

CHILDHOOD SEND-DOWN EXPERIENCE AND OLD-AGE SUPPORT TO PARENTS: THE TWINS EXPERIMENT IN CHINA

Hongliang Zhang
Zhejiang University, China

Junsen Zhang
Zhejiang University, China

Ning Zhang
University of Oxford, UK

Abstract

In the mass movement of sending urban youth to the countryside during China's Cultural Revolution, many families with multiple age-eligible children were forced to make a send-down choice among the siblings. We exploit this rare social experiment and employ data on urban twins in China to investigate the effect of childhood send-down experience on children's old-age support to parents. We find that compared with their twin siblings who had stayed in the city, send-downs were less likely to make a monetary transfer to parents and also tended to transfer less. We show that the inferior transfer behavior of send-downs was not due to any income disadvantage or selection of family's send-down choice in terms of children's altruism endowment. After ruling out the income and selection channel explanations, we posit that the inferior transfer behavior of send-downs is driven by the adverse effect of childhood send-down experience on children's willingness to provide old-age support to parents, which could work through both pure altruism and warm glow. (JEL: D64, J13, R28)

The editor in charge of this paper was Imran Rasul.

Acknowledgments: We thank Abi Adams-Prassl, Daniel Berkowitz, Osea Giuntella, Rufe Guo, Hidehiko Ichimura, Kamhon Kan, Jin Wang, and seminar/conference participants at Academia Sinica, Hong Kong Baptist University, Hong Kong University of Science and Technology, University of Oxford, University of Pittsburgh, University of Tokyo, the 2017 International Symposium on Contemporary Labor Economics, the 2018 Society of Labor Economists Conference, and the 2019 Asian Meeting of the Econometric Society for their valuable discussion and comments. We are also grateful to the editor and four anonymous referees for their helpful comments and feedback, and Uchechi Shirley Anaduaka and Baojun Wang for their excellent research assistance. Hongliang Zhang acknowledges partial financial support from the General Program of National Natural Science Foundation of China (Grant No. 71874051), and Junsen Zhang acknowledges partial financial support from Major Program of the National Social Science Fund of China (Grant No. 20&ZD076). Corresponding author: Junsen Zhang.

E-mail: zhang.hongliang@zju.edu.cn (Zhang); jszhang@cuhk.edu.hk (Zhang); ning.zhang@economics.ox.ac.uk (Zhang)

Journal of the European Economic Association 2023 21(4):1647–1685

<https://doi.org/10.1093/jeea/jvad011>

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1. Introduction

Children's willingness to provide old-age support is of critical concern to parents and is also related to parents' intra-household resource allocation decisions. In his Nobel Lecture, Becker (1993) envisages that "[P]arents worried about old-age support may try to instill in their children feelings of guilt, obligation, duty, and filial love that ... can commit 'children' to help them out." Becker, Murphy, and Spenkuch (2016) formalize this idea theoretically and demonstrate that parents' inculcation of child preferences that induces greater old-age support from children can also increase parents' human capital investment on children. Two recent studies also provide empirical evidence for parental efforts spent in fostering children's altruism within the family to differ by the exogenous differences in parents' incentives for inequality aversion (Yi 2019) and old-age support (Guo and Zhang 2020).¹ Although both the theoretical predictions and empirical observations in this strand of research regarding endogenous parental practices of instilling children's willingness to support in old age depend on the *anticipated* effects of childhood experiences on adult behavior toward parents, empirical support for such connection remains lacking.

The existing empirical work on familial transfers mainly focuses on bequests and inter-vivos transfers from parents to children, whereas the limited research on children's old-age support to parents (e.g. Altonji, Hayashi, and Kotlikoff 1996; Cai, Giles, and Meng 2006) only investigates the effects of children's income or wealth. While Yi (2019) and Guo and Zhang (2020) identify and document exogenous differences in children's childhood experiences in family altruistic education, neither study is able to provide evidence on how such differences in childhood experiences affect adult transfer behavior as the majority of the children in their sample were teenagers who had never made any real familial transfers. To the best of our knowledge, the current paper is the first to investigate the empirical connection between children's childhood experiences and old-age support to parents in adulthood. By taking advantage of a rare social experiment of sending urban youth to the countryside during China's send-down movement (also known as the rustication program) and employing data on the affected urban twins, we are able to establish a relationship between childhood experiences and adult behavior toward parents by linking the difference in adult transfers to parents within the twin pair to their difference in send-down experience.

To understand the determinants of the within-twin difference in adult transfer behavior toward parents, we present a theoretical framework that highlights two intrinsic incentives for children's giving to parents: *pure altruism* and *warm glow*. Whereas the former comes from children's genuine concern about parents' welfare, the latter derives from their private benefits from the act of giving per se driven by various psychological underpinnings such as self-satisfaction, recognition, duty, and

1. Specifically, Yi (2019) documents that parental efforts in teaching children the importance of fraternal love increase with the within-twin difference in birth weight due to parents' preference for inequality aversion among children, whereas Guo and Zhang (2020) demonstrate that children's gender composition affects both parents' expectation of old-age support and inculcation of filial piety.

guilt. In the economics literature on charitable giving (e.g. Andreoni 1989, 1990; Bénabou and Tirole 2006; Tonin and Vlassopoulos 2010), these two incentives are considered as two alternative forms of altruism with the seemingly selfish warm-glow incentive also called as “impure altruism”. We follow this conceptualization and use impure altruism and warm glow interchangeably throughout this paper, and consider children’s altruism toward parents as also combining both “pure” and “impure” forms.

