Altruism, Favoritism, and Guilt in the Allocation of Family Resources:

Sophie's Choice in Mao's Mass Send Down Movement

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The interactions between husbands, wives, parents, and children are more likely to be motivated by love, obligation, guilt, and a sense of duty than by self-interest narrowly interpreted.

Gary Becker, Nobel Lecture in Economics,1992.

1. Introduction

Although altruism is now well-accepted as a motivating force in the family, explaining in part the intergenerational flows of resources via human capital investments, transfers and bequests, the existence of behavior motivated by guilt has received little attention by economists. Psychologists emphasize the interpersonal aspect of guilt and define guilt as a negative emotional state associated with others' disapproving perceptions about one's actions. As concluded by Baumeister *et al*. (1994), "The prototypical cause of guilt would be the infliction of harm, loss or distress on a relationship partner."(p. 245). There are two implications highlighted: First, individuals will minimize actions that cause harm (guilt aversion). Second, individuals who have caused harm to others will seek to redress their actions by compensating the harmed parties. Guilt and restitution are importantly linked (Eisenberg, 2000). And, in this literature, guilt feelings are enhanced the closer the relationships among partners.[1]

In economics, guilt is hypothesized to play an important role in helping to solve commitment problems. Guilt aversion may induce cooperative behavior in

[1] There is one exception cited in the literature to the idea that guilt is based on acts perceived as wrongdoing, "survivor guilt" (Baumeister *et al*., 1994). The evidence for this is based on accounts of survivors of Hiroshima, the Holocaust, and homosexual men who survived in the early stages of the AIDS epidemic without any purposive or accidental action taken on their part to enhance their survival. We test for survival guilt among the twins cohorts experiencing the split family decision in the last section.

transactions where commitment cannot be externally enforced. Kandel and Lazear (1992), for example, argue that feeling guilty is a way to create incentives or to avoid shirking in teamwork.[2] If guilt exists and can be manipulated, it may be possible to design credit or insurance instruments, for example, that minimize commitment problems. In the context of the family, as pointed out by Becker (1992), if parents can instill guilt in children, then children would be more likely to provide old-age support to mitigate their feelings of guilt. While altruistic children may also provide such support, *ex ante* actions by parents can increase children's propensity to remit if such actions induce guilt. Forward-looking parents will then invest more in children when such investments enhance children's guilt. Without guilt, parental underinvestment due to lack of commitment would thus be greater. The key distinction between altruism and guilt is that for the former the utility of the agent depends on the utility state of the focal person while for the latter it is the actions taken (or not taken) by the agent that matter.[3]

There is little empirical evidence supporting the importance of guilt in motivating behavior; in particular, whether an agent compensates the specific person being harmed in order to reduce the psychic cost of guilt. Economists and psychologists have begun to carry out experiments to asses the role of guilt in games in which the commitment problem is endemic (e.g., prisoner's dilemma, dictator and ultimatum games). Miettinen and Suetens (2008) find that guilt feelings are induced by unilateral defection in prisoner's dilemma games. They do not assess if such feelings alter behavior. Ketelaar and Au (2003) find that those who followed a non-

[2] More generally, Arrow (1974: 23) suggests guilt may serve as an important lubricant of the social system. Kaplow and Shavell (2001) and Shavell (2002) argue that guilt or more generally morality, similar to the function of law, is an important way to channel human behavior.

[3] This distinction is analogous to that in the charitable giving literature between altruism and "warm glow" (e.g., Andreoni, 1989). Similarly, psychologists generally distinguish guilt from "shame" as the difference between feeling bad about an act (guilt) and about oneself (shame) (Eisenberg, 2000).

cooperative strategy in the first stage of an ultimatum game, and who tested as guilt-ridden as a consequence, were more likely to cooperate in the second round. Ellingsen *et al*. (2007), however, in trust and dictator games find no evidence of guilt aversion.

The lab experimental evidence is principally based on subjects who are unrelated university students, for whom psychologists argue guilt-inducing behavior is weak, compared with such behavior among those in close, long-term relationships. More importantly, while the experiments involve randomization of standard game parameters, the investigators do not randomize guilt-inducing behavior and thus do not provide estimates of how actions that induce guilt causally affect subsequent behavior. The principal problem is that there is heterogeneity in guilt aversion or propensity to feel guilty (perhaps induced by strategic parental behavior).[4] Thus, those who actually take guilt-inducing actions may be less prone to remedy them. Evidence outside the laboratory, in the psychology literature, is based mainly on anecdotal and autobiographical accounts (Tangney, 1995).

In this paper, we use new survey data on twins born in urban China among whom many experienced the consequences of the forced rustication movement of the Chinese "cultural revolution" to identify the distinct roles of altruism and guilt in affecting behavior within families. That is we combine a policy "experiment" with a natural natural experiment (Rosenzweig and Wolpin, 2000) to approximate the randomized experiment that has not and may never be possible to carry out – randomizing choices that are perceived to be harmful to third parties.

Between 1966 and 1976, schools and universities in China were shut down for varying periods and over 17 million urban secondary school graduates were sent to the countryside, representing probably the largest urban to rural migration in human

[4]Ketelaar and Au (2003), for example, using an instrument to identify guilt feelings, found that not all experimental subjects felt guilty when they chose the same non-cooperative strategy.

history. Researchers have used this historical episode in China to help identify the returns to schooling (e.g., Giles, Park and Zhang, 2007). We instead focus on the "send-down movement," and exploit a little known feature of this mass migration program, which is that in many families with two or more age-eligible children parents were made to select who among their children would be rusticated. In particular, some localities required at least one child from a family to go down, while other localities allowed each family to keep one child in the city (Bernstein, 1977; Zhou and Hou, 1999). Parents were thus forced to express favoritism towards one child versus another. Favoritism expressed within monozygotic (MZ) twin pairs is likely to have a large random component; and by comparing the twin-splitting choices of parents across MZ and dizygotic (DZ) twin pair households we show that we can assess to what extent such favoritism is biased towards better- or worse-endowed children and whether guilty actions followed such choices. We thus use the data to examine the "Sophie's choice" faced by families of whom to send away, the earnings and schooling of those sent away and those not sent away, and the subsequent large transfers made by parents to the children at the time of their marriages to identify three aspects of family behavior – altruism, favoritism and guilt.[5]

We set out a simple model with one parent and two children, with the parent being the only decision maker. Each child is endowed with a different amount of human capital. The parent is altruistic in that she cares about each child's utility. The parent also derives utility from spending time with a child and from giving monetary transfers to her. Favoritism exists if the parent derives more utility by spending the same time with or giving the same amount of transfers to one child versus another.

[5] In the novel *Sophie's Choice* (Styron, 1979), the main character in the novel is forced by Nazis to select which of her two children will be put to death and which sent to a concentration camp. The book depicts in part the guilt felt by Sophie over this act of choosing, which ultimately leads to suicide (it is impossible in this case to relieve guilt by compensating the "chosen" child).

Such favoritism could be based on the child's endowment. The model also incorporates guilt such that the parent derives more utility by giving transfers to a child who gets less parental time compared with the child who received more parental time.

The model shows the conditions under which we can separately identify altruism, favoritism and guilt based on an experiment in which there is exogenously-imposed variation in parents' time with children, as in the send-down program. The model also delivers decision rule for which child (twin) is rusticated. We show that altruism can be identified if the parent transfers more to a child that earns less, favoritism can be identified if the parent chooses to allocate her time across the children according to their endowments, and guilt can be identified if the parent transfers more to the child she spent less time with, given the current earnings state of the children. Moreover, we show that guilt can be identified only with information on at least two sibling children and their parents, and thus not from conventional survey data that typically provides information on one child and her parents.

There are two empirical challenges to identification arising from the fact that endowments of children and parental preferences are unobserved. First, we cannot directly estimate how endowments affect parental time with and transfers to children, as endowments are not measured. Second, the observed relationship between transfers and parental time with children, for example, would not identify guilt effects as both are endogenous. In addition, a measure of parental time with each child is required, which in standard surveys of adults would be inaccurately measured and missing for a respondent's sibling. In our sample, however, we can use the contrast between MZ and DZ twin pairs and the variation in the scope and rules of the send-down movement to overcome these empirical identification problems.

