

**Altruism, Favoritism, and Guilt in the Allocation of Family Resources:
Sophie's Choice in Mao's Mass Send-Down Movement**

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Abstract

Is it possible to identify behavior motivated by guilt? Although there are empirical findings on inter-household transfers consistent with the hypothesis that people care about others' state of well-being (altruism), the psychological literature on guilt suggests that transfers may also reflect compensation by one agent to another for prior harmful acts, given the harmed agent's current well-being. There is little evidence of guilt-motivated behavior, however. In this paper, we use new survey data on twins born in urban China, among whom many experienced the consequences of the forced mass rustication movement of the Chinese "cultural revolution," to identify the distinct roles of altruism and guilt in affecting behavior within families. In particular, we exploit the fact that many families were forced to select one of their adolescent children to be "sent down." We show the conditions under which guilt and altruism can be separately identified using such data and find that parents behaved altruistically, providing more gifts at the time of marriage to the sibling with lower earnings and schooling, but also exhibited guilt – given the subsequent income and education of the two children and despite the evident positive effects of rustication on earnings and political connections, the child experiencing more years of rustication received significantly higher transfers from the parents when married. These results thus provide 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.

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

Is it possible to identify behavior motivated by guilt? Although there are empirical findings on inter-household transfers consistent with the hypothesis that people care about others' state of well-being (altruism), the psychological literature on guilt suggests that transfers may also reflect compensation by one agent to another for prior harmful acts, given the harmed agent's current well-being. There is little evidence of guilt-motivated behavior, however, in part because harmful behavior cannot be induced in experimental settings and because of the empirical difficulty of separating harmful acts from their consequences that affect well-being. In this paper, we use new survey data on twins born in urban China, among whom many experienced the consequences of the forced mass rustication movement of the Chinese "cultural revolution", to identify the distinct roles of altruism and guilt in affecting behavior within families. The forced rustication or "send-down" movement resulted in one out of every three urban adolescents (most aged 16-19) being sent to the countryside for manual labor for from one or two to as many as 10 years during the period 1966-1976.¹ In particular, we exploit the fact that many families were forced to

¹ Bonnin (2005) provides the best estimates of the fraction of eligible youth sent down to the rural areas, which is based on the period 1970-77. In our data, the average years spent working in the countryside was 3.5 years.

select one of their adolescent children to be “sent down.”² Thus, there are many pairs of twins in the cohorts affected by this policy in which one twin, but not the other, experienced at least temporary and significant harm, or in which there was differential exposure to the harsh conditions of rural labor.³ We show that a combination of the unique policy “experiment” (send down movement) and twins data allow us to separately identify guilt and altruism under certain conditions: altruism is identified by examining whether parents provided more gifts at the time of marriage to the sibling with lower earnings and schooling, and guilt is identified by examining whether the child experiencing more years of rustication received significantly higher transfers when married, given the subsequent income and education of the two children.

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 avoid 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 – individuals will experience

² This is the translation from the Chinese phrase “sent down to the countryside.” The phrase thus refers to the fact that children are sent away and to rural areas, which are perceived as remote.

³ Two sociologists who experienced rustication when they were adolescents describe the experience: “As the sent-down youth were forced to move to villages and join peasants living in poverty and under strict political/mobility control, the immediate negative impacts of the policy were obvious. To many urban youth, including us, the send-down episode remains among the most difficult experiences in our lives - we suffered from a lack of material supplies, removal from our families, an unfamiliar environment, harsh physical labor, and so on.” (Chen and Cheng, 1999).

more guilt if they have harmed a family member than if they have harmed a casual acquaintance.

The key distinction between altruism and guilt is that for the former the utility of an agent depends on the utility state or well-being of the focal person while for the latter it is the actions taken (or not taken) by the agent that matter. 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).⁴

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 also test for survival guilt among the twins by examining whether twins who were not sent down compensated their formerly rusticated twin sibling in the form of contemporaneous transfers, given both twins’ current earnings and education.

In economics, guilt is hypothesized to play an important role in helping to solve commitment problems. Guilt aversion may induce cooperative behavior in 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.⁵ If guilt exists and can be manipulated, it may be possible to

⁴ Dictionary definitions of guilt and shame appear at odds with the definitions in the psychology literature. See, for example, Miriam-Webster. We will adopt the definitions in the psychology literature.

⁵ 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.

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.

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 assess 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-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).⁶ 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 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 toward one child versus another. Favoritism expressed within monozygotic (MZ) twin pairs is

⁶Ketelaar 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.

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 toward better- or worse-endowed children and whether guilty actions followed such choices. Comparisons of estimates using samples of MZ and DZ twins also will enable us to assess the bias in estimates obtained from samples of MZ twins of the effects of being sent down that arise from the fact that MZ twins are only identical at conception. 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.⁷

We set out a simple model with one parent and two children who have different human capital endowments – endowments that affect the returns to human capital investments in terms of earnings - with the parent being the only decision maker to make precise the definitions of altruism, guilt and favoritism and to make clear the challenges to empirical identification of these motives. 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. We show that altruism can be identified if the parent, subsequent to the choice of time with the child, transfers more to a child who as an adult earns less, favoritism can be identified if the parent chooses to

⁷In 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 her suicide. During the mass send down movement in China, parents did not view forced rustication as a beneficial treatment, and parental guilt was also directly observed by the children: “I can never forget my father's eyes that day, filled with torture, fear, sadness, and *guilt*.... Sending his three children to rural areas did not save my father's soul. Instead, he put upon himself an enormous psychological burden. He died of lung infection in the winter of 1971.” (Deng, 1993) [emphasis added].

allocate her time across the children or provide the child with transfers according to their human capital endowments, and guilt can be identified if the parent transfers later in life 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 children's 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 at conception and have a similar family background, the effects of unobserved endowments or family background should be similar for both twins. Thus, obtaining estimates based on within-MZ twin differences will reduce the influences of unobservables such as endowments and thus allow identification of causal impacts. The selection of one child to send down from within a pair of MZ twins thus 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. MZ twins are not perfectly identical. By comparing the estimates using the MZ twins sub-sample with those using the DZ twins sub-sample, in which the twins are less “identical” (Behrman, Rosenzweig and Taubman, 1994), we are able to both identify favoritism – was the less or more able child in terms of adult earnings chosen by the parents to be sent down - and sign the remaining bias in within-MZ estimates that arise from the fact that MZ twins are not perfectly identical.

