Sibling Rivalry: Endowment and Intrahousehold Allocation in Gansu Province, China *

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Abstract

This paper evaluates the strategies employed by households in rural China to allocate educational expenditure to children of different physical endowments, examining whether parents use educational funding to reinforce or compensate for these differences. Climatic shocks are employed as an instrument for endowment, measured as height-for-age, allowing for the identification of the impact of quasi-exogenous variation in endowment on parental allocations conditional on household fixed effects. The results suggest that educational expenditure is directed to the relatively weaker child; in response to the mean difference in height-for-age between siblings, parents redirect around 25% of discretionary educational spending to the child with lower height-for-age, and this effect is robust to the potentially confounding effects of gender and birth order. There is some evidence that time allocation may also be a relevant margin of compensation, but no evidence that medical expenditure responds to differences in height-for-age.

1 Introduction

For decades, social scientists have analyzed the decisions that households make about human capital accumulation and the implications of these decisions for individual economic outcomes (Strauss and Thomas, 1995). Given that the majority of educational investments are made in childhood, it is particularly crucial to understand the choices that parents make about education on behalf of their children – choices that, in multi-child families, entail not only identifying resources for education in the form of money or parental time, but allocating those resources among multiple children.

This paper provides evidence about the parental allocation of resources for education among children of varying endowments in a low-resource setting in rural China, seeking to identify whether parents employ a compensatory or a reinforcing strategy in response to variations in endowment among their children. I define endowment as the physical health of the child as shaped by his/her genes as well as the conditions faced in utero and early in life. The analysis thus seeks to explore how parents respond to variations in children's endowment resulting from the conjunction of the genetic lottery and early childhood shocks.

However, direct estimation of the relationship between parental behavior and relative health or physical robustness among children poses serious challenges. Any measurement of child endowment captured after birth will inevitably include a component of endogenous parental nurturing; thus, a parental preference may manifest itself in a higher endowment for a given child and in overtly preferential treatment of the same child. This generates spurious evidence of a positive relationship between a child's endowment and parental investments.

The principal methodological contribution of this paper is to address the endogeneity of any measurement of a child's endowment by employing as an instrument a variable capturing exogenous variation in resource availability correlated with physical health. The instrument used is grain yield as well as predicted grain yield in utero and 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 during the prenatal period and between birth and age three, has a substantial negative impact on physical and cognitive development (Pollitt et al., 1999; Grantham-McGregor and Ani, 2001). Shocks to a child's nutritional intake in this period are correlated with endowment, but exogenous to other intrahousehold decisions, given the use of 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 climatic shocks. This pattern of preferential allocations holds across multiple measures of expenditure, and is robust to the potentially confounding effects of gender and birth parity. There is also some evidence that time allocation may be a mechanism for parental compensation. However, parents do not seem to exhibit compensatory behavior in the allocation of medical expenditure.

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 (Hazarika, 2000; Behrman and Deolalikar, 1990; Ono, 2004). 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 and Morduch, 1998; Morduch, 2000), though the inverse relationship appears to hold in the United States (Butcher and Case, 1994). A separate literature has focused on the relationship between birth order and the intrafamily distribution of resources (Parish and Willis, 1993; Tenikue and Verheyden, 2007; Bommiere and Lambert, 2004).

A smaller literature has analyzed whether parents have a general preference for equality among their children. An early paper by Griliches presented evidence that parents attempted to limit intrafamily equality and attenuate preexisting differences in endowment, noting that the effect of IQ on schooling is significantly lower within families (Griliches, 1979). Behrman, Pollak and Taubman (1982) 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, employing functional form assumptions on the parental welfare function. Using developing-country data, Rosenzweig and Wolpin (1988) 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. Behrman (1988) finds evidence in India of a pro-male bias as well as parental inequality aversion, though such aversion declines in the lean season.

Two more recent papers analyzing the response of parental human capital investments to children's variation in endowments found that parents exhibit reinforcing behavior. Rosenzweig and Zhang (2009) find that parents exhibit higher educational expenditure for children of higher birth weight in China. They do not address the potential endogeneity of birth weight for siblings born as singletons – if, for example, children born at a certain parity or a certain stage of parental development are more wanted, the mothers could receive better prenatal nutrition, leading to higher birth weight for the child as well as enhanced subsequent endowments – but find parallel results for twin pairs for which endogenous determination of birth weight can be ruled out. Conti et al. (2010) also employ data from China to estimate a structural model of parental resource allocation given multidimensional child endowments, and find evidence of compensating investment in health but reinforcing investment in education. They exploit early health shocks, defined as a reported episode of serious disease, assumed to occur randomly within twin pairs.

In employing climatic shocks at birth as a source of variation in children's health endowment, this paper joins a robust literature that has examined the impact of early childhood shocks (climatic, economic, or political) on longer-term outcomes. Almond et al. (2006) and Meng and Qian (2009) have analyzed the long-term impact of famine caused by the Great Leap Forward in China. Almond (2006) and Almond and Chay (2006) exploit shocks to public health and social policy in the U.S. over time, while Banerjee et al. (2010) evaluate the impact of income shocks in nineteenth-century France caused by a vineyard-destroying insect. Most similar in spirit to this paper may be the work of Maccini and Yang (2009), which shows a relationship between early childhood shocks and long-run economic impacts in Indonesia. However, this paper is one of the first in this literature to show evidence of a relationship between in-utero shocks and physical robustness as measured by height-for-age.

Furthermore, this paper makes several contributions to the existing literature on parental intrahousehold allocation. It is the first study to estimate the response of parental allocations to quasi-exogenous variation in endowment without relying on the use of twin pairs. In addition, it builds on the existing literature about early childhood shocks, exploiting these shocks to address the potential endogeneity of a child's endowment. Finally, it provides robust evidence of an effect of climatic shocks, even in utero, on medium-term health outcomes as measured by height-for-age.

The paper proceeds as follows. Section 2 presents the data. Sections 3 and 4 present the main empirical results and robustness checks. The final section concludes.

2 Data

The data set employed in this analysis is the Gansu Survey of Children and Families (GSCF), a panel 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.

The survey employed a four-stage stratified random sample. First, counties were selected from the full sampling frame of counties, excluding autonomous minority regions. Townships within each county (and subsequently, villages within each township) were ranked according to per-capita income, and a sampling distance was calculated taking into account the relative population of each unit. Two to three townships within each county and two villages within each township were sampled, yielding 100 villages. Twenty children were then sampled in each village, selected randomly from the cohort of children aged 9–12 during the survey's first wave.

Gansu, located in northwest China, is one of the poorest and most rural provinces in China. Per capita income in this sample is around $200 a year, and the mean level of education observed among parents is only seven years for men and four years for women. This analysis will focus on a subsample of the families in the survey: those with two children in the household where both children are observed in the second wave survey. 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 the primary focus of the analysis. Such complete data is available on 413 families, and they constitute the relevant subsample. I will present the key results for a slightly larger sample, including households where these two children (the index child and the younger sibling) are the youngest two children of a larger family.[1]

Clearly, restricting to a subsample raises some question about external validity. However, it is important to note that the question of intrahousehold allocation among children is, in fact, irrelevant for the majority of the excluded sample, who are single-child households. These households do not form part of the theoretical sample of interest: households

[1]The average number of children in households in this sample is 2.2. While the One-Child Policy was in effect during the period in which these children were born, many rural households could nonetheless have two children legally under various exemptions to the policy (Gu et al., 2007). Other households may simply have defied the rules. It is not possible using this dataset to accurately identify for each household whether it was technically in compliance with the policy.

with a number of children greater than one. The primary results will also be shown to be robust to including households who have a number of children greater than two.

Panel A of Table 1 reports summary statistics for the subsample of two-children families and the overall sample for key demographic indicators of interest as measured in the second wave of the survey, as well as a t-test for equality between the two means. No statistically significant difference is apparent in income, parental education, or the age of the index child. 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 and households who have an older sibling in addition to the index child; these families will generally be headed by older parents. The index child is on average 15 during the second wave of the survey, and there is a small difference in average age comparing the sample and the subample. Height-for-age is also slightly lower in the subsample relative to the full sample.

The dependent variable of interest is educational expenditure per child per semester, reported by the head of household in six categories: tuition, educational supplies, food consumed in school, transportation and housing, tutoring, and other fees.[2] Each household reports separately expenditure for each child in each of these categories. In the Chinese educational context, supplies, tutoring, and other fees correspond to expenditure undertaken by the household to improve a child's academic performance, independent of the school attended. Expenses for transportation, housing, and food, on the other hand, may also vary in accordance with the choice of school and the choice of whether or not to have a child board at school. Discretionary expenditure 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 Panel B of Table 1. Total educational expenditure averages slightly less than 300 yuan per child per semester, or a total of 1040 yuan for two children over a year. This indicates that an average of 16% of mean household income is allocated to educational expenditure.

