in the year of birth for the younger sibling, holding the older sibling's shock constant, will increase the difference in height-for-age between them by .153. This is equivalent to 13% of the mean height-for-age in levels, and about 200% of the mean difference in height-for-age. Put another way, the mean difference in height between the older and younger siblings would be eliminated if there were a counterfactual increase in grain yield in the older child's year of birth corresponding to one half of the year-on-year standard deviation in that county. Using the first stage in rainfall, a one standard deviation increase in rainfall for the younger children will increase the difference in height for age by .22, a marginally larger effect. The similarity in these estimates suggests they reflect the same fundamental relationship between climatic shocks and nutrition.

Table 3: First stage cross-section

	Full sample	Full sample	Child 1	Child 1	Child 2	Child 2
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Grain yield						
Grain yield in utero	.125	.106	.094	.093	.160	.092
	(.023)***	(.028)***	(.043)**	(.044)**	(.027)***	(.040)**
Grain yield year 1	.114	.098	.068	.072	.147	.085
	(.023)***	(.027)***	(.047)	(.048)	(.029)***	(.042)**
Panel B: Rainfall						
Rainfall in utero	-.021	-.021	-.020	-.020	-.029	-.026
	(.005)***	(.005)***	(.002)***	(.004)***	(.005)***	(.005)***
Rainfall	-.021	-.020	-.028	-.028	-.034	-.027
	(.005)***	(.006)***	(.008)***	(.008)***	(.006)***	(.005)***
Panel B: Grain yield by region						
Grain yield year 1	.123	.257	.060	-.170	.163	.670
	(.035)***	(.234)	(.052)	(.147)	(.052)***	(.213)***
River county int.	.237		-.100		.420	
	(.149)		(1.049)		(.250)*	
Cohort fixed effects	No	Yes	No	Yes	No	Yes
Mean height-for-age	-1.19	-1.19	-1.27	-1.27	-1.17	-1.17
Num of obs.	2524	2524	705	705	759	759

Note: asterisks indicate significance at 1, 5 and 10 percent levels. All regressions include clustering at the county-year level.

Table 4: First stage in differences

	Height dif.	Height 1	Height 2
	(1)	(2)	(3)
Grain dif	.520		
	(.193)***		
Grain child 1		.233	-.443
		(.127)*	(.346)
Grain child 2		-.109	.523
		(.075)	(.296)*
Num of obs.	418	418	419
F stat.	6.765	3.703	9.001
Rain dif.	-.043		
	(.016)***		
Rain child 1		-.038	.017
		(.006)***	(.012)
Rain child 2		-.00005	-.044
		(.004)	(.012)***
Num of obs.	422	422	423
F stat.	6.592	15.858	7.679

Note: asterisks indicate significance at 1, 5 and 10 percent levels. All regressions include clustering at the county-birth year pair level.

4.3 Reduced form and two-stage least squares

Table 5 shows the reduced form results of regressing the dependent variables on the climatic shocks of interest, employing the household fixed effects specification. The first rows in Panel A and Panel B shows the preferred basic specification including cohort fixed effects for the elder child; standard errors are clustered at the county-birth year pair level. Any set of siblings born in the same county in the same two years is thus assumed to have experienced a common climatic shock. The second row includes grade fixed effects. The third and fourth row of each panel converts the four latter categories to dummy variables for spending, given that spending in these categories includes a large number of observations clustered at zero; this specification is also shown with grade fixed effects.

The results show a significant and negative relationship for grain yield shocks, and a significant and positive relationship for rainfall shocks: in both cases, educational spending favors the child who has been subject to a negative shock in infancy. In the case of grain yield (panel A), the relationship is consistent across all categories of expenditure and is consistently significant, though smaller in magnitude, when fixed effects are included. No effects on enrollment are evident.