In addition to altruism, children’s giving to parents may also be strategically motivated out of the expectation for *future* rewards from parents induced by the act of giving, that is, *exchange motive* (e.g. Bernheim, Schleifer, and Summers 1985; Cox 1987; Laferrère and Wolff 2006; Porter and Adams 2016).² We therefore incorporate both forms of altruism and exchange motive in modeling children’s transfer behavior toward parents. In our model, when choosing a financial transfer to parents, a child faces a trade-off between the benefits of transfer through increasing parents’ welfare (pure altruism), joy of giving (impure altruism), and *anticipated* reciprocal rewards from parents induced by the giving (exchange motive) and the cost of transfer through reducing own consumption. The send-down experience can influence a child’s transfer decision through both the income channel by changing the child’s budget constraint and the altruism channel by changing the child’s valuations of parents’ welfare (pure altruism) and warm glow of giving (impure altruism). Although pure and impure altruism have different implications regarding the crowding-out effect of parents’ income on child’s transfer,³ they are empirically indistinguishable in the within-twin comparison because of the lack of variation in parents’ income across the twins. Nonetheless, they distinguish from the pecuniary exchange motive as they are both non-pecuniary and reflect children’s underlying willingness for giving to parents. In terms of soliciting old-age support from children concerned by parents, instilling in their children either form of altruism can induce greater amount of old-age support.

We find that among identical (monozygotic (MZ)) urban twins born between 1948 and 1961 who were exposed to the send-down movement, send-downs were less likely to make a monetary transfer to parents and also tended to transfer less than their non-send-down counterparts. We first rule out the possibility for the inferior transfer behavior of send-downs to be driven by any income disadvantage as they actually earned more than their non-send-down counterparts. Nonetheless, concerns still remain over whether the difference in transfer behavior between rusticated and non-rusticated MZ twins may be the result of the selection of family’s send-down choice in terms of children’s altruism endowment at the time.⁴ Because of difference in genetic altruism

2. It is worth noting that child reciprocating kindness shown *previously* by parents to relieve an obligation is an impure altruism incentive, whereas child anticipating parental reciprocity in *future* interactions induced by his/her giving is an exchange motive.

3. The former implies that an increase in the recipient’s income leads to a one-for-one decrease in the amount of transfer, whereas the latter does not. See Ottoni-Wilhelm, Vesterlund, and Xie (2017) for an empirical test that distinguishes pure altruism from warm glow in an experimental setting.

4. Even if MZ twins are indeed identical in their genetic altruism endowment at conception, different early-life experiences in altruism formation shocks may still lead to differences in their degree of altruism toward parents by the time of their family’s send-down choice.

endowment at conception, non-identical (dizygotic (DZ)) twins would have a larger difference in their altruism at adolescence than MZ twins. Therefore, if the inferior transfer behavior of send-downs between MZ twins is indeed driven by the selection of family choice rather than send-down experience, then we expect the extent of the selection to be even more severe between DZ twins, thereby implying an even larger gap in transfer behavior between rusticated and non-rusticated DZ twins. Instead, we find that the gap in transfer behavior by send-down status is always smaller for DZ twins than MZ twins, which shows no evidence for the existence of negative selection of the send-down choice in terms of children's altruism endowment. After ruling out the income and selection channel explanations, we therefore posit that the inferior transfer behavior of send-downs is driven by the adverse effect of the send-down experience on children's willingness to provide old-age support to parents, which could work through both pure and impure altruism.

In a closely related study, Li, Rosenzweig, and Zhang (2010) also exploit the difference in the rustication experience of Chinese twins but investigate instead the effects on parents' monetary transfers to children at marriage. They find that parents provided more wedding gifts to the child who had experienced more years of rustication and perceive it as guilt-induced parental behavior to compensate for the child's traumatic send-down experience. Our paper complements their study by examining the effect of send-down experience on familial transfers heading in the opposite direction. Although the two papers use the same dataset, explore the same historical movement, and employ a similar identification strategy, they have different focuses, and our study aims to provide empirical evidence on childhood experiences shaping children's altruism and old-age support to their parents in adulthood.⁵

This paper also contributes to two other strands of literature. The first examines the roles of early-life experiences in the endogenous formation of child preferences (e.g. Malmendier and Nagel 2011; Alesina and Giuliano 2015; Chen and Yang 2015). As parents have enormous control over their children's early experiences and environmental conditions, a strand of research investigates in particular how forward-looking parents may shape child preferences through their attitudes or choices: For example, endogenously instill work ethic into their children in react to welfare-state arrangements (Lindbeck and Nyberg 2006) and technology changes (Doepke and Zilibotti 2008), deliberately lower their children's childhood consumption to increase their patience in adulthood (Bhatt and Ogaki 2012), and rationally choose among different parenting styles according to the socioeconomic environment (Doepke and Zilibotti 2017). Moreover, some parental actions may not have a willful intent to affect child preferences but end up doing so unintentionally as exemplified by evidence of children's beliefs about women's role or ideal family size being influenced by their own family experiences (Fernández, Fogli, and Olivetti 2004; Fernández and Fogli 2006). By showing that the rustication experience during the adolescent years reduces

5. It is worth noting that parents' guilt feelings toward rusticated children could also be a reason that led to the inferior transfer behavior of send-downs if such guilt feelings had lowered parents' expectation for old-age support, which weakened rusticated children's warm glow from "doing their part".

children's old-age support to parents, our paper becomes the first in this strand of literature to demonstrate empirically that children's altruism toward parents is affected by their early-life experiences.