[2]In China, textbook fees are mandatory and levied as part of the overall tuition, and here they are likewise reported in the tuition category. "Supplies" represents those other than textbooks.

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 (World Health Organization, 1995; Grantham-McGregor et al., 2007). It has also been employed as a measurement of nutritional status and malnutrition for adolescents up to age 18 (Prista et al., 2003; Sawaya et al., 1995; Leenstra et al., 2005). At the same time, evidence suggests that height-for-age largely reflects the history of nutrition or health prior to age three, as after this age, catch-up for a child stunted in infancy is limited (Martorell, 1999; Alderman, Hoddinott and Kinsey, 2006). Accordingly, a robust relationship between height-for-age and early childhood shocks is expected. Summary statistics on height-for-age for the index child in the sample and the subsample are also shown in Table 1. The average height-for-age is –1.1, suggesting, perhaps unsurprisingly, that this is a predominantly stunted population.

The primary data is supplemented by climatic data for Gansu. Grain yield data pre-1996 is from data tabulated by the Ministry of Agriculture; post-1996, the data is drawn from annual editions of the Rural Chinese Statistical Yearbook. Grain yield is measured annually at the county level in tons per hectare. These data are linked to rainfall data consisting of monthly reports of climate stations in China compiled by the China Climate Project, interpolated to the latitude and longitude of the villages in the sample using the inverse-distance weighting method. Data from stations within 200 kilometers of the county of interest are employed unless there are fewer than three stations in that radius, in which case the radius is increased to 250 kilometers. On average, each measure of rainfall at the village level is constructed by interpolating between rainfall reports at six stations.[3] Rainfall is observed for all villages in the sample, but the top and bottom 1% of all rainfall measures are trimmed to avoid the influence of outliers.

[3] The minimum number of stations used to construct a rainfall measure is three; the maximum is nine. The 25th and 75th percentiles of the distribution of stations across villages are four and seven, respectively.

3 Empirical evidence

3.1 Ordinary least squares

The primary relationship of interest in this analysis is equation (1), where the dependent variable is the reported expenditure on the education of child i in household h, school s, county c and born in year t, denoted E_{ihsct}. The independent variable is endowment as measured by height-for-age, denoted H_{ihsct}. A household fixed effect, denoted η_h, absorbs any household-level heterogeneity in income or the propensity for educational spending, and calendar birth month Mon_{ihsct} and gender G_{ihsct} are included as controls. The equation of interest is thus the following:

$$E_{ihsct} = \beta_1 H_{ihsct} + \beta_2 Mon_{ihsct} + \beta_3 G_{ihsct} + \eta_h + \epsilon_{ihsct} \tag{1}$$

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. The child-specific error term is denoted ϵ_{ihsct}, and standard errors are clustered at the county level. Any variable that is unchanging within the household (for example, parental health, parental attitudes toward education, or household wealth) is thus collinear with the household fixed effect η_h. I do not control for age directly in this specification, given that age (or year-of-birth) is the primary source of variation in the climatic shock of interest conditional on household fixed effects.

The equation is estimated for each of the six categories of educational expenditure, as well as for a dummy variable for enrollment, total expenditure, and total discretionary expenditure (excluding tuition). The results, shown in Table 2, are generally insignificant. However, there is the potential for bias in these results. The variable of interest is early childhood endowment, and height-for-age is believed to be a reasonable proxy, given that it primarily reflects nutritional availability prior to age three. However, it is impossible to rule out the hypothesis that height-for-age also embodies a significant component of prior parental investment. The child who has already been the target of greater parental investment will appear to have a greater endowment and, if there is some serial correlation in parental behavior, is likely to continue to receive more substantial investments. This will generate an upward bias in the estimated coefficients that may be problematic. Eliminating this bias is the goal of the identification strategy.

3.2 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: grain yield in utero and in the early years in life. Accordingly, the postulated first stage in its simplest form is the following, where S_{ct} denotes the climatic shock for children born in county c and year t, and month of birth and gender are again included as controls.

$$H_{ihsct} = \beta_1 S_{ct} + \beta_2 Mon_{ihsct} + \beta_3 G_{ihsct} + \epsilon_{ihsct} \tag{2}$$

There are 413 pairs of siblings for which anthropometric data is reported, or 826 children.[4]

As already noted, the first climatic measure employed is grain yield in the county and year of birth. Given that such statistics are only reported annually, grain yield in the first year of life for a given child is calculated as a weighted average of grain yield in the calendar year of birth and the following year, with the weights depending on the month of birth. Analogous strategies are used to calculate grain yield in the second year of life and in utero.[5]

The use of the grain yield measure may raise questions as to whether grain yield is correlated with other household or county covariates that could affect children's outcomes

[4] An additional four grain yield observations in a single county-year cell are missing; while no sampled children are born in that year, this yields missing observations for the regressions employing data in utero and in the second year of life.

[5] Defining G_1 as grain yield in the calendar year of birth and G_2 as grain yield in the next calendar year, the climatic shock in the year of birth is defined as G_1 for children born in January, $11/12G_1 + 1/12G_2$ for children born in February, and so on. The results of interest are robust to alternate constructions of the grain yield variable.

(e.g., the use of agricultural inputs, or other climatic or topological features). Accordingly, I will construct a second measure of predicted grain yield, denoted $\hat{S}_{ct}$, using rainfall as reported in the villages and years observed in the sample. In order to construct predicted grain yield, I regress grain yield on rainfall as measured in two periods of the calendar year (January to June, and July to December), as well as village and year fixed effects. (While grain yield is reported only at the level of the county and year, rainfall observations are available for each village in each month.) I then generate predicted values of grain yield shocks, denoted $\hat{S}_{ct}$, and regress height-for-age on predicted grain yield as an alternate first stage. The specification of interest for predicted grain yield can be written as follows.

$$H_{ihsct} = \beta_1 \hat{S}_{ct} + \beta_2 Mon_{ihsct} + \beta_3 G_{ihsct} + \epsilon_{ihsct} \tag{3}$$

Panel A of Table 3 shows the first stage in cross-section for the raw grain yield measures, estimating equation (2) with grain yield in utero and in years one and two of life, respectively, as the independent variables of interest. All specifications include standard errors clustered at the county level. The results show coefficients on grain yield that are positive and significant. Moreover, there is no significant difference in the impact of the climatic shock experienced over the three years examined; the pattern detected is entirely consistent. The magnitude suggests a 25% increase in grain yield relative to the mean in a single year in this critical period leads to an increase in height-for-age of around 6%. Panel B of the same table shows the first stage in cross-section for predicted grain yield, estimating equation (3). The pattern of results is very similar, and the magnitude of the estimated coefficients is comparable.[6]

Table 4 shifts the focus to a within-household specification, presenting the results from the first stage estimated with household fixed effects. The specification is thus analogous to the ordinary least squares specification already estimated, again employing both grain yield and predicted grain yield. The full sample of both first-born and second-born

[6]The number of observations reported for the specifications employing predicted grain yield as the instrument is slightly lower, given that the trimmed dataset does not include rainfall data for all villages, and thus does not generate values of predicted grain yield for all villages.

children is employed, and the specifications of interest are as follows.

$$H_{ihsct} = \beta_1 S_{ct} + \beta_2 Mon_{ihsct} + \beta_3 G_{ihsct} + \eta_h + \epsilon_{ihsct} \tag{4}$$

$$H_{ihsct} = \beta_1 \hat{S}_{ct} + \beta_2 Mon_{ihsct} + \beta_3 G_{ihsct} + \eta_h + \epsilon_{ihsct} \tag{5}$$

The results in columns (1) through (3) show a positive correlation between the within-household difference in raw grain yield in the county and year of birth and the observed difference in height-for-age that is significant for shocks in utero and in the first and second year of life. In order to maximize predictive power, I then define the total grain yield shock as the mean of grain yield in years zero (in utero), one, and two, and employ this variable in an analogous regression. This is the primary first stage of interest, and it can be seen in column (4) of Table 4. The same specifications are then replicated for the predicted grain yield measures in columns (5) through (8), and show a similar pattern of positive and significant coefficients.

Given that the standard errors are clustered at the level of the county, and twenty counties are observed in the sample, I also examine the robustness of the estimated effects to a correction for small-sample bias. I use the wild bootstrap procedure suggested by Cameron, Gelbach and Miller (2008) to bootstrap p-values, and report the corresponding wild bootstrap p-values in brackets in the final row of the table.[7] The estimated specifications are all robust to the use of the wild bootstrap. (Note that the wild bootstrap procedure generates only a t-statistic and corresponding p-value, not an estimated standard error; for this reason, only wild bootstrap p-values are reported.)