The inclusion of grade fixed effects may be troubling given the potential objection that grade level in itself represents an outcome reflective of past parental investment: children who have already benefited from greater parental investments may have advanced to a higher grade level. The extremely low magnitude of the coefficient on enrollment, however, suggests that the enrollment decision is not a significant margin of parental decision-making that would induce large endowment-based variation in grade level. Moreover, if grade level does reflect another compensating allocation by parents—so that less able children are in a higher-than-expected grade level by virtue of past preferential allocations, for example—then the coefficient estimates on endowment would be attenuated toward zero. The bias thus runs in an unfavorable direction, and the fact that endowment remains significant is evidence of a robust effect. There is little evidence to indicate that the inclusion of grade level biases the conclusions, particularly in light of the uniform consistency of the results.

In the instrumental variables specification, equation (13) is estimated using household fixed effects, instrumenting for the difference in endowment between the two children with the difference in climatic shocks (grain yield and rainfall) in infancy. The results reported in table 6 are clear and consistent. δ_1 is negative and highly significant, indicating that taller children receive less educational expenditure, for all categories of expenditure employing grain yield as an instrument. Employing rainfall, the results are significant for discretionary expenditure, transport and housing, and food. There is no significant impact on enrollment. The coefficients remain consistent in magnitude and sign when grade fixed effects are added.

To interpret the magnitude of the coefficients, consider that the average difference

Table 5: Reduced form

	Enrollment	Discretionary	Tuition	Supplies	Trans./Housing	Food	Tutoring	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Grain yield								
Grain yield	.026	-154.124	-24.653	-12.276	-26.997	-109.667	-8.158	-11.401
	(.024)	(46.979)***	(16.539)	(2.610)***	(9.930)***	(37.488)***	(3.370)**	(4.926)**
Grade FE	4.94e-18	-117.281	16.185	-5.279	-17.848	-77.287	-6.803	-10.064
	(5.34e-16)	(36.801)***	(10.530)	(3.263)	(5.920)***	(27.286)***	(2.712)**	(4.058)**
Spending dummies					-.187	-.187	-.161	-.100
					(.078)**	(.078)**	(.077)**	(.044)**
Grade FE					-.153	-.153	-.082	-.089
					(.063)**	(.063)**	(.074)	(.042)**
Panel B: Rainfall								
Rainfall	-.003	4.098	1.813	.242	.794	3.497	-.061	.112
	(.002)*	(2.584)	(.899)**	(.262)	(.504)	(1.969)*	(.175)	(.221)
Grade FE	2.20e-19	2.798	.954	.115	.448	2.384	-.139	-.010
	(9.05e-17)	(2.002)	(.794)	(.175)	(.371)	(1.507)	(.139)	(.194)
Spending dummies					.006	.006	.008	-.006
					(.004)	(.004)	(.004)**	(.004)
Grade FE					.007	.007	.010	-.004
					(.004)**	(.004)**	(.003)***	(.002)*
Mean (levels)	1.07	94.31	170.27	36.50	6.73	9.24	6.73	9.24
Median (levels)	1	40	130	30	0	0	0	0
Mean (difference)	.07	89.07	65.17	17.11	17.60	50.81	6.8	7.3
Num of obs.	419	419	375	375	375	375	375	375

Note: asterisks indicate significance at 1, 5 and 10 percent levels. All regressions include fixed effects for the older child's cohort and clustering at the county-birth year pair level.

in height-for-age between siblings is -.07. The estimates using grain yield suggest that in response to such a difference, the parents would redirect 20 yuan or 22% of mean individual discretionary spending toward the weaker child; the estimates using rainfall suggest that parents direct only 7% of discretionary expenditure toward the weaker child. The difference in magnitude between the estimates is unsurprising given the evidence previously examined about the first stage. The compliant households, those in which a difference in endowment between the two children is induced by shifts in relative grain yield in their birth years, are disproportionately drawn from the river-basin counties experiencing dramatic increases in yield. These counties were already characterized by higher agricultural yields and thus greater income. The estimated response in educational spending is thus much larger. The coefficient on grain yield corresponds to a shift of 9% of educational spending employing discretionary spending in the river basin counties as the reference mean, an effect of magnitude comparable to the estimated effect using rainfall.