Our estimates indicate that, controlling for the selectivity of the send-down choice in terms of both household-level and individual endowments, rustication actually had a large positive return for earnings, employment, and political status (communist party membership). The positive return to rustication is in part a result of the specific historical context during which urban schools were shut down and thus the return for remaining in the city was low.⁸ However, it has been argued by those who experienced the harsh conditions in rural areas that the harsh conditions themselves had a lasting positive effect.⁹ Note that the finding of a positive impact of the send-down experience means that observations of subsequent parental compensation if any for those sent down is less likely to reflect a response to the current scars from the experience (altruism) and is more reflective of parental feelings about the past choice.

We also find that OLS regressions tend to under-estimate by more than 50% the true earnings return to rustication, however, and estimates based on MZ twins also

⁸ 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.

⁹ Chen and Cheng (1999) speculate that: “It might just be that during those harsh days and nights, a spirit of self-striving, with which most urban youth were completely unfamiliar, was cultivated. Such a spirit, once developed, might not only help the sent-down youth to emerge from the negative impacts of the send-down life, but also might support their future career development. Apparently the motivation of sent-down youths to improve their poor lives was much stronger than that of urban youths with stable jobs and easier lives.”

yielded higher rustication returns than those estimated based on differences between DZ twins.¹⁰ These findings thus not only suggest that we have a lower-bound estimate of the returns to rustication, but imply 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 endowments. *De facto* policy and parents thus exhibited favoritism toward the better-endowed children with respect to who to retain in the household.

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 and guilt. First, we find that parents tend to transfer somewhat more resources at marriage to the child with lower earnings, consistent with altruism. Second, 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 and schooling), our FE estimate using the MZ twins sample suggests that parental gifts at the time of wedding were significantly higher for each additional year a child stayed in the countryside. The point estimates suggest that for each year a child spent as a youth in the countryside, transfers at marriage increased by 12 percent. As the average value of all gifts at marriage is approximately 1.5 times the yearly income of those marrying, given the average years spent in the countryside of approximately 3.5 years, the estimates suggest that a sent-down child received almost two-thirds of one year's earnings compared to his or her sibling who was kept at home and who subsequently

¹⁰ 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."

has identical earnings, schooling and at-conception endowment. We also show that our finding that transfers reflect being sent down is robust to the inclusion of the characteristics of the spouse and are not explained by either the facts that sent-down youth are less likely to co-reside with parents or are more likely to be Communist Party members. Thus, guilt appears to be an additional reason parents make transfers to children. We also examine transfers between siblings in the year prior to the survey, finding that those in the relevant cohort who were not sent down also appear to exhibit guilt – within MZ twin-pairs, the sibling experiencing separation from parents as a result of the send-down program received significantly higher levels of transfers from her sibling, for given the contemporaneous earnings and schooling of the pair. The magnitude of the inter-sib guilt effect on transfers was similar to that found for intergenerational transfers.

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 examines the robustness of the transfer results and Section 8 provides estimates of the determinants of transfers among siblings. Section 9 concludes, summarizing the findings.

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.

With respect to the forced relocation of urban youth, there are a number of key features that are relevant. 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. Some of 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.

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

Movement. During the 1960s and 1970s, the Chinese government sent millions of youth 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 policies 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 1946.¹¹ 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

¹¹ 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.

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 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,¹² 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.¹³ 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

¹² See Liu et al. (1995) and Zhou and Hou (1999).

¹³ See Deng and Treiman (1997) and Zhang *et al.* (2007) for more details.

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 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.

3. Theory

The rustication movement exogenously altered the amount of time parents spent with their adolescent children. We set up a simple model of parental behavior that incorporates altruism, favoritism and guilt and provides a precise definition of each of these motives. We use the model 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 be used to identify empirically each of these distinct 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. We show first that with one child, the reduced-form effect of varying the endowment of the child or the effect of varying exogenously the time parents spend with the child on the transfers parents make to the child later in life reflect a combination of favoritism, altruism and guilt. That is, guilt-motivated behavior cannot be identified using data pairing a parent and one child. Intuitively, even if parental consumption and time spent with the child are separable in the parental utility function, as long as the time a parent spends with the child is costly to the parent, then exogenously shifting her time away from the child lowers the marginal utility of the parent and thus can lead to compensatory transfers to the child solely as a result of altruism.

We then show that it is possible to identify both altruism and guilt using data on siblings. In the two-child model, if parental consumption and transfers to children are separable, shifting parental time from one child to another does not have any effect on the utility of the parent derived from consumption so that the relationship between differences in parental time allocated per child and the transfers made across the children identifies guilt if either of two conditions are met: (i) it is possible to condition on the adult earnings of both siblings or (ii) parental time with children adversely affects the child's adult earnings. Controlling for the earnings of the siblings eliminates the influence of the altruism motive, so that a finding that parents transferred more to the child with whom they spent less time must reflect compensation to alleviate guilt. However, if it is not possible to fully account for each

adult child's lifetime earnings, then if the child who was exposed less to parental time was as a result better off, the finding that despite being better off that child still received more transfers than her sibling would indicate that parental guilt must be a motive, as altruism would lead to such children receiving less transfers. Our empirical finding that children who were sent down, and thus who spent less time with their parents, benefited later in life thus makes our results on guilt robust to the incomplete measurement of lifetime child earnings.