One potential source of noise in these specifications is the timing of the relevant harvest vis-a-vis the birth. The definition of the grain yield variable assumes that the most important shock is the quality of the harvest, as proxied by grain yield, following conception and birth; thus, the measurement of grain yield proceeds forward from the date of conception or the date of birth and calculates average grain yield for the next

[7] The bootstrap is implemented using code adapted from that made public by Douglas Miller in conjunction with the 2008 paper.

nine or twelve months. However, given that grain crops are primarily harvested in the third quarter, it is possible that for children conceived later in the calendar year, the most important harvest actually precedes their conception, generating the grain stock that subsequently feeds the pregnant mother. (Analogously, one could argue that the most important harvest for the first year of life could precede birth for children born late in the year, generating the grain stock that feeds the infant.) In order to test this hypothesis, I estimate equation (12) adding an interaction term between the total grain yield measure (for raw grain yield) and month of birth. The result in column (9) of Table 4 shows the interaction term is insignificant.

As a robustness check, I also examine whether there is evidence of cross-dependence of shocks: controlling for his or her own shocks in infancy, the exclusion restriction requires that there is no dependence of one sibling's height-for-age on shocks experienced during the infancy of the other sibling. For example, one major threat to the identification strategy would be reallocation of resources by households in response to an adverse event: e.g., if following the birth of the second child, households preferentially direct resources to either the first or the second child when a negative shock occurs. This would be evident in a significant relationship between the older child's height and the shock to the younger child.

Note that coefficients on own shock and sibling shock cannot be estimated separately in a household fixed effects specification. Accordingly, I will estimate an analogous specification replacing household fixed effects with county fixed effects κ_c and year-of-birth fixed effects for the first-born child, $\gamma_{t,elder}$. (The same set of fixed effects will be used in all subsequent robustness checks that exclude household fixed effects.) The equation of interest is thus the following, where S_{ct}^{j} denotes the climatic shock for sibling j and ζ_{hsct} denotes the household-specific error component. The raw grain yield variable is employed in this test.

$$H_{ihsct} = \beta_1 S_{ct} + \beta_2 S_{ct}^{j} + \beta_3 Mon_{ihsct} + \beta_4 G_{ihsct} + \kappa_c + \gamma_{t,elder} + \zeta_{hsct} + \epsilon_{ihsct} \qquad (6)$$

Column (10) of Table 4 reports the results; the coefficient observed on the sibling shock variable is negative, insignificant, and less than a third of the magnitude (in absolute value) of the primary coefficient of interest.[8] This suggests that cross-dependence of shocks is not a major source of bias.

The magnitude of the coefficients for the grain yield measure indicates that a 25% increase in grain yield in early childhood relative to the mean for one sibling, holding the other sibling's shock constant, will increase the difference in height-for-age between them by about .40 on the scale of height-for-age Z-scores. This is equivalent to 34% of the mean height-for-age in levels, and about four times the mean difference in height-for-age.[9] In other words, 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 quarter of the standard deviation of grain yield across counties and years.

3.3 Reduced form and two-stage least squares

Table 5 shows the reduced form results. The equations estimated are the following, where again η_h denotes household fixed effects and controls for calendar month of birth and gender are included.[10]

$$E_{ihsct} = \beta_1 S_{ct} + \beta_2 Mon_{ihsct} + \beta_3 G_{ihsct} + \eta_h + \epsilon_{ihsct} \tag{7}$$

$$E_{ihsct} = \beta_1 \hat{S}_{ct} + \beta_2 Mon_{ihsct} + \beta_3 G_{ihsct} + \eta_h + \epsilon_{ihsct} \tag{8}$$

Panel A shows the results using the raw grain yield data. The first row shows the results for the base specification; standard errors are again clustered by county. The

[8] If equation (6) is estimated excluding the sibling shock variable entirely, the estimated coefficient β_1 remains positive and significant. For the sake of concision, this specification is not reported, but the tabulation is available upon request.

[9] The mean height-for-age Z score in this population is −1.1, and the mean difference in height is .1.

[10] Note that in this specification (as well as in the first stage already presented), there is no control included for the children's age or birth year. Conditional on household fixed effects, the primary source of remaining variation in early childhood climatic shocks is derived from birth year; accordingly, adding an additional control variable for birth year eliminates almost all residual variation in climatic shocks.

second row converts four spending categories (transport/housing, food, tutoring, and other) to dummy variables, given that a large number of zeros are observed in those categories.[11] The third row re-estimates the primary specification of interest, adding the vector of school-level controls ξ_s that absorbs any heterogeneity in school characteristics. There is variation within-household in school characteristics when the siblings attend different schools, as is common when the elder child is enrolled in middle school and the younger child is enrolled in primary school.[12] The fourth row re-estimates the primary specification, adding a linear control for grade level.

The coefficients are consistently negative, indicating that children born in a period with more positive grain yield shocks receive less parental expenditure. The estimated coefficient on enrollment is negative but insignificant; given that mean enrollment is well over 90%, there is limited power to detect an effect on enrollment. The significant effect on tuition evident in column (3) is rendered insignificant when grade level is added, suggesting there is no robust evidence of an effect on tuition. I again report wild bootstrap p-values for the primary reduced form results, and they are consistent with the estimated standard errors.

It should be noted that both school characteristics and grade level attained could be considered poor control variables insofar as they may represent outcomes that are also determined by parents as part of a compensatory strategy. The key results are evident both with and without these additional controls, and there is no significant difference in the coefficients of interest, again with the exception of the effect for tuition. However, given that older children have generally experienced more adverse climatic shocks in this province, and they are also enrolled in higher grades in school, it is useful to note that the

[11]All measures of expenditure are set to zero for children not enrolled in school. The results remain consistent if these measures of expenditure are instead coded as missing variables and the sample is restricted only to children enrolled in school.

[12]School-level controls included are the type of school (primary, middle, or high), whether the school is public or private, the number of classes per day, the number of weeks in the school year, the number of boarding students at the school, the total number of students reported in the school, the percentage with rural registration (hukou), the percentage who are ethnic minorities, the percentage of students absent from school on the preceding day, and the number of teachers. All school characteristics are reported by the principal. Distance from the household to the school is not reported by the household or the school. For children not enrolled in school, school characteristics are set equal to zero.

compensatory effect of more expenditure for children of lower height-for-age is evident even conditioning on grade level. Further evidence around the robustness of these results to variation in expenditure driven by sibling parity (i.e., whether the child is first- or second-born) will be provided in Section 4.1.

Panel B of Table 5 shows the same specifications employing predicted grain yield as the climatic shock variable; for concision, the specifications including school characteristics and grade level controls are omitted. The same pattern of negative coefficients is observed, generally of somewhat larger magnitude. The results are again robust to the use of the wild bootstrap.

In the instrumental variables specification, the following equation is estimated, employing grain yield or predicted grain yield as an instrument for height-for-age H_{ihsct}.

$$E_{ihsct} = \beta_1 H_{ihsct} + \beta_2 Mon_{ihsct} + \beta_3 G_{ihsct} + \eta_h + \epsilon_{ihsct} \tag{9}$$

Table 6 reports the two-stage least squares results, employing grain yield as the instrument in Panel A and predicted grain yield as the instrument in Panel B. The coefficients on height-for-age are negative and significant, indicating that children with a greater endowment receive less educational expenditure, and this is true with and without extensive controls for school characteristics and grade fixed effects, across a range of expenditure categories, and using both dummy and continuous measures of expenditure. I also report in both panels p-values from an Anderson-Rubin test that is robust to potential bias introduced by weak instruments; such bias may be a concern given that the F statistic in the first stage was slightly over five. The results of this test are, however, largely consistent with the primary results.

To interpret the magnitude of the coefficients, consider that the mean difference in height-for-age between siblings is $-.1$ of a standard deviation, with the younger child on average showing greater height-for-age. The estimates that employ grain yield as the instrument and include school covariates suggest that in response to such a difference, parents would redirect around 29% of discretionary expenditure to the weaker child; the estimates including a control for grade level suggest that the magnitude of the effect is around 24%. The corresponding estimate using predicted grain yield as an instrument suggest that the difference in discretionary expenditure in response to the average difference in height-for-age would be 17%. These differences in expenditure are evident across sibling pairs in a variety of grade levels, suggesting that the cumulative impact could be substantial if parents persist in such compensatory behavior.