Table 7 shows the first-stage F-statistics for the main specifications with and without grade fixed effects. F-statistics are well over 10, with the exception of the specification employing grain yield as the instrument with grade dummies. Since the magnitude and significance of the effects are consistent across specifications, weak instruments are unlikely to be a source of bias.

This evidence suggests the hypothesis that parental allocations of education are independent of children's endowment can be rejected. Parental allocations favor the child with lower endowment, though the hypothesis of full specialization in either child can also be rejected. In the modeling framework outlined, the observed pattern of parental allocations of educational expenditure is consistent with a parental preference for equality among children. This requires the use of education as a compensatory device for differences in endowment, as observed in the greater allocations to children of lesser endowment.

5 Robustness checks

5.1 Sibling parity effects

Given the difference in mean height-for-age between the older and the younger children, one potential concern is that height-for-age may simply proxy for birth order, and that a pattern of expenditure favoring the weaker child may reflect a pattern of expenditure favoring the first-born child. It is not possible to test for sibling parity effects directly using the household fixed effects specification. In order to address this point, (13) is re-estimated in a cross-section, thereby eliminating the household fixed effects, and including both own height and sibling's height as independent variables. H_j denotes the sibling's height; both measures of height are instrumented with the respective shocks in infabct.

$$E_i = \delta_1 H_i + \delta_2 H_j + \zeta_h + \epsilon_i \tag{14}$$

Table 6: Two-stage least squares

	Enrollment	Discretionary	Tuition	Supplies	Trans./Housing	Food	Tutoring	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Grain yield								
Height-for-age	.032	-299.873	-52.440	-26.112	-57.427	-233.276	-17.353	-24.252
	(.059)	(109.287)***	(48.033)	(8.466)***	(31.593)*	(113.055)**	(8.861)*	(11.198)**
Grade FE	2.70e-17	-288.743	39.848	-12.996	-43.941	-190.280	-24.777	-16.748
	(2.84e-15)	(136.351)**	(28.186)	(9.231)	(25.924)*	(91.476)**	(12.195)**	(8.521)**
Spending dummies					-.384	-.384	-.334	-.215
					(.181)**	(.181)**	(.170)**	(.126)*
Grade dummies					-.377	-.377	-.203	-.218
					(.216)*	(.216)*	(.181)	(.157)
Panel B: Rainfall								
Height-for-age	.056	-98.196	-41.496	-5.385	-17.694	-77.939	1.356	-2.506
	(.031)*	(35.708)***	(27.968)	(5.090)	(8.719)**	(23.250)***	(4.171)	(4.197)
Grade FE	-4.25e-23	-66.754	-22.756	-2.748	-10.688	-56.875	.233	3.323
	(2.17e-15)	(28.769)**	(25.701)	(4.169)	(6.854)	(19.405)***	(4.664)	(4.008)
Spending dummies					-.172	-.172	-.224	.105
					(.066)***	(.066)***	(.073)***	(.086)
Grade FE					-.178	-.178	-.227	.090
					(.051)***	(.051)***	(.023)***	(.073)
Mean (levels)	1.07	94.31	170.27	36.50	6.73	9.24	6.73	9.24
Median (levels)	1	40	130	30	0	0	0	0
Mean (difference)	.07	89.07	65.17	17.11	17.60	50.81	6.8	7.3
Num of obs.	419	419	375	375	375	375	375	375

Note: asterisks indicate significance at 1, 5 and 10 percent levels. All regressions include fixed effects for the older child's cohort and clustering at the county-birth year pair level.