Second, our paper also contributes to a growing empirical literature that examines how the traumatic experiences from the Chinese Cultural Revolution influence the beliefs and behavior of the affected children. Comparing the beliefs of high school graduates just before and after the Cultural Revolution ended with the college admissions resumed, Roland and Yang (2017) find that the earlier cohorts who were deprived of access to universities due to the suspension of higher education had lower beliefs in the payoffs of effort but displayed higher levels of distrust toward the government. Gong, Lu, and Xie (2015) use the China Family Panel Studies (CFPS) data and employ a regression discontinuity approach to examine the effect of children's send-down experiences on non-cognitive skills as measured by locus of control. They find that rusticated children had a lower external locus of control compared with their non-rusticated counterparts and attribute such behavior to their experiences in adapting to adversity and independent living. Although they also report one set of results on attitudes toward family and relationship in Online Appendix (Table C.A) using respondents' rating of the value of family and relationship on a five-point scale, our study differs substantially from Gong, Lu, and Xie (2015) in focus, identification strategy, and measurement. While Gong, Lu, and Xie (2015) employ the regression discontinuity approach to examine the effect of send-down on children's non-cognitive skills based on self-reported answers to survey questions, we use the within-MZ twins estimation to investigate the effect of send-down on children's altruism toward parents based on actual monetary transfer to parents for old-age support.

The rest of this paper is organized as follows. Section 2 briefly describes the send-down movement and the employed dataset. Section 3 develops a theoretical framework to guide our empirical analysis. Section 4 introduces our empirical strategy. Section 5 presents our main empirical results. Section 6 discusses the findings of our robustness checks and some alternative explanations. Section 7 concludes the paper.

2. Background and Data

2.1. Background

Soon after the launch of the Cultural Revolution in August 1966, Mao Zedong initiated the large-scale rustication program of sending out urban educated youth to rural areas. As the most intensive political and social mobilization movement during the Cultural Revolution, this rustication program had profound effects on the life course of a generation of urban youth in China. Between 1967 and 1978, over 17 million junior and senior secondary school graduates in cities were sent to the countryside, who account for about one-third of the urban population in the affected 1948–1961 birth cohorts (Zhou and Hou 1999).

Although some young people were indeed inspired by the revolutionary propaganda and signed up for the program voluntarily (especially at the beginning of the

movement), the vast majority of rusticated youth were reluctant and forced to go to the countryside owing to the poverty and hardship associated with the move (Bernstein 1977; Chen et al. 2020). After Mao Zedong's directive on December 21, 1968 that urged educated young people to "*go to the countryside to be re-educated by the poor and lower-middle peasants*", the rustication program became a nationwide compulsory political movement with a top-down mobilization system to ensure compliance. At the top, the State Revolutionary Committee set rustication plans for the State Council every year. At the municipal level, the authorities distributed the quota on the basis of socio-political background. Specifically, children from bad-class origins (e.g. counter-revolutionaries, pre-liberation capitalists, and landlords) were among the priority group to be sent down, whereas those from good-class origins (e.g. revolutionary cadres, pre-liberation industrial workers, and peasants) might be given opportunities to be exempted (Chan, Rosen, and Unger 1980). At the bottom level, the children's schools, parents' work units, and residential organizations acted in concert to mobilize support for compliance with the rustication program (Rene 2013). During mobilization of this program, various ways of ensuring compliance were used, including forced transfer of residency, termination of the distribution of food ration tickets, and suspension of the parents' wages (Bonnin 2013).

The intensity of the rustication program varied considerably over time (Pan 2002). In the first 2 years of the movement (1967–1968), nearly two million urban youth were sent to the countryside. In accordance to Mao Zedong's December 21, 1968 directive, the number of rusticated children rose sharply to 2.7 million in 1969. A slowdown was reported in the early 1970s, as the annualized flow of rusticated children decreased to around 850,000 between 1970 and 1973. In 1974, the leadership renewed its interest in the movement, and the flow of rusticated children consequently more than doubled to an annualized average of over 1.7 million up until 1977. Some rules that governed the number of children in a family that can stay in the city were also imposed. In the early 1970s when the rate of rustication was low, if one sibling had already been sent to the countryside, then the other siblings can stay in the city. Meanwhile, in 1974 when the send-down movement was intensified, the rule was changed to allow only one sibling of each family to stay in the city (Bernstein 1977). The family-related selection rules were widely applied and forced many families to make a send-down choice among their multiple age-eligible children. In our sample of families with twins born between 1948 and 1961, one-third of these families sent one of their twins to the countryside while the other remained in the city.

The destination of send-down varies across families. According to the 2010 CFPS, 79% of send-down youths were sent to rural areas in the same province, whereas the rest were sent to a different province. Among those who were sent to a different province, 40% were sent to a province adjacent to their home provinces, whereas 60% were sent to a province far from their home province. Although most of urban youths were sent to rural areas in the same province, government rules prevented them from returning home or visiting their parents frequently. The send-down youths could apply to return home during important holidays, such as the spring festival, or in the case of a family emergency (Xu 1998). The majority of the urban youths were sent to rural villages,

collective farms, and state farms to conduct agricultural work. Although some worked as study counselors, agricultural technicians, barefoot doctors, and non-state-funded teachers, the majority worked as farmers (Gu 2009).⁶