The evidence presented here suggests that the hypothesis that parental allocations of education are independent of children's endowment can be rejected. Parental allocations seem to favor the child with lower endowment, though there is no evidence of full specialization in either child. Given the nature of the sample employed, there are clearly some important questions about the external validity of these results. Households represented in this sample will generally be characterized by unusually strong preferences for multiple children, as they have potentially risked adverse consequences and/or fines by having an unsanctioned child; some of them may not have faced official sanctions, but simply made use of exemptions to the policy that other households did not exploit.[13] It is also possible that they are characterized by unusually strong preferences for male children, and the implications of this preference will be discussed further below.

Extrapolating from these results to a broader sample of multichild families requires assumptions about whether parents who have a large number of children also have a different preference for equality between children. The direction of this bias is not intuitively obvious, nor is it clearly predicted by basic theoretical models of intrahousehold allocation that have primarily focused on identifying a relationship between the number of children and the mean of parental investment, rather than intrahousehold variation in that investment.

In this dataset, data is available on some sibling pairs who are the youngest two chil-

[13]Gu et al. (2007) estimate that "policy fertility" in Gansu, the average fertility rate if stated policies were perfectly implemented, is 1.7. While family size in this purely rural sample is larger at 2.2, clearly there are a substantial number of legal exemptions.

dren of larger families. I also re-estimate the primary specifications, the reduced form in equation (7) and the two-stage least squares in equation (9), using the larger sample, and employing raw grain yield as the instrument of interest. The results are shown in Table 7.[14] The estimated coefficients are again negative and significant, and in fact slightly larger in magnitude (though the difference is not statistically significant). This is suggestive evidence that the observed patterns may not be limited to two-child families. The opposite exercise might also be of interest: examining intrahousehold allocation patterns in households with a number of children, and thus a presumed preference for household size, that is below the mean. Clearly, this empirical test cannot be implemented using Chinese data given low overall fertility rates. However, given that low or rapidly declining fertility rates characterize much of Europe as well as the richer Asian economies, patterns of intrahousehold allocation in low-fertility environments where one-child families are increasingly the norm may still be of interest from a policy perspective.

3.4 Other outcomes

In this dataset, disaggregated investments by child are reported for only one other category of spending, investments in medical care. Data is available from two sources: 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. Separately, the head of household (normally the father) reports medical expenses for each child and the number of days ill over the last month.[15]

Investments in health care may be quite different from investments in education: these investments are rarer (only around half of the sampled children are reported to have utilized any medical care in the previous year), and may respond primarily to short-term health challenges or emergencies. To test whether medical investments are also responsive to measures of long-term endowment, I regress the available measures of medical invest-

[14]These estimates do not include the additional controls for school characteristics or grade level.

[15]There are a number of missing reports about doctors' visits in the mother's surveys, leading to the slightly lower number of observations observed for these specifications.

ment (the number of visits to a doctor and clinic, and actual monetary expenditure) E^m_{ihsct} on height-for-age in a specification parallel to the primary specification, estimated with and without a control for medical necessity as reported by that parent, M_{ihsct}. (For maternal reports, this is the number of medical episodes or the days of school missed due to sickness; for paternal reports, this is the number of days ill over the past month.)

$$E^m_{ihsct} = \beta_1 H_{ihsct} + \beta_2 Mon_{ihsct} + \beta_3 G_{ihsct} + \beta_4 M_{ihsct} + \eta_h + \epsilon_{ihsct} \tag{10}$$

This specification is estimated using two-stage least squares, instrumenting for height-for-age with raw grain yield.[16]

The results are shown in Panel A of Table 8. The coefficients on height-for-age are insignificant, while measures of medical expenditure are highly predictive as expected.[17] This suggests that medical expenditure is not an important margin for compensation for early childhood shocks.

In addition, the time invested by each household member, including children, in various activities such as leisure, household labor, and homework, is reported. Allowing children to abstain from household chores or spend more time on academic pursuits may also be plausibly considered a form of parental investment that could be responsive to children's endowments. Accordingly, I re-estimate equation (9) with dummy variables equal to one if a child is reported to invest time in the specified activity as the dependent variable. Dummy variables are employed given that, with the exception of television, no more than 50%–60% of children are reported to spend time on each activity enumerated. Note that there is no adding-up constraint imposed in the data collection procedure; some parents report that their children spend very few hours on these activities combined, while the mean total time reported is 22 hours.

The results are shown in Panel B of Table 8. It is evident that children of greater

[16]The results including predicted grain yield are comparable, but for concision are not reported.

[17]It should be noted, however, that the coefficients on height-for-age are of substantial magnitude, albeit imprecisely estimated.

height-for-age allocate significantly more time to household labor, including fetching water, chopping wood, and eldercare (though the latter coefficient is narrowly insignificant). They are also significantly less likely to spend time on their own homework. Both phenomena are consistent with children of lower height-for-age as a result of early childhood shocks benefiting from a lighter allocation of household labor, and investing more time in academic work, as a form of parental compensation. The magnitude suggests that the probability of engaging in chores increases by around 10% in response to the average difference in height-for-age between children; the probability of investing time in homework decreases by slightly less than 30%.

Taken together, these results constitute suggestive evidence that time allocation may be another strategy of parental compensation for children of relatively weaker endowment. There is, however, little evidence of an effect on investments in health care, with the caveat that the estimated coefficients are imprecise.

3.5 Violations of the exclusion restriction

The exclusion restriction in the primary two-stage least squares specification assumes that the only channel through which early childhood climatic shocks affect educational expenditure, conditional on household fixed effects, is via nutritional availability and hence the determination of the child's height-for-age. One obvious set of outcomes that are presumably affected by grain yield shocks would include the household's wealth and income. However, given that there is no variation in wealth or income conditional on household fixed effects, this is not a source of bias in the main specification.

There are several potential additional channels through which early shocks could affect height-for-age or educational investment in children other than via nutritional availability. However, they all rely on shifts within the household that differentially affect the child who is young at the time of the shock. First, if parents reallocate labor in response to a climatic shock and this differentially affects the child who is infancy (and not, for example, an older child), this could be a source of bias.[18] Second, if climatic shocks lead to changes in household composition (due to temporary or permanent outmigration, for example), these changes could also affect subsequent investment in education. Again, it is necessary to assume that the shifting household composition differentially affects the child who is an infant at the time of the shock. In this sample, it can be verified that household composition as measured in the household survey is not correlated with the climatic shocks in the children's years of birth.[19] It is not possible, however, to rule out temporary migration in response to these shocks.

Third, if climatic shocks lead to changes in household attitudes toward education (e.g., if parents change their perception of the relative returns of agricultural versus non-agricultural occupations), this could be a source of bias. Again, it is necessary to assume that this change primarily affects expenditure on the child who was in infancy during the adverse shock. It is not obvious that this assumption is plausible, given that attitudinal shifts would presumably affect educational expenditure on both children.

In addition, it is useful to note that if the primary channel through which climatic shocks affect later educational expenditure is via shifts in household composition or parental labor shocks, is is plausible that the effect of such shocks could persist beyond the so-called "critical period" prior to age three. Changes in household composition or attitudes presumably would shift parental expenditure on children, even if these changes occur at a point when the children have already entered school. However, evidence presented in Section 4.4 will suggest that climatic shocks have no effect on educational expenditure if they occur after the first three years of life. This is consistent with the assumption that there are no meaningful channels through which early childhood climatic shocks affect subsequent educational expenditure, conditional on household fixed effects, other than via the determination of height-for-age.

[18]There is some evidence in a companion paper using the same dataset, Leight et al. (2015), that labor supply shocks are an important channel through which climatic shocks affect height-for-age in a cross-household analysis.

[19]Tabulations are not reported, but are available upon request.

4 Robustness checks

4.1 Sibling parity effects

There is a trend in climatic shocks in Gansu in the period of interest – more specifically, the mean of the grain yield difference between the first-born and second-born children is positive. This suggests that sibling parity (i.e., birth order) could be a source of bias in this specification, if the evident preference for the relatively weaker child in fact reflects a preference for the first-born child. Accordingly, I re-estimate the primary specification adding a linear control for sibling parity, P_{ihsct}. The equation estimated can be written as follows, and grain yield is employed as an instrument for height-for-age.

$$E_{ihsct} = \beta_1 H_{ihsct} + \beta_2 Mon_{ihsct} + \beta_3 G_{ihsct} + \beta_4 P_{ihsct} + \eta_h + \epsilon_{ihsct} \tag{11}$$

The results of estimating equation (11) are shown in Panel A of Table 9; standard errors are again clustered by county. For this and subsequent robustness checks, I report only the results using the raw grain yield measure as an instrument for concision. The results show the same negative relationship between height-for-age and educational expenditure evident in the primary results, and coefficients on sibling parity that are small in magnitude and generally insignificant. There is some evidence that first-born children are more likely to be enrolled in school – perhaps unsurprising, given their older age – and they seem to spend more on school supplies. However, these effects are small in magnitude relative to the estimated coefficients on height-for-age (with the exception of the coefficient on tuition, where the magnitudes are comparable).