As is argued in the literature on education and earnings (Ashenfelter and Krueger, 1994; Behrman and Rosenzweig, 1999), as MZ twins are genetically identical and have a similar family background, the effects of unobserved endowment or family background should be similar for both twins. Thus, obtaining estimates based on within-MZ twin differences will, to a great extent, reduce the influences of unobservables such as endowments and thus allow identification of causal impacts. Second, by comparing the estimates using the MZ twins sub-sample with those using the DZ twins sub-sample (Behrman, Rosenzweig and Taubman, 1994), we are able to identify the impact of unobserved individual endowments on our variables of interest. Finally, the selection of one child to send down from within a pair of MZ twins comes close to mimicking the randomization of the send-down treatment, and thus for this sub-sample we can identify the causal effect of the choice of who to send away on subsequent interpersonal parental and child behavior.

Our estimates indicate that, controlling for unobserved family and individual endowments and the selectivity of the send-down choice, rustication actually had a large positive return for earnings, employment, and political status. OLS regressions tend to under-estimate by more than 50% the true earnings return to rustication, however, and estimates based on MZ twins also yielded higher rustication returns than those estimated based on differences between DZ twins.[6] These findings suggest that during the send-down movement, children with disadvantageous family backgrounds were sent down and stayed in the countryside for a longer period and that parents choosing among children also sent away the child with less favorable earnings

[6] There are some studies that measure the effects of send-down in the sociology and China studies literature (Deng and Treiman, 1997; Zhou and Hou, 1999; Yang, 2003), but these studies do not take into account either the cross-household or within-household selectivity of the "program."

endowments. *De facto* policy and parents thus exhibited favoritism towards the better-endowed children with respect to who to retain in the household.[7]

Our estimates of the parental transfer equation, making use of information on the large parental gifts and transfers made at the time of each child's wedding and exploiting the within-MZ twin variation in such transfers, also reveal altruistic behavior, favoritism and guilt. First, we find that parents tend to transfer somewhat more resources to the child with lower earnings, consistent with altruism. Consistent with the evidence on parental favoritism based on earnings, we also find that parents favored the child sent down for a shorter period in terms of parental wedding gifts.

Finally, we find evidence that is consistent with parents feeling guilt over sending a child to the countryside. In particular, after controlling for the effect of altruism and favoritism (net of endowments and contemporaneous earnings), our FE estimate using the MZ twins sample suggests that parental gifts at the time of wedding were 12 percent higher for each additional year a child stayed in the countryside. Our finding that guilt is an additional reason parents make transfers to children thus provides some empirical foundation for theoretical models that use guilt or morality to explain human behavior and socioeconomic phenomena.

The remainder of the paper is organized as follows. Section 2 briefly describes the Cultural Revolution and the send-down movement in the 1960s and 1970s in China. Section 3 sets up the theoretical model that guides our empirical tests. Section 4 describes the data and variables. Section 5 presents estimates of the returns to and determinants of rustication, and Section 6 reports the transfer function estimates identifying altruism, favoritism and guilt. Section 7 concludes, summarizing the

[7]The positive return to rustication is a result of specific historical environment, during which urban schools were shut down and thus the return for remaining in the city was low. Indeed, as shown by Meng and Gregory (2002) and Giles, Park and Zhang (2007), education and earnings are lower for all cohorts affected by the Cultural Revolution.

findings and presenting additional evidence that those children not sent away by parents felt survivor guilt.

2. The Cultural Revolution

On August 8, 1966, the Central Committee of the Chinese Communist Party chaired by Mao Zedong issued a formal document, which marks the start of the 10-year long "great proletarian Cultural Revolution" in China. Two aspects of the Cultural Revolution are particularly important for our study: the "send down" of urban youth to the countryside and the shutdown of schools.

2.1. The Cultural Revolution, the Red Guards and the Send Down Movement.

During the 1950s and 1970s, the Chinese government sent millions of youths to the countryside. Although small scale send-down movements started in the 1950s, the large scale send-down movement started in 1967, and was made official in December 1968, when Chairman Mao stated in a speech that "it is very necessary for the urban educated youth to go to the countryside to be re-educated by the poor farmers!" (Zhang *et al*. (2007)).

The large scale rustication program followed the initiation of the "Cultural revolution" in 1966. In the initial stages urban youngsters, who were called the "Red Guards," were mobilized. The Red Guard organization was formed by teenagers, most of whom were junior or senior high school students. The main functions of the Red Guards, as documented by Bridgham (1967) and Heaslet (1972), were to harass those persons in authority opposed to Mao's polices or "intellectuals" with capitalist leanings, and to transform education and culture to conform to socialism. The Red Guards attacked, tortured and even executed those Party cadres and intellectuals who refused to follow Mao's revolutionary order but then began to fight among themselves

and rob factories, shops and schools. Essentially, these mobilized teenagers turned into uncontrolled mobs.

In 1967, Mao then initiated the large scale rustication program that would simultaneously discharge the Red guards, reduce unemployment in urban areas (mainly high among the young) and increase agricultural productivity: sending urban youth to the countryside. Some youth were inspired by the revolutionary and patriotic propaganda associated with the send down movement, and went down to the countryside voluntarily. However, most did not want to be separated from their parents or give up the better work opportunities and life in urban areas. Thus, coercion was used, in particular the threat of job loss for parents.

The send-down movement is probably the largest urban-to-rural migration in the history of the world. The youth to be sent down were junior high school and high school graduates. Because junior high school was universalized in cities during the Cultural Revolution, the "educated youth" essentially included all urban youth in the affected birth cohorts. The oldest cohorts affected by this large scale send-down movement was those who graduated from senior high school in 1966, or those born in 1946.[8] Because senior high schools ceased admitting new students in 1966, sent-down youth after 1969 were mostly junior high school graduates. From 1967 until the send-down movement was ended in 1978, 17 million urban youth (from birth cohorts 1946-1961) or one tenth of the urban population, were sent down to rural areas.

Not all youth in the affected cohorts were required to go. There is a substantial variation in the proportion of sent-down youth in the urban population of affected cohorts both over time and across localities. For example, as documented by Bernstein (1977), in Wuhan City in Hubei Province all age-eligible youth were

[8] During those years, children initiated schooling at the age of 8. Completion of primary schooling took 6 years; junior and senior high school completion required 3 years each.

rusticated in 1974. However, in most cities during most years over the whole period, the proportion of eligible youth sent down was much lower. Each city was given a quota of sent-down youth which differed each year, and send-down policies were adjusted according to the quota. When the quota was binding (less than 100 percent of the high school graduates were needed to migrate) a selection rule was needed. The selection rule varied by locality and time. As documented by Bernstein (1977) and Zhou and Hou (1999), some localities required at least one child from a family to go down, while other localities allowed each family to keep one child in the city. Such rules thus required that families choose from among their age-eligible children who would go down. We will attempt to identify the within-family rules used by parents in this exogenously-induced choice.

It is important to note that selection *across* households was also not random. Among the priority households subject to rustication (Bernstein, 1977) were those headed by intellectuals, businessmen, landlords, rich peasants, and those with relatives in Taiwan or the United States. Moreover, children of cadres or well-connected families were more able to find a way to escape from being sent down or to be able to return to the cities earlier based on priority job needs, determined by government or party officials.

Zhou and Hou (1999) describe the typical send-down experiences. Most sent-down youth were forced to do hard manual work in the field for as long as 12 hours a day and 7 days a week. Some were sent to the poor distant parts of the country, and were allowed to visit their urban homes only every three years. Many sent-down youth in their later years, however, note the positive aspects of the experience - the hard manual work and harsh environment they claim made them stronger in both body

and mind, and helped them to develop important interpersonal skills to deal with difficult people and situations (Chen and Cheng, 1999; Yang, 2003).

