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ABSTRACT

Substitution Effects in Parental Investments*

The paper estimates how parents adjust bride-prices and land divisions to compensate their sons for differences in their schooling investments in rural China. The main estimate implies that when a son receives one yuan less in schooling investment than his brother, he will obtain 0.7 yuan more in observable marital and post-marital transfers as partial compensation. Controlling for unobserved household heterogeneity, planned consumption differences across sons, and a fuller accounting of lifetime transfers are quantitatively important. The empirical findings strongly support the unitary model as a model of resource allocation for sons in traditional agricultural families.

JEL Classification: D13, J12, J13

Keywords: household model, parental investment, marriage market, transfers

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1 Introduction

Parents have to decide how much to invest and the types of investment to make in each of their children. As summarized in his 1991 book, Becker argues that parents should first choose investments in their children to maximize total household wealth, and then use intra-household transfers to distribute consumption among their children to maximize parental utility. Family members' consumption levels are independent of their income contributions to the family wealth.

The above model is known as the unitary or wealth maximization model of the family. There are three classes of tests of the model based on the above two predictions. One class is the income pooling test (e.g., Thomas (1990); Altonji et. al. (1992); Lundberg et. al. (1997); Duflo (2003); Bobonis (2009)). If family member i gains a dollar of exogenous own income while member j loses a dollar of exogenous own income, there should be no change in i's or j's consumption because total family wealth remains the same. This literature generally finds that the distribution of income is a crucial determinant of the distribution of consumption among the family members, contradicting the unitary model.

The second class is the income difference transfer test (e.g., Menchik (1980, 1988); Cox (1987); Cox and Rank (1992); Altonji et. al. (1997); Raut and Tran (2005)). If family member i gains a dollar of exogenous own income and member j loses a dollar of exogenous own income, i will receive one dollar less in intra-household transfers and j will receive one dollar more in transfers; hence every family member's consumption remains the same. Empirical results of this literature are mixed.[1]

A third class of test is based on the hypothesis that families invest to maximize household wealth. Efficiency in investments requires more resources to be directed towards family members with higher marginal return or lower marginal cost, which serves as a testable implication in this literature (e.g., Pitt et. al.(1990); Udry (1996); Qian (2008)). The results are mixed.[2]

The choice of which test to use is primarily driven by data availability. Many, if not most, traditional agricultural societies are patrilocal. Sons either live with their parents or stay in the same village after they marry. Parents and adult sons often farm and conduct other business together. Thus, there are closer interactions between parents and adult sons than observed in modern urban environments. Allocation decisions within rural households are more likely to be made consistently with the unitary model. However, tests of the unitary model among these rural societies are

[1] Based on US bequests data, Menchik (1980, 1988) argues that there are no compensating transfers within most families. Using pension and household survey data for the US, Cox (1987), Cox and Rank (1992) find that transfers are reinforcing rather than compensatory. Using the PSID data, Altonji et. al. (1997) estimate a marginal compensation effect of 0.13, which is very small. Using data from Indonesia, Raut and Tran (2005) estimate the same specification as Altonji et. al. (1997), but with data on transfers from children to parents. They find a compensation effect of 0.95, which is much higher than estimates with US data.

[2] Pitt et. al. (1990) found that food consumption within Indian rural families were positively correlated with calories expended in labor supply. Udry's (1996) study of African rural households reveals that, within the same household, plots controlled by women have significantly higher marginal productivity of capital and labor input than the plots controlled by men, indicating that production inputs are not efficient among these households. Comparing across different regions of China, Qian (2008) found that daughters had higher survival rates when their adult wages are higher. She also found that daughters and sons both had more education when the female adult wage is higher. The former result is consistent with the unitary model, while the latter is not.

limited because when household incomes are jointly earned, data on the income contribution of each adult household member is usually unavailable.[3] In addition, financial transfers among household members, normally identified by the difference between individual specific consumption and income, are also hard to measure. To get around the measurement problems, some studies use policy or geographic variations to proxy for individual-specific income changes;[4] other studies ignore the co-resident sample.[5]

This paper provides a new test of the unitary model for traditional rural households called the investment difference transfer test. It is based on studying how parental transfers adjust to compensate for differences in parental investments. We apply our test using a unique household survey dataset from rural China, where patrilocality is a common practice: 38 percent of the sons live with their parents, and 64 percent live in the same village with their parents after marriage. To test the unitary model in this environment, we study the relationship between a significant investment, educational expenditure, and two inter-vivos transfers made by Chinese villagers to their sons: marital transfers (bride-price), and post-marital land divisions. As suggested by our interviews with parents, and also confirmed by our data, educational expenditures are the largest component of monetary parental investment. Bride-prices plus land divisions are typically the largest transfers from parents to sons. In this sense, they are closer to the lifetime inter-vivos transfer in Becker's model.[6]

We present a unitary model to guide our regression specifications and interpret the empirical results. The model delivers a regression of a measure of parental transfers on schooling expenditure for each child in the household, controlling for household and sibling fixed effects in a multiplicative form. The regression provides a causal estimate of how parents adjust a particular transfer to their children in response to differences in schooling expenditures on these children. We call this regression coefficient the *marginal compensation coefficient*. In the standard unitary model, this coefficient should identify the marginal gross return (payoff) to a child's schooling expenditure, r, which should be larger than 1.0. However, when a rural family invests in a child, a portion of the gross return to the investment may accrue to the family rather than to the child directly. If a child directly appropriates a fraction f of the gross returns from his investment, the marginal compensation coefficient estimates $f \cdot r$. When $f < 1$, the 1.0 benchmark is suggestive but not compelling.

The main empirical finding of our paper is that our point estimate of the marginal compensation coefficient for differences in schooling expenditures across sons is 0.7. The estimate implies that when a son receives one yuan less in schooling investment than his brother, he will obtain 0.7 yuan more in transfers that are *observed by us* as partial compensation. This point estimate

[3] Udry (1996) is an exception.

[4] For example, Qian (2008) uses inter-regional variation in tea production as a proxy for female wages. Bobonis (2009) uses a child care subsidy program to proxy for an increase in wife's income and rain shocks to proxy for the variations in jointly-held income by the couple.

[5] E.g., Raut and Tran (2005), Altonji et. al. (1992,1997). Kaplan (2009) shows that moving back to the parents' home is important for poor families in the United States.

[6] While we use the term bride-price for convenience, most if not all the transfer is given to the son to help him set up his new household. It is not a transfer payment to the bride's family.

is significantly larger than previous estimates of compensating parental transfers for differences in intra-household investments in children. In the farming households considered here, full appropriation of the returns to schooling expenditure ($f = 1$) is not appropriate. Thus the evidence here provides significant support for the unitary model for the communities in our study.

Four factors are important in obtaining our point estimate. First, it is quantitatively important to control for unobserved household heterogeneity. Due to data limitations, most researchers cannot control for household unobservables.[7] In the context of the income difference transfer test, if children from richer families receive more in parental investments and have higher own incomes, failing to control for household unobservables will result in a substantial underestimate of the parental compensation response. Our results confirm the downward biases in the OLS regressions: the estimated marginal compensation coefficient increases by a third in absolute value when we control for unobserved household heterogeneity.

Second, fuller accounting for lifetime transfers matters. Becker's model is static. Most empirical work, including ours, tests the model using partial lifetime transfers and partial lifetime income observed over a window of only a few years.[8,9] We show that using partial lifetime transfers to carry out our test leads to a systematic bias in our estimate of the marginal compensation coefficient. The extent of the bias can be tested with multiple transfers and we do so here. The household fixed effects regression of marital transfers on schooling expenditure delivers an estimated marginal compensation coefficient of 0.4. By adding post-marital land transfers to the bride price, we increase the value of the estimated compensation coefficient to 0.7.

Third, controlling for other observed sibling differences in allocated consumption does not change the estimated homogenous marginal compensation coefficient. Most studies based on sibling variation assume "equal concern", i.e., parents put equal weights on the consumption of their children in the utility function. Some previous rejections of the unitary model may be due to "unequal concern" rather than non-unitary behavior. Our empirical results show that parents do not equalize consumption across children. We further relax the homogenous marginal compensation coefficient assumption and find that children's attributes affect the magnitude of the marginal compensation coefficients as predicted by the theoretical model, but these effects are not precisely estimated.

Lastly, the optimal transfer to a child may be negative and infeasible because the child refuses to cooperate. In these cases, optimal parental investments are more complicated because the two-step optimization breaks down (Behrman et. al. (1995)).[10] When negative transfers are infeasible, tests

[7] An exception is the Health and Retirement Study (HRS) data used by McGarry and Schoeni (1995). They found in their fixed effect regression that parental transfers are larger to children in the low income categories. It is hard to compare their study to the aforementioned tests, as they include both income and schooling of the children in their regressions.

[8] The President's Commission on Pension Policy (PCPP) survey used by Cox (1987) reports transfers received by children in 1979. The Panel Study of Income Dynamics (PSID) data used by Altonji et. al. (1997) include transfers of more than 100 dollars in 1988. The Health and Retirement Study (HRS) data used in McGarry and Schoeni (1995) reports transfers of 500 dollars or more made to each child over the past two years.

[9] Altonji et. al. (1997) show that, with credit constraints, using partial lifetime transfers may be valid.

[10] Two-step optimization may also fail when the marginal utlities of consumption and labor supply are not separable in the household utility function (Pitt et. al. (1990)).

using data with only positive transfers may suffer from selection bias.[11] Our current framework does not deal with corner solutions in our measure of parental transfers where the two-step optimization fails. But this problem is not quantitatively significant in our context because nearly all of the sons receive strictly positive bride-prices.[12]

We check the robustness of our results along several other dimensions. Accounting for measurement error in schooling expenditure, discounting of the timing of school expenditures and bride prices, or accounting for the public goods' components in children's consumption do not increase the estimate of the compensation coefficient.

Our paper contributes to a growing literature on intra-household resource allocation in rural China where 60 percent of the population in China still lives. Using a dataset of monozygotic twins, Li et. al. (2008) find that urban parents provide more marital gifts to children who suffered from involuntary rustication during the Cultural Revolution (1966-1976). They also study within gender differences. Qian (2008) found mixed support for the unitary model when she studied gender differences in parental investments. Thus these studies, including ours, suggest that the unitary model of parental resource allocation applies within gender in rural China. Its application across gender is less clear.[13]

This paper is organized as follows. Section 2 presents a model of parental investment, derives testable implications, and discusses identification issues. Section 3 introduces the data and descriptive results. Section 4 reports the estimation results. Section 5 addresses some related issues. Section 6 concludes the paper.

2 A Unitary Model of Parental Investment

Consider a household h with two of their children, denoted by $i = 1, 2$ respectively. The parents have a wealth endowment of m_h. Child i has ability level a_{ih}. The parents decide how to allocate family resources to its members.

Let the parents' utility function be

$$U_h = u(c_h, c_{1h}, c_{2h}) \tag{1}$$

where $u(\cdot)$ is a twice differentiable, concave function; c_h represents the consumption of the parents; c_{ih} is child i's consumption.[14] A family may have more than two children; the consumption of the

[11] Cox (1987); Altonji et. al. (1997); Raut and Tran (2005) correct for selection in their empirical work.

[12] It is an issue with dowries for daughters where one-third of the households provide no dowry to at least one daughter.

[13] Our paper does not address gender differences. The gender difference patterns in our data are similar with other developing counties (e.g. villages examined by Quisumbing's (1994) in Philippines): girls receive comparable schooling investments with their brothers, but they generally receive significantly less dowry and nearly no bequests.

[14] As later on we will use the marital transfer as a measure of inter-vivos transfers, we might think of incorporating the marital market clearing conditions into our framework. This can be easily done by assuming positive assortative matching among spouses. Imagine that parents care not only about the consumption level of their own children, but also about the consumption of to whom they are married. For example, we can consider a family h thinking of finding potential spouses for their sons 1 and 2 by equipping them with consumption level c^g_{1h} and c^g_{2h} so that they

other children in the household is subsumed in parental consumption.

Parents can invest in their children's schooling. Let the parents spend s_{ih} on child i's schooling. This expenditure generates $R(s_{ih}, a_{1h}, a_{2h}, m_h)$ revenue from i's schooling for the family. $R(.)$ may be labor earnings if the child works outside the home. In rural households, $R(.)$ is the contribution of the child to family income. We expect $R(.)$ to be increasing in its arguments. The total cost of s_{ih} to the family is $C(s_{ih}, a_{1h}, a_{2h}, m_h)$. The cost function $C(\cdot)$ contains both the monetary and time cost to the family associated with each yuan spent on schooling.

The budget constraint of the family is

$$\begin{aligned} c_h + c_{1h} + c_{2h} &= m_h + \sum_{i=1}^{2} (R(s_{ih}, a_{1h}, a_{2h}, m_h) - C(s_{ih}, a_{1h}, a_{2h}, m_h)) \\ &\equiv w_h \end{aligned}$$

where w_h denotes the total family wealth available for consumption.