This suggests that a preference for the first-born child is unlikely to be the primary omitted variable driving the main results. Comparing the magnitude of the coefficient on height-for-age to the coefficient estimated in the primary specification, the estimated coefficients in specification (11) are slightly smaller, though the difference is not statistically significant.

4.2 Gender and intrahousehold allocation

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. While the primary specifications included gender as a control variable, this may not be regarded as fully satisfactory given the abundant anthropological and demographic evidence on abortion, abandonment, or underreporting of female children in China (Coale and Banister, 1994; Qian, 1997); it is implausible to assume that the gender of both children can be assumed to be random.[20] In Gansu, the sex ratio in 2000 was 111.2, close to the national average of 113.6, and indicative of substantial household determination of child gender (Banister, 2004). Accordingly, households with different gender balances among their children are likely to differ materially along other observable and unobservable dimensions.

The gender of the first child, however, may be a plausibly exogenous observation, as anthropological evidence indicates that selection for gender occurs principally in births subsequent to a first-born daughter, and selective abortion prior to the birth of a first child is unusual (Gu et al., 2007; Banister, 2004). The evidence in this sample is consistent with this hypothesis. The sex ratio for the first child is not significantly different from .5, while for the second-born child, the sex ratio is highly imbalanced: 67% of second-born children are male. However, the sex ratio for the second child in households with a first-born male is likewise not significantly different from .5. Households choosing to bear a second child following the birth of a son do not engage in costly sex-selection methods in order to bear a second son.

Accordingly, in households with a first-born son, the gender of both children can plausibly be considered to be quasi-random. In order to test the robustness of the primary results to bias introduced by unobservable gender preferences among parents, I re-estimate the primary specification equation (9), restricting the sample to households with a first-born son and employing grain yield as an instrument. The estimation results

[20]The primary results are also robust to the exclusion of the gender variable.

are shown in Table 9, and the pattern of negative and significant coefficients is consistent with the primary results. While the coefficients observed are generally somewhat smaller in magnitude, the difference is not statistically significant.

4.3 Selection bias

Selection into the sample of two-child families observed in this analysis would also constitute a violation of the exclusion restriction. If families with certain characteristics are more or less likely 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. An alternative strategy to test for selection effects exploits the presence 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 likely produce an attenuation toward zero in an otherwise positive relationship between grain yield shocks and height-for-age. This reflects the fact that surviving children, while weakened by adverse conditions in infancy, are nonetheless likely to be genetically more robust and thus have a propensity toward greater health, weakening the correlation between grain yield and height-for-age.

On the other hand, if selection via differential mortality is not an important phenomenon, there should not be an attenuation of the relationship between shocks and health outcomes as the severity of the shock increases – assuming that the relationship between grain yield and height-for-age is otherwise linear. This is clearly a strong assumption, and accordingly the results should be interpreted with caution.

To test this hypothesis, dummy variables are defined to capture severe climatic shocks of varying intensity. A severe shock is identified when total grain yield in the critical period of interest (in utero and in the first and second years of life) falls below the 20th, 30th, 40th, 50th, and 60th percentiles, respectively. The first stage equation conditional on household fixed effects is then re-estimated, adding these dummy variables and an interaction between the dummy variable and the linear grain yield term. For example, the following equation shows the specification employing a dummy variable for grain yield below the 20th percentile.

$$H_{ihsct} = \beta_1 S_{ct} + \beta_2 D^{20}_{ihsct} \times S_{ct} + \beta_3 D^{20}_{ihsct} + \eta_h + \epsilon_{ihsct} \tag{12}$$

The objective is to test whether the slope of the positive relationship between grain yield and height-for-age is attenuated toward zero in the lower part of the climatic shock distribution, a phenomenon that would be evident in a negative coefficient on β_2.

The results are shown in Panel A of Table 10. The coefficients β_2 are positive and generally significant, suggesting that selective mortality is not a particularly relevant phenomenon in this sample. In fact, the positive relationship between grain yield shocks and height-for-age is larger in magnitude for children in the lower part of the height-for-age distribution. While these results may partially reflect non-linearities in the relationship between grain yield and height-for-age independent of selective mortality, the results here would only be consistent with selective mortality if there are very large non-linearities in this relationship at low levels of grain yield, rendering the relationship of interest positive rather than negative. While these results must be interpreted cautiously, they seem to be consistent with the absence of any significant phenomenon of selective mortality.

Another channel through which selection bias could occur is if parents respond to the climatic shock experienced in the first year of their eldest child's life by altering their fertility. If the probability that they have a second (or higher parity) child is altered, this would also affect their probability of entering the sample. In order to examine this correlation, dummy variables equal to one if a household has a second (or third) child, as well as dummy variables corresponding to the gender of the second (or third) child, are regressed on the climatic shock variable for the first (or second) child, conditional on county and year fixed effects. Years between the birth of the first and second child is also employed as a dependent variable, given that previous evidence has suggested that increased spacing between births may be a strategy employed by some parents to enable greater investment in the child born earlier (Rosenzweig and Wolpin, 1988).

The specifications of interest are thus the following, where F_{hct} denotes a fertility outcome (a dummy for the birth of a second or third child, gender of the second or third child, or years between the first and second birth) for household h with first-born child in year t in county c, and $\tilde{F}_{hcy}$ denotes a fertility outcome (dummy for the birth of a third child and gender of the third child) for household h with second-born child in year y in county c. S denotes the corresponding climatic shock for the child of interest, and γ are year of birth fixed effects for the child of interest.[21] ζ again denotes the household-specific error component.

$$F_{hct} = \beta S_{tc} + \kappa_c + \gamma_{t,elder} + \zeta_{hct} \tag{13}$$

$$\tilde{F}_{hcy} = \beta S_{cy} + \kappa_c + \gamma_{y,younger} + \zeta_{hcy} \tag{14}$$

$$\tag{15}$$

The results are shown in Panel B of Table 10.[22] In general, there is no evidence of a relationship between early-life shocks for infants and the probability of a later birth. This suggests that parents' subsequent fertility decisions are not shaped by the climatic shock experienced in the first year of life by their first-born child.

There is some evidence, apparent in column (2) of the same table, of a negative

[21] The climatic shock S is again defined as the mean of grain yield in utero and in the first and second years of life, parallel to the main analysis. I use raw grain yield measures rather than predicted grain yield.

[22] The sample for these specifications include all households for whom month of birth is reported for the first-born child, allowing the construction of the grain yield variable of interest. Month of birth was only reported for children who participated in the anthropometric data collection. For this reason, the sample size is lower than the full sample size of 1918.

relationship between the grain yield shock for the first-born child and the gender of the second child. Parents who have a positive shock in their first child's year of birth are subsequently more likely to have a boy; this could be interpreted as evidence of an income effect, as households having experienced a positive shock may be more able to invest in costly technology to manipulate the gender of a subsequent child. Given that the effect of gender on educational expenditure is not large in this sample, however, this is not a significant source of bias.

Taken together, these results suggest that selection into the observed subsample of two-children families is unlikely to be a major phenomenon, and households represented in this subsample have not exhibited significantly different patterns of fertility or mortality.

4.4 Placebo tests

A large existing literature already cited suggests that climatic shocks after the critical period of development in early childhood do not plausibly have a substantial impact on height-for-age, conditional on household fixed effects. While there is no consensus on when this critical period ends, age three is often cited as a cut-off point. Moreover, the postulated exclusion restriction for the main specification of interest here suggests that if climatic shocks later in childhood do not affect height-for-age, they should not subsequently determine the intrahousehold allocation of educational expenditure. Accordingly, estimating the impact of shocks after age three on the dependent variables of interest serves as a useful placebo test.

In order to conduct this test, I identify grain yield shocks in the county of residence in years four and five of life, denoted S_{ct}^{+4} and S_{ct}^{+5} respectively, and re-estimate the reduced form for four expenditure outcomes (tuition, total discretionary expenditure, supplies, and transportation/housing). I employ shocks in year four as the earliest placebo shock given the identification of the period before age three as the critical period of early childhood development in the literature.

For each shock, the sample is restricted to sibling pairs in which the younger child is at least three by the year of the shock: i.e., for grain yield at age four, the sample is restricted to sibling pairs where the age gap is no more than a year, such that the younger child is three by the time the older child is four. The equation of interest can thus be written as follows for the shock four years after birth.

$$E_{ihsct} = \beta_1 S_{ct}^{+4} + \beta_2 Mon_{ihsct} + \beta_3 G_{ihsct} + \eta_h + \epsilon_{ihsct} \tag{16}$$

I also re-estimate the main reduced form specification (7) with the primary grain yield shock as the independent variable, but employing the restricted sample, in order to compare the coefficients for the sample of interest. For concision, I employ the simple grain yield measure, rather than predicted grain yield.