Table 7: First stage F-statistics

	Enrollment	Total	Discretionary	Supplies	Transport	Food	Tutoring	Other
	(1)	(2)	(3)	(4)	(5)	(5)	(7)	(8)
Panel A: Grain yield								
Height-for-age	307.01	307.01	32.05	32.05	32.05	32.05	32.05	32.05
Grade dummies	5.35	5.35	5.36	5.36	5.36	5.36	5.36	5.36
Panel B: Rainfall								
Height-for-age	13.00	13.00	15.18	16.10	16.10	16.10	16.10	16.10
Grade dummies	14.91	14.91	14.95	14.95	14.95	14.95	14.95	14.95

In order to estimate unbiased coefficients on H_i and H_j, this equation requires a stronger exclusion restriction than the household fixed effects specification: the climatic shock employed to instrument for each sibling's height must be orthogonal not only to the unobserved residual for that child, but also to the residual for the household. In particular, climatic shocks must have no impact on the household's overall budget constraint. This restriction is a priori implausible. However, it can be directly tested by assuming that the exclusion restriction holds and noting that in this case, the household fixed effects specification can be derived from equation (14) by virtue of simple algebraic manipulation.

$$E_i - E_j = (\delta_1 H_i + \delta_2 H_j + \zeta_h + \epsilon_i) - (\delta_1 H_j + \delta_2 H_i + \zeta_h + \epsilon_j) \tag{15}$$

$$\Delta E = (\delta_1 - \delta_2)(H_i - H_i) + (\epsilon_i - \epsilon_j) \tag{16}$$

This indicates that if the exclusion restriction holds, $\delta = \delta_1 - \delta_2$, the linear combination of the coefficients in the cross-sectional specification should equal the coefficient in the fixed household effects specification. The most convenient way to test this hypothesis as a restriction on a single specification is to further rearrange equation (14) by subtracting E_j from the right side.

$$E_i = \delta_1 H_i + \delta_2 H_j - E_j + \zeta_h + \epsilon_i \tag{17}$$

$$E_i = (\delta_1 - \delta_2)H_i + (\delta_2 - \delta_1)H_j + \zeta_h + \epsilon_i \tag{18}$$

In a regression of own expenditure on own height, sibling height, and the negative of sibling expenditure, the coefficients on own height and sibling height should be equal and opposite in sign, and equal in magnitude to the coefficient using household fixed effects. If this identity does not hold, the exclusion restriction for the cross-sectional specification can be rejected. Implementing this test for each category of expenditure yields a rejection of the hypothesized restriction on the coefficients, and thus a rejection of the accompanying exclusion restriction, for one out of eight categories of expenditure employing grain yield, and for five out of eight categories of expenditure employing rainfall. This suggests that income effects or other household-level impacts of climatic shocks may be relevant and the household fixed effects specification is more robust.

The cross-sectional specification does have the advantage of allowing the direct implementation of a test for the presence of sibling parity effects. The specification is re-estimated adding a dummy variable for sibling parity and interacting the dummy with own height, as shown in (19); P_i denotes a child's sibling parity.

$$E_i = \delta_1 H_i + \delta_2 H_j + \alpha_1 P_i + \alpha_2 P_i * H_i + \zeta_h + \epsilon_i \tag{19}$$

The results are shown in table 8, employing grain yield as an instrument in panel A and rainfall in panel B; all specifications include cohort fixed effects and a full set of grade dummies. The results are largely consistent with the previous specification despite the known source of bias. Own height is generally insignificant, and sibling height is positive and significant for three categories of expenditure using rainfall and five categories using grain yield. More importantly, the uniform insignificance of the parity dummy and interaction effect indicates that the hypothesis that there are any differences between the specifications with and without parity effects can be rejected.