Many send-downs considered themselves exiles who were forced to leave their homes and thereby abandoned both physically and morally. These send-downs also experienced difficult conditions in the countryside as they were required to do heavy manual labor in the fields. These rusticated youth stayed in rural areas for about 5–6 years on average before eventually returned to cities.⁷ By the time these children returned to cities (usually through job recruitment), most were already grown-ups and many lived in the dormitory of their work units, which means that they never returned to their family since their send-down. Previous studies show that the rustication experience has changed the preferences of rusticated children in adulthood, including their attitudes toward their families (Gong, Lu, and Xie 2015) and their risk aversion (Fan 2020). With respect to altruism within the family, both separation from the family and hardship of rural life could lead send-down experience to reduce children's altruism. On one hand, the early and prolonged separation from the family could weaken the family connections of send-downs, and consequently reduce their valuations of their parents' welfare. On the other hand, some rusticated children may view their send-down experience as a way of fulfilling their responsibility to their families and thus have smaller warm-glow incentives to support their families thereafter. Moreover, being (forced to be) sent down may create resentment of the rusticated children toward parents, thus reducing both their pure-altruism and warm-glow incentives for old-age support to parents in adulthood.⁸

Although the rusticated children had experienced extreme hardship in the countryside, send-down experience did not adversely affect their economic well-being after their return. This outcome is largely attributable to various government policies that were implemented to facilitate these children's transition to the urban labor force, the most significant of which counted the years spent in rural areas as work experience to determine their wage grades and promotion in the public sector (Zhou and Hou 1999). Given that the majority of the Chinese urban population of the affected 1948–1961 birth cohorts worked in the public sector, the send-downs had higher income than their non-send-down counterparts. Using the same dataset as we use in this study, Li, Rosenzweig, and Zhang (2010, Table 3) report a positive effect of send-down years on adult income among MZ twins. Using the 2010 CFPS and exploiting the discontinuity in the send-down probability across birth cohorts, Gong, Lu, and Xie (2015, Table 5) also find a positive send-down effect on adult income.

6. According to Chen et al. (2020), approximately 11.7% of the send-down youths were assigned to technical jobs from 1962 to 1972.

7. In both the CFPS data and the twins data used in this paper, more than 95% of send-down youths returned to cities after the send-down was over.

8. On one hand, resentment reduces a child's genuine concern over parents' welfare (pure altruism); on the other hand, resentment also reduces a child's senses of duty and guilt toward parents, thus lowering their private benefit from the act of giving to parents (impure altruism).

2.2. Data

The dataset used in this paper is the Chinese Adult Twins Survey (CATS), which was carried out by the Urban Survey Unit of the National Bureau of Statistics (NBS) in June and July 2002 in five Chinese cities: Chengdu, Chongqing, Harbin, Hefei, and Wuhan. This survey covers a wide range of socioeconomic information, including education, marriage, employment, income, and important life events, such as the send-down experience during the Cultural Revolution.⁹ Completed questionnaires were collected from 1,495 pairs of same-sex adult twins (2,990 respondents), including 919 pairs of MZ twins (1,838 individuals) and 576 pairs of DZ twins (1,152 individuals).

Since income plays a significant role in determining an adult child's transfer to elderly parents, we restrict our sample to families with both twins being in the labor force and having reported positive income at the time of the survey, thereby leaving us with a sample of 536 MZ twin pairs (1,072 individuals) and 336 DZ twin pairs (672 individuals).¹⁰ Moreover, given that rustication is a temporary social movement that affects only those cohorts who were attending secondary school between 1967 and 1978, we further restrict our main analysis sample to those twins who belonged to the affected cohorts (i.e. those who were born between 1948 and 1961).¹¹ With these criteria, we are left with a send-down cohort sample of 156 MZ twin pairs (312 individuals) and 56 DZ twin pairs (112 individuals).¹² Columns (1) and (2) of Table 1 report the summary statistics of the send-down cohort of MZ and DZ twins, respectively. The two types of twins have the identical mean age (45.6 years), and are similar in terms of the send-down proportion (50% for MZ twins vs. 48% for DZ twins), average years of schooling (11.4 years vs. 11.1 years), average annual income (10,524 yuan vs. 10,477 yuan), marriage rate (93.6% vs. 93.8%), spousal years of schooling (11.0 years vs. 11.1 years), and number of siblings (3.8 vs. 3.7). In the year

9. For a detailed description of the survey (see Li, Rosenzweig, and Zhang (2010)).

10. The CATS contains rich information on individual income, including labor income (salary or net income from business), capital income (rent, interests, and dividend), and other income (unemployment benefits). We make use of income information from each sub-category to calculate total income. Our results are robust to alternative income definitions such as only considering salary income.

11. Our results are robust to alternative send-down cohort definition as defined by those who were born between 1946 and 1961 (Li, Rosenzweig, and Zhang 2010).

12. It is worth noting that send-down experience has little effect on the chances that adult children were in the labor force and reported a positive income at the time of the survey. In the Online Appendix Table B1, we estimate a family fixed-effect (FE) regression of the dummy indicator of being in the labor force and reporting a positive income on the send-down dummy. For both MZ and DZ twins, the point estimate of the coefficient on the send-down dummy is small in magnitude (approximately 0.02) and statistically insignificant, showing little evidence that send-down experience affects the probability of working and earning an income to be able to provide financial support to elderly parents.

TABLE 1. Summary statistics.