3.1. The One-child Model

3.1.1. Model Setup

Consider the following utility function for a parent with one child

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

(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 lifetime income when an adult, 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 when the child is young (prior to the child earning income), i.e., $\alpha_r \equiv \partial \alpha / \partial r > 0$, and by giving a monetary transfer t to the child when the child is an adult and the child's earnings (defined below) are observed by the parent, 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.¹⁴

¹⁴ As can be seen, we have set up the model so that parental consumption, the child's income, and time and resources allocated to the child are additively separable. The separability of parental consumption and child income is standard in the literature, as is our definition of altruism. We discuss below how

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.¹⁵ 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,

$$Y = c + t + Pr, \quad (2)$$

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

$$W = \beta(e)r + t + \varepsilon, \quad (3)$$

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. However, we have also assumed that parental transfers to the child do not directly affect the child's

relaxing the additional separability assumptions affects inferences about guilt from observations on transfers and parental time.

¹⁵ 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.

human capital, as they are transfers made when the child is an adult (and ε is observed). We will be examining the relationship between transfers made by the parents at the marriage of the children and parental time spent with the children when they were adolescents. The assumption that transfers and parental time are additively separable in the budget constraint, but not necessarily so in the utility function, is thus a reasonable restriction given our data.¹⁶

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

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

The first order conditions with respect to r and t are

$$-PU_c + \delta\beta V_w + \alpha_r = 0 \quad (5)$$

$$-U_c + \delta V_w + \alpha_t = 0 \quad (6)$$

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 reduced-form 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

$$\frac{dr}{de} = \frac{A_1 + A_2 + A_3}{\Delta_2}, \quad (7)$$

¹⁶ Finding that parental transfers at a child's marriage and time spent with the parents when the child was less than 19 are negatively correlated thus is not likely to reflect the substitutability of time and money in the production of human capital.

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 $A_3 = \delta r \beta_e V_{cc} [(P - \beta)U_{ww} + \alpha_{rt} - \beta \alpha_{tt}]$.¹⁷ 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 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

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

¹⁷ The second order conditions are $P^2 U_{cc} + \delta \beta^2 V_{ww} + \alpha_{rr} < 0$, $U_{cc} + \delta V_{ww} + \alpha_{tt} < 0$ and

$$\Delta_2 \equiv \begin{vmatrix} P^2 U_{cc} + \delta \beta^2 V_{ww} + \alpha_{rr} & PU_{cc} + \delta \beta V_{ww} + \alpha_{rt} \\ PU_{cc} + \delta \beta V_{ww} + \alpha_{rt} & U_{cc} + \delta V_{ww} + \alpha_{tt} \end{vmatrix} > 0, \text{ where the first two inequalities}$$

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).

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,

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

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

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

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

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

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

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

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

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.¹⁸ 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 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:

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

and

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

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

¹⁸ The proof can be obtained from authors on request.

the own and cross-transfers effects from exogenous variation in the earnings of child one in (15) and (16) is

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

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

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

and the cross effect is

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

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

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

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 either know how endowments affect the returns to parental time, the sign of β_e , or to condition on the earnings of the children in order to identify favoritism arising purely from preferences, given altruism. Conditioning on the earnings of both siblings eliminates the first two terms in the denominator and numerator in (20), yielding:

$$\frac{dt^1}{de^1} - \frac{dt^2}{de^1} = \frac{-\alpha_{te}}{\alpha_{tt}}, \quad (21)$$

so that favoritism can be identified based on the differences in transfers received by higher- and lower-endowed siblings.

It may not be possible to control completely for the lifetime incomes of the children, in which case knowledge of the effects of endowments on the returns to human capital investments must be known. 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 $\alpha_{te} > 0$, meaning that parents would choose to provide more financial assistance to more endowed children or that they favor higher-endowed children.

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 also permits the identification of guilt ($\alpha_{rt} < 0$). In this case, if it is not possible to completely account for the earnings of the siblings, it is only necessary to have information 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

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

and the corresponding cross effect on the second child is

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

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 (22) and (23), which is

$$\frac{dt^1}{dr^1} - \frac{dt^2}{dr^1} = \frac{\delta \beta V_{ww} + \alpha_{rt}}{-(\delta V_{ww} + \alpha_{tt})}. \quad (24)$$

The sign of $dt^1 / dr^1 - dt^2 / dr^1$ will be the same as that of $\delta \beta V_{ww} + \alpha_{rt}$. Thus, only if $\beta < 0$, does the finding that $dt^1 / dr^1 - dt^2 / dr^1 < 0$ imply $\alpha_{rt} < 0$, so that guilt can be identified.

Conditioning on the earnings of the siblings, makes it unnecessary to know how parental time with children affects their human capital, as then (24) becomes

$$\frac{dt^1}{dr^1} - \frac{dt^2}{dr^1} = -\frac{\alpha_{rt}}{\alpha_{tt}}. \quad (25)$$

With the differences in the earnings of the two siblings accounted for, the finding that the sibling with whom the parents spent less time received greater transfers as an adult would thus identify guilt. 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), so that that even if the information on child earnings incompletely characterizes the lifetime earnings of the siblings, guilt can be identified from estimates of how differences in transfers across the siblings are related to their time at home with parents.

3.2.3. Relaxing Separability

We have assumed until now that the utility parents derive from each child and the components of the child-specific utility functions are additively separable.

Relaxing these assumptions yields the more general objective function

$$U(c) + \delta V(W^1, W^2, r^1, r^2, t^1, t^2 : e^1, e^2).^{19} \quad (26)$$

In this more general specification with two children, we need to take into account cross-effects in the definition of guilt. In particular, we will define guilt as $V_{t^1 r^1} - V_{t^2 r^1} < 0$. In this case, we not only consider the increase in the marginal utility of transferring income to a child i when parental time with child i is reduced ($V_{t^i r^i}$) but also the decrease in the marginal utility of transferring income to child j ($V_{t^j r^i}$).