To maximize utility, schooling investments should be chosen to maximize total family wealth. The first order condition

$$\frac{\partial R(s_{ih}, a_{1h}, a_{2h}, m_h)}{\partial s_{ih}} = \frac{\partial C(s_{ih}, a_{1h}, a_{2h}, m_h)}{\partial s_{ih}} \tag{2}$$

determines the optimal schooling investment level $s^*_{ih}(a_{1h}, a_{2h}, m_h)$ of child i. The two schooling decisions will determine optimal family wealth $w^*_h(s_{1h}, s_{2h}, a_{1h}, a_{2h}, m_h)$.

We now solve for the optimal consumption allocations. Following Becker, we employ a homothetic utility function $u(\cdot)$. A convenient property of homotheticity is that the expenditure on each consumption good will be a fixed proportion of total wealth. In our context, the utility maximizing level of consumption will be

$$c^*_{1h} = k_{1h} w^*_h \tag{3}$$

$$c^*_{2h} = k_{2h} w^*_h \tag{4}$$

$$c^*_h = (1 - k_{1h} - k_{2h}) w^*_h \tag{5}$$

where k_{ih} denotes the proportion of total family wealth allocated to child i.[15] We do not interpret

can attract brides with expected consumption levels of $\widehat{c}^b_{1h}$ and $\widehat{c}^b_{2h}$ respectively. Here we use superscript g and b to denote the groom and bride. The parental utility function is then $\widetilde{u}(c_h, c^g_{1h}, \widehat{c}^b_{1h}, c^g_{2h}, \widehat{c}^b_{2h})$.

This equation, however, can be simplified to equation (1) through positive assortative matching in the marriage market. To illustrate this point, assume that the matching function takes the form $c^b_{ih} = \phi_0 + \phi_1 c^g_{ih} + \nu_{ih}$, where $i = 1, 2$ and ν_{ih} is the deviation (with zero mean) from positive assortative matching in wealth due to love and other match-specific factors. From the parents' point of view, the expected daughter-in-law should possess $\widehat{c}^b_{ih} = \phi_0 + \phi_1 c^g_{ih}$. Plugging into the utility function yields $\widetilde{u}(c_h, c^g_{1h}, \widehat{c}^b_{1h}, w^g_{2h}, \widehat{w}^b_{2h}) = \widetilde{u}(c_h, c^g_{1h}, \phi_0 + \phi_1 c^g_{1h}, c^g_{2h}, \phi_0 + \phi_1 c^g_{2h}) = u(c_h, c^g_{1h}, c^g_{2h})$ which is the utility function (1) we use in the model.

[15] k_{ih} represents the weight parents put on child i's consumption level in their utility function. To illustrate this point, consider two examples of the parental utility functions. If (1) $U_h = c_h^{1-\alpha_{1h}-\alpha_{2h}} c_{1h}^{\alpha_{1h}} c_{2h}^{\alpha_{2h}}$, then $k_{1h} = \alpha_{1h}, k_{2h} = \alpha_{2h}$; if (2) $U_h = [(1-\alpha_{1h}-\alpha_{2h})c_h^\rho + \alpha_{1h} c_{1h}^\rho + \alpha_{2h} c_{2h}^\rho]^{\frac{1}{\rho}}$, then $k_{1h} = \frac{\alpha_{1h}^{\frac{1}{1-\rho}}}{\alpha_{1h}^{\frac{1}{1-\rho}} + \alpha_{2h}^{\frac{1}{1-\rho}} + (1-\alpha_{1h}-\alpha_{2h})^{\frac{1}{1-\rho}}}, k_{2h} =$

k_{ih} as parental preference parameters. Different values of k_{ih} serve as a convenient form to capture alternative concerns as to why different children may have different levels of consumption. Parents may give more consumption to their older sons not because they "like" them more than their other sons, but because the older sons are more likely to live with them after marriage and provide them elder support. Such dynamic concerns from the parents are embedded in our model through k_{ih}. Becker refers to the case where $k_{1h} = k_{2h}$ as equal concerns. In our context, equal concerns has no normative implication.

The parents can affect child i's consumption through two channels: revenue from schooling investment for the whole family, R^*_{ih}, and parental transfers, t_{ih}. The total consumption of child i is

$$c^*_{ih} = k_{ih} w^*_{ih} = t^*_{ih} + f_{ih} R^*_{ih}$$

In rural households, $R(s^*_{ih}, a_{1h}, a_{2h}, m_h)$ may include family income that is jointly earned. Thus, not all of it may be appropriated by the child. $f_{ih} \in [0, 1]$ is the proportion of $R(s^*_{ih}, a_{1h}, a_{2h}, m_h)$ retained by the child for his own consumption.[16] Substituting in the optimal consumption and schooling investment levels, the optimal inter-vivos transfer to child i is

$$t^*_{ih} = k_{ih} w^*_h - f_{ih} R^*_{ih} \tag{6}$$

So far, we have characterized the parental investment behavior under a framework that allows heterogeneity in both parents and children's attributes, and unequal parental valuations of their investments across children. The model presented here is a unitary model that nests all tests proposed in the literature. The choice of tests crucially depends on data availability.[17]

In our specific context, as in most developing countries, individual income (fR) is not well defined and hard to measure. Instead, we observe parental investments (s) and transfers (t). In this case, what we can learn from the data is the compensation/substitution relationship between s and t based on equation (6). We will develop a new test using this relationship to examine the fitness of the unitary model to the data.

2.1 Empirical specification

To turn (6) into a regression equation of transfers on schooling expenditure, consider a constant-marginal-return form of revenue $R(.)$ on s^*_{ih}:

$\dfrac{\alpha_{2h}^{\frac{1}{1-\rho}}}{\alpha_{1h}^{\frac{1}{1-\rho}} + \alpha_{2h}^{\frac{1}{1-\rho}} + (1-\alpha_{1h}-\alpha_{2h})^{\frac{1}{1-\rho}}}$.

[16] Parents and other family members obtain $(1 - f_{ih}) R^*_{ih}$.

[17] With proxies for marginal returns (R_s) and investment data (s) as in Qian (2008), one can test the model using the first order condition (2) and see if members with higher prospective returns receive more in investments; with income (fR) and consumption data (c), as in Lundberg et. al. (1997), one can use consumption decision equations (3) and (4) and see if family members' consumption remains the same with a shift in income between family members; with income (fR) and transfer data (t) as in Altonji et. al. (1997), one can test the model using equation (6) and see if, holding family wealth (w_h) constant, a dollar increase in income decreases transfers received by one dollar (so that the consumption is unchanged).

$$R(s^*_{ih}, a_{1h}, a_{2h}, m_h) = R(s^*_{ih}(a_{1h}, a_{2h}, m_h), a_{ih}) = r_{ih}s^*_{ih} + e_{ih}$$

where e_{ih} is an IID random shock with $cov(s^*_{ih}, e^*_{ih}) = 0$ and r_{ih} is the marginal rate of return to schooling expenditure. For empirical tractability, we assume that other siblings' attributes a_{jh} $(j \neq i)$ and parental wealth m_h only affect child i's schooling return function R through his optimal schooling investment s^*_{ih}.

Substituting the above into (6), we have:

$$t^*_{ih} = k_{ih}w^*_h - f_{ih}r_{ih}s^*_{ih} - f_{ih}e_{ih} \tag{7}$$

We call $f_{ih}r_{ih}$ the *marginal compensation coefficient*. It measures the marginal effect of a child's schooling expenditure on his parental transfer.

The distinction between r_{ih} and $f_{ih}r_{ih}$ deserves attention. The former is the marginal return at the household level, while the latter is at the individual level. $(1 - f_{ih})r_{ih}$ captures the externalities that other family members benefit from the schooling investment in child i. In our context, parents are the decision makers. When making schooling investment decisions, they only care about the efficiency at the household level. In other words, they will choose s^*_{ih} as determined by (2) even if $f_{ih}r_{ih} < \frac{\partial C(s^*_{ih}, a_{1h}, a_{2h}, m_h)}{\partial s^*_{ih}}$ for child i.[18]

Given a random sample of households $h = 1, .., H$, consider the regression

$$t_{ih} = \kappa_{ih}F_h + \beta_{ih}s_{ih} + \varepsilon_{ih} \tag{8}$$

where $\{F_h\}_{h=1}^H$ are household fixed effects capturing household wealth, κ_{ih} are sibling fixed effects, and β_{ih} are the child-specific coefficients on schooling expenditure. The estimation and interpretation of κ_{ih} and β_{ih} warrant detailed discussions.

2.1.1 Sibling fixed effects κ_{ih}

First, unlike the usual specification in the literature, in equation (8), inter-vivos transfers are linear in schooling expenditure but multiplicative in household and sibling fixed effects.

Second, κ_{ih} is child dependent. Empirically, we allow κ_{ih} to depend on a set of binary indicators: whether (1) the son is taller than his sibling, (2) has more years of agricultural experience at the time of his marriage, (3) has more years of non-agricultural experience, (4) lived outside the home before marriage, (5) live with the parents after marriage, and (6) he is the first son.

We now discuss the normalization of κ_{ih}. To ease exposition, let son i in household h be characterized by two indicator variables $D_{ih} = (D^a_{ih}, D^b_{ih}) \in \{0, 1\} \times \{1, 0\}$. Let the sons with $(D^a_{ih}, D^b_{ih}) = (0, 0)$ be the base group denoted by κ_0. Not all households have a son where $\kappa_{ih} = \kappa_0$.

[18] While beyond the scope of this paper, the divergence between household and individual optimality creates intra-household conflict outside the unitary framework.

For any type of son i,

$$\kappa_{ih}F_h = \frac{\kappa_{ih}}{\kappa_0} \cdot \kappa_0 F_h$$
$$= (1 + \delta_a D^a_{ih} + \delta_b D^b_{ih}) \cdot \omega_h \quad (9)$$

where ω_h estimates $k_0 w^*_h$, which is the consumption level enjoyed by a child with hypothetical characteristics κ_0 in household h. δ_a or δ_b is the increase in k_{ih} due to the presence of characteristic a or b for child i relative to the hypothetical child 0 within household h.

2.1.2 Marginal compensation for differences in schooling investment

$-\beta_{ih}$ in equation (8) estimates $f_{ih}r_{ih}$, the marginal compensation coefficient in equation (7). Because of the multiplicative form between f_{ih} and r_{ih}, we cannot identify them separately without imposing further assumptions.

Proposition 1 (Homogenous marginal compensation) *If (1) the marginal return to schooling investments is homogenous across children and families ($r_{ih} = r$), and (2) the appropriation coefficient is also homogenous across children and families ($f_{ih} = f$), then $-\beta_{ih} = -\beta$ in equation (8) identifies the marginal compensation coefficient, i.e., $-plim\ \widehat{\beta} = f \cdot r \leq r$.*

First, the above proposition is true even if we cannot observe abilities of the children. When the gross return to educational investments is homogenous, abilities affect the level of investments only through the marginal cost of schooling. Variations in s_{ih} capture all variations in unobservable a_{ih}. Therefore $cov(s_{ih}, \varepsilon_{ih}) = 0$ holds in equation (8).

Second, the marginal benefit of schooling investments should be equalized with marginal cost at the optimum. The marginal cost of schooling will be larger than one with a positive interest rate and if there are non-pecuniary costs to schooling. Then if we observe positive schooling expenditure, the marginal benefit (r) must be greater than or equal to one as well. Proposition 1 says that $-\beta$ estimates $f \cdot r \leq r$. The standard version of the income difference transfer test of Becker's unitary model tests whether $-\beta > 1$, which is only valid if there is full appropriation, $f = 1$. Otherwise, there is no reason to expect $-\beta > 1$.

Even if we are willing to accept that $-\beta$ estimates $f \cdot r$, there are other empirical issues.

Proposition 2 (Regression without fixed effects) *Suppose that $f_{ih}r_{ih} = f \cdot r$. A regression without household fixed effects will deliver $-plim\ \widehat{\beta} < f \cdot r$.*

If we work with a dataset without sibling variation in the same household, a household fixed effects' regression is infeasible. In this case, we are estimating the equation

$$t_{ih} = \eta + \beta s_{ih} + \upsilon_{ih} \quad (10)$$
$$= \overline{k_{ih}w_h} - f \cdot r s_{ih} + k_{ih}w_h - \overline{k_{ih}w_h} + \varepsilon_{ih} \quad (11)$$

where $\overline{k_{ih}w_h} = (\sum_{h=1}^{H}(k_{1h}+k_{2h})w_h)(2H)^{-1}$. Since $cov(s_{ih}, w_h) > 0$ and $cov(s_{ih}, k_{ih}) > 0$, $-plim\ \widehat{\beta} = f \cdot r - \frac{Cov(s_{ih}, k_{ih}w_h)}{Var(s_{ih})} < f \cdot r$. The result is intuitive. Richer families tend to invest more in both schooling and marital transfers, therefore a regression with imperfect controls for family wealth or fixed effects will bias the estimates of $f \cdot r$ downwards. However, controlling for household effects itself is not enough. The importance of sibling effects is emphasized in the following proposition.