Variables	Send-down cohort		Non-send-down cohort
	MZ twins (1)	DZ twins (2)	MZ twins (3)
Send-down proportion	0.503 (0.501)	0.482 (0.502)	n.a.
Send-down years (conditional on being sent down)	3.2 (2.7)	2.9 (1.3)	n.a.
Age	45.6 (3.4)	45.6 (3.5)	36.7 (2.7)
Schooling years	11.4 (2.7)	11.1 (2.3)	11.4 (3.3)
Having non-twin sibling(s)	0.920 (0.272)	0.911 (0.286)	0.777 (0.417)
Number of non-twin siblings	2.8 (1.6)	2.7 (1.7)	1.7 (1.4)
Father with high school degree or above	0.238 (0.427)	0.184 (0.389)	0.380 (0.486)
Mother with high school degree or above	0.185 (0.389)	0.122 (0.329)	0.217 (0.413)
Married	0.936 (0.245)	0.938 (0.243)	0.876 (0.330)
Spousal school years	11.0 (3.1)	11.1 (3.0)	11.1 (3.1)
Annual income, self-reported	10,524 (6,990)	10,477 (7,042)	10,151 (6,304)
Annual income, reported by the co-twin	11,338 (8,741)	9,697 (7,467)	10,971 (7,450)
Transfer incidence	0.436 (0.497)	0.580 (0.496)	0.518 (0.500)
Annual transfer amount (excluding zeros)	667 (644)	620 (698)	826 (879)
Annual transfer amount (including zeros)	291 (538)	360 (612)	428 (643)
Observations	312	112	282
Families	156	56	141

Note: For each variable denoted by the row heading, the table reports the mean for the sample defined by the column heading with the standard deviation in parentheses. The send-down cohort consists of all twin pairs born between 1948 and 1961 and the non-send-down cohort consists of all MZ twin pairs born between 1962 and 1970.

prior to the survey, 44% of MZ twins and 58% of DZ twins had made a transfer to their parents. Conditional on making a transfer, the average transfer amount is 667 yuan for MZ twins and 620 yuan for DZ twins, which accounts for approximately 6% of the adult children's annual income.¹³

13. To guide our estimated send-down effect on transfer amount against the excessive outliers, we top code the transfer amount at 3,000 yuan (i.e. the 99th percentile of the transfer amount distribution in our sample).

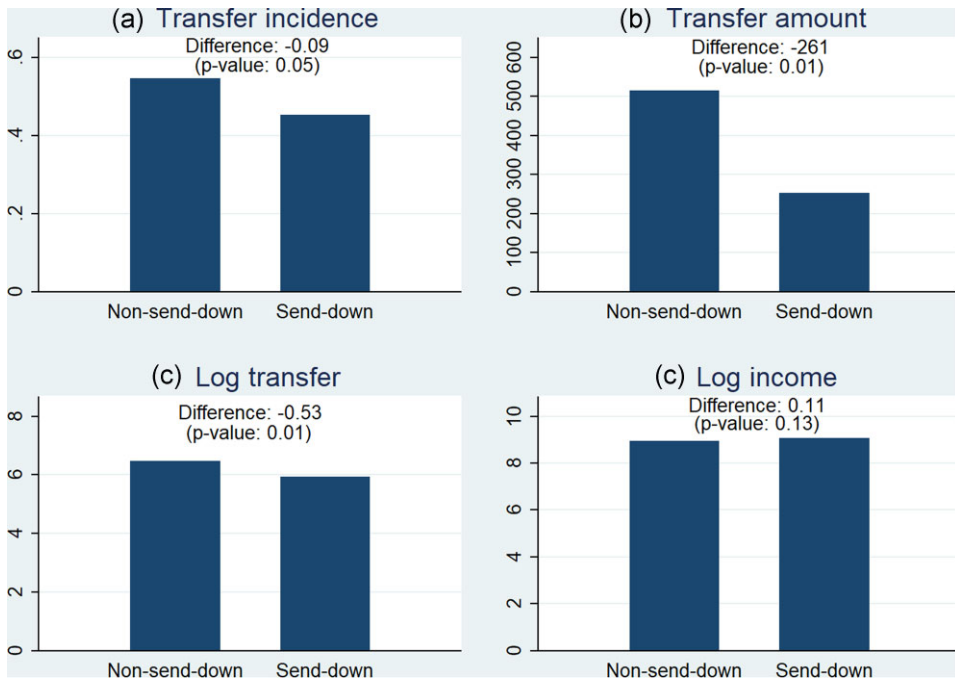


FIGURE 1. Transfer behavior, income and send-down status. The data are drawn from the subsample of send-down cohort of MZ twin pairs with only one twin sent down. Panel (c) further restricts the sample to both twins making a positive transfer to parents. The p -value reported in parentheses is for a one-tailed t -test of the matched twin pairs.

In addition to the main analysis sample of send-down cohort of MZ and DZ twins, we further employ a subsample of non-send-down cohort of MZ twins born between 1962 and 1970 to estimate the income effect on adult children's transfer behavior toward parents in the two-step procedure described in detail in Section 4.3. Column (3) of Table 1 reports the summary statistics of this subsample of non-send-down cohort of MZ twins. Compared with the send-down cohort sample, these non-send-down cohort MZ twins are younger, slightly less likely to be married, and have fewer siblings. However, in terms of income and the probability of making a positive transfer to parents, they are largely comparable with the send-down cohort, except that conditional on making a transfer they seem to tend to transfer somewhat a greater amount (826 yuan in column (3) vs. 667 yuan in column (1)).

Figure 1 presents a descriptive analysis of the differences in the transfer behavior and income by send-down status for a subsample of MZ twins from families with one twin sent down and the other staying in the city. Panel (a) shows that send-downs were 9 percentage points less likely to make a transfer to their parents than their non-send-down counterparts, and the difference is significant at the 5% level (in a one-tailed test). In terms of the overall transfer amount (including zeros), Panel (b)

shows that send-downs on average transferred 261 yuan less than their non-send-down counterparts, and the difference is significant at the 1% level. Conditional on both twins making a transfer to parents, the rusticated children transferred 41% (53 log points with a p -value of 0.01) less than their non-rusticated counterparts (Panel (c)). However, regardless of the measurement used, the gap in the transfer behavior of send-downs to their non-send-down counterparts is not due to any income disadvantage. Instead, Panel (d) indicates that, if anything, send-downs actually earned more (11 log points) than their non-send-down counterparts. Although the difference is not statistically different at conventional levels, it is quantitatively substantial and has a p -value of 0.13.