Sent-down youth returned to cities during, and especially near the end of, the Cultural Revolution, but only on a small scale. Official reasons for returning included going to college, obtaining an urban job, and looking after parents. Again, the literature suggests that children from well-connected families were more likely to come back this way, which was called the "back door". In 1978, two years after Mao died and the Cultural Revolution ended, large-scale protests and strikes of sent-down youth and their urban relatives began to emerge. Finally, in October 1978, the Party issued a document to stop sending young people to the countryside and to start arranging the sent-down people to return to cities. About five percent of sent-down youth, or less than a million in number,[9] never returned to the urban areas, because they were married to local farmers or were assigned non-agricultural local jobs.

2.2. Education during the Cultural Revolution.[10] At the onset of the Cultural Revolution, all primary schools in urban China were closed for 2 to 3 years, and secondary and tertiary level institutions were closed for much of the period. No teaching was carried out and no new students were admitted. Some primary and junior high schools reopened in 1968-69, so those who would have completed primary school in 1966-68 were able to go on to high school and children aged 7-9 began primary school. However, teachers were not allowed to follow the standard curriculum, and instead students were asked to study Mao's thought and learn farming and manual labor from peasants and workers. Those of normal graduation age for junior high school or senior high school were given diplomas even though they had missed out on a traditional junior high or senior high education. Senior high schools

[9] See Liu et al. (1995) and Zhou and Hou (1999).
[10] See Deng and Treiman (1997) and Zhang *et al*. (2007) for more details.

stopped admitting new students during 1966-1972, and when it reopened in 1972, its curriculum focused on factory and farm work.

Universities were closed from 1966 to 1970-71, although those who had entered university before the Cultural Revolution and had not completed their degrees were allowed to stay there without formal teaching until 1970-71. They were then given a university degree and assigned jobs. After 1970-71, universities began to admit students, with new admission criteria based on the political attitudes or family background of the students, which meant manual workers, peasants, soldiers, Party cadres, or students whose parents were from these groups. Admission was not based on academic merit, and no senior high school graduates were allowed to go to college directly. As in the lower-level schools, students in universities did not receive the education provided by a normal curriculum; instead political study was given emphasis.

2.3. Summary. First, the send-down movement was mostly compulsory, and was an unanticipated shock to most people. Second, the send-down movement affected the birth cohorts 1946-1961. Third, different cohorts stayed in the countryside for different years. The earliest sent-down cohorts stayed in the countryside more than 10 years (1967-1978), but the latest cohorts stayed for only a few months. Fourth, there is also large, non-random between-family variation in send-down years. The likelihood of send-down and the duration of stay vary by location and family background. Fifth, there is within-family and within-twin variation in send-down. In many cases, parents were permitted to send only one child (twin) down, and even if all children (twins) were sent down, the children stayed in the countryside for different durations. Sixth, although those youth who remained in cities during this period may have been able to

continue their education, the quality of education was low during the period compared with pre-revolution times.

3. Theory

The rustication movement exogenously altered the amount of time parents spent with their children. We set up a simple model of parental behavior that incorporates altruism, favoritism and guilt to assess how observations on the earnings of children and parental transfers to them resulting from exogenous or experimental variation in parental time spent with children may identify each of these motivations for intra-family resource allocations. We begin with a model in which there is one parent and one child, and then expand the model to two children to show that sibling data are necessary for identifying guilt.

3.1. The One-child Model

3.1.1. Model Setup

Consider the following utility function for a parent with one child

$$\max_{\{c,r,t\}} \quad U(c) + \delta V(W) + \alpha(r,t:e). \tag{1}$$

(1) has three components. First, the parent derives utility $U(c)$ from her own consumption c, where $\partial U / \partial c \equiv U_c > 0$ and $\partial^2 U / \partial c^2 \equiv U_{cc} < 0$. Second, the parent also cares about the child's utility $V(W)$, where W is the child's income, and we assume that $V_w > 0$ and $V_{ww} < 0$. δ is a weighting parameter, denoting altruism. Third, the parent also derives utility α by spending time r with the child, i.e., $\alpha_r \equiv \partial\alpha / \partial r > 0$, and by giving a monetary transfer t to the child, i.e., $\alpha_t \equiv \partial\alpha / \partial t > 0$. We also assume that $\alpha_{rr} < 0$ and $\alpha_{tt} < 0$. Moreover, the marginal utility the parent derives from *r* and *t*, i.e., α_r and α_t, depends on an environmental variable e, the child's ability or endowment.

The goal of our empirical tests is to identify altruism, favoritism and guilt. We say that the parent is altruistic if $\delta > 0$, which is in contrast to the case of $\delta = 0$, when the parent does not care about the child's utility. We call it favoritism if $\alpha_{re} > 0$ and/or $\alpha_{te} > 0$, meaning that the parent derives more utility from spending time with or giving transfers to the better-endowed child. Finally, consistent with the psychological literature on interpersonal guilt, we define guilty behavior as $\alpha_{rt} < 0$: the parent derives increased utility from giving more transfers to a child who receives less parental time.[11] That is, the parent feels guilty for spending less time with a child and as a consequence derives more utility from increasing transfers to her. The principal question we now address is whether *the signs* of the objects δ, α_{te}, and α_{rt} can be identified from observed parental behavior.

The optimization problem is subject to both the parent's budget constraint,

$$Y = c + t + \Pr, \tag{2}$$

where Y is parental earnings and P is the cost of time, and the child's income function,

$$W = \beta(e)r + t + \varepsilon, \tag{3}$$

where β, as a function of e, is the return to parental time, and ε is a random shock to the child's income. Note that because child income is directly affected by parental time r, parental time is also an investment good, as in standard models of human capital. In this model r also directly augments the utility of the parent and thus the allocation of parental time reflects both selfish and altruistic motives.

Substituting (2) and (3) into (1), we can rewrite the maximization problem as

[11] In the model set out by Becker (1992), guilt is also defined by the positive cross-partial between transfers and the guilt-inducing behavior. The guilt function in that model characterizes children's behavior.

$$\max_{\{r,t\}} \quad U(Y - t - \Pr) + \delta V(\beta(e)r + t + \varepsilon) + \alpha(r, t : e). \tag{4}$$

The first order conditions with respect to *r* and *t* are

$$-PU_c + \delta\beta V_w + \alpha_r = 0 \tag{5}$$

$$-U_c + \delta V_w + \alpha_t = 0 \tag{6}$$

3.1.2. Comparative Statics

As written, the only exogenous variables in the model are the child endowment e and the income shock ε. Assuming e is observed, the relationship between the child's endowment and the parent's allocation of time to the child even in this simple model does not identify either favoritism, guilt, or altruism. The relevant comparative static relationship is

$$\frac{dr}{de} = \frac{A_1 + A_2 + A_3}{\Delta_2}, \tag{7}$$

where $A_1 = -\alpha_{re}(U_{cc} + \delta V_{ww} + \alpha_{tt}) + \alpha_{te}(PU_{cc} + \delta\beta V_{ww})$, $A_2 = \alpha_{te}\alpha_{rt}$ and $A_3 = \delta r \beta_e V_{cc}[(P - \beta)U_{ww} + \alpha_{rt} - \beta\alpha_{tt}]$.[12] The first term in the numerator, A_1, is the effect of favoritism, and its sign is undetermined. A_2 is the effect of guilt, which has a negative sign if $\alpha_{rt} < 0$, suggesting that guilt makes the parent spend more time with a weaker child. Finally, A_3, which also involves the efficiency term β_e (the effect of the endowment on the return to parental time), is the effect of altruism and also has an ambiguous sign. Thus, as dr/de involves all three effects: altruism, favoritism and guilt, and its sign is ambiguous, we cannot identify any of these effects by observing

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[12] The second order conditions are $P^2U_{cc} + \delta\beta^2 V_{ww} + \alpha_{rr} < 0$, $U_{cc} + \delta V_{ww} + \alpha_{tt} < 0$ and $\Delta_2 \equiv \begin{vmatrix} P^2U_{cc} + \delta\beta^2 V_{ww} + \alpha_{rr} & PU_{cc} + \delta\beta V_{ww} + \alpha_{rt} \\ PU_{cc} + \delta\beta V_{ww} + \alpha_{rt} & U_{cc} + \delta V_{ww} + \alpha_{tt} \end{vmatrix} > 0$, where the first two inequalities follow from the assumptions of $U_{cc} < 0$, $V_{ww} < 0$ and $\alpha_{tt} < 0$ and the last one is assumed. These second-order conditions guarantee a unique optimal solution to (5) and (6).

how the endowment of the child affects the amount of time parents devote to the child dr/de.