Proposition 3 (Unobservable preference bias controls) *Suppose that* $f_{ih}r_{ih} = f \cdot r$, *and* $cov(k_{ih}, a_{ih}) > 0$. *Controlling for household fixed effects while ignoring the sibling fixed effects* k_{ih} *will bias the estimates of* $f \cdot r$ *downwards, i.e.,* $-plim\ \widehat{\beta} < f \cdot r$.

When k_{ih} is determined by children's ability, which we do not observe in our data, we are not able to control for k_{ih} at all. The transfers equation becomes

$$t_{ih} = \overline{k}w_h - f \cdot rs_{ih} + (k_{ih} - \overline{k})w_h$$

Even though $\overline{k}w_h$ is picked up with the household fixed effect, $(k_{ih} - \overline{k})w_h$ will be treated as part of the error term. Given that $cov(\widetilde{s_{ih}}, w_h) > 0$ and $cov(k_{ih}, s_{ih}) > 0$, $-plim\ \widehat{\beta} = f \cdot r - \frac{Cov(\widetilde{s_{ih}}, (k_{ih}-\overline{k})w_h)}{Var(\widetilde{s_{ih}})} < f \cdot r$, where $\widetilde{s_{ih}}$ is the deviation of schooling investments from the household mean.

With the "equal concern" assumption, $k_{ih} = k$, hence $-plim\ \widehat{\beta} = f \cdot r$. The bias discussed here is relevant when parental "unequal concern" towards children is quantitatively significant and "unequal concern" is unobserved.

Proposition 4 (Heterogenous marginal compensation) *If* $f_{ih} = f$ *but* $r_{ih} = \overline{r} + \psi^a \varepsilon_{ih}^a$, *where* $\varepsilon_{ih}^a = a_{ih} - E(a_{ih})$ *and* $\psi^a > 0$, *then* $-plim\ \widehat{\beta} > f \cdot \overline{r}$.

When the return to schooling is positively correlated with children's unobserved abilities, we are estimating the equation

$$\begin{aligned} t_{ih} &= k_{ih}w_h - f \cdot \overline{r}s_{ih} + \upsilon_{ih} \\ &= k_{ih}w_h - f \cdot \overline{r}s_{ih} - f \cdot \psi^a \varepsilon_{ih}^a s_{ih} - e_{ih} \end{aligned}$$

If $Cov(s_{ih}, -\psi^a \varepsilon_{ih}^a s_{ih}) < 0$, β in equation (8) will be biased downwards. In other words, the average marginal compensation coefficient $f \cdot \overline{r}$ will be over-estimated.

To test if the coefficient on the schooling expenditure $f \cdot r$ is child specific, we consider the following regression:

$$t_{ih} = (1 + \delta D_{ih})\omega_h + \beta s_{ih} + \gamma D_{ih}s_{ih} + \varepsilon_{ih} \tag{12}$$

where we interact schooling investments with children observables (D_{ih}). The sign of γ will tell us if children with certain attributes tend to receive higher or lower marginal compensation for differences in schooling investments ($f_{ih}r_{ih}$).

To sum up, the full specification that we estimate, without restricting $f_{ih}r_{ih} = f \cdot r$, is equation (12). The parameters $(\delta, \omega, \beta, \gamma)$ will be estimated through non-linear least squares. In the appendix, we discuss in detail the estimation algorithm and econometric issues including the incidental parameters problem.

2.2 Investment difference multiple transfers test

All existing tests of the unitary model that are based on income-transfer differences suffer from a common measurement issue: researchers only observe a part of the total inter-vivos transfers that are made by parents over their children's lifetime.

Proposition 5 (Partially observable inter-vivos transfer) *Suppose we observe* $t'_{ih} = \alpha t_{ih} + \mu_{ih}$*, with* $0 < \alpha < 1$*, IID* $\mu_{ih} \sim N(0, \sigma_\mu^2)$ *and* $cov(t_{ih}, \mu_{ih}) = 0$*. Let* $f_{ih}r_{ih} = f \cdot r$*. Then* $-plim\widehat{\beta} = \alpha f \cdot r < f \cdot r$*.*

This proposition illustrates a useful case: If the observed transfers are proportional to the real total value, the magnitude of the coefficient on schooling will be proportional to $f \cdot r$.

We use the above proposition to derive an investment difference multiple transfers test. If we increase α by adding more transfers to the dependent variable, the magnitude of the coefficient on schooling should increase and converge towards the true value of the marginal compensation coefficient. Formally, let the researcher observe two transfers: $t_{ih,1}$ and $t_{ih,2}$. We can sum these two transfers to get a more complete measure of lifetime parental transfer, denoted as $t_{ih,3}$. Note that

$$t_{ih,g} = \alpha_g k_{ih} w_h - \alpha_g f \cdot r s_{ih} + \alpha_g \varepsilon_{ih} + \mu_{ih,g} \tag{13}$$

where $g = 1, 2, 3$ and $\alpha_3 = \alpha_1 + \alpha_2$. When $Cov(s_{ih}, \varepsilon_{ih}) = 0$,

$$-plim\widehat{\beta_{t_i}} < -plim\widehat{\beta_{t_3}} = (\alpha_1 + \alpha_2) f \cdot r \tag{14}$$

where $i = 1, 2$, and $\widehat{\beta_{t_g}}$ denotes the estimated β when the transfer at time g is the dependent variable. We refer to the test with null hypothesis:

$$H_0\text{: } \frac{\beta_{t_3}}{\beta_{t_g}} > 1,\ g = 1, 2 \tag{15}$$

as the *investment difference multiple transfers test* of the unitary model.

One nice feature of this test is that the result is independent of the other concerns affecting the consistency of our estimate of $-\beta$. In (13), since $Cov(t_{ih,g}, \mu_{ih,g}) = Cov(s_{ih}, \mu_{ih,g}) = 0$ by construction, the potential bias associated with $-\beta_{t_g}$ in each transfer regression only comes from the possible correlation between s_{ih} and ε_{ih}, which is invariant across transfers. Denoting this bias term as ψ, then $-plim\widehat{\beta_{t_g}} = \alpha_g(f \cdot r - \psi)$. The bias term ψ may change the signs of the coefficients in different transfer regressions, but it will not affect their ratios. Specifically, when $\psi < f \cdot r$, β should always be negative and adding more transfers to the dependent variable should decrease the coefficient on schooling investments towards $-f \cdot r$ (increasing its magnitude). Readers are referred to appendix C for a concrete example of ψ and formal derivation of the above arguments.

Since testing hypothesis (15) involves comparing coefficients across different regressions (with different dependent variables), we employ the bootstrap method to obtain confidence bounds for our test statistics.

3 Data

The data used in this study come from the "Survey on Family and Marriage Dynamics in Hebei Province", which was carried out in rural Hebei in the summer of 2005 by the authors and their Chinese colleagues. Rural Hebei province is culturally, economically, and socially representative of North China. Altogether, 600 households from 30 villages, in 15 townships in 3 counties were surveyed.[19] Figure 1 locates each of the counties in Hebei.

The survey was designed to address some of the issues discussed in the previous section. Each household was required to have at least one married child in order to be included in the sample. Our respondents are parents in the household between the ages of 50 to 69. We exclude parents older than 70 because of concerns about recall.

For each child, information on significant events over his/her life was collected from the parents, including (1) education, (2) pre-marital work experience, (3) engagement and marriage, (4) fertility and post-marriage intra-family arrangements and (5) pre-mortem household division if applicable. To minimize the burden of the interview, for households with more than three married children, three of them were selected.[20] In total, our survey covers 600 households with 1688 children (853 sons and 830 daughters) in total, among which 1276 (628 sons and 648 daughters) have been married. In the following parts of this section, we discuss the key variables used in this study and the construction of our working sample.

3.1 Education Expenditure

In rural Hebei, parents shoulder almost all of the educational costs of their children. Self-financing by children is negligible. For each child in our dataset, we collected data on educational expenditure since middle school (grade 7), including tuition fees, books, and room and board, all deflated to 1980 price levels. We do not have expenditure data for elementary school, because most parents could not accurately recall such information. Instead, we impute the spending on

[19] The three counties, Feng Run, Zhao Xian, and Chi Cheng, were selected after extensive analysis of county and township-level economic and demographic information from the 1980s and 1990s. Feng Run is the richest of the three in terms of per capita GDP, and Chi Cheng is the poorest. Within each county, townships were ranked on the basis of incomes, and then one was randomly selected from each quintile. Two villages were then randomly selected in each township, one for the upper half of the income distribution, and one from the lower half. Finally, from each village, 20 households satisfying age requirements for the household head and his spouse were randomly selected.

[20] The selection criteria were: (1) Choose the children who married first and last; (2) If the two selected children are of the same sex, choose a third of the opposite sex; (3) If the two selected children are of the opposite sex, choose a third one about whom the parents were most worried when s/he was 15; (4) If none of these criterion are applicable, daughters are preferred.

elementary school for each child. A recent study by Liu et. al. (2006) shows that the total cost of elementary education is about half the cost of middle school education in rural China. In light of this, we regress observed middle school expenses on gender, cohort and village dummies, and halve the predicted value before using it as imputed elementary schooling expenditure.[21]

Figure 2 illustrates the dynamics of educational expenditures in our sample. Total educational expenditure on sons in 1980 yuan increased significantly along with household annual income following economic reform. These expenditures rose especially fast early on, but the increase slowed beginning in the late 1980s. Total household expenditure on a son's education amounts to about four-fifths of annual household income. Increases in educational expenditures arose mainly from an increase in years of schooling, rather than increases in annual costs (Refer to figure A1).[22]

3.2 Marital Transfers

The first significant inter-vivos transfer in our dataset is marital transfers. Currently, a large part of the marital transfers in rural Hebei is given by parents to their marrying children, and not to their in-laws. It is an inter-generational transfer rather than an inter-familial transfer. At the time of the marriage, both the groom's and the bride's families provide furniture, major home appliances, farm equipment, and sometimes cash payments to the newly-wed couple.[23] The groom's family usually spends more because traditionally the groom is responsible for building a new house for the newly weds. In our dataset, for each marriage, we have a complete inventory of marital transfers along with their monetary value.

Parents must save for years in order to finance these expenditures. Escalating marital expenses in Chinese villages during the past two decades have been well documented in the literature (E.g. Siu (1993); Min and Eades (1995); Zhang (2000)). Our dataset confirms high and rising marital transfers, especially in bride-prices, since the economic reform. Refer to Figure 2. Despite some fluctuations in the early 1990s and 2000s, the bride-price is roughly two and a half times annual household income. With reported rural household savings running about thirty percent of reported household income,[24] it takes parents 7 to 8 years to accumulate the bride-price. By contrast, the dowry is normally equal to about half of annual household income.

In this paper, we focus on parental transfers toward sons. This sample restriction is based on several concerns. First, nearly all sons receive strictly positive marital transfers. When both sons receive positive transfers, transfer decisions reflect interior solutions to the parents' optimization

[21] The mean of this imputed variable is around 300 yuan.

[22] We also examine educational expenditures on daughters over the same period. In the 1980s, spending on girls' education was below that on boys, but by the mid-1990s investments in children's human capital are about the same between genders.

[23] Actually, the cash transfer paid by the groom's family, which accounts for about 20 percent of the bride price, is usually given directly to the bride's family. However, once the bride's parents receive this cash payment, they can either keep it or use it to purchase items for the dowry. The dowry financed by the cash component of the bride-price is often referred to as "indirect dowry" in the sociology literature (see Goody (1978)). According to our dataset, on average, nearly all of the cash in bride-price became indirect dowry. This suggests that the cash component in the bride price should be included in the marital transfers enjoyed by the grooms, even though they are initially given to the bride's parents as inter-familial transfers.

[24] National Bureau of Statistics, China Statistical Yearbook (2007)

problem. Thus, the two-step maximization is valid and all the implications of our model should apply. The same argument does not hold for daughters, where about 20 percent of the sampled daughters do not receive a dowry. For these cases, implications of the model's corner solution have to be derived, which is beyond the scope of this paper. Second, due to the nature of patrilocal society and village land rules, only sons are entitled to receive household land as part of household divisions. As a result, our investment difference multiple transfers test is only feasible for the sons sample. Lastly, given the magnitude of the bride-price and sons' eligibility to receive land transfers, our measure of total inter-vivos transfers is a better measure of lifetime transfer for sons than for daughters.

3.3 Land Division

Some families in our sample made household property divisions before the parents were deceased. These agreements were typically verbal, with only 20 percent relying on formal written contracts. Noteworthy, daughters in rural China are not entitled to any forms of (pre-mortem) bequests. Only sons are potential recipients.