Additional tests can be implemented by making use of an alternate source of expenditure data, namely data on medical expenditure. Data on medical expenditure on children is available from two sources. First, the mother reports the number of visits to a doctor or to a clinic or hospital for each child, as well as the number of medical episodes and days of school missed due to sickness for each child. Second, the head of household (normally the father) separately reports medical expenses for each child and the number of days ill over the last month. Table 9 shows these results, and, again, sibling parity is found to be uniformly insignificant, indicating no pattern of systematic favoritism by birth order. The fact that this is corroborated by reports from both parents is encouraging, as one might hypothesize that if one parent systematically favors the older or younger child, he or she might over-report his or her episodes of illness in order to justify the expenditure. However, the coincidence of results from both parents suggests that reporting bias is unlikely to be a factor biasing the results.

5.2 Gender effects

A second potential confounding factor is gender. Given the evidence from other sources of gender bias in household decision-making in China, the effect of gender on parental allocations may outweigh any observed effect for endowment. On the other hand, simply controlling for gender is unlikely to be satisfactory. Given the abundant anthropological and demographic evidence on abortion, abandonment, or underreporting of female children in China (Coale & Banister, 1994; Hull, 1990; Gu & Roy, 1995; Yi *et al.*, 1993; Qian, 1997), it is implausible to think that the gender of both children can be assumed to be random, and households with different gender balances among their children are likely to differ materially along other observable and unobservable dimensions.

However, in this case, the gender of the first child is a plausibly exogenous observation, as anthropological evidence indicates that selection for gender occurs principally in births subsequent to a first-born daughter and that selective abortion prior to the birth of a first child is unusual (Johansson & Nygren, 1991; Baochang *et al.*, 2007; Banister, 2004; Junhong, 2001). The evidence in this sample is consistent with this hypothesis. The sex ratio for the first child in this sample is not significantly different from .5, while for the second-born child, the sex ratio is highly imbalanced. The observations on the gender of

Table 8: Educational expenditure and sibling parity

	Enrollment	Discretionary	Tuition	Supplies	Trans./Housing	Food	Tutoring	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Grain yield								
Height-for-age	2.68e-17	514.421	82.958	14.073	48.747	373.527	44.191	33.882
	(1.43e-14)	(512.825)	(138.341)	(45.853)	(90.523)	(361.544)	(33.545)	(36.552)
Sibling height-for-age	-1.29e-17	187.846	92.694	21.763	23.859	108.575	12.121	21.527
	(2.61e-15)	(124.400)	(43.009)**	(5.838)***	(18.889)	(90.837)	(4.847)**	(13.673)
Parity int.	-1.58e-17	-346.890	-4.058	-3.127	-39.459	-259.078	-23.996	-21.231
	(8.21e-15)	(250.592)	(105.144)	(24.220)	(46.949)	(199.272)	(19.196)	(14.233)
Parity	-2.40e-17	-441.283	-2.720	-.630	-47.987	-333.822	-30.103	-28.741
	(1.14e-14)	(352.249)	(157.643)	(39.904)	(64.354)	(280.069)	(25.823)	(19.569)
Panel B: Rainfall								
Height-for-age	-1.98e-21	116.529	106.282	19.884	6.829	57.628	13.846	18.343
	(2.44e-14)	(103.840)	(64.762)	(39.192)	(25.184)	(70.083)	(7.114)*	(5.973)***
Sibling height-for-age	2.35e-22	62.765	62.054	3.220	6.601	44.233	.434	8.276
	(3.67e-15)	(25.967)**	(26.751)**	(5.699)	(3.948)*	(22.091)**	(2.509)	(2.415)***
Parity int.	1.26e-21	-89.253	-55.281	-9.769	-9.669	-57.252	-6.336	-6.228
	(1.74e-14)	(55.186)	(51.005)	(26.059)	(14.064)	(37.078)	(3.856)	(2.346)***
Parity	1.82e-21	-111.191	-78.496	-12.816	-10.083	-70.377	-7.993	-9.923
	(2.66e-14)	(88.733)	(82.726)	(40.465)	(20.901)	(60.639)	(4.711)*	(6.716)
Num. of obs.	771	771	770	770	770	770	770	770

Note: asterisks indicate significance at 1, 5 and 10 percent levels. All regressions include cohort fixed effects and clustering at the county-year level.