Suppose that we could perform an experiment by exogenously varying parental time *r* and then can observe the effects of that variation on the only other choice variable, transfers *t*. We now show that it is still not possible to identify the existence of guilt, although it is possible to identify altruism if there is information on child earnings, that is, on the child's income net of parental transfers *w*, where $w = \beta(e)r + \varepsilon$. Note that fixing (experimentally) parental time *r* and the child's endowment, variation in earnings is due solely to variation in ε.

First, conditioning on *r*, the relationship between the exogenous component of child's earnings *w*, given her endowment, and transfers identifies altruism, as given by

$$\frac{dt}{d\varepsilon} = \frac{\delta V_{ww}}{-(U_{cc} + \delta V_{ww} + \alpha_{tt})}. \tag{8}$$

As can be seen, the sign of $dt/d\varepsilon$ is the opposite of the sign of the altruism parameters δ, and is zero if there is no altruism. Thus, we can identify altruism by estimating dt/dw controlling (experimentally) for parental time allocation *r* and for the child endowment *e*: an altruistic parent ($\delta > 0$) will transfer less to the child when the child's earnings increase, given its time allocation to the child.

However, the sign of the effect of exogenous variation in parental time *r* allocated to the child on transfers *t* does not only reflect guilt. In particular,

$$\frac{dt}{dr} = \frac{PU_{cc} + \delta\beta V_{ww} + \alpha_{rt}}{-(U_{cc} + \delta V_{ww} + \alpha_{tt})}. \tag{9}$$

As $PU_{cc} < 0$, finding that parents who spend less time with a child (because, say, of the send down) remit to her more transfers ($dt/dr < 0$) cannot determine the sign of α_{rt}, and thus we cannot identify guilt by simply estimating dt/dr, even if *r* can be

varied exogenously, from families with one child or with information on only one child. Even if there is no guilt, a parent forced to spend less time with the child will allocate more money to the child simply because of altruism.

3.2. The Two-child Model

3.2.1. Model Setup

With two children, the parent's problem becomes

$$\max_{\{c,r,t\}} \quad U(c) + \sum_i \delta V(W^i) + \sum_i \alpha(r^i, t^i : e^i), \tag{10}$$

$$\text{s.t.:} \quad Y = c + \sum_i t^i + P\sum_i r^i, \tag{11}$$

$$W^i = \beta(e^i) r^i + t^i + \varepsilon^i, \tag{12}$$

where the superscript i=1,2 represents child i. The four first order conditions are

$$-PU_c^i + \delta\beta V_w^i + \alpha_r^i = 0 \qquad \text{for } i = 1,2 \tag{13}$$

$$-U_c^i + \delta V_w^i + \alpha_t^i = 0 \qquad \text{for } i = 1,2 \tag{14}$$

Assuming the second order conditions hold, there will be a unique optimal solution to these four first order conditions.

3.2.2. Comparative Statics

For the two-child model, it is straightforward to show that, just as in the one-child model, the relationship between child-specific parental time and child-specific endowments dr^i / de^i, or even the difference between the children in parental time allocations by endowment $dr^1 / de^1 - dr^2 / de^1$, involve all three motives (altruism, favoritism and guilt) and their signs are ambiguous.[13] Thus, we cannot identify any one motive by estimating dr^i / de^i or the difference $dr^1 / de^1 - dr^2 / de^1$. However, in the two-child case it is possible to separately identify altruism, favoritism and guilt if

[13] The proof can be obtained from authors on request.

variation in parental time r^i ($i=1,2$) can be controlled experimentally and, as assumed above, endowments can be measured or also controlled for.

First, if there is altruism, an increase in the (exogenous component of) earnings of child one on transfers to her will be negative and those to her sibling will be positive:

$$\frac{dt^1}{d\varepsilon^1} = \frac{-\delta V_{ww}(U_{cc} + \delta V_{ww} + \alpha_{tt})}{(2U_{cc} + \delta V_{ww} + \alpha_{tt})(\delta V_{ww} + \alpha_{tt})}, \tag{15}$$

and

$$\frac{dt^2}{d\varepsilon^1} = \frac{\delta V_{ww} U_{cc}}{(2U_{cc} + \delta V_{ww} + \alpha_{tt})(\delta V_{ww} + \alpha_{tt})}. \tag{16}$$

The sign of $dt^1 / d\varepsilon^1$ will be the same as $-\delta$, and thus $dt^1 / d\varepsilon^1 < 0$ would identify $\delta > 0$, or altruism. Similarly, the cross effect of sibling one's earnings on transfers to sibling two $dt^2 / d\varepsilon^1 > 0$ will also identify altruism. Moreover, the difference between the own and cross-transfers effects from exogenous variation in the earnings of child one in (15) and (16) is

$$\frac{dt^1}{d\varepsilon^1} - \frac{dt^2}{d\varepsilon^1} = \frac{-\delta V_{ww}}{(\delta V_{ww} + \alpha_{tt})}, \tag{17}$$

which again signs the altruism parameter.

Conditioning on the allocation of parental time, the relationships between the endowments of the children and transfers to them, unlike in the one-child case, can identify whether parents favor lower- or higher-endowment children. In particular, the own endowment effect on transfers is

$$\frac{dt^1}{de^1} = \frac{-(\delta\beta_e V_{ww} + \alpha_{te})(U_{cc} + \delta V_{ww} + \alpha_{tt})}{(2U_{cc} + \delta V_{ww} + \alpha_{tt})(\delta V_{ww} + \alpha_{tt})}, \tag{18}$$

And the cross effect is

$$\frac{dt^2}{de^1} = \frac{(\delta\beta_e V_{ww} + \alpha_{te})U_{cc}}{(2U_{cc} + \delta V_{ww} + \alpha_{tt})(\delta V_{ww} + \alpha_{tt})}. \tag{19}$$

The difference between the two eliminates the role of parental preferences for their own consumption and is

$$\frac{dt^1}{de^1} - \frac{dt^2}{de^1} = \frac{-\delta\beta_e V_{ww} - \alpha_{te}}{\delta V_{ww} + \alpha_{tt}}. \tag{20}$$

Parents will allocate transfers differently to children with different endowments for two reasons – because endowments may affect the return (in terms of child earnings) to parental time ($\beta_e \neq 0$) and parents are altruistic, and because the utility from providing transfers differs depending on the child endowment ($\alpha_{te} \neq 0$). That is, gifts to children will vary with their endowments because parents value them differently or there are differential returns to endowments.

Because the sign of the difference between the own and the cross endowment effects on transfers will be the same as the sign of the sum $\delta\beta_e V_{ww} + \alpha_{te}$ it is necessary to know how endowments affect the returns to parental time, the sign of β_e, in order to identify favoritism arising purely from preferences, given altruism. Most estimates in the literature imply that there is positive "ability bias" ($\beta_e > 0$). In that case, only if we find that higher-endowed children receive smaller transfers relative to lower-endowed children could we infer that parents *prefer* lower-endowed children. When $\beta_e > 0$, the finding that (20) is positive would imply that parents would choose to provide more financial assistance to more endowed children, and are either not biased strongly against remitting to higher-endowed children or that they favor higher-endowed children $\alpha_{te} > 0$.