These pre-mortem bequests include housing, land, and other producer durable goods. 75 percent of our households have divided the house, 65 percent of the households have divided the land, and a few households (about 10 percent) have divided other items. The value of housing and items are reported. For land divisions, we know the amount of the land and the time of transfer. For purposes of valuing the inheritance associated with land, a few institutional details regarding property rights are required.

Land in rural China is not privately owned. Rather, ownership rights reside with the village (collective), and households are allocated usufruct rights. Since the introduction of economic reform in the late 1970s, these rights have largely been allocated to households on a per capita basis, with the length of tenure governed in principal by a series of land laws. For example, the first national land law extended tenure for 15 years. The land laws have not always been respected, and land has often been reallocated among households by villages before the expiration of the land law, however in Hebei province, use rights have been very secure.

As part of the household division of property, sons are effectively receiving usufruct rights to the land that has been allocated to the household by the village. At a minimum, these rights are secure until the next land law, but in practice, the claims run much longer. Conservatively, we calculate the value of the land as the total real return to the land from cultivation over the first 10 years after the land is given to the son.[25]

Panel A of Table 5 reports the summary statistics for each of the pre-mortem bequests. Notice that the value of the housing given at the time of the marriage represents a large share of the total value of the bequest, but these transfers have been counted as part of the bride-price. Therefore,

[25] We assume that the share of land in value-added is fifty percent. Because of discounting, increasing the number of years does not significantly change the value of the inheritance.

land is the only important transfer besides the marital transfers captured by our data.[26,27]

3.4 Sons Sample

There are 178 households with more than one married son in our dataset. Further restrictions have to be imposed for purpose of our analysis. First of all, children with missing values for educational expenditure or marital transfers were dropped. There are also 16 households, or less than 10 percent, when the groom's family did not pay any bride-price. We exclude them in our analysis, as they do not represent the marriage arrangement we usually observe. We also exclude 18 matrilocal marriages,[28] since this marriage pattern deviated from the typical patrilocal custom in the region. We keep all households with more than one married son surviving the above criteria. Altogether, 127 households with 264 sons/grooms are used in the analysis of this paper.

Table 1 presents descriptive information on the characteristics of these households and their children. To show the relative importance of the between-sibling variation in our dataset, we calculate for individual-level variables the within-household standard deviations in column 4. The results are generally half of the overall standard deviations (column 3 of Table 1). Column 5 reports the number of households with within-household variations in children's characteristics.

Sons examined in this paper were born between the 1950s and the 1990s, with most marriages occurring between 1980 and 2000. Mean age at marriage was 23. Before marriage, sons were more likely to participate in agricultural activities than non-agricultural activities.

We consider the education of the father as an important determinant of parental wealth. In addition, we use the total real value of housing and total value of fixed assets in agriculture as proxies for family wealth. These measures are constructed based on a complete inventory of the investments in housing and agricultural producer durables made by the parents since they were married.

4 Empirical Results

4.1 $f_{ih}r_{ih} = f \cdot r$

We first estimate the model assuming homogenous marginal compensation for sibling differences in educational expenditure, $f_{ih}r_{ih} = f \cdot r$. Columns (1) to (8) in Table 2 report the estimation results for equation (8) with various children's attributes as controls for k_{ih}. These attributes include: an indicator if they are the taller of the sons in the household, indicators for having more agricultural and non-agricultural experience in the household, an indicator if they lived with

[26] In fact, other than marital transfers and household divisions, we ask in our survey about every transfer larger than 100 yuan that parents give to the children after their marriages. Only 43 sons (16 percent of total sample of sons) receive post-marital transfers, with an average of 1700 yuan. This value is only about one third of the mean bride-price these sons received. The evidence suggests that offering large post-marital transfers is not a common practice in rural Chinese villages.

[27] We also estimate our model adding both post-marital transfers and the value of other property received as part of household division into the measure of inter-vivos transfers. The results are not significantly changed.

[28] In matrilocal marriages, the newly-wed couples live with or in the same village as the bride's natal family.

parents before or after marriage, and an indicator if they were the first son.[29] The "hypothetical" son for comparison in each household has zero values in all of these dummies. In column (8), we only select the significant factors in the full specification in order to focus our discussions. In all tables, the estimated coefficient on school expenditure is β, where $-\beta$ is the marginal compensation coefficient.

The point estimate of $-\beta$ using OLS without any other covariates in column O1 is 0.23 with a standard error of 0.06. Since marital transfers and schooling expenditure are positively related with household income, the estimate of $-\beta$ is biased downward without controls for household income. Column O2 adds household fixed effects to the regression (while maintaining $k_{1h} = k_{2h}$, i.e., the equal concerns assumption). We obtain an estimate of $-\beta$ of 0.39 with a standard error of 0.15. Controlling for household fixed effects, the estimated magnitude of β increases by over one third in the direction predicted by the theory.

As we add other household and sibling interaction effects, the point estimate for $-\beta$ generally remains in the vicinity of 0.35. With a conservative expectation of $r = 1$, this estimate of $f \cdot r$ implies a seemingly low f of 0.35, i.e., children can only retain less than half of their schooling returns. It is suspected that our results may suffer from downward bias. We will test and resolve this underestimation problem of $f \cdot r$ in the subsequent sections.

Our preferred specification is in column (8). Here, compared with the "hypothetical son" in the household, taller sons enjoy 22 percentage more in total consumption. Sons who live with their parents after marriage obtain nearly 40 percent more consumption than those who do not. Last, older sons tend to receive one fifth less than their younger siblings, which at first sight is inconsistent with popular wisdom about first-son bias in rural China. Below, we show that this inconsistency is resolved with appropriate discounting.

The results also suggest that parents tend to provide more consumption to children with more premarital agricultural or non-agricultural experience, and to children who do not live outside the home before marriage (column (7)). Even though these coefficients are imprecisely estimated, their magnitudes suggest that they are not economically insignificant.

Estimates of $k_0 w_h$ are summarized in Table A2. Panels A and B show the summary statistics and distribution of $k_0 w_h$ estimated under alternative specifications in Table 2. Both the graph and the Kolmogorov–Smirnov statistics show that the distributions of $k_0 w_h$ are not significantly different from log-normal. In panel C, we examine the determinants of $k_0 w_h$, by regressing it against household level characteristics. The estimates are consistent across columns. The coefficients on the number of sons and daughters in the household are negative, suggesting that the hypothetical son enjoys less consumption if there are more sons or more girls in the family. The level of consumption is also higher for children from families that are richer, with better educated fathers, and that are

[29] In an unreported specification, we try to control for year effects using sons' (1) age at marriage and (2) age at the time of the survey. The former's coefficient is -0.02 and statistically insignificant. The latter carries a significant coefficient of -0.05, suggesting that one will receive 5 percent less in consumption if he is 1 year older than his brother. As we shall see later, this result is consistent with what we obtain from the first son dummy. However, it is not wise to put the "first son indicator" and "age" in the same regression since they are highly correlated. Given that our main conclusions are not affected in either way, we decide not to put these time controls in our final regressions.

located in richer counties.

Our behavioral model generates a linear regression model with multiplicative family and sibling fixed effects rather than log linear or linear fixed effects regression models. In appendix D, we present and discuss the estimation results with the traditionally used level-on-level and log-on-log regressions. We find that functional form matters: a level-on-level specification is more appropriate under the (restricted) "equal concern" assumption, but this lacks behavioral justification in more general cases; a log-on-log specification imposes a strong restriction in our multiple transfers test, which is rejected by our data.

4.1.1 Sensitivity to discount rates

The framework discussed in section 2 is a static model while actual intra-household transfers are made over time and in different periods for different children. To make these transfers comparable in present value, all transfers made in each household are discounted at rate d to the year in which the first son was married. In columns (1)-(6) of Table 4, we compare different specifications (the basic ones assuming "equal concerns" and the parsimonious ones with only the significant controls) under different discount rates in order to test the sensitivity of our results.

Two patterns are notable in Table 4. First, estimated coefficients on schooling investments decline in magnitude as the discount rate increases. Therefore, taking the discount rate into account does not solve our problem of underestimating $f \cdot r$. Second, estimates of δ do not vary across alternative discount rates either in terms of their magnitudes or standard errors, except for a time related factor: the first son indicator. Coefficients on the first son dummy consistently increase with discount rates, and become positive eventually, suggesting that first sons receive higher consumption than other siblings.

These patterns are due to the correction for discounting. Educational expenditures occurred before marital transfers. If we discount everything to the time when the first son got married,[30] the differences in education expenditure across siblings by age will widen. Meanwhile, the difference in marital transfers by age will be compressed. So discounting increases the value of schooling expenditure of the first son and reduces the value of marital transfers to the second son. This logic explains why the estimates of the first son effect increases with the discount rate.

4.1.2 An investment difference multiple transfers test

Parents may use transfers other than marital transfers to make up the difference in human capital investments among their children. Proposition 5 says that when we use a part of the life-time transfers to estimate equation (12), $f \cdot r$ will be biased downwards. To address this issue, we construct a more complete measure of life-time parental transfers by adding to marital transfers other significant parental transfers captured by our survey - the value of land division - and perform our multiple transfers test.

[30] Choosing a different timing as baseline for discounting will generate the same trend.

We re-estimate equation (12) after adding the value of land to the dependent variable (t_{ih}). The sample size becomes smaller, because we concentrate on the sub-sample of households that have made land divisions. The results are reported in Table 5. Consistent with proposition 5, taking into account the value of inherited land substantially increases the absolute value of the coefficients on schooling expenditures.[31] This result is robust across all discount factors. For example, with a ten percent discount rate, our preferred specification in column 6 now suggests $f \cdot r = 0.37$ rather than 0.16. That is, the estimated compensatory coefficient more than doubles when we are able to capture more of lifetime parental transfers.

Moreover, the magnitudes of the δs in Table 5 become smaller but with similar standard errors as before. Overall, they become statistically insignificant. However, their signs are the same and the patterns under alternative discount rates remain as in Table 4.[32]

To obtain confidence intervals for our multiple transfers test, we bootstrap our sample with 100 iterations and store the estimates of the marginal compensation coefficients (β_{t_g}) with the value of the bride price ($t_g = t_1$) and the value of the bride price plus land division ($t_g = t_3$) as the dependent variable. Table 6 presents the summary statistics of the ratio β_{t_3}/β_{t_1} of these 100 iterations. Across alternative specifications and discounting rates, the mean value of the ratio is greater than 1. In addition, $\beta_{t_3}/\beta_{t_1} > 1$ occurs with more than 95 percent chance. The evidence suggests that we cannot reject the hypothesis that "adding in more parental transfers increases the magnitude of marginal compensation coefficients" at conventional significance level.

Our theoretical model predicts that estimating equation (8) using a partial measure of parental transfer will underestimate $f \cdot r$. Comparison between Table 4 and 5, along with the bootstrap results in Table 6 shows that this concern is empirically significant.

To sum up section 4.1, two findings particularly stand out. First, controlling for unobserved household heterogeneity is important. Second, a fuller accounting of lifetime parental transfers significantly increases the estimate of the compensatory effect, consistent with the unitary model.

4.2 $f_{ih}r_{ih} \neq f \cdot r$

Relaxing the homogenous marginal compensation effect, we expect $f_{ih}r_{ih}$ to depend on sons' activities and living arrangements with the parents. These patterns can be tested by examining the interaction terms between schooling expenditure and the children's attributes in equation (12).

We present in Table A1 the expected signs of the interaction terms. The main occupation of the parents in our sample is farming. Parents will benefit more from their sons' education when their sons participate in agricultural activities or live with them after marriage. Therefore, we expect the interaction term to be positive for these sons (lower $f_{ih} \cdot r_{ih}$). Note that children with more agricultural experience have lower $f_{ih} \cdot r_{ih}$ not only because of the lower f_{ih}, but also because of

[31] Even though the difference is not statistically significant at times due to the larger standard errors.

[32] The difference we observe may due to the fact that we are using a more restricted sample. To address this concern, we replicate marital transfer regressions in Table 4 using the same sub-sample as Table 5. The results are shown in Table A3. The coefficients on schooling expenditures and parental bias factors in this table are very similar with those in Table 4, suggesting that the increase in estimates of $f \cdot r$ in Table 5 cannot be attributed to the sample selection.

their lower r_{ih}. i.e., returns to schooling are lower in agriculture than non-agriculture. Following the same logic, we expect sons with more agricultural experience or live independently to have higher $f_{ih} \cdot r_{ih}$, therefore negative interaction terms. Traditionally, the first son usually takes more responsibility for caring for their parents and siblings. They are expected to have lower $f_{ih} \cdot r_{ih}$. We do not have a prior on the interaction between height and schooling.

Table 3 reports the results. For each specification in columns (1) to (8), we interact the same attributes with schooling investments as used to control for the parental bias.[33] The first thing to notice is that our estimates of γs in equation (12) are very imprecise. The large standard errors are mainly due to our relatively small sample size. Second, because of the weak explanatory power of the interaction terms,[34] adding them into the regressions does not significantly alter our estimates of β and δs (comparing Table 3 with Table 2).