The results are shown in Table 11; in each panel, columns (1) through (4) report the placebo tests, while columns (5) through (8) report specifications employing the restricted sample and the primary grain yield shock of interest. It is evident that the coefficients on the shocks in years four and five are small in magnitude and not statistically significant. By contrast, the effect of the primary grain yield shock remains large in magnitude, even in this restricted sample, and significant in all specifications except one.

In the bottom row of each panel, I report the results of Wald tests for equality of coefficients across the specifications using the shocks in years four and five and the analogous specifications using the primary grain yield shock. For each specification, the p-value tests the equality of the coefficient on the placebo shock and the primary grain yield shock using the same sample; for example, the p-value in column (1) of Panel A tests equality of the coefficients across columns (1) and (5) of that panel. For the year four shocks, equality of the coefficients can be rejected for two of the four measures of expenditure, and for the third, the specification narrowly fails to reject at the 10 percent level. For the year five shocks, equality of the coefficient in the placebo test and the primary coefficient of interest can be rejected for all four measures of expenditure. This evidence suggests that climatic shocks later in childhood do not significantly determine parental allocation of educational expenditure, consistent with the assumption that the channel of causality for the primary results runs through the effect of climatic shocks on child development during a critical period in infancy.

5 Conclusion

In the previous literature on intrahousehold allocation, the question of the presence or absence of family aversion to inequality has received extensive analytical attention. However, little evidence has been presented regarding the nature of parental responses to systematic differences in physical endowment among children, particularly non-twins.

I employ an identification strategy that relies on the correlation between climatic variation and height-for-age, mediated through the impact of nutritional shocks in infancy. I find a pattern of preferential allocations of discretionary educational expenditure to children of lower endowment, consistent with a parental preference for equality of outcomes. The relationship is robust across multiple specifications and measures of expenditure, and robust to the inclusion of both gender and sibling parity. These results imply that, at least in education, the household is serving as a mechanism for the mitigation of existing inequalities. There is, however, no evidence that health expenditure follows a similar pattern.

These results raise the question of whether the observed allocation favoring the weaker child 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 – or more accurately, is perceived by parents to have higher returns – then the observed strategy could be interpreted as maximizing returns to educational investment.

While no robust evidence on this point is available in my sample, a survey of the mothers of the sampled children does collect information on the level of education she expects each to attain. A simple test of the perceptions of the returns to educational expenditure on different children can be implemented by regressing this expectation on reported expenditure, height-for-age, and the interaction between the two, conditional on household fixed effects, and instrumenting for height-for-age with grain yield in infancy. This test shows no evidence that returns to expenditure are perceived by mothers to be systematically different for children of varying height-for-age.[23] While this evidence must be considered only suggestive, it is consistent with parents seeking to provide a consumption-like benefit to children of lower initial endowment, rather than responding to systematic differences in returns to expenditure across different children.

For the purposes of the welfare analysis of potential household interventions, this is an encouraging result that suggests that household-level interventions may improve welfare outcomes for the weakest members of a family. Policies that aim to increase human capital investment for struggling children are typically targeted at a household level. Even if a transfer is specifically designated for a particular child, this provision is challenging to observe or enforce, and parental reallocation of other consumption may undo any intended benefit for the child of interest. However, this study provides evidence that household processes of allocation in the rural Chinese context favor the direction of human capital resources toward children that have experienced negative shocks and the associated negative health effects. This may provide a higher degree of confidence that external transfers to these households will in fact benefit their more vulnerable children.

[23] Tabulations available on request.

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6 Tables

Table 1: Summary statistics

Panel A: Demographic data				Panel B: Educational expenditure per child			
	Sample	Subsample	p-value		Mean	Std. Dev.	Max.
Net income	6791.97	6169.01	.352	Total	260.27	311.48	4240
Net income agri.	2618.84	2444.59	.276	Discretionary	97.88	181.11	2240
Income per capita	1705.01	1525.74	.234	Tuition	162.4	170.08	2000
Father educ.	7.11	6.88	.142	Supplies	35.67	38.26	300
Mother educ.	4.29	4.1	.199	Transport/Housing	11.81	45.66	600
Father age	42.62	38.95	.061	Food	35.5	115.02	1700
Mother age	39.25	36.88	0	Tutoring	5.41	16.02	100
Index child age	15.1	15.01	.081	Other fees	9.49	27.73	360
Height-for-age	-1.19	-1.31	.017				
Obs.	1918	413					

Note: The sample encompasses the full sample of households that report income data; this is 1918 out of the full sample of 2000 households in the survey. The subsample is households with two-children families in which both children report anthropometric data. There are 413 households in the subsample of interest, and 826 children. Income is reported in yuan; educational expenditure is reported in yuan per semester. The third column in Panel A reports the p-value for a test of equality of means across the sample and subsample. All variables are reported in the second survey wave collected in 2004.

Table 2: OLS

	Enrollment	Total	Discretionary	Tuition	Supplies	Trans./Housing	Food	Tutoring	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Height-for-age	.009	-7.259	-6.676	-.584	-1.897	-.503	-4.473	.715	-.519
	(.009)	(11.079)	(7.254)	(7.135)	(1.016)*	(1.246)	(5.938)	(.502)	(.708)
Mean (levels)	.95	260.27	97.88	162.4	35.67	11.81	35.5	5.41	9.49
Median (levels)	1	180	40	125	25	0	0	0	0
Mean (dif.)	-.07	-145.06	-79.15	-65.9	-13.37	-14.16	-39.78	-5.58	-6.26
Obs.	826	826	826	826	826	826	826	826	826

Note: The dependent variable is educational expenditure on a given child in the specified category; the independent variable is height-for-age. All regressions include household fixed effects, controls for birth month and gender, and standard errors clustered by county. The mean in differences reports the mean intra-household difference in expenditure (between the first and second child) in the specified category.
* $p < .1$
** $p < .05$
*** $p < .01$

Table 3: Cross-household variation in height-for-age

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Panel A: Grain yield					
Utero shock	.138	.131	.139						
	(.039)***	(.043)***	(.047)***						
Year 1 shock				.130	.117	.136			
				(.040)***	(.046)**	(.044)***			
Year 2 shock							.134	.134	.131
							(.035)***	(.040)***	(.042)***
Sample	All	First-born	Second-born	All	First-born	Second-born	All	First-born	Second-born
Obs.	822	409	413	826	413	413	822	409	413
F	12.586	3.548	6.99	13.51	3.23	6.962	15.406	4.337	7.191
				Panel B: Predicted grain yield					
Utero shock	.130	.117	.136						
	(.042)***	(.042)***	(.054)**						
Year 1 shock				.130	.117	.140			
				(.042)***	(.042)***	(.055)**			
Year 2 shock							.128	.118	.136
							(.042)***	(.043)***	(.054)**
Sample	All	First-born	Second-born	All	First-born	Second-born	All	First-born	Second-born
Obs.	812	408	404	817	407	410	819	407	412
F	13.086	3.367	5.133	12.064	3.67	4.781	13.097	3.206	4.712

Note: The dependent variable is height-for-age; the independent variables are grain yield in the specified year in Panel A, and predicted grain yield in the specified year in Panel B. Month of birth and gender are included as control variables. The sample employed is as specified in the table; standard errors are clustered by county.
* $p < .1$
** $p < .05$
*** $p < .01$

Table 4: First stage

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Utero shock	.537 (.175)*** [.040]**									
Year 1 shock		.513 (.164)*** [.010]*								
Year 2 shock			.430 (.149)*** [.010]*							
Total shock				.579 (.176)*** [.020]**					.605 (.213)***	.450 (.134)***
Utero pred. shock					.846 (.313)*** [.000]***					
Year 1 pred. shock						1.160 (.384)*** [.020]**				
Year 2 pred. shock							1.444 (.418)*** [.000]***			
Total pred. shock								1.446 (.434)*** [.020]**		
Month int.									-.003 (.008)	
Sibling shock										-.115 (.118)
Fixed effects		Household			Household			Household		County + elder year-of-birth
Obs.	822	826	822	826	795	801	806	821	826	826
F	4.409	4.612	3.927	5.137	3.379	4.249	5.515	5.201	2.652	5.629

Note: The dependent variable is height-for-age; the independent variables are grain yield in the specified year or predicted grain yield in the specified year, total grain yield or predicted total grain yield, the interaction of total grain yield with month of birth, and the total grain yield shock experienced by the sibling. Total grain yield and total predicted grain yield are defined as the mean of the corresponding shocks in utero and in the first and second year of life. All specifications include month of birth and gender as controls. The fixed effects included are specified in the table; wild bootstrap p-values are reported in brackets in Columns (1) through (8). Standard errors are clustered by county.