The experiment of exogenously varying the time spent by parents with one child (Sophie's choice) and observing how that affects the *difference* in the transfers

across the two children, however, permits the identification of guilt (α_{rt}<0) with information only on the sign of β, the return to parental time. In particular, the effects of increasing parental time spent with child one on the transfers it receives is

$$\frac{dt^1}{dr^1} = \frac{-PU_{cc}(\delta V_{ww} + \alpha_{tt}) - (U_{cc} + \delta V_{ww} + \alpha_{tt})(\delta\beta V_{ww} + \alpha_{rt})}{(2U_{cc} + \delta V_{ww} + \alpha_{tt})(\delta V_{ww} + \alpha_{tt})}, \tag{21}$$

and the corresponding cross effect on the second child is

$$\frac{dt^2}{dr^1} = \frac{-PU_{cc}(\delta V_{ww} + \alpha_{tt}) + U_{cc}(\delta\beta V_{ww} + \alpha_{rt})}{(2U_{cc} + \delta V_{ww} + \alpha_{tt})(\delta V_{ww} + \alpha_{tt})}. \tag{22}$$

Because $-PU_{cc}(\delta V_{ww} + \alpha_{tt}) < 0$, we cannot identify the sign of α_{rt} even if $dt^1 / dr^1 < 0$. However, we can identify the sign of α_{rt} from the difference between the own and cross-effects of (21) and (22), which is

$$\frac{dt^1}{dr^1} - \frac{dt^2}{dr^1} = \frac{\delta\beta V_{ww} + \alpha_{rt}}{-(\delta V_{ww} + \alpha_{tt})}. \tag{23}$$

The sign of $dt^1 / dr^1 - dt^2 / dr^1$ will be the same as that of $\delta\beta V_{ww} + \alpha_{rt}$. Under the condition that $\beta < 0$, finding $dt^1 / dr^1 - dt^2 / dr^1 < 0$ implies $\alpha_{rt} < 0$, and thus we can identify guilt.[14]

4. Data

The data that we use are derived from the Chinese Twins Survey, which was carried out by the Urban Survey Unit (USU) of the National Bureau of Statistics (NBS) in June and July 2002 in five cities of China. The survey was funded by the Research Grants Council of Hong Kong. Based on existing twins questionnaires in the United States and elsewhere, the survey covered a wide range of socioeconomic information. The questionnaire was designed by Mark Rosenzweig and Junsen Zhang

[14] Empirically, we indeed find that $\beta < 0$, or there is a negative return to parental time with a child (or a positive return to being sent down to the countryside).

in close consultation with Chinese experts from the NBS. Adult same-sex twins aged between 18 and 65 were identified by the local Statistical Bureaus through various channels, including colleagues, friends, relatives, newspaper advertising, neighborhood notices, neighborhood management committees, and household records from the local public security bureau. Overall, these channels permitted a roughly equal probability of contacting all of the twins in these cities, and thus the twins sample that was obtained is approximately representative.[15] Questionnaires were completed through household face-to-face personal interviews.

This is the first socioeconomic twins dataset in China, and perhaps the first in Asia. The dataset includes rich information on the socioeconomic situation of respondents in the five cities of Chengdu, Chongqing, Harbin, Hefei, and Wuhan. Altogether there are 4,683 observations, of which 2,990 observations are from twins households. For the sample of twins, care was taken to distinguish whether twins are identical (monozygotic or MZ) or non-identical (fraternal of DZ) twins, based on standard questions used in prior twins surveys. We consider a pair of twins to be identical if *both* twins respond that they have identical hair color, looks, and gender. Completed questionnaires were collected from 919 pairs of MZ twins (1838 individuals) and 576 pairs of DZ twins (1152 individuals). However, for each variable, there may be a slightly different number of observations due to missing values. The summary statistics of MZ and DZ twins are reported in Table 1. For each variable reported, we restrict the sample such that it is non-missing for both twins in a pair.

Column 1 shows that 56 percent of the identical twins are male, and on average the twins were 37 years old and had 11 years of schooling. For the whole MZ twins sample, the twins had been sent down (away from parents) for an average of 0.7

[15] Our inferences are based on estimates obtained from within-twin pair differences, so that the influence of first-order effects of any unobserved characteristics that may have led to the selection of twin pairs into the sample is eliminated.

years. For those who fall in the affected cohorts (born during 1946-1961 or aged 41-56 in 2002), however, about half (51 percent) were sent down for rustication and they on average stayed in the countryside for 1.7 years. The MZ twins in our sample had monthly average earnings of 888 *yuan* in 2002, where earnings include wages, bonuses, and subsidies.

Because we rely on estimates based on variation within pairs of twins in the amount of time spent away from parents due to forced rustication for identification of key parameters, an important feature of the data is the extent of within-twin pair variation in send-down time. In total, 362 pairs of MZ twins and 157 pairs of DZ twins are in the affected cohorts (age 41-56 in 2002). Table 2 shows that for 34 percent of the affected MZ twin pairs, neither twin was sent down; for 29 percent of the twin pairs, one of them was sent down; and for the remaining 37 percent of the twin pairs, both were sent down. The within-twin variation in send-down years is even larger. In almost half (48 percent) of the MZ twin pairs, the twins spent a different number of years in the countryside: 23 percent had 1-2 years' difference in send-down years, about 21 percent had 3-5 years' difference, and the remaining 4 percent had a difference of more than 5 years. The within-twin pair differences for DZ twins are also large, and have a similar distribution.

5. Estimation Results: Economic Returns to Rustication

We first estimate the returns to rustication β, which as we have shown is critical for identification of guilt. We first estimate the following equation, which corresponds to the children's earnings in the model:

$$w^{ij} = X^{j}\alpha + Z^{ij}\beta + \mu^{j} + e^{ij} + \varepsilon^{ij}, \qquad (24)$$

where the superscript j refers to family j and i refers to individual i, w^{ij} is the logarithm of earnings for child (twin) i in family j, X^{j} is a set of observed family

variables, and Z^{ij} is a set of observed child-specific variables that affect earnings. Included in Z^{ij} are the number of send-down years, which corresponds to $-r$ in the model. We also include in (24) the child's education, age, and gender, as well as city dummies. μ^j is a family effect, and e^{ij} represents the child-specific endowment. ε^{ij} is the disturbance term, which is assumed to be independent of the Z^{ij} and μ^j.

5.1. Empirical Methodology

5.1.1. Identifying β Using MZ Twins

The OLS estimate of the effect of send-down years in equation (24), β_{OLS}, is likely to be biased. This bias arises because we normally do not have perfect measures of μ^j and e^{ij}, which are likely to be correlated with the Z^{ij}. As discussed in section 2, those who were sent-down for a longer period are likely to come from disadvantageous families, and if the family background effect is not completely accounted for, then the OLS estimation will pick up the negative effect of disadvantageous family background. It is therefore difficult to ascertain how much of the empirical association between earnings and send-down years is due to the causal effect of the rustication, and how much is due to unobserved family background and individual endowment that influences both earnings and send-down years. Moreover, as incorporated in the model, within the family the parents' choice of who to send down may reflect bias, and thus may be related to unobserved child-specific endowments.

We remove the influence of both the family effect and child endowment effect by applying the fixed effects estimator to the monozygotic (MZ) twins sample. As MZ twins are genetically identical, there will be no within-family individual variation in the endowment term ($e^{1j} = e^{2j}$). And because they have a similar family

background, the twin pair should have the same μ^j. Thus, taking the within-twin difference will also eliminate the unobservable family effect μ^j. To identify endowment effects, we also apply the same estimation procedure to the sample of DZ twins, which does not eliminate the influence of the child-specific endowments. We show below that the comparison of OLS, within-MZ and within-DZ estimates can be used to identify the effects of endowments and thus allocation rules and motives.