While imprecisely estimated, most coefficients for the interaction terms in Table 3 have the predicted signs (as Table A1). In column 7, sons living with parents before marriage or after marriage retain less income from schooling investments. The first sons are also found to directly benefit less from schooling than their siblings. By contrast, sons with more non-agriculture experience tend to receive 0.17 yuan more from each yuan spent on schooling, consistent with the view that their parents as farmers are less likely to benefit directly from them. On the other hand, sons with more agricultural experience retain more of their schooling returns which is anomalous. Finally, taller sons benefit more from schooling.

The last three columns in Table 4 show that our main conclusions are robust to different discounting rates. Columns 7-9 in Table 5 show that including the value of land division parental transfers does not change the point estimates of δs substantially from that in Table 4.

In summary, we find evidence suggesting that $f_{ih}r_{ih} \neq f \cdot r$. However the large standard errors preclude us from being able to make precise quantitative statements on this heterogeneity in marginal compensation.

5 Other Issues

5.1 Measurement error in schooling investments

The low absolute value of the estimated marginal compensation coefficients may be due to attenuation bias. Given that our education expenditure variable mainly comes from recall data, measurement error might exist, which could bias estimates of $-\beta$ towards zero. One way to correct for this bias is to adopt an instrumental variable approach. "Years of schooling" is an ideal candidate to serve as an instrument for education expenditure: (1) it is positively correlated with education expenditure and (2) its measurement error, if there is any, is not likely to be correlated with that of education expenditure.

[33] We also try specifications where all of the attributes are interacted with the schooling investments regardless of the way we control for k_{ih}. The conclusions are essentially the same.

[34] Notice that Table 2 and 3 have the same R^2 for all specifications.

In Panel A of table 7, we replicate all specifications in Table 4 with 2SLS estimation. The first stage regressions and reduced form regressions are presented in Panel B and Panel C, respectively.

As expected, "years of schooling" is a good predictor of education expenditure in the first stage. (One year of schooling costs about 800 yuan on average.) In the second stage, estimates of δs and γs do not change much. However, the precision of $f \cdot r$ is lost due to the increased standard errors. Compared with the OLS results, the IV estimates are slightly smaller, but the Hausman tests cannot reject that their difference is statistically insignificant. We conclude that the underestimation of $f \cdot r$ is not likely due to the attenuation bias.[35]

5.2 Strategic behavior of the children

So far, our framework assumes children are passive and only parents make decisions in the family. Full compensation in transfers amounts to full insurance for children. If the returns to education depend on children's efforts, two problems emerge: free riding (among siblings) and moral hazard. Both of them cause the children to exert less effort than optimal. To avoid these problems, parents would break the linkage between education achievement and inter-vivos transfers and commit an equal amount of transfers to their children. Such strategic behaviors tend to drive the coefficient on education investment towards zero in our regression. Thus one interpretation of our results is that strategic concerns are second order in our families under study.

5.3 Public goods in transfers

The externality to the parents and other family members may not only exist in individual's schooling investment, but also in the transfers. For example, when a son lives with parents after marriage, his parents may enjoy the transfer made to this son at the time of marriage as well, such as a new TV or a new house. However, it should be emphasized that the first order condition determining the optimal schooling is not distorted. As long as $MR(s_{ih}) = MC(s_{ih})$ holds, schooling investments are efficient, and family income is maximized at $w_h^*(s_{ih}^*)$. Suppose that parents own expenditure is c_h, and they also benefit a proportion of b_{ih} from the transfer to the children t_{ih}. The consumption of the parents and children are

$$
\begin{aligned}
c_h^* + b_{1h}t_{1h} + b_{2h}t_{2h} &= (1 - k_{1h} - k_{2h})w_h^* \\
c_{ih}^* = f_{ih}R(s_{ih}^*) + t_{ih} &= k_{ih}w_h^*
\end{aligned}
$$

where $i = 1, 2$. Notice that for given s_{ih}^*, the final consumption enjoyed by the parents, $c_h^* + b_{1h}t_{1h} + b_{2h}t_{2h}$, is the same regardless of the value of b_{ih}. In other words, when $b_{ih}t_{ih}$ increases, either because the parents live with the child (higher b_{ih}) or the child requires a larger transfer to

[35] Table A7 reports the reduced form results of this excercise, suggesting that an additional year of schooling only contributes an increase of 120 yuan in life-time consumption (see preferred specification with discounting rate 0.05), which is much lower than the monetary cost suggested by the first stage results. It is not surprising that such a low estimate in the marginal return to schooling is consistent with our low point estimates of the marginal return to schooling expenditure in Table 4.

obtain the desired consumption level (higher t_{ih}), parents will decrease their own expenditure c_h^* accordingly, so that their consumption is not changed.

6 Conclusion

This paper examines how parents adjust bride-prices and land divisions to compensate their sons for differences in their schooling expenditures in rural China. The main estimate implies that when a son receives one yuan less in schooling investment than his brother, he will obtain 0.7 yuan more in observable marital and post-marital transfers as partial compensation. This marginal compensation estimate is quantitatively larger than any comparable estimate using North American data, suggesting that the unitary model is a useful model of resource allocation for sons in traditional agricultural families. Controlling for unobserved household heterogeneity, planned consumption differences across sons, and a fuller accounting of lifetime transfers are quantitatively important.

There are a number of avenues for future research. First, the discrepancies between within gender and across gender tests of the unitary model in China need further study. In our data, we will have to deal with the failure of two stage optimization for daughters because a significant number of daughters receive zero dowry. Second, it will be useful to consider strategic models of family interaction when $f_{ih} < 1$ and there is a conflict between the household's investment optimum and the individual's optimum. Finally, we have ignored the question of young adults leaving their villages and moving to cities. Since this trend will grow, it is important to extend the framework to incorporate this phenomenon.

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Appendix

A Estimation of the Multiplicative Model

Given a random sample of households $h = 1, .., H$, each with 2 children $i = 1, 2$, consider the regression

$$y_{ih} = (1 + D_{ih}\delta) \cdot \omega_h + X_{ih}b + \varepsilon_{ih} \tag{16}$$

where y_{ih} is the dependent variable; D_{ih} and X_{ih} are vectors of children's characteristics; and ω_h is the household fixed effect. The parameters to be identified are $\theta = (\delta, \{\omega_h\}_{h=1}^{H}, b)$.[36]

A nonlinear least squares (NLS) estimator of θ, $\widehat{\theta}$, solves

$$\min_{\theta} \sum_{h=1}^{H}\sum_{i=1}^{2} [y_{ih} - (1 + D_{ih}\delta) \cdot W_h - X_{ih}b]^2$$

To reduce the computational burden of a global search for all parameters, we take advantage of the partial linearity in (16): given a value of δ, δ_0, equation (16) is a linear model. $\{\omega_h\}_{h=1}^{H}$ and b can be estimated through OLS. Denoting these estimators as $\{\omega_h^{ols}(\delta_0|X_{ih}, D_{ih})\}_{h=1}^{H}$ and $b^{ols}(\delta_0|X_{ih}, D_{ih})$, our problem is to find

$$\widehat{\delta} = \arg\min_{\delta_0} \sum_{h=1}^{H}\sum_{i=1}^{2} (y_{ih} - (1 + D_{ih}\delta_0) \cdot \omega_h^{ols}(\delta_0|X_{ih}, D_{ih}) - X_{ih}b^{ols}(\delta_0|X_{ih}, D_{ih}))^2$$

Given $\widehat{\delta}$, the final estimator $\widehat{\theta}$ is

$$\widehat{\theta} = (\widehat{\delta}, \{\omega_h^{ols}(\widehat{\delta}|X_{ih}, D_{ih})\}_{h=1}^{H}, b^{ols}(\widehat{\delta}|X_{ih}, D_{ih}))$$

B The Incidental Parameter Problem

The least square estimators outlined above are subjected to a so-called "incidental parameter" problem. As first noticed by Neyman and Scott (1948), standard estimators of nonlinear panel data model are usually inconsistent if the length of the panel is small relative to the number of observations. In this case, the finite sample bias in the fixed effects parameters ($\{\omega_h\}_{h=1}^{H}$ in our context) will contaminate estimates of other parameters (δ and b in (16)).

Given the partial nonlinearity feature of our model, an alternative approach that can help get around the incidental parameter problem is quasi-differencing. To see this, divide $1 + D_{ih}\delta$ on both sides of (16),

$$\frac{y_{ih}}{1 + D_{ih}\delta} = W_h + \frac{X_{ih}}{1 + D_{ih}\delta}b + \frac{\varepsilon_{ih}}{1 + D_{ih}\delta}$$

[36] In our context, comparing (16) and (12), we can see the following correspondence: $y_{ih} = t_{ih}$, $\omega_h = k_0 w_h$, $X_{ih} = (s_{ih}, D_{ih}s_{ih})$, and $b = (\beta, \gamma)$.

and then take the sibling difference within the same household

$$\Delta \frac{y}{1+D\delta}_h = \Delta \frac{X}{1+D\delta}_h b + \upsilon_h \tag{17}$$

where $\Delta \frac{x}{1+D\delta}_h = \frac{x_{1h}}{1+D_{1h}\delta} - \frac{x_{2h}}{1+D_{2h}\delta}$ and $\upsilon_h = \frac{\varepsilon_{1h}}{1+D_{1h}\delta} - \frac{\varepsilon_{2h}}{1+D_{2h}\delta}$. Notice that household fixed effects are eliminated in (17). The parameters to be determined are (δ, b), which can be consistently estimated through NLS.[37]

In order to compare the accuracy and efficiency between our least-square estimate and the above quasi-differencing estimate, we conduct Monte Carlo experiments. The number of households is 127, consistent with our dataset. In each simulation, each household is endowed with a $k_0 w_h$ assumed to be log normal distributed. Its distribution parameters are calibrated from the moments of the household fixed effects $\{\omega_h\}_{h=1}^{H}$ that we estimate from the data.

Each household has two children, one of whom is the "first son". We randomly generate two other attributes of the children, "indicator of the taller son" (D_{ih}^{taller}) and "indicator of living with parents post marriage" (D_{ih}^{livep}), such that their variance-covariance matrices with the first son indicator (D_{ih}^{1stson}) replicate the ones in the real data.

We regress schooling expenditure on $k_0 w_h$ and children's attributes using the actual dataset. We then use the coefficients and the distribution parameters of the error terms to generate the simulated schooling investments. Note that the error terms here serve as the ability endowments of the children. The inter-vivos transfers are simulated using equation (8) with error terms randomly generated to match the sample moments of the marital transfers (t_{ih}). The resulting data set is $\{s_{ih}, t_{ih}, D_{ih}^{taller}, D_{ih}^{livep}, D_{ih}^{1stson}\}$. We estimate the model

$$t_{ih} = (1 + \delta_1 D_{ih}^{taller} + \delta_2 D_{ih}^{livep} + \delta_3 D_{ih}^{1stson}) \cdot \omega_h + \beta s_{ih} + \varepsilon_{ih}$$

with the "true" parameters being set as $(\beta, \delta_1, \delta_2, \delta_3) = (-0.3, 0.2, 0.4, -0.2)$. Note that we allow $cov(s_{ih}, \omega_h) > 0$ and $cov(s_{ih}, D_{ih}) > 0$, but keep $cov(s_{ih}, \varepsilon_{ih}) = 0$ in the simulated data, which are basic identification assumptions maintained throughout our paper.

Table A4 reports the results of 500 simulations. Panel A contains our least square results and panel B contains the quasi-differencing results. The coefficients are the mean value of the 500 simulated estimates. In the brackets are their corresponding standard errors. The null hypothesis being tested is that "the mean of the estimates has the same value as the true parameters".

With different specifications, the least square approach used in our paper delivers highly precise estimates. By contrast, the quasi-differencing results exhibit larger bias and larger standard errors, especially when all three attributes of the children are controlled for simultaneously. We conclude that, even though our least square estimates may be theoretically inconsistent, their finite sample bias is negligible and they are more efficient than the consistent quasi-differencing estimates.[38] We

[37] For consistency we need $cov(\Delta \frac{X}{1+D\delta}_h, \upsilon_h) = 0$ in (17). This condition is ensured under our assumptions in the model.

[38] This conclusion is similar to Greene (2004). Using Monte Carlo methods, he finds that the coefficients in the Tobit model with fixed effects are "unaffected" by the incidental parameter problem. He observes that the estimators'

therefore base our inferences on the least squared results in our paper.