* $p < .1$

** $p < .05$

*** $p < .01$

Table 5: Reduced form

	Enroll. (1)	Total (2)	Discret. (3)	Tuition (4)	Supplies (5)	Transportation/Housing (6)	Food (7)	Tutoring (8)	Other (9)
Panel A: Grain yield									
Grain yield	-.055	-288.166	-207.786	-80.380	-21.423	-35.643	-121.839	-12.165	-16.716
	(.035)	(34.316)***	(32.731)***	(16.921)***	(3.522)***	(6.077)***	(25.509)***	(2.260)***	(3.251)***
Wild bootstrap p-values	[.100]*	[.020]**	[.020]**	[.020]**	[.020]**	[.010]***	[.020]**	[.020]**	[.020]**
Grain yield					-.322	-.307	-.206	-.085	
					(.036)***	(.053)***	(.022)***	(.035)**	
Primary spec. + school controls	-.019	-199.851	-159.203	-40.648	-10.412	-29.924	-98.613	-8.814	-11.440
	(.032)	(39.380)***	(32.717)***	(16.295)**	(3.158)***	(7.529)***	(25.511)***	(2.069)***	(3.519)***
Primary spec. + grade level	-.083	-109.839	-129.583	19.744	-3.906	-21.669	-84.902	-8.189	-10.917
	(.049)*	(43.531)**	(39.421)***	(13.340)	(4.914)	(5.939)***	(28.081)***	(2.638)***	(4.781)**
Panel B: Predicted grain yield									
Predicted grain yield	-.213	-622.112	-382.914	-239.197	-50.257	-65.576	-216.546	-23.036	-27.499
	(.091)**	(110.894)***	(93.587)***	(37.441)***	(7.372)***	(19.517)***	(66.076)***	(5.313)***	(7.639)***
Wild bootstrap p-values	[.100]*	[.020]**	[.020]**	[.020]**	[.020]**	[.020]**	[.020]**	[.020]**	[.020]**
Predicted grain yield						-.538	-.601	-.457	-.152
						(.133)***	(.118)***	(.079)***	(.085)*
Mean (levels)	.95	260.27	97.88	162.4	35.67	11.81	35.5	5.41	9.49
Median (levels)	1	180	40	125	25	0	0	0	0
Mean (differences)	-.07	-145.06	-79.15	-65.9	-13.37	-14.16	-39.78	-5.58	-6.26
Obs.	826	826	826	826	826	826	826	826	826

Note: The dependent variable is educational expenditure on a given child in the specified category; the sample is children living in two-children households. In Panel A, the independent variable is the mean of reported grain yield in utero and in the first and second years of life; in Panel B, the independent variable is the mean of predicted grain yield in utero and in the first and second years of life. All regressions include household fixed effects, controls for birth month and gender, and standard errors clustered by county. School-level controls included are the type of school (primary, middle or high), whether the school is public or private, the number of classes per day, the number of weeks in the school year, the number of boarding students at the school, the total number of students reported in the school, the percentage with rural registration (hukou), the percentage who are ethnic minorities, the percentage of students absent from school on the preceding day, and the number of teachers; all school characteristics are reported by the principal. The mean in differences reports the mean intra-household difference in expenditure (between the first and second child) in the specified category. Wild bootstrap p-values are reported for the simple reduced form specification in brackets.

* $p < .1$
** $p < .05$
*** $p < .01$

Table 6: Two-stage least squares

	Enroll. (1)	Total (2)	Discret. (3)	Tuition (4)	Supplies (5)	Transportation/Housing (6)	Food (7)	Tutoring (8)	Other (9)
Panel A: Grain yield									
Height-for-age	-.096	-497.501	-358.730	-138.772	-36.985	-61.535	-210.348	-21.002	-28.860
	(.065)	(157.107)***	(124.401)***	(44.810)***	(10.364)***	(21.581)***	(79.820)***	(7.718)***	(11.353)**
Anderson-Rubin p-value	.209	.001	.037	.000	.062	.422	.065	.013	.004
Height-for-age						-.555	-.530	-.356	-.146
						(.179)***	(.180)***	(.118)***	(.087)*
Primary spec. + school controls	-.034	-350.793	-279.445	-71.348	-18.276	-52.525	-173.093	-15.471	-20.080
	(.057)	(124.296)***	(103.037)***	(33.466)**	(6.877)***	(20.975)**	(68.836)**	(6.168)**	(9.751)**
Primary spec. + grade level	-.151	-199.936	-235.875	35.940	-7.110	-39.444	-154.544	-14.906	-19.871
	(.095)	(90.530)**	(87.091)***	(24.823)	(9.086)	(13.991)***	(58.807)***	(5.794)**	(10.550)*
Panel B: Predicted grain yield									
Height-for-age	-.149	-436.651	-268.762	-167.889	-35.275	-46.027	-151.990	-16.169	-19.301
	(.083)*	(163.870)***	(106.942)**	(65.095)***	(10.886)***	(20.504)**	(67.200)**	(6.351)**	(7.780)**
Anderson-Rubin p-value	.909	.004	.033	.002	.214	.326	.028	.039	.083
Height-for-age						-.377	-.422	-.321	-.107
						(.147)**	(.158)***	(.120)***	(.065)
Mean (levels)	.95	260.27	97.88	162.4	35.67	11.81	35.5	5.41	9.49
Median (levels)	1	180	40	125	25	0	0	0	0
Mean (differences)	-.07	-145.06	-79.15	-65.9	-13.37	-14.16	-39.78	-5.58	-6.26
Obs.	826	826	826	826	826	826	826	826	826

Note: The dependent variable is educational expenditure on a given child in the specified category and the independent variable is height-for-age; the sample is children living in two-children households. In Panel A, height-for-age is instrumented by the mean of grain yield in utero and in the first and second years of life; in Panel B, height-for-age is instrumented by the mean of predicted grain yield in utero and in the first and second years of life. All regressions include household fixed effects, controls for birth month and gender, and standard errors clustered by county. School-level controls included are the type of school (primary, middle or high), whether the school is public or private, the number of classes per day, the number of weeks in the school year, the number of boarding students at the school, the total number of students reported in the school, the percentage with rural registration (hukou), the percentage who are ethnic minorities, the percentage of students absent from school on the preceding day, and the number of teachers; all school characteristics are reported by the principal. The mean in differences reports the mean intra-household difference in expenditure (between the first and second child) in the specified category. The Anderson-Rubin p-value is reported in both panels for the base two-stage least squares specification, including no additional controls.
* $p < .1$
** $p < .05$
*** $p < .01$

Table 7: Reduced form and two-stage least squares: Full sample

	Enroll. (1)	Total (2)	Discretionary (3)	Tuition (4)	Supplies (5)	Transportation/Housing (6)	Food (7)	Tutoring (8)	Other (9)
Panel A: Reduced form									
Grain yield	-.067	-301.218	-209.951	-91.267	-22.627	-35.160	-121.323	-12.066	-18.775
	(.036)*	(34.610)***	(31.100)***	(20.962)***	(3.607)***	(5.801)***	(24.547)***	(1.954)***	(3.059)***
Obs.	1034	1034	1034	1034	1034	1034	1034	1034	1034
Panel B: Two-stage least squares									
Height-for-age	-.131	-590.187	-411.365	-178.823	-44.334	-68.891	-237.712	-23.641	-36.786
	(.086)	(194.835)***	(141.476)***	(68.619)***	(13.599)***	(24.542)***	(88.200)***	(8.543)***	(13.874)***
Mean (levels)	.93	251.08	92.81	158.27	34.42	10.96	32.46	6.26	8.72
Median (levels)	1	174.5	40	125	21	0	0	0	0
Mean (differences)	-.07	-137.78	-73.89	-63.89	-13.15	-13.6	-37.88	-3.04	-6.21
Obs.	1034	1034	1034	1034	1034	1034	1034	1034	

Note: The dependent variable is educational expenditure on a given child in the specified category; the sample includes all sibling pairs surveyed. In Panel A, the independent variable is the mean of reported grain yield in utero and in the first and second years of life; in Panel B, the independent variable is height-for-age instrumented by this grain yield variable. All regressions include household fixed effects, controls for birth month and gender, and standard errors clustered by county. School-level controls included are the type of school (primary, middle or high), whether the school is public or private, the number of classes per day, the number of weeks in the school year, the number of boarding students at the school, the total number of students reported in the school, the percentage with rural registration (hukou), the percentage who are ethnic minorities, the percentage of students absent from school on the preceding day, and the number of teachers; all school characteristics are reported by the principal. The mean in differences reports the mean intra-household difference in expenditure (between the first and second child) in the specified category.