The expression relating how transfers shift between child 1 and child 2 in response to a change in the time parents spend with child 1 in this non-separable case, conditional on the earnings of both siblings and thus analogous to (25), is then

$$\frac{dt^1}{dr^1} - \frac{dt^2}{dr^1} = \frac{V_{t^1 r^1} - V_{t^2 r^1}}{V_{t^1 t^2} - V_{t^1 t^1}}. \quad (27)$$

As in the separable model, finding that $dt^1 / dr^1 - dt^2 / dr^1 < 0$, parents transfer more resources later in life to the child who received less attention, implies that parental transfers reflect guilt $V_{t^1 r^1} - V_{t^2 r^1} < 0$, as long as child-specific transfers are not too complementary.²⁰ An analogous expression can be derived for the effects of differences in earnings of the children on transfers, which yields the same implication

¹⁹ We still retain the assumption that parents' utility from own consumption and the utility they derive from children are additively separable, as is imposed in all models incorporating altruism. Relaxing this assumption would require more information on the cross partials in the parental utility function between parental consumption and the time allocated and transfers provided to each child in order to identify behavior motivated by guilt from observations on transfers and time allocation.

²⁰ A sufficient condition is that $V_{t^i t^j} \geq 0$ or that child-specific transfers are substitutes.

as in the additively-separable model – a rise in the relative earnings of one child leads to a relative reduction in transfers going to that child.

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 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.²¹ Questionnaires were completed through 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 respondents, of which 2,990 are twins. For the sample of

²¹ One component of the selectivity of the sample, in addition to that associated with identifying twins in the population, is due to “abnormally” high death rates in rural areas among the urban youth at the time. Estimates suggest that death rates for those sent down were 50% higher than normal (Xia, 2003). This suggests that rural life was not pleasant for those rusticated. However, the degree of mortality selectivity is small as death rates for the adolescent population are minimal. The elevated death rate is approximately 0.2 deaths per thousand. 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.

twins, care was taken to distinguish whether twins are identical (monozygotic or MZ) or non-identical (fraternal or 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 matched pairs of MZ twins (1838 respondents) and 576 matched pairs of DZ twins (1152 respondents).²² 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 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 those sent down on average stayed in the countryside for 3.5 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

²² The questions used to distinguish MZ and DZ twins are standard in the psychological literature on twins. It is possible that these lead to misclassification, most likely in DZ twins characterized as MZ. We show below that any differences between MZ twins, due to epigenetic factors and other factors that differentiate MZ twins or incorrect classification of MZ twins, biases downward the effects we find of send-down on transfers given evident favoritism of parents for better-endowed children. In our sample MZ twin pairs are over-represented relative to the population of twins (60% versus approximately 50%); this does not necessarily reflect misclassification, as among MZ twin pairs, both members of the pair are more likely to be found or to respond compared with DZ twin pairs because they are more alike. MZ twin pairs are also over-represented, for example, in the Minnesota Twins Survey (Behrman *et al.*, 1994).

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. Estimating the Economic Returns to Rustication and the Selection Rule

In this section we first discuss how the twins sample can be used to estimate the returns to rustication β , which as we have shown is important for the identification of guilt, and then show how we can identify the governmental rule selecting households and whether parents exhibited favoritism in choosing the child to be sent down according to his or her earnings endowment. We exploit the differences in estimates of β that are obtained using three estimation methods: Least squares estimates obtained from the cross-section of households, and fixed-effects methods in which an estimate is obtained of β based on variation in send-down years within MZ and DZ twin pairs. Identification comes from the fact that there are greater differences in unmeasured endowments across DZ than across MZ twins and thus the within-DZ bias component of the estimate of β will be larger in absolute magnitude than that in the within-MZ estimate. The difference between the two estimates of β signs the bias,

which then tells us the sign of the correlation between rustication and the endowment (favoritism).

The empirical results based on estimates that exploit variation in send-down within MZ twin pairs, and thus which are least biased by the presence of unobserved endowments, suggest that β is positive - increased rustication while an adolescent increased earnings later in life, increased the likelihood of being employed at the time of the survey, and increased the likelihood of being a member of the Communist Party while having no adverse effect on subsequent adult health. The results based on the contrast between the within-DZ and within-MZ estimates also suggest that parents favored higher endowment children, choosing the child with lower earnings endowments to be sent away. Conventional estimates of the consequences of rustication based on variation in send down years across households are biased downward and the positive and significant within-MZ twins estimates of β are lower bound to the extent that there is any variation in unmeasured endowments within MZ twin pairs.

5.1. Identifying β Using MZ Twins

We first estimate the following equation, which corresponds to the children's earnings in the model:

$$w^{ij} = X^j \alpha + Z^{ij} \beta + \mu^j + e^{ij} + \varepsilon^{ij}, \quad (28)$$

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.²³

Included in Z^{ij} are the number of send-down years, which corresponds to $-r$ in the

²³ In the model we specified a linear child earnings function, while here we use a semi-log specification as is standard in the earnings literature. We consider below the appropriate functional form for the transfer equation, in which the semi-log specification is shown to be empirically preferred.

model. We also include in (28) 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 .

The OLS estimate of the effect of send-down years in equation (28), 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} , for example if parents favor children based on their endowments. 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 parental bias, and thus may be related to unobserved child-specific endowments.

We remove the influence of both the family effect and reduce the child endowment effect by applying the fixed effects estimator to the monozygotic (MZ) twins sample. Taking the within-twin difference eliminates the unobservable family effect μ^j . Because MZ twins are genetically identical at conception, there will be less within-family individual variation in the endowment term compared with spaced siblings and DZ twins.²⁴ To identify endowment effects, we also apply the same

²⁴ MZ twins are not identical at birth. Differences in nutrient intake in the womb across MZ twins, for example, can have important effects on adult outcomes. Behrman and Rosenzweig (2004) show that there is considerable variation in birthweight across MZ twins, and this variation is significantly related to differences in their educational outcomes and earnings.

estimation procedure to the sample of DZ twins. We show below that the comparison of OLS, within-MZ and within-DZ estimates can be used to identify the effects of endowments on allocation rules and motives as well as indicate the direction of the remaining bias in the within-MZ estimates arising from any differences in endowments among MZ twins.