C Robustness of investment difference multiple transfers test to the biases in the transfer regression

This section provides an example of how our investment difference multiple transfers test is immune to the potential bias in the transfer regression. Consider the case where we regress marital transfer on a child's educational expenditure without household or sibling fixed effects, as discussed in proposition 2. The transfer equations can be written as

$$t_{ih,g} = \alpha_g \eta - \alpha_g f \cdot r s_{ih} + \alpha_g \upsilon_{ih} + \mu_{ih,g},\ g = 1,2,3,\ \alpha_3 = \alpha_1 + \alpha_2$$

where $\eta = \overline{k_{ih} w_h} = (\sum_{h=1}^{H} (k_{1h} + k_{2h}) w_h)(2H)^{-1}$ and $\upsilon_{ih} = k_{ih} w_h - \overline{k_{ih} w_h}$. It is easy to show that

$$-plim\widehat{\beta_{tg}} = \alpha_g (f \cdot r - \frac{Cov(s_{ih}, \upsilon_{ih})}{Var(s_{ih})}),\ g = 1,2,3,\ \alpha_3 = \alpha_1 + \alpha_2 \tag{18}$$

Hence

$$\frac{plim\widehat{\beta_{t_3}}}{plim\widehat{\beta_{tg}}} = \frac{\alpha_1 + \alpha_2}{\alpha_g} > 1,\ g = 1,2$$

which is independent of the bias term.

D Regressions where sibling effects and household fixed effects enter linearly

In this section we present the results and discuss the limitations of the commonly-used regression with linear household and sibling fixed effects. We regress the bride-price on schooling investments using both level-on-level form (Table A5) and log-on-log form (Table A6), with (panel As) and without household fixed effects (panel Bs).[39] Different children characteristics are controlled in different columns.

Controlling for household level heterogeneities is important. Without household fixed effects, the coefficients on schooling investments are significantly negative in level regressions but insignificant (sometimes even positive) in log form regressions. After adding in household dummies, the coefficients become substantially smaller (more negative) and significant throughout all specifications, indicating larger compensation effects in bride-prices. Children from richer families tend to

behavior in models with a continuous dependent variable (whether truncated or not) are quite different from binary choice models.

[39] Family level attributes, such as father's years of schooling, total number of sons and daughters, and wealth indicators are also controlled for in the regression without household fixed effects. These coefficients are not reported to save space.

receive more in both education investments and inter-vivos transfers, therefore simple OLS regression will result in an upward bias in the coefficients on schooling investments if we cannot perfectly control for family wealth. For this reason, we just focus our discussions on household fixed effect regressions.

Our level-on-level fixed effect regressions suggest that when one son receives 1 yuan less in his schooling investments than his brothers, he will be compensated by about 0.30 to 0.38 yuan in his marital transfers. The log form regressions support a similar compensation story.

Children's characteristics are generally not significant in fixed effect regressions. However, some of the indicators have relatively large coefficients in magnitude. For example, sons living with their parents after marriage receive over 1000 yuan more than their brothers in marital transfers, but the larger standard error of this estimate precludes us from drawing any inference at conventional confidence levels.

The level-on-level specification is problematic when parents have "unequal concern" towards their children. For example, if parents love their first son 10 percent more than the others, they will give him 10 percent, rather than 0.1 yuan, more in marital transfers. In this sense, one might consider the log-on-log form as a better approximation to the transfer decision model. However, the log-on-log specification imposes a strong implication in the multiple transfers test. To see this, consider the life-time transfer determination equation

$$\ln t_{ih} = \ln \omega_h - e \ln s_{ih} + \varepsilon_{ih}$$

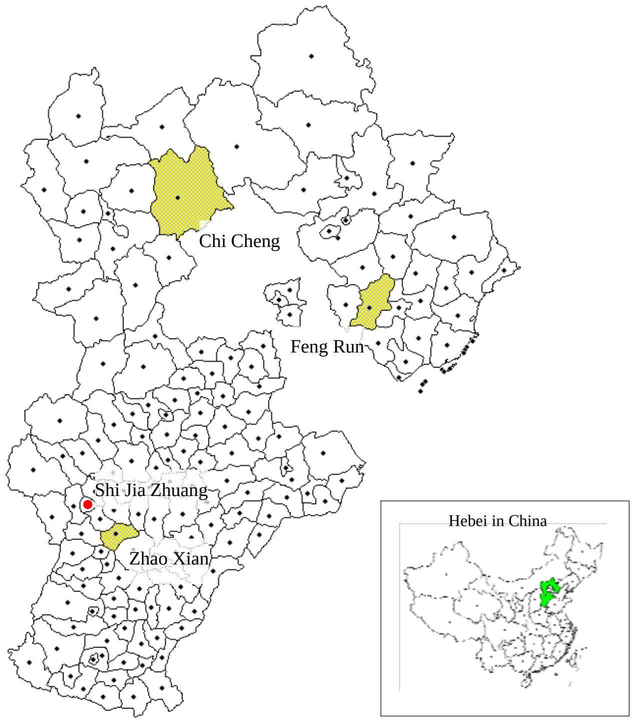
where e is the marginal compensation elasticity. Now suppose we only observe αt_{ih} instead of t_{ih}, the above equation becomes

$$\ln \alpha t_{ih} = \ln \alpha + \ln \omega_h - e \ln s_{ih} + \varepsilon_{ih}$$

The effect of α is buried in the constant term and has no impact on the estimation of e. That is to say, the log-on-log specification is robust to the problem of "partially observable life-time transfer". Adding more transfers into the dependent variable should not change the coefficient on the log of schooling expenditure.

Table A8 tests this implication by replicating the panel B of Table A6 while adding the value of land to the dependent variable. Compared with Table A6, estimates of e nearly double throughout the specifications in Table A8, providing strong evidence for rejecting the use of a log-on-log specification in the transfer regressions.

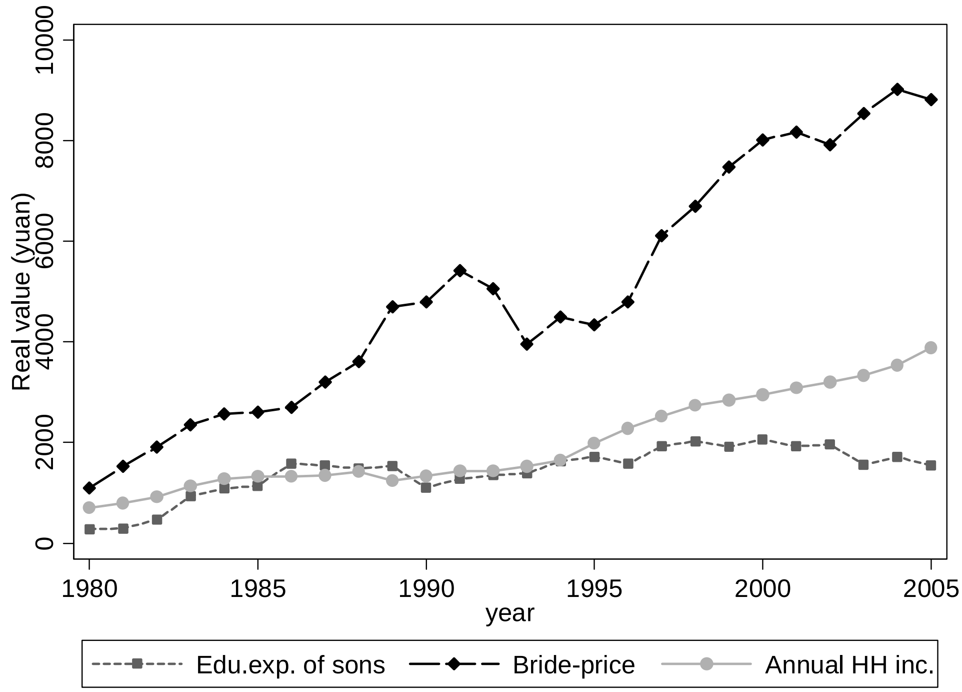
Figure 1. Surveyed Counties in Hebei



Notes:

The three surveyed counties have been shaded in. The large white area in the middle of the province is the municipalities of Beijing and Tianjin, both of which have provincial administrative status.

Figure 2. Marital Transfers and Educational Expenditure Received by Sons



Data Sources:

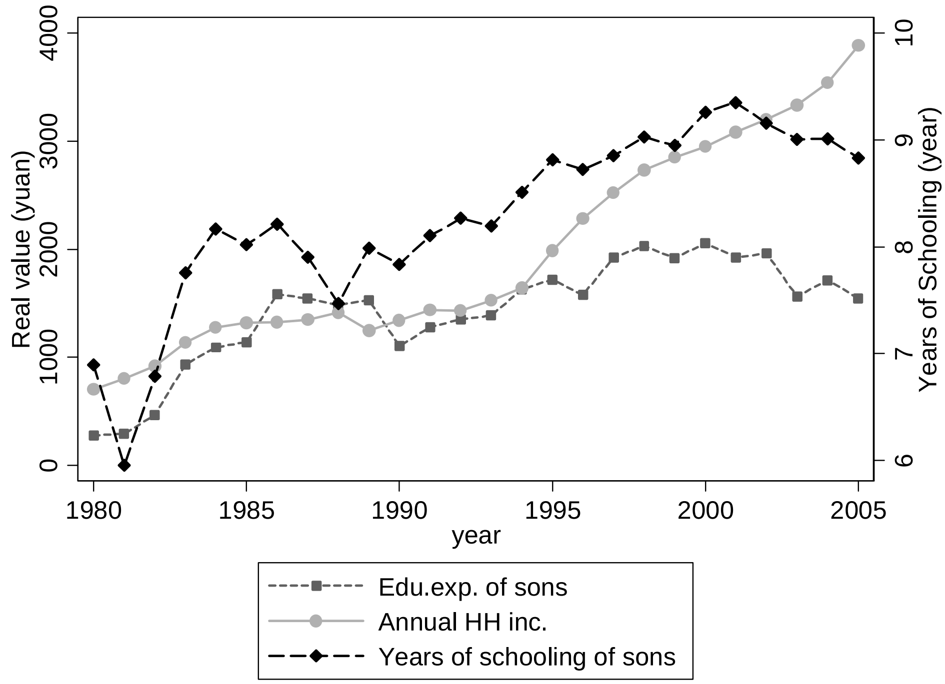
Annual household income: Hebei Bureau of Statistics (http://www.hetj.gov.cn/);

Marital transfers and educational expenditure: Survey on family and marriage dynamics in Hebei province.

Notes:

We first calculate the mean marital transfers (bride-price) and educational expenditure for children married in each year, then smooth the graph by calculating the moving average of the four adjacent years. All monetary values have been deflated to the 1980 price level.

Figure A1. Educational Expenditure versus Years of Schooling of Sons



Data Sources:

Annual household income: Hebei Bureau of Statistics (http://www.hetj.gov.cn/);

Educational expenditure and years of schooling: Survey on family and marriage dynamics in Hebei province.

Notes:

This graph plots the mean educational expenditure and the mean years of schooling of sons married in certain years.

The graph has been smoothed using the moving average of the four adjacent years. All monetary values have been deflated to the 1980 price level.

Table 1. Variable Definitions and Summary Statistics

Variable Name	Unit	Definition or Notes	Obs	Mean	Std	Std w/i HH	# of HH w/ intra-HH variations
			(1)	(2)	(3)	(4)	(5)
Key Variables							
yr_sch	year	Years of schooling.	264	8.42	(2.90)	(1.62)	127
edu_exp	yuan†	Total educational expenditure.	264	1556	(2785)	(1917)	127
mar_trans	yuan	Total monetory value of bride-price, including house, items and cash.	264	5085	(5151)	(2469)	127
land	yuan	Value of the land division.‡	172	2328	(2007)	(946)	83
Sons' Attributes							
age		Age.	264	33.87	(5.68)	(2.83)	127
age_mar		Age at marriage.	264	23.34	(2.53)	(1.64)	105
taller		Indicator of the taller son.*	264	0.39	(0.49)	(0.44)	99
more_ag_expr		Indicator of the son with more years of experience in agriculture.*	264	0.29	(0.46)	(0.38)	72
more_nonag_expr		Indicator of the son with more years of experience in nonagriculture.*	264	0.34	(0.47)	(0.42)	88
live_in_pre		Indicator of living with parents before marriage.	264	0.37	(0.48)	(0.28)	40
live_in_post		Indicator of living with parents afrer marriage.	264	0.38	(0.49)	(0.30)	46
1st_son		Indicator of the first born son.	264	0.48	(0.50)	(0.50)	126
Variables used to construct the dummy variables in sons' attrinbutes							
height	cm	Height.	264	170.34	(5.80)	(2.82)	99
ag_expr	year	Agricultural experience before marriage.	264	3.60	(4.60)	(2.26)	72
nonag_expr	year	Non-agricultural experience before marriage.	264	2.91	(3.04)	(1.60)	88
Household Attributes							
yr_sch_fa	year	Father's years of schooling.	127	5.32	(3.11)		
n_sons		Total number of sons in the household.	127	2.42	(0.76)		
n_daughters		Total number of daughters in the household.	127	0.76	(0.82)		
ttl_house	yuan	Total value of houses ever built by the parents.	127	11659	(12311)		
ttl_ag_equip	yuan	Total value of agricultural equipment ever possessed by the parents.	127	2224	(5085)		
fengrun		Indicator of residents in Fengrun county.	127	0.35	(0.48)		
zhaoxian		Indicator of residents in Zhaoxian county.	127	0.29	(0.45)		
chicheng		Indicator of residents in Chicheng county.	127	0.37	(0.48)		

Note:

† All monetary values are deflated to the 1980 price level.