5.1.2. Selection of Families for Send-down: β_{OLS} **versus** β_{MZ} . The fixed effects model can be specified as follows. The earnings equations for a pair of MZ twins are given as

$$w^{1j} = X^j\alpha + Z^{1j}\beta + \mu^j + e^{1j} + \varepsilon^{1j} \quad (25)$$

$$w^{2j} = X^j\alpha + Z^{2j}\beta + \mu^j + e^{2j} + \varepsilon^{2j}, \quad (26)$$

where the superscript 1 and 2 refer to twin 1 and 2 in a pair. A within-twin or fixed effects estimator of β for MZ twins β_{MZ} is based on the first-difference of equations (25) and (26):

$$w^{1j} - w^{2j} = (Z^{1j} - Z^{2j})\beta + \varepsilon^{1j} - \varepsilon^{2j}. \quad (27)$$

OLS estimates of equation (27) provide an unbiased estimate of the effect of the rustication. Comparisons of the estimates obtained from (25) or (26) with the within-estimates obtained from MZ and DZ twin-pairs also shed light on how endowments affect send-down selection across households and across children within households, the parental selection rule (favoritism).

Comparing β_{OLS} with β_{MZ} provides the direction of the bias caused by the non-random selection of households combined with the family selection rule for send-down. In particular, it can be easily shown that the sign of β_{OLS} - β_{MZ} will be the sign of $\text{cov}(Z^{ij}, \mu^j + e^{ij})$. More specifically, if $\beta_{OLS} > \beta_{MZ}$ ($\beta_{OLS} < \beta_{MZ}$), then the

unobserved family background and endowment ($\mu^j + e^{ij}$) are positively (negatively) correlated with send-down years (r^{ij}). In other words, children of better (poorer) family background or/and endowment were sent down for a longer period.

5.1.3. Within-Family Selection for Send-down: β_{MZ} **versus** β_{DZ}. We can also obtain the within-family selection rule for send-down, and identify favoritism, by comparing the fixed effects estimate of β using DZ twins β_{DZ} with the fixed effect estimate using MZ twins β_{MZ}. Because $e^{1j} \neq e^{2j}$ for DZ twins, the fixed effect estimator cannot remove the endowment effects and thus is biased. Algebraically, the first difference becomes

$$w^{1j} - w^{2j} = (Z^{1j} - Z^{2j})\beta + e^{1j} - e^{2j} + \varepsilon^{1j} - \varepsilon^{2j}. \quad (28)$$

Because we do not observe the endowments $e^{1j} - e^{2j}$, if corr($Z^{1j} - Z^{2j}, e^{1j} - e^{2j}$) $\neq 0$, then β_{DZ} will be biased.

As shown in Behrman, Rosenzweig and Taubman (1994) with respect to the allocation of any resource allocated to children that is based on endowments, the comparison of β_{MZ} with β_{DZ} identifies the allocation rule if the variance of the difference in the child input for MZ twins is less than the variance of that difference for DZ twins. In our case, the input is the number of years spent in the countryside, and the variance condition is met. Thus if we find that $\beta_{DZ} > \beta_{MZ}$, then the cross-child difference in the unobserved endowment ($e^{1j} - e^{2j}$) is positively correlated with the cross-child difference in send-down years ($r^{1j} - r^{2j}$): the better-endowed child in a family was sent down for a longer period. On the other hand, if $\beta_{DZ} < \beta_{MZ}$, then it means that parents favored the stronger child, preferred having the higher-endowed child spend more time with them in the household.

5.2. Empirical Results

5.2.1. Estimates of the consequences of rustication for earnings using MZ Twins

In the first three columns of Table 3, we report the estimates from OLS earnings regressions using the MZ twins sample. The dependent variable is the logarithm of monthly earnings. The standard errors are robust to heteroscedasticity and clustering at the family level. We have 447 pairs of MZ twins or 994 observations, which have complete information for both twins.

Column 1 shows a simple regression with send-down years, age, gender and city dummies as independent variables. This simple regression shows that the overall effect of send-down years is essentially zero - the coefficient on the send-down years is very small, and it is not significantly different from zero. Age is not correlated with earnings. Men have 18.6 percent higher earnings than women.

In column 2, we add education as a covariate. Controlling for education, the effect of send-down years is more than tripled, and it is significantly different from zero at the 10 percent level. This suggests two things. First, send-down years are generally negatively correlated with education, and the lack of effect of send-down years in column 1 is in fact due to the negative correlation between send-down years and education. Second, once education is controlled for, send-down years become positively correlated with earnings. As expected, education itself has a positive effect on earnings. An additional year of education increases earnings by as large as 8.5 percent, which is comparable to the OLS estimate of the returns to education in previous studies that draw on Chinese data (Zhang *et al*., 2005).

In column 3, we report estimates from a specification that controls for another important measure of human capital, work experience. Our survey instrument allows us to compute the total years of actual formal work for an individual. To allow for the

non-linear effect of work experience, we include both experience and experience squared in the model. Note first that experience has a concave effect on earnings, with earnings increasing with the first 14 years of experience at a decreasing rate and then decreasing with it. Moreover, formal work experience is negatively associated with send-down years, as the effect of send-down years on earnings increases to 2.5 percent after controlling for the experience variables.

The family fixed effects estimates using MZ twins are reported in columns 4-6 of Table 3. They consistently show that send-down years have a large positive effect on earnings. Moreover, the estimates do not change much when we control for education and experience. The point estimates suggest that one year spent "down" in the countryside increases earnings by 3.4 percent. This is larger than the return to schooling, corrected for endowment effects, of 2.7 percent (column 5). To interpret the high return to rustication it is important to note the historical counterfactual at this time – staying in the city, where there was high unemployment and where schools were closed down or providing a low-quality curriculum. The significant positive return to rusticating youth cannot thus be readily generalized to other contexts, such as China today or any other country at any time. It is of relevance, as we have shown, for understanding the fundamentals of family behavior.

The fact that the within-MZ pair send-down point estimates are consistently larger than the OLS estimates implies that there was a negative selection effect across families: children from unfavorable backgrounds stayed in the countryside for a longer time. For example, for the most complete specification (column 6 versus column 3), the within twin pair estimate of the return to rustication is 68% larger than the comparable OLS estimate (0.041 versus 0.025).

In the last three columns of Table 3, we report fixed effect estimates of the same model specifications using the DZ twins sample. We have 322 pairs of DZ twins or 644 observations for which have complete information for both twins. The results using the DZ twins sample are also different from those using the MZ twins sample. The coefficient on the send-down variable is very small and statistically insignificant for all three specifications. There is thus a downward bias in the DZ estimates ($corr(e_{1j} - e_{2j}, D_{1j} - D_{2j}) < 0$), which implies that within the family the weaker child was sent down for a longer time. The rustication "program' was thus negatively selective both across and within families.

The results regarding other variables for the DZ twins sample are as expected. Education has a positive and significant coefficient, and the estimates are larger than those using the MZ twins sample. These results thus suggest that there was positive within-family selection with respect to both schooling and parental time: the stronger sibling in a family received more education and stayed home longer with parents during the send-down period. Parents clearly exhibited favoritism towards the more able child.

5.2.2. Other Outcome Variables

In this section, we briefly examine the impact of rustication on three additional outcome variables: employment status, party membership, and health. Employment status is a dummy variable that equals 1 if one is working and 0 if not.[16] The party membership dummy, which equals 1 if one is a member of the Communist Party and zero otherwise, indicates one's political status. Being a Party member not only is an important political achievement in China, but also involves economic gains from

[16] If a person is not working, she could be unemployed or not in the labor force. We include those who are not in the labor force, such as retirees and housewives, because they could be discouraged workers. See Giles, Park and Cai (2005) for how unemployment, retirement and other terms are defined in urban China. Using a smaller sample of those in the labor force generates similar results.

obtaining a well-paying job in the government, possibly inclusive of bribes and side payments.[17] The third variable is a health condition dummy, which equals 1 if the respondent reports that she has none of the diseases listed in our questionnaire and 0 if she has at least one of them. In the sample, 64 percent of the respondents are "healthy" while the rest have at least one disease.

The OLS regressions reported in the top panel of Table 4 show that the associations between the number of years of send-down and these three outcome measures are mixed. Send-down has a positive relationship with employment and party membership but is associated with lower health, though the latter is not significant. The FE estimates using MZ twins (middle panel) suggest that send-down years have a positive effect on all three outcome variables, although the health effect is not statistically significant.