5.2. 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

$$w^{1j} = X^j \alpha + Z^{1j} \beta + \mu^j + e^{1j} + \varepsilon^{1j} \quad (29)$$

$$w^{2j} = X^j \alpha + Z^{2j} \beta + \mu^j + e^{2j} + \varepsilon^{2j}, \quad (30)$$

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 (29) and (30):

$$w^{1j} - w^{2j} = (Z^{1j} - Z^{2j})\beta + \varepsilon^{1j} - \varepsilon^{2j}. \quad (31)$$

We have assumed in this case that the e terms reflect only at-conception endowments and thus are eliminated in (31); the remaining ε^{ij} terms thus contain a component of the endowment that is known by the parents. OLS estimates of equation (31) provide an unbiased estimate of the effect of the rustication only if that part of the ε^{ij} containing endowments is small. Comparisons of the estimates obtained from (29) or (30) 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 $\beta_{OLS} - \beta_{MZ}$ 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.

5.3. 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 certainly biased. Algebraically, the first difference becomes

$$w^{1j} - w^{2j} = (Z^{1j} - Z^{2j})\beta + e^{1j} - e^{2j} + \varepsilon^{1j} - \varepsilon^{2j}. \quad (32)$$

Because we do not observe the endowments $e^{1j} - e^{2j}$, if $\text{corr}(Z^{1j} - Z^{2j}, e^{1j} - e^{2j}) \neq 0$, 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. The sign of the difference between the within-DZ and within-MZ estimates of β also indicates the direction of any bias in the within-MZ estimate of β due to any remaining differences in endowments across MZ twins, assuming that parents respond in the same way to child endowment differences across type of twin pairs. Thus, if $\beta_{DZ} < (>) \beta_{MZ}$, parents exhibit favoritism towards the stronger (weaker) child, and any bias in the within-MZ estimate β_{MZ} must be negative (positive).

5.4. Estimates of the consequences of rustication for earnings

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 (twin-pair) 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 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 excluding any work experience gained during the time spent in the countryside. 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, or within twin-pair, 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 rustication 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 ($\text{corr}(e_{1j} - e_{2j}, D_{1j} - D_{2j}) < 0$), 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. This also means that we have a lower bound estimate of the positive effect of rustication years on earnings from the MZ twin-pairs sample.

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 toward the more able child.

5.5. Rustication and 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.²⁵ 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 obtaining a well-paying job in the government, possibly inclusive of bribes and side payments. 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.²⁶

²⁵ 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.

²⁶ The estimates in Li *et al.* (2007) of the effects of party membership on earnings, obtained from the same sample of twins as used here, suggest that the greater access to party membership experienced by those rusticated explains only a small part of their earnings advantage. We assess below whether having a child who is a party member affects parental transfers.

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

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 and assess whether parents provided more resources to the child who spent more time away from parents due to rustication, given the earnings and education of the twins at the time of marriage. We also compare within-MZ and within-DZ estimates of the effects of rustication on transfers to identify favoritism with respect to financial help. We find that parents, in accord with the model, do exhibit guilt: parents provided significantly more resources to the rusticated child at marriage despite the fact that rustication evidently increased earnings and despite control for differences in earnings between the twins at the time of marriage. The results also suggest that parents mildly exhibit altruism, giving more to the child with lower earnings net of their rustication experience, but also favored the child with the higher earnings endowment. Given that we found that less-endowed children were more likely to be sent down, this latter result also implies that the

statistically-preferred within-MZ estimate of the effects of rustication on parental transfers, which identifies guilt, is also lower-bound.

In China, one major means by which parents provide children with financial resources is with wedding gifts.²⁷ 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 large, almost 1.5 times the average annual earnings of the twin respondents at the time of their marriage (over half of annual earnings at the time of the survey), and three-quarters of the twins pairs report receiving different amounts from their parents.²⁸ 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

$$T^{1j} = aw^{1j} + bw^{2j} + cD^{1j} + dD^{2j} + Z^{1j}f + Z^{2j}g + \eta^j + he^{1j} + me^{2j} + \xi^{1j} + o\xi^{2j} \quad (33)$$

$$T^{1j} = aw^{2j} + bw^{1j} + cD^{2j} + dD^{1j} + Z^{2j}f + Z^{1j}g + \eta^j + he^{2j} + me^{1j} + \xi^{2j} + o\xi^{1j}, \quad (34)$$

²⁷ Typically, at marriage the couple sets up their own home or in some cases moves into a newly-furnished room in the groom's parents' house. The groom's parents also provide more financial resources at the time of marriage than the bride's parents. In our data, the average value of transfers at marriage from groom households is 7500 *yuan*; for bride households it is 3912 *yuan*. Because our sample only contains same-sex twin pairs, any differences in transfers across twins are not due to gender.

²⁸ Transfers at marriage dominate financial transfer flows to households. Contemporaneous (survey year) financial transfers from parents to children, as in the United States, are very sparse and one-tenth the size of the transfers at the time of marriage. Five percent of the respondents received unemployment insurance in the year preceding the survey, with an average monthly payment of only 190 *yuan*. Only 21% of the sample have any formal health insurance, almost all employer-paid.

where T^{ij} is (log) 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^{ij} are observable individual-specific child variables such as age and schooling at the time of marriage, η^j captures the set of common family characteristics, e^{ij} are the unobserved child endowments, and ξ^{ij} are random errors. The parameters, a, b, c, d, f and g are parameters to be estimated.²⁹

Differencing (33) and (34) for the sub-sample of MZ twins, we get:

$$T^{1j} - T^{2j} = \pi_1(w^{1j} - w^{2j}) + \pi_2(D^{1j} - D^{2j}) + (Z^{1j} - Z^{2j})\pi_3 + (\xi^{1j} - \xi^{2j}), \quad (35)$$

where $\pi_1 = a - b$, $\pi_2 = c - d$, and $\pi_3 = f - g$. We have again assumed that the endowments of the MZ twins are identical in obtaining (35) so that we get consistent estimates of π_1 , and π_2 , which in turn identify altruism and guilt, respectively.³⁰ In particular, $\pi_2 = -dt_1/dr_1 - dt_2/dr_1$ (expression (24) or (27)) and thus identifies guilt and, analogously, π_1 identifies altruism. Note that 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. A negative estimated π_1