‡ The Hebei Statistiscal Yearbook provides the net income from cultivation per mu in each year. We assume that half this income is the net return to land. Given the area of the land recieved by each son, the value of the land is defined as the sum of the total return to land over the 10 years since the land was given by the parents.

* Comparison among the selected sibling in the household.

Table 2. Multiplicative Specification

	OLS	HH FE	Multiplicative FE							
	O1	O2	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
edu_exp (β)	-0.23	-0.39	-0.40	-0.36	-0.38	-0.31	-0.29	-0.35	-0.20	-0.25
	[0.06]***	[0.15]***	[0.15]***	[0.15]**	[0.16]**	[0.17]*	[0.13]**	[0.14]**	[0.11]*	[0.12]**
δ										
taller			0.16						0.18	0.22
			[0.1]						[0.1]*	[0.11]**
more_ag_expr				0.11					0.04	
				[0.15]					[0.13]	
more_nonag_expr					0.10				0.17	
					[0.12]				[0.12]	
live_in_pre						0.29			0.24	
						[0.3]			[0.22]	
live_in_post							0.40		0.45	0.40
							[0.25]		[0.16]***	[0.17]**
1st_son								-0.20	-0.23	-0.22
								[0.13]	[0.12]**	[0.12]*
R-squared	0.02	0.79	0.80	0.79	0.80	0.80	0.81	0.81	0.84	0.84
F-test (P)†									0.00	0.00

Note:
The regressions are based on the sons' sample with 264 observations in 127 households.
Robust standard errors are in brackets, clustered at the household level.
* significant at 10%; ** significant at 5%; *** significant at 1%
† P value of the F test for the joint significance of k_{ih}.

Table 3. Multiplicative Specification with Interaction Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
edu_exp (β)	-0.37	-0.36	-0.33	-0.32	-0.29	-0.44	-0.18	-0.29
	[0.19]*	[0.15]**	[0.17]*	[0.13]**	[0.12]**	[0.28]	[0.22]	[0.2]
δ								
taller	0.17						0.22	0.25
	[0.07]**						[0.1]**	[0.08]***
more_ag_expr		0.10					0.17	
		[0.13]					[0.13]	
more_nonag_expr			0.16				0.21	
			[0.12]				[0.17]	
live_in_pre				0.26			0.15	
				[0.15]*			[0.22]	
live_in_post					0.39		0.43	0.40
					[0.17]**		[0.21]**	[0.12]***
1st_son						-0.22	-0.24	-0.26
						[0.1]**	[0.12]**	[0.08]***
γ: Interaction with educational expenditure (edu_exp)								
taller	-0.07						-0.12	-0.16
	[0.24]						[0.16]	[0.17]
more_ag_expr		0.06					-0.63	
		[0.87]					[0.86]	
more_nonag_expr			-0.34				-0.17	
			[0.25]				[0.24]	
live_in_pre				0.14			0.20	
				[0.48]			[0.34]	
live_in_post					0.10		0.15	0.12
					[0.57]		[0.59]	[0.51]
1st_son						0.12	0.05	0.14
						[0.28]	[0.19]	[0.21]
R-squared	0.80	0.79	0.80	0.80	0.81	0.81	0.84	0.84
F-test1†							0.00	0.00
F-test2‡							0.90	0.71

Note:
The regressions are based on the sons' sample with 264 observations in 127 households.
Robust standard errors are in brackets, clustered at the household level.
* significant at 10%; ** significant at 5%; *** significant at 1%
† P value of the F test for the joint significance of k_{ih}.
‡ P value of the F test for the joint significance of the interaction terms.

Table 4. Multiplicative Specification with Discounting

	Basic			Preferred Specification			with Interactions		
	d=0	d=0.05	d=0.1	d=0	d=0.05	d=0.1	d=0	d=0.05	d=0.1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
edu_exp (β)	-0.40	-0.26	-0.14	-0.25	-0.21	-0.16	-0.29	-0.16	-0.06
	[0.15]***	[0.12]**	[0.1]	[0.12]**	[0.1]**	[0.1]	[0.2]	[0.18]	[0.17]
δ									
taller				0.22	0.21	0.22	0.25	0.23	0.25
				[0.11]**	[0.11]**	[0.11]*	[0.08]***	[0.07]***	[0.07]***
live_in_post				0.40	0.39	0.36	0.40	0.38	0.34
				[0.17]**	[0.18]**	[0.18]**	[0.12]***	[0.08]***	[0.08]***
1st_son				-0.22	0.11	0.47	-0.26	0.13	0.64
				[0.12]*	[0.11]	[0.12]***	[0.08]***	[0.07]*	[0.07]***
γ: Interaction with educational expenditure (edu_exp)									
taller							-0.16	-0.02	0.00
							[0.17]	[0.16]	[0.16]
live_in_post							0.12	0.07	0.07
							[0.51]	[0.32]	[0.26]
1st_son							0.14	-0.04	-0.17
							[0.21]	[0.18]	[0.15]
R-squared	0.80	0.82	0.80	0.84	0.83	0.84	0.84	0.83	0.84
F-test1†				0.00	0.00	0.00	0.00	0.01	0.00
F-test2‡							0.71	0.97	0.38

Note:
The regressions are based on the sons' sample with 264 observations in 127 households.
Robust standard errors are in brackets, clustered at the household level.
* significant at 10%; ** significant at 5%; *** significant at 1%
† P value of the F test for the joint significance of kih.
‡ P value of the F test for the joint significance of the interaction terms.

Table 5. Adding Pre-mortem Bequests as Parts of the Transfers

Panel A. Summary Statistics of Pre-mortem Bequest

Items	# Household	# Household making pre-mortem bequest	Mean Value
House	127	94	4259
Land†	127	83	2328
Others	127	12	2542

Note:

† Construction of the value of land: The Hebei Statistiscal Yearbook provides the net income from cultivation per mu in each year. We assume that half this income is the net return to land. Given the area of the land recieved by each son, the value of the land is defined as the sum of the total return to land over the 10 years since the land was given by the parents.

Panel B. Regression Results

	Basic			Preferred Specification			with Interactions		
	d=0	d=0.05	d=0.1	d=0	d=0.05	d=0.1	d=0	d=0.05	d=0.1
edu_exp (β)	-0.69	-0.50	-0.32	-0.54	-0.44	-0.37	-0.75	-0.55	-0.36
	[0.2]***	[0.19]***	[0.17]*	[0.19]***	[0.2]**	[0.2]*	[0.18]***	[0.17]***	[0.17]**
δ									
taller				0.02	0.02	0.04	0.02	0.04	0.10
				[0.08]	[0.09]	[0.1]	[0.08]	[0.07]	[0.08]
live_in_post				0.16	0.16	0.18	0.05	0.07	0.12
				[0.14]	[0.16]	[0.18]	[0.11]	[0.09]	[0.09]
1st_son				-0.19	0.01	0.25	-0.23	-0.05	0.23
				[0.1]*	[0.11]	[0.12]**	[0.08]***	[0.08]	[0.09]***
γ: Interaction with educational expenditure (edu_exp)									
taller							-0.01	-0.11	-0.18
							[0.3]	[0.26]	[0.26]
live_in_post							1.33	0.74	0.43
							[0.96]	[0.68]	[0.59]
1st_son							0.28	0.17	0.02
							[0.29]	[0.27]	[0.23]
R-squared	0.84	0.87	0.87	0.87	0.87	0.87	0.87	0.87	0.88
F-test1†				0.00	0.39	0.00	0.01	0.86	0.32
F-test2‡							0.22	0.36	0.47

Note:

The regressions are based on sons sample with land divisions (172 observations in 83 households).

Robust standard errors are in brackets, clustered at the household level.

* significant at 10%; ** significant at 5%; *** significant at 1%

† P value of the F test for the joint significance of kih.

‡ P value of the F test for the joint significance of the interaction terms.

Table 6. Bootstrap Results of β_{t3}/β_{t1} (Sammary Statitics)

			mean	std	min	max	$P(\beta_{t3}/\beta_{t1}>1)$
Basic	d=0	(1)	1.76	0.68	1.09	5.97	1.00
	d=0.05	(2)	1.47	0.60	1.05	6.00	1.00
	d=0.1	(3)	1.29	1.09	-3.77	9.77	0.94
Preferred Specification	d=0	(4)	2.27	1.76	-6.03	12.37	0.97
	d=0.05	(5)	1.22	7.10	-67.00	16.64	0.99
	d=0.1	(6)	1.42	0.81	-2.59	6.11	0.98

Note:

This table reports the summary statistics of the ratio of coefficients on schooling investment (β) in regressions with the bride-price as the dependent variable (t1) and the bride-price plus land division (t3) as the dependent variable. The number of bootstrap iterations is 100.

Each row reports the results of a corresponding specification of Table 4.

Table 7. Instrumenting Educational Expenditure by Years of Schooling

Panel A:

	Basic			Preferred Specification			with Interactions		
	d=0	d=0.05	d=0.1	d=0	d=0.05	d=0.1	d=0	d=0.05	d=0.1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
edu_exp (β)	-0.35	-0.24	-0.16	-0.20	-0.19	-0.14	-0.21	-0.12	-0.04
	[0.26]	[0.17]	[0.13]	[0.21]	[0.17]	[0.14]	[0.25]	[0.21]	[0.18]
δ									
taller				0.22	0.21	0.22	0.27	0.26	0.28
				[0.11]**	[0.11]*	[0.12]*	[0.09]***	[0.07]***	[0.07]***
live_in_post				0.40	0.39	0.36	0.40	0.39	0.36
				[0.18]**	[0.18]**	[0.19]*	[0.13]***	[0.1]***	[0.09]***
1st_son				-0.22	0.11	0.47	-0.28	0.09	0.64
				[0.12]*	[0.12]	[0.12]***	[0.09]***	[0.08]	[0.09]***
γ: Interaction with educational expenditure (edu_exp)									
taller							-0.24	-0.11	-0.07
							[0.22]	[0.18]	[0.16]
live_in_post							0.58	0.30	0.10
							[0.42]	[0.33]	[0.25]
1st_son							0.19	0.00	-0.17
							[0.22]	[0.19]	[0.19]
R-squared	0.78	0.81	0.82	0.83	0.83	0.83	0.84	0.83	0.84
F-test1†				0.00	0.00	0.00	0.00	0.02	0.00
F-test2‡							0.93	1.00	0.64

Panel B: 1st Stage

yr_sch	769.24	883.35	1044.11	755.48	839.61	955.74	755.48	839.61	955.74
	[178.6]***	[232.84]***	[307.54]***	[165.72]***	[215.65]***	[284.44]***	[165.72]***	[215.65]***	[284.44]***
R-squared	0.73	0.72	0.71	0.76	0.74	0.73	0.76	0.74	0.73

Panel C: Reduced Form

	Basic			Preferred Specification			with Interactions		
	d=0	d=0.05	d=0.1	d=0	d=0.05	d=0.1	d=0	d=0.05	d=0.1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
yr_sch	-208.52	-183.42	-153.10	-63.02	-126.16	-95.79	-149.26	-104.26	-75.70
	[206.33]	[151.01]	[138.97]	[137.37]	[147.66]	[131.88]	[176.26]	[144.88]	[130.6]
R-squared	0.78	0.81	0.82	0.83	0.82	0.83	0.83	0.83	0.84

Note:
The regressions are based on the sons' sample with 264 observations in 127 households.
Robust standard errors are in brackets, clustered at the household level.
* significant at 10%; ** significant at 5%; *** significant at 1%
† P value of the F test for the joint significance of kih.
‡ P value of the F test for the joint significance of the interaction terms.