3. Theoretical Framework

In this section, we present a simple theoretical framework of an adult child's transfer decision to elderly parents that incorporates three motives underlying children's financial transfers to parents: pure altruism, warm glow, and exchange motive.

3.1. Model Setup

The basic unit in the model is a family consisting of a parent–adult child pair. The parent's utility, denoted by $V(c_p, s)$, depends on own numeraire-good consumption (c_p) and the service to the child (s). The first argument c_p is linked to the parent's income (y_p) and the child's transfer (t) such that $c_p = y_p + t$, whereas the second argument s is linked to the child's transfer (t) through a transfer–service relationship $s(t)$ with $s_t = ds/dt \geq 0$ and $s_{tt} = d^2s/dt^2 \leq 0$. We assume that parent's service to the child (s), for example, housekeeping and childcare is not fully fungible to the numeraire-good consumption. For the parent, providing such services does not directly reduce own consumption c_p but requires time and effort; for the child, the services offered by the parent cannot be perfectly substituted by those purchasable from the market because of the genuine quality differences. Moreover, we make the following functional form assumptions for $V(c_p, s)$: (i) it is increasing in c_p and decreasing in s , that is, $V_c = \partial V/\partial c > 0$ and $V_s = \partial V/\partial s < 0$; (ii) it is strictly concave in both of its arguments and has a non-positive cross-partial derivative, that is, $V_{cc} = \partial^2 V/\partial c^2 < 0$, $V_{ss} = \partial^2 V/\partial s^2 < 0$, and $V_{cs} = \partial^2 V/\partial c \partial s \leq 0$; and (iii) the parent enjoys a net utility gain from every dollar of transfer from the child even after taking into account the reciprocal reward induced by the giving, that is, $dV/dt = V_c + V_s s_t > 0$.

The adult child is endowed with income y_k , a weighting parameter δ denoting degree of pure altruism toward the parent, and a weighting parameter θ denoting degree of warm glow (impure altruism) toward parents, all of which are potentially affected by the send-down experience (r). When choosing the amount of transfer t to

the parent, the child maximizes the following utility function:

$$\begin{aligned} \max_t \quad & \{U(c_k, s) + \delta V(c_p, s) + \theta W(t)\} & (1) \\ \text{subj. to:} \quad & c_k = y_k - t, \\ & c_p = y_p + t, \\ & s = s(t), \text{ and} \\ & t \geq 0, \end{aligned}$$

where $U(c_k, s)$ denotes child's utility derived from his/her own numeraire-good consumption (c_k) and the service from the parent (s), and $W(t)$ denotes the warm-glow utility that the child derives directly from the giving. We assume that $U(\cdot, \cdot)$ is strictly increasing and strictly concave in both of its arguments, and the cross-partial derivative is non-negative (i.e. $U_z = \partial U / \partial z > 0$ and $U_{zz} = \partial^2 U / \partial z^2 < 0$ $\forall z \in \{c, s\}$, and $U_{cs} = \partial^2 U / \partial c \partial s \geq 0$), and $W(t)$ is strictly increasing and strictly concave (i.e. $W_t = dW/dt > 0$ and $W_{tt} = d^2W/dt^2 < 0$).

3.2. Transfer Decision

Let us first consider the child's transfer decision. Without loss of generality, we assume that the parent's reward to the child $s(0) = 0$ when $t = 0$.¹⁴ In this case, at the initial endowment point, the child's utility is $U(y_k, 0) + \delta V(y_p, 0) + \theta W(0)$. Taking its full derivative with respect to t yields the child's net marginal utility from the first dollar of transfer (denoted as NMU_t^o) as follows:

$$NMU_t^o = -U_c^o + U_s^o s_t^o + \delta(V_c^o + V_s^o s_t^o) + \theta W_t^o, \quad (2)$$

where superscript o indicates that the parameter is evaluated at the child's initial endowment point.

In equation (2), when deciding whether to make the first dollar of transfer to the parent, the child makes a trade-off between the benefits from the transfer through increasing reciprocal parental reward ($U_s^o s_t^o$), parents' welfare ($\delta(V_c^o + V_s^o s_t^o)$) and warm-glow utility (θW_t^o), and the cost of the transfer through reducing own consumption (U_c^o). The child's transfer decision rule is $t > 0$ if and only if $NMU_t^o > 0$, and $t = 0$ otherwise. Thus, the effect of rustication experience (r) on the child's transfer decision can be expressed by its effect on the child's net marginal utility of transfer at the initial endowment point, that is,

$$\frac{d NMU_t^o}{dr} = (-U_{cc}^o + U_{cs}^o s_t^o) \frac{dy_k}{dr} + \left((V_c^o + V_s^o s_t^o) \frac{d\delta}{dr} + W_t^o \frac{d\theta}{dr} \right). \quad (3)$$

14. In practice, $s(0)$ may be positive, which reflects the parent's voluntary reward to the child even without the latter's transfer.

Given equation (3), when a child’s rustication experience (r) is altered exogenously, which may change y_k , δ , and θ , the total effect on the child’s transfer decision is the sum of the effects through (i) *the income channel*

$$\left((-U_{cc}^o + U_{cs}^o s_t^o) \frac{dy_k}{dr} \right),$$

which potentially depends on the child’s exchange motive if c and s are complementary in determining the child’s utility (i.e. $U_{cs}^o > 0$), and (ii) *the altruism channel*

$$\left((V_c^o + V_s^o s_t^o) \frac{d\delta}{dr} + W_t^o \frac{d\theta}{dr} \right),$$

which combines the potential influences of the rustication experience on both the child’s pure and impure altruism toward parents.