Consistent with the findings on earnings, the difference between the within-MZ twin pair and OLS results suggests a negative selection effect across families. For example, the FE estimates of the effect of the send-down years on employment and the Party membership are about one third larger than the OLS estimates. While the OLS estimates suggest that send-down years are negatively correlated with health the FE estimates show that send-down years have no effect on health. Finally, and also consistent with the findings for earnings, the difference between the FE estimates from the MZ sample (middle panel of Table 4) and those from the DZ sample (bottom panel) suggests a negative within-family selection rule – parental favoritism towards the better-endowed child.

6. Parental Transfers: Altruism, Favoritism and Guilt

[17] See Li *et al*. (2007) for a recent study of the economic returns to Communist Party membership.

In this section, we obtain estimates of the determinants of parental transfers to the twins to identify altruism, favoritism and guilt. We use the log of parental wedding gifts as our measure of transfers. In China, one major means by which parents provide children with financial resources is with wedding gifts. As almost all children marry, we have many observations on these transfers. As shown in Table 1, over 75 percent of the twins received a gift or transfer from parents at the time of their marriage. The average amount of the gifts at marriage is almost 1.5 times average earnings, and three-quarters of the twins pairs report receiving different amounts from their parents.[18] To examine the determinants of wedding gifts and transfers, we restrict the sample to those pairs of twins in which both twins ever married. A feature of the survey that facilitates this analysis is that we also have information on the twin's earnings, age and schooling at the time of marriage, the appropriate state variables for decisions about transfers at the time of marriage.

Taking a linear approximation to the parental transfer decision rule, conditional on parental time spent with the child, as indicated by send down years, the transfer equations for the two twins can be written as

$$T^{1j} = aw^{1j} + bD^{1j} + Z^{1j}c + X^{j}d + \eta^{j} + fe^{1j} + \xi^{1j} \tag{29}$$

$$T^{2j} = aw^{2j} + bD^{2j} + Z^{2j}c + X^{j}d + \eta^{j} + fe^{2j} + \xi^{2j}, \tag{30}$$

where T^{ij} is transfer of parent *j* to child *i (i=1, 2)*, D^{ij} represent send-down years ($D^{ij} = -r^{ij}$, where r^{ij} is defined as the parental time with a child as in the model), w^{ij} again represents child earnings (in logs), Z^{ji} are observable individual-specific child variables such as age and schooling at the time of marriage, X^{i} are observable family (parent) characteristics, η^{j} is an unobserved family effect, the e^{ji} are

[18]We also have information on contemporaneous (survey year) financial transfers from parents to children, but these, as in the United States, are very sparse and one-tenth the size of the transfers at the time of marriage.

the unobserved child endowments, and the ξ^{ji} are random errors. The parameters, a, b, c, d and f are parameters to be estimated.

Differencing (29) and (30) for the sub-sample of MZ twins, we get:

$$T^{1j} - T^{2j} = a(w^{1j} - w^{2j}) + b(D^{1j} - D^{2j}) + (Z^{1j} - Z^{2j})c + (\xi^{1j} - \xi^{2j}), \qquad (31)$$

which will provide a consistent estimates of a and *b,* which in turn identify altruism and guilt, respectively. Note that $a = dt_1 / d\varepsilon_1 - dt_2 / d\varepsilon_1$ in the model (expression (17)) because, given that the child endowments are differenced out and we are conditioning on parental time (send down years) and schooling attainment differences, the only source of differential wage variation is from the random terms ε in the earnings function. The coefficient b, given that β < 0, identifies guilt, as seen in expression (23) in the model. A negative estimated a would suggest the existence of altruism; a positive b is consistent with guilt. That is, parents providing more resources at marriage to the twin with lower earnings at marriage is consistent with altruism; net of earnings and human capital differences, providing more resources to the child with more years away from parents would imply parental guilt.

Regression results reported in Table 5 are indeed consistent with the operation of guilt. The OLS estimates using the MZ twins sample in columns 1-2 suggest that years sent down have a negative effect on transfers, though the effect is not significant. However, once we remove the effects of the unobserved family effect and child-specific endowments, the coefficient on sent-down years becomes positive and statistically significant (columns 3 and 4). Given that $dt^1 / dr^1 < 0$ and our finding that the return to send-down years is positive ($\beta < 0$), this means that $\alpha_{rt} < 0$, or parents indeed pay for guilt.

The magnitude of the guilt-related send-down effect on the parental provision of wedding gifts is also large. The point estimate implies that each additional year in

the countryside raises the parental gift by about 12 percent - if one twin stayed in the countryside for 8 years and the other twin did not go, then the send-down twin's wedding gift is twice as large as that for the one who stayed home. This compensation is more striking, considering that the sent-down experience itself also had a large positive return on earnings. Indeed, guilt appears to be a stronger parental motivation than altruism: although the coefficient on children's earnings on parental transfers is negative, consistent with altruism, it is not statistically significant.[19]

Finally, our finding that children with lower endowments were sent away for longer periods than their better-endowed siblings is consistent with parents having a preference for spending more time with better-endowed children, but also may reflect their perceptions at the time that rustication would reduce human capital so that the bias may merely reflect the higher expected human capital returns to endowments (and altruism). By looking at the relationship between endowments and *ex post* transfers, net of sent-down years and earnings, however, we can directly identify whether parents favor *per se* the more endowed child. We again compare estimates from the MZ and DZ twins sub-samples. If $b_{MZ} > b_{DZ}$ in the transfer equations, then it must be that $corr(f(e_{1j} - e_{2j}), D_{1j} - D_{2j}) < 0$ for DZ twins. As we have shown that $corr(e_{1j} - e_{2j}, D_{1j} - D_{2j}) < 0$ from the estimated earnings equations, this means $f>0$, i.e., that the better-endowed child is favored by parents with more resources. That is, the better-endowed child would receive more transfers from parents, if the two children were sent-down for the same duration.

Comparing the FE estimates using MZ twins to the FE estimates using the DZ twins (columns 3-4 vs. columns 5-6) indeed suggests that the stronger child is the favored one. Although the estimated coefficient for send-down years on the amount of

[19] Cox (1987) in a pioneering study of transfer motives also does not find strong support for altruism.

the parental gift for the MZ twins is large, positive and significant, that for the DZ twins is almost zero and insignificant. That $b_{MZ} > b_{DZ}$ means that the choice of the weaker child to be sent down or sent down longer reflects parental bias towards better-endowed children, and not just investment efficiency *cum* altruism. It is not surprising, therefore, to observe that post-send-down transfers reflect parental guilt.

8. Conclusion

The send-down movement in China was a unique, traumatic experience for many families, requiring in many cases parents to make horrific distinctions among their children. Although we have found that, given the peculiar conditions of the time, among children with similar family background and individual endowments those forcibly sent down to the countryside ended up no less healthy and with higher earnings, greater political connections and a higher likelihood of employment compared with their identical siblings, it is clear that this policy-relevant finding has little relevance for any policies in the current time because it so violates contemporary values of human rights. Such values also bar any experiments that would force agents to make any decisions that would harm others, thus also precluding direct tests of guilt motivations. However, the unique policy experiment and survey design allow us to obtain insights on fundamental aspects of human behavior including the presence of guilt-motivated behavior.

We have collected data documenting the experiences of the victims of forced rustication movement during China's Cultural Revolution, specifically those who were twins, among whom many experienced the Sophie's choice-like decisions of their parents, in order to identify whether and how guilt, as distinct from altruism, affects behavior. The program forced, non-randomly, parents to make decisions that they perceived as more harmful to some than to others within the family. By looking

at the experiences of split twin pairs and their parents, we can thus come close to an experimental design with a guilt-inducing treatment that would not otherwise be possible.

We developed a simple theoretical model incorporating favoritism, altruism and guilt to show the conditions and data required for identifying these distinct behavioral motives when harm can be induced exogenously. The key condition, following the psychological literature, is that guilt is an interpersonal emotion so that it is necessary to distinguish guilt-motivated behavior from altruistic behavior with information on transactions from the agent causing harm to the harmed in comparison with the unharmed who have both a similar personal relationship to the agent and similar state variables.