²⁹ We chose to estimate the transfer equation in log form because of the non-symmetric distribution of transfers. We then applied the non-nested functional-form test developed by MacKinnon, White and Davidson (1983), which has the advantage over earlier models in that it is asymptotically insensitive to failures of the normality assumption (Davidson and MacKinnon, 1993). Both our log-linear model and the linear model (in both wages and transfers) are tested against the hypothesized “correct” specification, an artificial compound model (a weighted function of the centered log-linear and linear models), by Gauss-Newton test regressions. The test statistic (t-statistic) for our log-linear specification is 1.93, which means that we cannot reject the null hypothesis (at the 5% level) that the log-linear model is the correct specification. The test statistic for the linear model is 2.91, which means that the null hypothesis that the linear model is the correct specification is rejected at the 1% level. We also ascertained if there is an independent effect of being rusticated, in addition to that associated with the number of years sent down. While being rusticated has a positive effect on at-marriage transfers, the coefficient is not statistically significant.

³⁰ Analogous to our analysis of the effects of rustication on earnings, we compare below within-MZ and within-DZ estimates of the π 's to assess the sign of the bias in the within-MZ estimates arising from any remaining unobserved differences across MZ twins.

would suggest the existence of altruism; a positive π_2 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 (and especially since rustication improved earnings ($\beta > 0$)), 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.³¹ 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 $dt_1 / dr_1 - dt_2 / dr_1 < 0$ and our finding that the return to send-down years is positive ($\beta < 0$), this means that $\alpha_{rt} < 0$ (or $V_{11r1} - V_{12r1} < 0$ in the non-separable model) - 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 4 years, just slightly above the mean stay in the sample, and the other twin did not go, then the value of the send-down twin's wedding gift is 50% larger than 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:

³¹ The estimated age-at-marriage coefficient indicates that parental transfers are significantly smaller the more marriage is delayed. Given that our estimates are from twin pairs, this result might be due to parents providing more gifts to the child who marries first. This finding is robust to estimation procedure.

although the coefficient on children's earnings on parental transfers is negative, consistent with altruism, it is not statistically significant.³²

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 parental 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 $\pi_{2MZ} > \pi_{2DZ}$ in the transfer equations, then it must be that $\text{corr}((h-m)(e_{1j} - e_{2j}), D_{1j} - D_{2j}) < 0$ for DZ twins. As we have shown that $\text{corr}(e_{1j} - e_{2j}, D_{1j} - D_{2j}) < 0$ from the estimated earnings

equations, this means $h - m = \frac{dt^1}{de^1} - \frac{dt^2}{de^1} > 0$, or that the better-endowed child is

favored by parents with more resources ($\alpha_{te} > 0$). 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 the parental gift for the MZ twins is large, positive and significant, that for the DZ twins is almost zero and insignificant. This means that the within-MZ estimates are lower-bound estimates of the parental transfer response to rustication, to the extent

³² Cox (1987) in a pioneering study of transfer motives also does not find strong support for altruism.

that there are unobserved endowment differences within MZ twin pairs. Moreover, that $\pi_{2MZ} > \pi_{2DZ}$ 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.

7. Alternative Explanations

In this section we consider alternative explanations for our finding that parents provided more resources at marriage to the child who was sent down longer. The most obvious concern is that we have not fully taken into account the lifetime earnings of the children, having only controlled for earnings at marriage. Parents may have had a more informed forecast of their children's lifetime earnings than is measured by their children's earnings at marriage. Although our finding that rustication increased average earnings obviates the need to control for earnings in the transfer equation, we can use the survey information on the current earnings of the twins as an additional control variable in the transfer equation to assess if this affects our results. It does not.

An alternative explanation for our rustication-transfer result is that rusticated youth were more likely to be Communist Party members, as we found, and thus the higher transfers at marriage they received reflects reciprocity – parents expect that such children will be more likely to be able to provide for them later in life because of their superior influence and thus the payoffs to the parents from assisting such children at marriage will be higher than assisting those children without party affiliation. We can assess if this is the reason for our finding by controlling for party membership. We find that it is not – party membership has no effect on parental

transfers at marriage, and the rustication effect on parental transfers is unaffected by the inclusion of this variable.

Another potential confounding influence is associated with the fact that we use parental transfers when the twins marry. On average half of the transfers at marriage come from the spouse's parents, which we do not observe. It is possible that those who were rusticated longer were at a disadvantage in the urban marriage market to which they returned compared with those who stayed at home. If rusticated twins married spouses from poorer households (including spouses from rural areas), then their parents may have compensated them for the lower transfers they received from their in-laws by increasing their contribution. We assess the importance of this by looking at how rustication affected the characteristics of the spouse and also add the characteristics of the spouse at the time of the marriage to our transfer specification. We find that rustication has no effect of the quality of the spouse, and controlling for spouse characteristics has no effect on our result.

Finally, a large fraction of twins (40%) co-reside with parents. If parents consider co-residence a form of support for their children, then transfers may mechanically be negatively correlated with co-residence. If rusticated twins, who were also evidently the less preferred twins, are less likely to co-reside with parents, then they may be receiving greater financial resources simply because they are less likely to live with parents as adults. We examine the effects of rustication on parental co-residence and then estimate the relationship between transfers and rustication controlling in addition for whether or not the twin is co-residing with parents. Although we do find that children rusticated as adolescents are significantly less likely to co-reside with their parents later in life, we find that co-residing children

actually received more transfers at marriage and that controlling for co-residence increases the positive effect of rustication on marital transfers.