Table A1. Predictions on the Interaction Terms

Son's Attributes	Proportion of total educational return appropriated by the son	Sign of the interaction effects with schooling expenditure
Taller son	?	?
More agiculture experience before marriage	less	+
More non-agiculture experience before marriage	more	-
Live outside the home before marriage	more	-
Live with the parents after marriage	less	+
First son	less	+

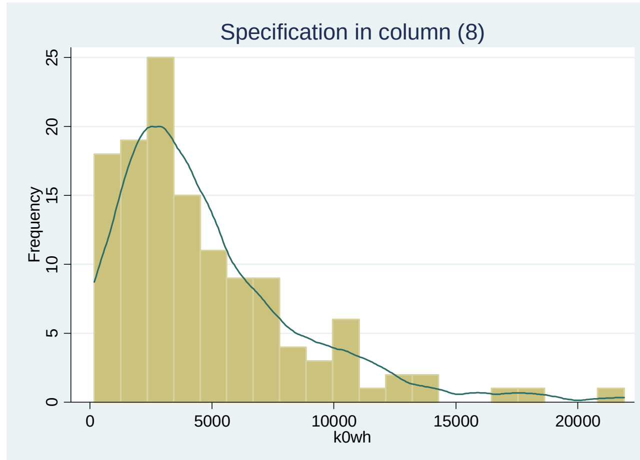
Table A2. Estimates of the consumption level enjoyed by the hypothetical son 0 in each household (k_0w_h)

Panel A. Summary of k_0w_h estimated from specifications in Table 2

Spec	Obs	Mean	Std. Dev.	Log-normality Test
(1)	127	5458	4360	0.21
(2)	127	5574	4480	0.08
(3)	127	5542	4402	0.16
(4)	127	4833	3956	0.24
(5)	127	4775	3818	0.26
(6)	127	6303	5017	0.19
(7)	127	3957	3294	0.19
(8)	127	4787	3877	0.14

Note: The "log-normality test" column reports the p-value of the Kolmogorov-Smirnov distribution test with the null hypothesis "the log of k_0w_h follows a normal distribution".

Panel B. Histogram and kernel density of k_0w_h



Panel C. Regressions of k_0w_h on household level characteristics

Dependent variable: estimates of k_0w_h	Specifications in Table 2 used to estimate k_0w_h							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
n_sons	-665.2	-739.8	-690.3	-784.7	-616.9	-840.3	-629.0	-643.5
	[329.4]**	[340.0]**	[339.5]**	[295.7]***	[307.3]**	[383.9]**	[256.3]**	[312.0]**
n_daughters	-707.9	-790.4	-710.8	-724.2	-591.5	-779.0	-454.8	-517.0
	[400.0]*	[393.7]**	[403.1]*	[337.9]**	[315.1]*	[466.5]*	[263.9]*	[325.2]
yr_sch_fa	34.1	18.3	29.5	-8.9	8.6	24.7	-5.5	11.0
	[92.4]	[94.1]	[93.2]	[86.6]	[80.3]	[108.1]	[69.8]	[81.0]
ttl_house	0.1	0.1	0.1	0.1	0.1	0.2	0.1	0.1
	[0.1]***	[0.1]**	[0.1]**	[0.0]***	[0.0]**	[0.1]**	[0.0]**	[0.0]**
ttl_ag_equip	0.3	0.3	0.2	0.3	0.2	0.3	0.2	0.3
	[0.1]***	[0.1]***	[0.1]***	[0.1]***	[0.1]***	[0.1]***	[0.1]***	[0.1]***
fengrun	427.0	512.8	479.1	418.5	543.3	669.4	482.1	576.3
	[932.1]	[960.3]	[959.9]	[809.5]	[825.5]	[1,047.3]	[664.1]	[803.8]
chicheng	-1071.5	-1103.7	-1116.9	-589.7	-582.4	-1187.1	-369.0	-590.9
	[875.9]	[903.6]	[903.4]	[782.7]	[773.1]	[1,010.1]	[641.5]	[762.7]
Obs	127	127	127	127	127	127	127	127
R-squared	0.38	0.38	0.36	0.38	0.33	0.37	0.36	0.36

Note:
Standard errors robust to heteroscedasticity are in brackets.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table A3. Multiplicative Specification Using Housholds with Land Division

	d=0	d=0.05	d=0.1	d=0	d=0.05	d=0.1	d=0	d=0.05	d=0.1
edu_exp (β)	-0.43	-0.37	-0.27	-0.27	-0.30	-0.29	-0.54	-0.45	-0.30
	[0.22]**	[0.19]*	[0.16]	[0.18]	[0.19]	[0.19]	[0.25]**	[0.2]**	[0.19]
δ									
taller				0.09	0.06	0.07	0.09	0.06	0.11
				[0.12]	[0.11]	[0.11]	[0.09]	[0.08]	[0.1]
live_in_post				0.25	0.21	0.19	0.16	0.14	0.15
				[0.2]	[0.2]	[0.19]	[0.1]*	[0.09]	[0.1]
1st_son				-0.31	-0.04	0.25	-0.37	-0.11	0.23
				[0.14]**	[0.13]	[0.13]*	[0.09]***	[0.09]	[0.1]**
γ: Interaction with educational expenditure (edu_exp)									
taller							-0.03	-0.04	-0.12
							[0.29]	[0.27]	[0.26]
live_in_post							0.93	0.46	0.32
							[0.77]	[0.6]	[0.55]
1st_son							0.37	0.21	0.03
							[0.3]	[0.26]	[0.23]
R-squared	0.82	0.87	0.88	0.86	0.86	0.87	0.87	0.87	0.87
F-test1†				0.00	0.35	0.01	0.00	0.52	0.38
F-test2‡							0.19	0.50	0.64

Note:
The regressions are based on sons sample with land divisions (172 observations in 83 households).
Robust standard errors are in brackets, clustered at the household level.
* significant at 10%; ** significant at 5%; *** significant at 1%
† P value of the F test for the joint significance of kih.
‡ P value of the F test for the joint significance of the interaction terms.

Table A4. Sensitivity of Estimate of Beta to Use of Different Estimators

Panel A. Non-linear Least Square

	True Value	Estimated Value			
β	-0.30	-0.29	-0.30	-0.30	-0.30
		[0.099]	[0.096]	[0.104]	[0.106]
δ					
height	0.2	0.20			0.20
		[0.047]			[0.05]
live_in_post	0.4		0.40		0.40
			[0.063]		[0.076]
1st_son	-0.2			-0.20	-0.20
				[0.034]	[0.035]

Panel B. Quasi-differencing

	True Value	Estimated Value			
β	-0.30	-0.28	-0.28	-0.31	-0.24
		[0.104]	[0.103]	[0.103]	[0.158]
δ					
height	0.2	0.27			0.39
		[0.056]			[0.666]
live_in_post	0.4		0.51		0.71
			[0.097]		[0.911]
1st_son	-0.2			-0.10	-0.03
				[0.046]**	[0.349]

Note:

Reported in the tables are the mean and standard deviation (in bracket) of the 500 simulations. The null hypothesis being tested is H0: estimated value = true value.

** means that the null can be rejected at 5 percent significance level.

Table A5. Marital Transfers Regression (bride-price as the dependent varaible)

Panel A: Without Household Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
edu_exp	-0.30	-0.30	-0.28	-0.29	-0.25	-0.25	-0.30	-0.20
	[0.08]***	[0.08]***	[0.08]***	[0.08]***	[0.08]***	[0.08]***	[0.08]***	[0.08]**
taller		660.69						622.92
		[416.12]						[410.47]
more_ag_expr			404.93					186.45
			[553.03]					[564.47]
more_nonag_expr				378.79				638.08
				[474.69]				[569.07]
live_in_pre					865.23			957.31
					[696.08]			[811.19]
live_in_post						1498.27		1412.04
						[608.82]**		[605.65]**
1st_son							51.92	69.23
							[503.87]	[484.19]
R-squared	0.33	0.33	0.33	0.33	0.33	0.35	0.33	0.36

Panel B: With Household Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
edu_exp	-0.38	-0.38	-0.37	-0.37	-0.35	-0.32	-0.38	-0.30
	[0.14]**	[0.14]***	[0.14]**	[0.15]**	[0.16]**	[0.15]**	[0.15]**	[0.18]*
taller		491.72						487.29
		[527.63]						[520.87]
more_ag_expr			63.09					-100.45
			[752.20]					[820.57]
more_nonag_expr				98.52				238.68
				[708.47]				[781.78]
live_in_pre					517.71			503.12
					[972.00]			[1,029.51]
live_in_post						1146.18		1070.42
						[1,143.19]		[1,158.68]
1st_son							-184.32	-228.35
							[846.33]	[842.42]
R-squared	0.80	0.80	0.80	0.80	0.80	0.81	0.80	0.81

Note:
The regressions are based on the sons' sample with 264 observations in 127 households.
In panel A, family level attributes, including father's years of schooling, total number of sons and daughters, and wealth indicators are controlled for, but their coefficients are not reported to save space.
Robust standard errors are in brackets, clustered at the household level.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table A6. Marital Transfers Regression (log(bride-price) as the dependent varaible)

Panel A: Without Household Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(edu_exp)	-0.05	-0.05	-0.03	-0.05	0.01	-0.02	-0.05	0.05
	[0.08]	[0.08]	[0.08]	[0.08]	[0.08]	[0.08]	[0.08]	[0.08]
taller		0.07						0.06
		[0.12]						[0.13]
more_ag_expr			0.16					0.09
			[0.16]					[0.16]
more_nonag_expr				0.02				0.14
				[0.13]				[0.13]
live_in_pre					0.40			0.41
					[0.16]**			[0.16]**
live_in_post						0.44		0.39
						[0.15]***		[0.15]***
1st_son							0.07	0.09
							[0.14]	[0.14]
R-squared	0.34	0.34	0.34	0.34	0.36	0.36	0.34	0.38

Panel B: With Household Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(edu_exp)	-0.30	-0.30	-0.31	-0.30	-0.28	-0.27	-0.31	-0.28
	[0.15]*	[0.16]*	[0.16]*	[0.15]**	[0.16]*	[0.15]*	[0.16]*	[0.16]*
taller		-0.02						-0.02
		[0.16]						[0.16]
more_ag_expr			-0.08					-0.09
			[0.24]					[0.25]
more_nonag_expr				-0.05				-0.03
				[0.18]				[0.20]
live_in_pre					0.14			0.09
					[0.29]			[0.32]
live_in_post						0.32		0.30
						[0.27]		[0.29]
1st_son							0.17	0.16
							[0.28]	[0.29]
R-squared	0.75	0.75	0.75	0.75	0.75	0.76	0.75	0.76

Note:
The regressions are based on the sons' sample with 264 observations in 127 households.
In panel A, family level attributes, including father's years of schooling, total number of sons and daughters, and wealth indicators are controlled for, but their coefficients are not reported to save space.
Robust standard errors are in brackets, clustered at the household level.
* significant at 10%; ** significant at 5%; *** significant at 1%

Tabke A7. Reduced Form Regression - Expenditure Replaced by Years of Schooling

	d=0	d=0.05	d=0.1	d=0	d=0.05	d=0.1	d=0	d=0.05	d=0.1
yr_sch	-208.52	-183.42	-153.10	-63.02	-126.16	-95.79	-149.26	-104.26	-75.70
	[206.33]	[151.01]	[138.97]	[137.37]	[147.66]	[131.88]	[176.26]	[144.88]	[130.6]
δ									
taller	0.16	0.10	0.07	0.22	0.21	0.22	0.39	0.41	0.44
	[0.1]	[0.11]	[0.14]	[0.11]*	[0.11]**	[0.12]*	[0.12]***	[0.1]***	[0.1]***
live_in_post				0.40	0.39	0.36	0.61	0.56	0.44
				[0.17]**	[0.18]**	[0.18]**	[0.17]***	[0.14]***	[0.11]***
1st_son				-0.22	0.11	0.47	-0.39	0.04	0.65
				[0.12]*	[0.11]	[0.13]***	[0.11]***	[0.1]	[0.1]***
γ									
taller							-180.63	-149.36	-141.89
							[126.67]	[92]	[75.08]*
live_in_post							-150.15	-66.82	13.56
							[160.45]	[167.69]	[151.92]
1st_son							155.85	23.05	-111.22
							[115.46]	[88.46]	[122.39]
R-squared	0.78	0.81	0.82	0.83	0.82	0.83	0.83	0.83	0.84
F-test1†	0.17	0.01	0.00	0.00	0.00	0.00	0.00	0.01	0.00
F-test2‡							1.00	1.00	1.00

Note:
The regressions are based on the sons' sample with 264 observations in 127 households.
Robust standard errors are in brackets, clustered at the household level.
* significant at 10%; ** significant at 5%; *** significant at 1%
† P value of the F test for the joint significance of kih.
‡ P value of the F test for the joint significance of the interaction terms.

Table A8. Adding the Value of Land to Parental Transfers (log-log specifications with household fixed effects)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(edu_exp)	-0.54	-0.55	-0.49	-0.53	-0.50	-0.49	-0.56	-0.44
	[0.15]***	[0.16]***	[0.17]***	[0.15]***	[0.16]***	[0.14]***	[0.16]***	[0.15]***
taller		-0.10						-0.10
		[0.17]						[0.16]
more_ag_expr			0.18					0.24
			[0.19]					[0.20]
more_nonag_expr				0.14				0.22
				[0.15]				[0.15]
live_in_pre					0.17			0.10
					[0.22]			[0.30]
live_in_post						0.47		0.44
						[0.26]*		[0.28]
1st_son							0.15	0.21
							[0.31]	[0.28]
R-squared	0.73	0.73	0.73	0.73	0.73	0.75	0.73	0.77

Note:
The regressions are based on sons sample with land divisions (172 observations in 83 households).
Robust standard errors are in brackets, clustered at the household level.
* significant at 10%; ** significant at 5%; *** significant at 1%