Besides measuring the causal effect of rustication on earnings and other outcome variables based on the different rustication experience of twins, we found that parents selected children with lower endowments to be sent down and this selection in part was motivated by preferences for more able children as well as possibly efficiency motives (combined with altruism). We also found, based on subsequent transfer behavior at the time of the marriage of the children, that parents behaved altruistically, providing more gifts to the sibling with lower earnings and schooling. But parents also exhibited guilt – given the current state variables of the two children, the child experiencing more years of rustication received significantly higher transfers.

Finally, although Becker (1992) emphasizes the potential guilt felt by children with respect to the sacrifices made by parents in remedying family commitment problems, we have focused on the guilt felt by parents from deliberately neglecting a child. We can test for another form of guilt, among the children subject to the

Sophie's Choice treatment - "survivor" guilt, which has been recorded among surviving victims of impersonal forces of mass death such as the Holocaust, Hiroshima, and AIDS epidemics (Baumeister *et al*., 1994). In particular, we can see if the sibling not selected to be rusticated or with fewer years separated from parents, provides more resources to the sibling more affected, given their current state variables.

Within-twin estimates (not reported) of transfers across twins in the year preceding the survey, for the MZ sub-sample, indicate both altruistic behavior and survivor guilt among the siblings. In particular, differences across the twins in current earnings were statistically significantly and negatively associated with differences in contemporaneous net transfers, for given send-down experience, while differences in years sent down were positively associated with transfers, for given earnings and education. The inter-sib guilt effects on transfers were as large as those exhibited by parents – each year of difference in years away was associated with a statistically significant 15.7 percent increase in transfers to the more victimized sibling.

Guilt, independent of altruism, thus appears to be an important force counteracting adverse experiences among family members, whether caused by the agents themselves or by others. Our finding that guilt is one important motive for intra-family resource transfers thus provides some empirical foundation for theoretical models that use guilt or morality to explain human behavior and suggest that the guilt motive should be considered in the design of contracts where enforcement and commitment issues, such as in the family, are important.

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Table 1: Descriptive Statistics, by Twin pair Type

Variable	MZ twins		DZ twins	
	Mean	Standard deviation	Mean	Standard deviation
Sent-down years for whole sample	0.71	(2.11)	0.45	(1.75)
Proportion sent down for affected cohorts (age 41-56 in 2002)	0.51	(0.50)	0.46	(0.50)
Sent-down years for affected cohorts	1.74	(2.90)	1.67	(3.04)
Age	37.31	(10.22)	34.80	(10.04)
Proportion male	0.56	(0.50)	0.59	(0.49)
Years of education	11.24	(2.96)	11.35	(3.07)
Proportion with Party membership	0.18	(0.38)	0.14	(0.34)
Earnings in 2002 (monthly wage, bonus and subsidies in RMB)	888.50	(517.93)	835.33	(548.30)
Proportion employed	0.70	(0.46)	0.70	(0.46)
Proportion self-assessed as 'Healthy'	0.64	(0.48)	0.68	(0.47)
Proportion of twins with wedding gifts from parents	0.77		0.76	
Wedding gifts (2002 *yuan*)	5,595	(9,696)	6,029	(10,430)
Proportion of twin pairs with wedding gifts different	0.75		0.74	
Within-twin difference in wedding gifts	2,818	(7,778)	3,145	(8,536)
Earnings at the time of wedding (2002 *yuan*)	322	(605)	335	(420)
Number of twins (Pairs)	1,838 (919)		1,152 (576)	

Note: For each of the variables, we restrict the sample to those twin pairs, for which we observe the variable for both twins in a pair.

Table 2: Within-twin Variation in Rustication and Sent-Down Years for Affected Cohorts (Age 41-56 in 2002)

	MZ twins		DZ twins	
Variable	Count	Percent	Count	Percent
Within-twin variation in send-down dummy				
Neither sent down	123	33.98	61	38.85
One sent down	106	29.28	49	31.21
Both sent down	133	36.74	47	29.94
Total pairs	362	100	157	100
Within-twin variation in send-down years				
0 year	187	51.66	83	52.87
1-2 years	85	23.48	44	28.02
3-5 years	77	21.27	22	14.01
6- years	13	3.59	8	5.10
Total pairs	362	100	157	100

Table 3: Estimates of the Effect of Sent-Down Years on Log Wage, by Estimation Method and Twin Pair Type

	OLS (MZ Twins)			Fixed Effects (MZ Twins)			Fixed Effects (DZ Twins)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sent-down years	0.005	0.017*	0.025***	0.032**	0.034***	0.042***	-0.003	-0.006	-0.008
	(0.010)	(0.009)	(0.010)	(0.013)	(0.012)	(0.014)	(0.036)	(0.034)	(0.035)
Age	0.003	0.008***	-0.003						
	(0.003)	(0.002)	(0.006)						
Male	0.186**	0.212***	0.213***						
	(0.044)	(0.038)	(0.038)						
Education		0.085***	0.085***		0.027*	0.030**		0.046***	0.045***
		(0.006)	(0.006)		(0.015)	(0.014)		(0.012)	(0.012)
Experience			0.028***			0.022			-0.017
			(0.009)			(0.021)			(0.026)
Experience squared			-0.001**			-0.000			0.000
			(0.000)			(0.001)			(0.001)
Observations	994	994	994	994	994	994	644	644	644
R-squared	0.04	0.22	0.23	0.01	0.02	0.02	0.00	0.04	0.04

Note: Standard errors in parentheses are robust to heteroscedasticity and clustering at the family level. * significant at 10% ** significant at 5% *** significant at 1%. All OLS regressions control for city dummies.

Table 4: OLS and Fixed Effect Estimates of the Effect of Sent-Down Years on Additional Outcomes, by Twin pair Type

	Dependent variables		
	Employed	Party member	Healthy
MZ Twins: OLS			
Years sent down	0.019***	0.015***	-0.008
	(0.007)	(0.005)	(0.005)
Observations	1836	1796	1838
MZ Twins: FE			
Years sent down	0.026***	0.020***	0.006
	(0.009)	(0.008)	(0.008)
Observations	1836	1796	1838
DZ Twins: FE			
Years sent down	0.005	0.010	-0.021*
	(0.007)	(0.009)	(0.011)
Observations	1150	1132	1152

Note: All regressions include education, experience and experience squared. OLS regressions also control for city dummies. Standard errors in parentheses are robust to heteroscedasticity and clustering at the family level. * significant at 10% ** significant at 5% *** significant at 1%

Table 5: OLS and Fixed Effect Estimates of the Effect of Sent-Down Years on Log Parental Transfers and Gifts at Marriage, by Twin Pair Type

	OLS (MZ Twins)		Fixed Effects (MZ Twins)		Fixed Effects (DZ Twins)	
	(1)	(2)	(3)	(4)	(5)	(6)
Years sent down	-0.028	-0.002	0.117**	0.119**	0.001	0.002
	(0.034)	(0.038)	(0.053)	(0.053)	(0.066)	(0.066)
Age at marriage	-0.215***	-0.239***	-0.234***	-0.236***	-0.099*	-0.095*
	(0.031)	(0.030)	(0.043)	(0.043)	(0.056)	(0.057)
Male	1.196***	1.287***				
	(0.263)	(0.259)				
Education at marriage		0.192***		0.067		0.007
		(0.045)		(0.069)		(0.069)
Log wage at marriage		0.095		-0.012		-0.094
		(0.094)		(0.116)		(0.214)
Co-twin characteristics						
Years sent down	-0.145***	-0.121***				
	(0.035)	(0.036)				
Education at marriage		0.124***				
		(0.047)				
Log wage at marriage		0.106				
		(0.097)				
Observations	1106	1106	1106	1106	608	608
R-squared	0.15	0.19	0.10	0.11	0.02	0.02

Note: All OLS regressions include city dummies. Standard errors in parentheses are robust to heteroscedasticity and clustering at the family level. * significant at 10% ** significant at 5% *** significant at 1%.