* $p < .1$

** $p < .05$

*** $p < .01$

Table 8: Other outcomes: Medical investments and time allocation

Panel A: Medical investments

	Doctor			Clinic			Med. expend.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Height-for-age	-.002	.033	-.007	-.139	-.085	-.142	-408.183	-420.997
	(.046)	(.056)	(.042)	(.086)	(.071)	(.091)	(429.875)	(433.259)
Med. episodes		.268			.341			
		(.084)***			(.121)***			
Days missed			.413			.468		
			(.091)***			(.177)***		
Days sick								2533.150
								(1292.865)*
Mean (levels)	.4	.4	.4	.49	.49	.49	164.52	164.52
Median (levels)	0	0	0	0	0	0	10	10
Mean (differences)	-.05	-.05	-.05	-.08	-.08	-.08	-14.13	-14.13
Obs.	799	799	799	826	826	826	826	826

Panel B: Time allocation

	TV	Cooking	Washing	Chopping wood	Water	Elder care	HW help	Playing	Homework
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Height-for-age	.037	-.083	-.085	.163	.232	.119	.065	.037	-1.366
	(.044)	(.094)	(.085)	(.105)	(.125)*	(.080)	(.085)	(.101)	(.404)***
Mean (levels)	.86	.33	.59	.13	.29	.21	.32	.55	.45
Median (levels)	0	1	0	0	0	0	1	0	
Mean (differences)	-.03	-.09	-.06	.14	.21	.07	-.02	-.01	-.86
Obs.	826	826	826	826	826	826	826	826	826

Note: The dependent variables in Panel A are measure of medical investment (whether the child has seen a doctor or visited a clinic in the last year, and medical expenditure in the last year). The independent variables are height-for-age instrumented by the mean of reported grain yield in utero and in the first and second years of life, and various measures of medical necessity/need as reported by the parents. The dependent variables in Panel B are a dummy variable equal to one if the child reports time invested in the specified activity and zero otherwise, and the independent variable is height-for-age instrumented by the mean of reported grain yield in utero and in the first and second years of life. All regressions include household fixed effects, controls for birth month and gender, and standard errors clustered by county.
* $p < .1$
** $p < .05$
*** $p < .01$

Table 9: Educational expenditure, sibling parity and gender

	Enrollment (1)	Total (2)	Discretionary (3)	Tuition (4)	Supplies (5)	Transportation/Housing (6)	Food (7)	Tutoring (8)	Other (9)
Panel A: Sibling Parity									
Height-for-age	.003	-406.523	-349.329	-57.194	-23.201	-58.970	-220.316	-18.782	-28.060
	(.066)	(146.366)***	(128.354)***	(30.461)*	(10.806)**	(21.833)***	(84.462)***	(8.119)**	(13.030)**
Parity	-.063	-58.365	-6.031	-52.334	-8.843	-1.645	6.395	-1.425	-.513
	(.023)***	(53.217)	(39.147)	(17.700)***	(3.353)***	(7.127)	(23.995)	(2.613)	(3.917)
Mean (levels)	.95	260.27	97.88	162.4	35.67	11.81	35.5	5.41	9.49
Median (levels)	1	180	40	125	25	0	0	0	0
Mean (differences)	-.07	-145.06	-79.15	-65.9	-13.37	-14.16	-39.78	-5.58	-6.26
Obs.	826	826	826	826	826	826	826	826	
Panel B: Gender									
Height for age	-.028	-307.171	-276.489	-30.682	-24.393	-36.352	-176.854	-17.201	-21.689
	(.050)	(96.567)***	(85.345)***	(23.594)	(6.630)***	(11.683)***	(58.679)***	(5.542)***	(6.144)***
Mean (levels)	.95	259.06	105.42	153.64	38.09	12.48	40.33	5.52	9.01
Median (levels)	1	177.5	45	125	30	0	0	0	0
Mean (differences)	-.03	-141.76	-80.35	-61.41	-16.68	-13.07	-39.08	-5.27	-6.25
Obs.	422	422	422	422	422	422	422	422	

Note: The dependent variable in both panels is educational expenditure on a given child in the specified category. In Panel A, the independent variable is height-for-age, instrumented by the mean of reported grain yield in utero and in the first and second years of life, and sibling parity. In Panel B, the sample is restricted to households with a first-born male child. All regressions include household fixed effects, controls for birth month and gender, and standard errors clustered by county. The mean in differences reports the mean intra-household difference in expenditure (between the first and second child) in the specified category.

* $p < .1$

** $p < .05$

*** $p < .01$

Table 10: Selection into the sample

Panel A: Early shocks and selective mortality

	Cutoff for severe shock					
	20th perc.	30th perc.	40th perc.	50th perc.	60th perc.	70th perc.
	(1)	(2)	(3)	(4)	(5)	(6)
Grain yield	.484	.474	.456	.486	.479	.478
	(.124)***	(.120)***	(.117)***	(.108)***	(.109)***	(.114)***
Yield x Severe shock	1.156	1.513	1.629	1.816	1.219	.693
	(.860)	(.777)*	(.620)***	(.922)**	(1.149)	(1.070)
Severe shock	-2.120	-2.670	-3.158	-3.062	-2.379	-1.807
	(1.269)*	(1.336)**	(1.457)**	(1.985)	(2.609)	(2.678)
Obs.	826	826	826	826	826	826

Panel B: Early shocks and subsequent fertility

	Second child				Third child		
	Birth	Gender	Spacing	Birth	Gender	Birth	Gender
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Grain yield child 1	-.003	-.024	-.032	.003	-.052		
	(.010)	(.006)***	(.026)	(.003)	(.344)		
Grain yield child 2						.006	-.171
						(.012)	(.357)
Obs.	1743	711	1743	1743	125	693	99

Note: In Panel A, the dependent variable is height-for-age, and the independent variable is the primary grain yield shock (the mean of grain yield in utero and in the first and second year of life), a dummy for the grain yield shock falling under the specified percentile threshold, and an interaction between the dummy and the linear grain yield term. All regressions include household fixed effects, controls for birth month and gender, and standard errors clustered by county. In Panel B, the dependent variables are dummy variables for the birth of a second- or third-parity child, the gender of a second- or third-parity child, and the number of years between the births of the first and second child. The independent variable is the primary grain yield shock for the specified child. All regressions include year of birth fixed effects for the specified child, county fixed effects, and standard errors clustered at the county level. Asterisks denote significance at 1, 5 and 10 percent levels.

Table 11: Placebo tests

Panel A: Discretionary expenditure and tuition

	Discretionary		Tuition		Discretionary		Tuition	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Year 4 shock	-81.086		-34.183					
	(65.615)		(70.516)					
Year 5 shock		-4.169		27.729				
		(24.022)		(30.750)				
Grain yield shock					-300.489	-358.913	-381.150	-253.257
					(212.521)	(69.499)***	(165.864)**	(63.633)***
Sample	$\Delta Age \leq 1$	$\Delta Age \leq 2$	$\Delta Age \leq 1$	$\Delta Age \leq 2$	$\Delta Age \leq 1$	$\Delta Age \leq 2$	$\Delta Age \leq 1$	$\Delta Age \leq 2$
Wald test p-value	.322	.000	.053	.000				
Obs.	120	434	120	434	120	434	120	434

Panel B: Supplies and Transportation/Housing

	Supplies		Transportation/Housing		Supplies		Transportation/Housing	
Year 4 shock	-17.588		-11.650					
	(16.738)		(19.967)					
Year 5 shock		2.927		-7.201				
		(4.659)		(7.143)				
Grain yield shock					-94.091	-51.687	-61.866	-103.128
					(37.025)**	(19.027)***	(33.223)*	(28.106)***
Sample	$\Delta Age \leq 1$	$\Delta Age \leq 2$	$\Delta Age \leq 1$	$\Delta Age \leq 2$	$\Delta Age \leq 1$	$\Delta Age \leq 2$	$\Delta Age \leq 1$	$\Delta Age \leq 2$
Wald test p-value	.111	.002	.015	.000				
Obs.	120	434	120	434	120	434	120	434

Note: The dependent variables are four measures of educational expenditure as employed in the primary analysis. The independent variables are measures of grain yield four and five years after birth, and the primary grain yield shock of interest (grain yield measured in utero, in the first year, and in the second year of life). All regressions include household fixed effects and standard errors clustered by county, and the sample is restricted to sibling pairs where the age difference is as specified in the table. The Wald test p-value reports a test of equality of the coefficients on the placebo grain yield shock and the primary grain yield shock for the specified sample and dependent variable.
* $p < .1$
** $p < .05$
*** $p < .01$