Table 9: Medical expenditure and sibling parity

	Doctor visit	Doctor visit	Clinic visit	Clinic visit	Medical expenditure
	(1)	(2)	(3)	(4)	(5)
Med episodes	.122		.032		
	(.015)***		(.024)		
Days missed		-.023	.105		
		(.004)***	(.019)***		
Days sick					98.817
					(5.844)***
Parity	.055	.036	-.260	-.222	4.006
	(.033)*	(.034)	(.161)	(.158)	(77.841)
Num of obs.	817	817	846	846	839

Note: asterisks indicate significance at 1, 5 and 10 percent level.

Table 10: Educational expenditure and gender

	Enrollment	Discretionary	Tuition	Supplies	Transport/Housing	Food	Tutoring	Other Fees
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Height for age	.023	-309.448	-57.865	-26.362	-59.872	-240.757	-17.992	-25.195
	(.056)	(59.967)***	(22.683)**	(12.592)**	(25.987)**	(72.207)***	(7.549)**	(9.370)***
Gender	.050	58.687	30.146	1.389	13.589	41.570	3.554	5.239
	(.021)**	(47.375)	(15.858)*	(6.627)	(8.890)	(37.956)	(2.811)	(5.082)
Grade dummies	2.82e-17	-296.726	39.236	-12.121	-45.831	-195.367	-17.527	-25.880
	(3.06e-15)	(99.166)***	(34.705)	(6.183)**	(23.303)**	(65.664)***	(8.863)**	(7.874)***
Gender	-3.64e-18	24.532	1.881	-2.688	5.807	15.632	2.394	3.387
	(5.35e-16)	(40.543)	(14.881)	(3.417)	(5.600)	(26.596)	(2.825)	(4.034)
Height for age	.054	-98.971	-42.032	-5.329	-17.866	-78.333	1.351	-2.544
	(.033)	(35.756)***	(26.799)	(5.141)	(8.775)**	(23.242)***	(4.187)	(4.274)
Gender	.046	18.666	15.695	-2.124	6.566	15.065	.153	1.436
	(.015)***	(22.508)	(13.250)	(3.033)	(5.211)	(18.042)	(1.395)	(2.461)
Grade dummies	-5.24e-18	-66.757	-22.747	-2.751	-10.686	-56.876	3.323	.233
	(2.16e-15)	(28.636)**	(24.576)	(4.158)	(6.899)	(19.390)***	(3.987)	(4.673)
Gender	3.74e-18	-2.838	10.484	-3.776	1.613	-.493	-.363	.181
	(1.12e-15)	(16.540)	(11.246)	(2.353)	(2.501)	(13.016)	(1.508)	(2.463)
Obs.	418	418	375	375	375	375	375	375

Note: asterisks indicate significance at 1, 5 and 10 percent levels. All regressions include cohort fixed effects and clustering at the county-year level.

the first and second child are thus assumed to constitute one exogenous observation (the gender of the first child) and one endogenous observation (the gender of the second child). In a family fixed-effects framework such as that employed here, the relevant dependent variable is the difference in gender between the children, which is likewise endogenous. However, the gender of the first child is an appropriate instrument if the assumption of exogeneity is plausible. The main equation of interest is again estimated employing climatic shocks as instruments, and using the gender of the first child as an instrument for the difference in gender, yielding the following estimating equation.

$$E_i = \delta H_i + \beta G_i + \zeta_h + \epsilon_i \tag{20}$$

The estimation results are shown in table 10, again employing both grain and rainfall as instruments and cohort fixed effects for the elder cohort, and including grade dummies in the second row. The coefficient on endowment is again significant and negative for total expenditure and discretionary categories. Gender, perhaps surprisingly, proves to be uniformly insignificant with the exception of a positive coefficient on enrollment in the specifications lacking grade fixed effects. This indicates that the relative gap in enrollment between the older and younger child increases when the gap in their genders becomes positive: e.g., when the first child is a girl and the second child is a boy. However, this effect disappears when grade fixed effects are added. Conditional on endowment, discretionary allocations of educational funding between boys and girls appear to be equal. There is no evidence of preferential allocations to male children.