3.3. Optimal Transfer Amount

Assuming $NMU_t^o > 0$ such that the child will make a transfer to the parent, then the optimal transfer amount is determined by the following first-order condition of equation (1):

$$-U_c + U_s s_t + \delta(V_c + V_s s_t) + \theta W_t = 0. \tag{4}$$

Applying the implicit function theorem¹⁵ to equation (4) yields the following comparative statics:

$$\frac{dt}{dr} = \frac{(-U_{cc} + U_{cs} s_t) \frac{dy_k}{dr} + \left((V_c + V_s s_t) \frac{d\delta}{dr} + W_t \frac{d\theta}{dr} \right)}{\Lambda}, \tag{5}$$

where $\Lambda = -U_{cc} + 2U_{cs} s_t - U_{ss} s_t^2 - U_s s_{tt} - \delta(V_{cc} + 2V_{cs} s_t + V_{ss} s_t^2 + V_s s_{tt}) - \theta W_{tt} > 0$. Similar to equation (3), a child’s rustication experience also influences the optimal transfer amount to the parent through the income

$$\left((-U_{cc} + U_{cs} s_t) \frac{dy_k}{dr} \right) \text{ and altruism channels } \left((V_c + V_s s_t) \frac{d\delta}{dr} + W_t \frac{d\theta}{dr} \right).$$

4. Empirical Strategy

We now turn to our empirical strategy to estimate the effect of childhood send-down experience on adult transfer behavior toward parents using data on urban twins in

15. We treat variable r (rustication experience) as a continuous variable in the model for ease of illustration of the comparative statics results. However, our conclusions still hold if we treat r as a discrete variable. Milgrom and Shannon (1994) and Ashworth and Bueno de Mesquita (2006) have developed a powerful yet simple method for deriving comparative statics conclusions for a wide variety of models, including discrete choice variable cases. The method, namely, “monotone comparative statics”, is an analogy to comparative statics with respect to continuous variables.

China. In Section 4.1, we set up an empirical framework that relates the within-twin difference in transfer to parents to the within-twin differences in income, send-down experience, and degree of altruism (pure and impure forms combined) at the time of the send-down choice. We show that if the send-down choice within a family is correlated with the children's degree of altruism at the time, then a multivariate regression yields a biased estimate of the send-down effect on children's willingness for giving to parents because of the existence of correlation and selection bias terms. In Section 4.2, we propose a two-step estimation strategy that eliminates the correlation bias by netting out the income effect via an estimate obtained from a cohort of MZ twins who are not affected by the send-down movement, and show that the remaining difference in transfer across the twins results from the combined effects of the differences in their send-down experience and degree of altruism at the time of the send-down choice. In Section 4.3, we take advantage of the fact that MZ twins are genetically more similar than DZ twins in terms of altruism endowment, and show that a comparison between the MZ and DZ estimates can determine the sign of the selection bias term.

4.1. Setup

Equations (3) and (5) in Section 3 identify income and altruism as the two channels through which childhood send-down experience may affect an adult child's transfer toward his/her parents. Following these insights from the theoretical framework, we assume that a child's transfer to parents t_{ij} depends linearly on his/her income Y_{ij} , the latent degree of *combined pure and impure altruism* (hereafter "altruism") toward parents δ_{ij} , which is normalized to have the same measurement unit as t_{ij} , a family FE accounting for any family-level observed and unobserved determinants of child's transfer to parents μ_j , and an independently and identically distributed (*i.i.d.*) disturbance term ξ_{ij} . Thus, the transfer equation for child i from family j can be written as follows:

$$t_{ij} = \rho Y_{ij} + \delta_{ij} + \mu_j + \xi_{ij}. \quad (6)$$

For ease of illustration, equation (6) imposes two further restrictions in addition to linearity regarding the income effect on child's transfer to parents. First, a child's transfer to parents depends solely on his own income but not the co-twin's. Second, the income coefficient ρ is unaffected by a child's send-down experience. In the Appendix, we consider the relaxation of the former assumption, allowing for the cross-twin income effect on transfer to parents. In Online Appendix, we consider the relaxation of the latter assumption, allowing for heterogeneous income coefficients between the send-down and non-send-down children.

Taking the within-twin difference of (6) eliminates the family FE μ_j and yields the following first-difference equation:

$$\Delta t_j = \rho \Delta Y_j + \Delta \delta_j + \Delta \xi_j, \quad (7)$$

where Δ is an operator of the within-twin difference.

Let D_{ij} be a dummy indicator for the send-down treatment of child i in family j (i.e. corresponding to r in the model), and let e_{ij} denote the degree of altruism of child i in family j at the time of the send-down choice. We specify the following linear equation that relates a child's degree of altruism in adulthood $\delta_{ij} - D_{ij}$, e_{ij} , and an *i.i.d.* random shock ζ_{ij} :

$$\delta_{ij} = \beta D_{ij} + e_{ij} + \zeta_{ij}. \quad (8)$$

Taking the first difference of equation (8) within the twin pair and substituting that first-difference equation for $\Delta\delta_j$ in equation (7) yields the main empirical specification of this paper:

$$\Delta t_j = \rho \Delta Y_j + \beta \Delta D_j + \Delta \varepsilon_j, \quad (9)$$

where $\Delta \varepsilon_j = \Delta e_j + \Delta \zeta_j + \Delta \xi_j$, ρ represents the effect of the child's income on transfer to parents given altruism, and β (our main parameter of interest) represents the effect of send-down experience on the child's transfer to parents through the altruism channel given income.