To assess the robustness of our results on transfers to the inclusion of earnings at more life-cycle points, we must use a smaller subsample of twin pairs, as fewer twin pairs provided information on earnings both at marriage and in the year prior to the survey than provided just earnings at marriage. In column 1 of Table 6 we report within-MZ estimates of the effects of rustication on transfers at marriage for this subsample, replicating the specification employed in obtaining the estimates reported in Table 5 for the larger sample. In this subsample, the estimated effect of rustication on transfers is larger than that obtained from the main sample. We are interested here, however, in the robustness of this coefficient to alternative specifications. Column 2 of the table reports the estimates including both the at-marriage and at-survey log wages of the twins in this sub-sample. As can be seen, this leaves the rustication coefficient virtually unchanged.

The estimates reported in column 3 are from a specification that includes both the set of wages and party membership. Party membership does not appear to affect parental transfers at marriage, nor does the effect of rustication (column 4, interaction between rustication years and party membership) depend on party affiliation. The main effect of rustication on parental transfers is again invariant to taking into account the relationship between rustication and membership in the Communist Party.

Rustication did not evidently affect the quality of marital matches of the twins independent of their marriage age and schooling. In Table 7 we report within-MZ estimates of the effects of the age of the twins, their education and the number of years sent down on the education and log earnings at marriage of their spouses in columns one and two, respectively. Years sent down have no statistically significant

effect on either spouse variable. Moreover, the estimated effect of rustication on parental transfers at marriage is still positive and statistically significant when the two spouse characteristics are included in the transfer equation (columns three and four) and the magnitude of the coefficient is little changed compared to that obtained when spouse characteristics are excluded (Table 5).

Rustication does appear to affect the likelihood of a twin residing with parents. The first two columns of Table 8 report within-MZ estimates of the effects of rustication, age at marriage and education at marriage on the probability of co-residing with the parents at the time of the survey. The point estimate of the rustication effect suggest that for every year in the countryside the probability of parental co-residence declines by 4.5 percent (0.018/0.40), so that on average rusticated youth, who spent on average 3.5 years in the rural areas, are 16% less likely to be co-residing with their parents at the time of the survey. However, the within-MZ estimate of the effect of co-residence on parental transfers is positive, and inclusion of co-residence in the transfer equation increases the positive estimated effect of rustication on parental transfers at marriage (column 4 versus column 3).³³

8. “Survivor” Guilt

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

³³ To the extent that MZ twins differ in endowments of which parents are aware, the relationship between send-down years and parental co-residence and between co-residence and parental transfers may in part reflect favoritism (bias towards better-endowed children), as we have seen that favored children were more likely to stay with parents at adolescence and to be provided more gifts at marriage. Thus, the rustication coefficients in columns 1 and 2 of Table 8 and the co-residence coefficient in column 4 are likely upper-bound estimates of the effect of rustication on co-residence and parental transfers, respectively.

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). These anecdotal studies suggest that those who took no actions against their close relatives nevertheless experienced guilt about their not having been as adversely affected as if they were responsible. In our data, 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 earnings and schooling attainment, despite not having made the decision about the sibling's send-down.³⁴

The survey elicited information not only on intergenerational transfers, but also on the transfers between the twins in the year prior to the survey. In that year, 40% of the twins had received a transfer from their co-twin. The average amount of the transfer was non-trivial, 642 *yuan*, about 72% of the average monthly wage. We can thus estimate a transfer equation, similar to equations (29) and (30) except that the transfers are inter-sibling and all variables refer to the year prior to the survey rather than to the marriage years of the respondents. While the model does not incorporate inter-sib transfers, it seems reasonable to interpret a negative effect of a wage increase on the receipt of inter-sibling transfers as indicating the presence of sibling altruism and, given wages, the effect of differences in years sent down on transfers as reflecting guilt, in this case the guilt of the less- or non-rusticated twin.

Table 9 reports OLS (based on the MZ sample), within-MZ and within-DZ estimates of the effects of sibling wages and rustication on sibling transfers received

³⁴ It is possible that in some cases the weaker twin "volunteered" to go in place of the stronger twin, in which case that could account for at least part of any subsequent guilt-motivated behavior we see among siblings.

in the year preceding the survey. The OLS and within-MZ estimates for the MZ subsample indicate both altruistic behavior and survivor guilt among the siblings. In particular, differences across the twins in current earnings are statistically significantly and negatively associated with differences in contemporaneous net transfers received, for given send-down experience, while differences in years sent down are positively associated with the receipt of transfers, for given earnings and education. The wage elasticity of transfers is large – a 10% relative decrease in wages for a sibling increases the financial transfers she receives from her sibling by 8%. Siblings thus appear to be more altruistic toward each other than are parents to their children. Surprisingly, the effects of send-down on transfers are as large as those exhibited by parents. The preferred within-MZ estimates indicate that each year of difference in years away is associated with a statistically significant 15.7 percent increase in transfers to the more victimized sibling. The sibling “survivors” thus also appear to exhibit guilt.

9. 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 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 finding has little relevance for any contemporaneous policies because of the peculiar conditions of the time, including the absence of or reduced schooling opportunities in all parts of China, and because the policy clearly 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 earnings and schooling attainment of the two children (and the characteristics of their spouses and co-residence with parents) and despite the fact that rustication increased the earnings of those rusticated relative to those who were not, the child experiencing more years of rustication received significantly higher transfers. We also found that a twin who had a co-twin who was rusticated also provided more resources to that co-twin later in life despite not having been responsible for the send-down decision. This finding appears to be consistent with reports of “survival” guilt expressed by surviving family members of catastrophes, both man-made and natural.