5.3 Selection bias

Selection into the sample of two-child families observed in this analysis would constitute a violation of the exclusion restriction. If families with certain characteristics are more or less probable to suffer an adverse mortality event as the result of the same climatic shock, then the pattern of shocks may affect the ultimate pattern of allocations by determining the surviving number of children, and hence inclusion or exclusion in the sample. Due to the absence of complete data on retrospective familial mortality, it is not possible to directly examine child mortality as a function of varying climatic shocks. The alternative strategy employed is the identification of extremely severe climatic shocks that are most likely to be associated with increased mortality. If there is selective survival among children born in those years, this would be expected to produce a pattern of greater physical robustness among those born in years with severely adverse climatic shocks as weaker children die prematurely, and a positive relationship between adverse climatic shocks and height-for-age when shocks are severe would be observed. On the other hand, if selection via differential mortality is not an important phenomenon, a severe harvest shock should yield a decline in indices of physical robustness or health.

To test this hypothesis in this data set, height-for-age in the pooled sample of older and younger siblings is regressed on four dummy variables indexing negative harvests of different severity and timing. A severe shock is defined as a year in which the grain yield in that county was below the 10th percentile for average grain yield over all counties in the years of interest; a bad shock is a year in which grain yield was below the 25th percentile; a moderately bad shock is below the 50th percentile. In each case, a dummy variable is employed indexing whether a shock occurred in the first year of life, and the regressions are estimated with cohort fixed effects and county-year clustering.

The results, shown in table 11, show coefficients that are uniformly negative. For both grain yield and rainfall, there is evidence of attenuation of coefficients toward zero for the most severe shocks, and in the case of grain yield, this renders the coefficient insignificant. However, the consistently negative sign suggests that selective mortality is minimal and selection into the observed subsample of two-children family is unlikely to be a factor biasing the results.

6 Effectiveness of educational expenditure and endowment

These results raise the question of whether the observed pattern favoring the weaker child exhibited is a compensatory response intended to provide consumption-like educational benefits to children with lower endowments, or whether this allocation strategy reflects differential returns to educational expenditure for children of differing levels of endowment. If, for example, educational expenditure has higher returns for the child with a lower endowment, then the observed strategy could be interpreted as maximizing returns to

Table 11: Height-for-age and severe shocks

	Height-for-age	Height-for-age
	(1)	(2)
Panel A: Grain yield		
Negative shock	-.297	
	(.151)**	
Severe shock		-.028
		(.093)
Panel B: Rainfall		
Negative shock	-.529	
	(.195)***	
Severe shock		-.637
		(.279)**
Num. of obs.	845	845

Note: asterisks indicate significance at 1, 5 and 10 percent levels. All regressions include cohort fixed effects and clustering at the county-year level.

educational investment. In order to test this hypothesis, the panel data of observations on the older sibling is employed to evaluate gains in academic attainment between wave 1 and wave 2 as a function of height-for-age, again instrumented with climatic shocks, educational expenditure and the interaction between the two. The equation to be estimated is (21), where ΔT denotes the gain in academic skills as measured by the difference in wave 1 and wave 2 scores on grade-specific academic tests in Chinese and mathematics. T_{1i}, the test score on the first round, is included as an independent variable, though not reported, in order to account for mean reversion. The test scores are normalized by grade to mean zero and standard deviation one.