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)
Monthly wage in 2002 (includes 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 received (2002 <i>yuan</i>)	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)
Monthly wage at the time of wedding (2002 <i>yuan</i>)	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)

Variable	MZ twins		DZ twins	
	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 (0.010)	0.017* (0.009)	0.025*** (0.010)	0.032** (0.013)	0.034*** (0.012)	0.042*** (0.014)	-0.003 (0.036)	-0.006 (0.034)	-0.008 (0.035)
Age	0.003 (0.003)	0.008*** (0.002)	-0.003 (0.006)						
Male	0.186** (0.044)	0.212*** (0.038)	0.213*** (0.038)						
Education		0.085*** (0.006)	0.085*** (0.006)		0.027* (0.015)	0.030** (0.014)		0.046*** (0.012)	0.045*** (0.012)
Experience			0.028*** (0.009)			0.022 (0.021)			-0.017 (0.026)
Experience squared			-0.001** (0.000)			-0.000 (0.001)			0.000 (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*** (0.007)	0.015*** (0.005)	-0.008 (0.005)
Observations	1836	1796	1838
MZ Twins: FE			
Years sent-down	0.026*** (0.009)	0.020*** (0.008)	0.006 (0.008)
Observations	1836	1796	1838
DZ Twins: FE			
Years sent down	0.005 (0.007)	0.010 (0.009)	-0.021* (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 (0.034)	-0.002 (0.038)	0.117** (0.053)	0.119** (0.053)	0.001 (0.066)	0.002 (0.066)
Age at marriage	-0.215*** (0.031)	-0.239*** (0.030)	-0.234*** (0.043)	-0.236*** (0.043)	-0.099* (0.056)	-0.095* (0.057)
Male	1.196*** (0.263)	1.287*** (0.259)				
Education at marriage		0.192*** (0.045)		0.067 (0.069)		0.007 (0.069)
Log wage at marriage		0.095 (0.094)		-0.012 (0.116)		-0.094 (0.214)
<u>Co-twin characteristics</u>						
Years sent down	-0.145*** (0.035)	-0.121*** (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%.

Table 6: Within-MZ Estimates of the Effect of Sent-Down Years, Current Wage and Party Affiliation on Log Parental Transfers and Gifts at Marriage

	Dependent variable: Log parental transfers and gifts at marriage			
	(1)	(2)	(3)	(4)
Years sent down	0.280*** (0.078)	0.279*** (0.081)	0.278*** (0.082)	0.262*** (0.093)
Age at marriage	-0.248*** (0.059)	-0.248*** (0.059)	-0.248*** (0.059)	-0.248*** (0.059)
Education at marriage	0.164* (0.096)	0.163* (0.097)	0.163* (0.097)	0.166* (0.098)
Log wage at marriage	0.023 (0.171)	0.022 (0.168)	0.021 (0.168)	0.022 (0.168)
Log current wage		0.013 (0.320)	0.003 (0.316)	0.008 (0.316)
Party membership			0.149 (0.359)	0.079 (0.367)
Party*send-down years				0.062 (0.160)
Observations	534	534	534	534
Pairs of twins	267	267	267	267
R-squared	0.16	0.16	0.16	0.16

Note: Standard errors in parentheses are robust to heteroscedasticity and clustering at the family level.

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

Table 7: Within-MZ Twin Estimates of the Effect of Sent-Down Years on Spouse Characteristics and Log Parental Transfers and Gifts at Marriage

	Dependent variables:			
	Spousal education at marriage	Log spousal earnings at marriage	Log parental transfers and gifts at marriage	
	(1)	(2)	(3)	(4)
Years sent down	0.097 (0.084)	-0.003 (0.014)	0.117** (0.054)	0.117** (0.054)
Age at marriage	-0.017 (0.025)	0.083*** (0.018)	-0.241*** (0.043)	-0.239*** (0.044)
Education at marriage of twin	0.143** (0.060)	0.034 (0.025)	0.065 (0.069)	0.066 (0.070)
Log wage of twin			-0.004 (0.123)	0.006 (0.130)
Spousal education at marriage			0.012 (0.051)	0.012 (0.051)
Log spousal earnings at marriage				-0.034 (0.101)
Observations	1092	1092	1092	1092
R-squared	0.02	0.06	0.11	0.11

Note: Standard errors in parentheses are robust to heteroscedasticity and clustering at the family level.

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

Table 8: Within-MZ Estimates of the Effect of Sent-Down Years on Proximity with Parents and Parental Transfers and Gifts at Marriage

	Dependent variables			
	Live with parents (1)	Live with parents (2)	Log Parental gifts and transfers (3)	Log Parental gifts and transfers (4)
Send down years	-0.018** (0.008)	-0.018** (0.008)	0.125* (0.068)	0.136* (0.071)
Live with parents now				0.632** (0.307)
Age at marriage	-0.005 (0.006)	-0.004 (0.006)	-0.262*** (0.049)	-0.259*** (0.049)
Education at marriage	-0.018* (0.010)	-0.016 (0.011)	0.117 (0.081)	0.127 (0.082)
Log wage at marriage		-0.028 (0.021)	0.027 (0.120)	0.045 (0.121)
Observations	874	874	874	874
Number of hhs	437	437	437	437
R-squared	0.01	0.02	0.12	0.13

Note: Standard errors in parentheses are robust to heteroscedasticity and clustering at the family level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 9: OLS and Fixed Effect Estimates of the Effect of Sent-Down Years on intra-twin Gifts and Transfers
(MZ twins sample and DZ twins sample)

	Dependent variable: log of gifts and transfers from co-twin (in survey year)					
	OLS (MZ Twins)		Fixed Effects (MZ Twins)		Fixed Effects (DZ Twins)	
	(1)	(2)	(3)	(4)	(5)	(6)
Years sent-down	0.003 (0.068)	0.022 (0.064)	0.138 (0.094)	0.157* (0.088)	0.037 (0.162)	0.033 (0.164)
Age	-0.003 (0.015)	-0.005 (0.015)				
Male	-0.601** (0.272)	-0.800*** (0.276)				
Education		-0.041 (0.039)		-0.119** (0.051)		0.054 (0.063)
Log wage		0.142 (0.177)		-0.786*** (0.201)		-0.024 (0.221)
<u>Co-twin characteristics</u>						
Years sent-down	-0.135** (0.056)	-0.135** (0.055)				
Education		0.078** (0.039)				
Log wage		0.927*** (0.182)				
Observations	946	946	946	946	616	616
R-squared	0.07	0.11	0.01	0.08	0.00	0.00

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%.