$$\Delta T = H_i + E_i + H_i * E_i + T_{1i} + \eta_h + \epsilon_i \tag{21}$$

Because the younger sibling was not included in the first wave of testing, as a result of their extremely young age, this equation can only be estimated in the cross-section without household fixed effects. This, again, raises the concern of income effects that violate the exclusion restriction. The equation is estimated with county-year fixed effects in order to absorb variation in income at the county level; standard errors are clustered at the county-year level. The results in table 12 show that the interaction effect is generally insignificant, though it is positive and highly significant for tutoring in specifications employing both grain yield and rainfall, and positive and marginally significant for total expenditure and discretionary expenditure for specifications employing grain yield. This indicates that while there is little variation in the relative effectiveness of educational expenditure for children of different endowments, the interaction effect is positive when it is significant. Returns to at least one type of educational expenditure are increasing in endowment.

Table 12: Educational expenditure and endowment

	Total	Discretionary	Tuition/Text	Food/Trans./Housing	Tutoring	Other
	(1)	(2)	(3)	(4)	(5)	
Panel A: Grain yield						
Height for age	-.417	-.369	-.457	-.394	-.163	-.361
	(.107)***	(.025)***	(.133)***	(.039)***	(.095)*	(.046)***
Expenditure int.	.0005	.002	.0006	.002	.083	.001
	(.0007)	(.003)	(.001)	(.003)	(.034)**	(.008)
Expenditure	.001	.003	.001	.002	.113	.003
	(.0004)***	(.003)	(.0005)**	(.004)	(.044)**	(.009)
e(N)	1569	1569	1548	1548	1548	
Panel B: Rainfall						
Height for age	-1.514	-.349	.095	-.425	-.281	-.351
	(28.818)	(.071)***	(3.239)	(.244)*	(.042)***	(.018)***
Expenditure int.	.012	-.001	-.004	-.007	.038	.003
	(.284)	(.012)	(.028)	(.063)	(.009)***	(.002)
Expenditure	.009	-.00005	-.001	-.007	.053	.004
	(.196)	(.012)	(.014)	(.068)	(.013)***	(.002)*
Num. of obs.	1585	1585	1564	1564	1564	1564

Note: asterisks indicate significance at 1, 5 and 10 percent levels. All regressions include cohort fixed effects and clustering at the county-year level.

For this reason, the hypothesis that parents preferentially direct expenditure to children of lower endowments maximize the total returns on their educational spending can be rejected.

7 Conclusion

In the previous literature on intrahousehold allocation, the question of the presence or absence of family aversion to inequality has received limited analytical attention. However, little evidence has been presented regarding the nature of parental responses to systematic differences in endowment among children. This paper analyzes the relationship between parental allocations of educational expenditure and endowment, testing a model in which parents seek to maximize the total returns to all educational investments against one in which they have a preference for equality of expected lifetime income across their offspring.

Employing an identification strategy that relies on the correlation between climatic variation and children's endowment, mediated through the impact of nutritional shocks in infancy on cognitive and physical development, I find a pattern of preferential allocations of discretionary educational expenditure to the less able child, conditional on the attained grade level. This is consistent with a parental preference for equality of outcomes. The relationship is robust across multiple specifications and measures of expenditure, and is unaffected by the inclusion of gender or sibling parity. However, evidence suggests that this pattern of allocations is not a response to higher returns to educational expenditure for children of lower endowment. In some categories of expenditure, returns are higher for children of higher endowment.

These results suggest that, at least in the area of education, the household is serving as a mechanism for the mitigation of existing inequalities. It is impossible to rule out the possibility that the observed allocative decisions would be altered by an external shock. However, based on this evidence it seems a priori plausible that a positive income shock to the households examined would disproportionately benefit the more vulnerable among the children, where vulnerability is measured as a lower physical endowment. For the purposes of the welfare analysis of potential household interventions, this is an encouraging result that suggests that household-level interventions will in fact improve welfare outcomes for the weakest members of a family.

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