Similar as the income effect, equation (8) assumes that a child's send-down experience only affects his/her own altruism but not the co-twin's. In the Appendix, we present an extended empirical model allowing for cross-twin send-down effects on altruism and cross-twin income effects on transfer to parents. With both cross-twin effects, the main empirical specification, equation (A.5), still links the within-twin difference in transfer to parents (Δt_j) to the within-twin differences in income (ΔY_j) and send-down experience (ΔD_j) as equation (9), although the coefficients carry different interpretations. Specifically, the income coefficient of ΔY_j in equation (A.5) corresponds to the *net* effect (i.e. own minus cross effect) of income given altruism, and the send-down coefficient of ΔD_j corresponds to the *net* effect of send-down through the altruism channel given income. As the two equations are empirically indistinguishable, our estimates of the income and send-down coefficients are applicable to both specifications but carry different interpretations: They correspond to *own* income and send-down effects when no cross-twin effects exist but *net* income and send-down effects when cross-twin effects exist.¹⁶

4.2. Unconditional and Conditional Estimates of the Send-Down Effect

Denote $\hat{\beta}_u$ the unconditional estimate of equation (9) that ignores the within-twin income difference ΔY_j and $\hat{\beta}_c$ the conditional estimate that accounts for it. With ζ_{ij} and ξ_{ij} both being *i.i.d.* random shocks, we derive in equations (B1) and (B2) in the

16. It is worth noting that with the cross-twin effects, the within-twin estimates actually identify the net effect of the *relative* send-down treatment on the rusticated twins compared with their non-rusticated counterparts for the families in which one of the twins was sent to the countryside but the other remained in the city.

Online Appendix the following formulas for these two estimators:

$$\text{plim}\hat{\beta}_u = \beta + \frac{\sigma_{\Delta D \Delta Y}}{\sigma_{\Delta D}^2} \rho + \frac{\sigma_{\Delta D \Delta e}}{\sigma_{\Delta D}^2} \quad (10)$$

and

$$\text{plim}\hat{\beta}_c = \beta - \frac{\sigma_{\Delta D \Delta Y}}{\sigma_{\Delta D}^2 \sigma_{\Delta Y}^2 - \sigma_{\Delta D \Delta Y}^2} \sigma_{\Delta Y \Delta e} + \frac{\sigma_{\Delta Y}^2}{\sigma_{\Delta D}^2 \sigma_{\Delta Y}^2 - \sigma_{\Delta D \Delta Y}^2} \sigma_{\Delta D \Delta e}, \quad (11)$$

where $\sigma_{\Delta D}^2 = \text{var}(\Delta D_j)$, $\sigma_{\Delta Y}^2 = \text{var}(\Delta Y_j)$, $\sigma_{\Delta D \Delta Y} = \text{cov}(\Delta D_j, \Delta Y_j)$, $\sigma_{\Delta D \Delta e} = \text{cov}(\Delta D_j, \Delta e_j)$, and $\sigma_{\Delta Y \Delta e} = \text{cov}(\Delta Y_j, \Delta e_j)$.

The formulas for $\hat{\beta}_u$ and $\hat{\beta}_c$ in equations (10) and (11) deserve some special remarks.

- (1) The unconditional estimate $\hat{\beta}_u$ in equation (10) is subject to two potential sources of bias. The first is an omitted variable bias arising from the omission of ΔY_j in the unconditional estimation.¹⁷ The second is a selection bias that exists if the send-down choice is correlated with children's degree of altruism at the time (i.e. $\sigma_{\Delta D \Delta e} \neq 0$). In the absence of the selection bias, the unconditional estimate $\hat{\beta}_u$ identifies the gross effect of send-down experience on a child's transfer to parents through both the altruism and income channels.
- (2) Despite the inclusion of ΔY_j , the conditional estimate $\hat{\beta}_c$ in equation (11) still contains two bias terms. The first bias term, which we refer to as *the correlation bias*, arises from the correlation of ΔY_j with both ΔD_j and Δe_j . The second bias term, which we refer to as *the selection bias*, is attributable to the correlation between ΔD_j and Δe_j . Only when the degree of altruism at the time of the send-down decision is uncorrelated with both income and send-down choice (i.e. $\sigma_{\Delta D \Delta Y} = 0$ and $\sigma_{\Delta D \Delta e} = 0$) will a multivariate regression of Δt_j on ΔY_j and ΔD_j yield a consistent estimate for β .

4.3. Two-Step Estimation Procedure

The within-twin difference in degree of altruism at the time of the send-down choice Δe_j may result from either the difference in altruism endowment at birth (if any) or the difference in childhood experiences in altruism formation shocks. Even if MZ twins are indeed identical in their genetic altruism endowment at conception, the zero correlation assumption for $\sigma_{\Delta D \Delta e}$ may still be invalidated if the send-down choice within a family is correlated with the children's early-life altruism formation. When this is indeed the case (i.e. $\sigma_{\Delta D \Delta e} \neq 0$), $\hat{\beta}_c$ contains not only the selection bias term but also the correlation bias term because a non-zero $\sigma_{\Delta D \Delta e}$ transmits to a non-zero $\sigma_{\Delta Y \Delta e}$ when ΔD_j and ΔY_j are correlated with each other as we document in Panel (d) of Figure 1. While the former bias term is a direct result of the selection of the send-down choice in terms of the children's degree of altruism at the time (i.e. $\sigma_{\Delta D \Delta e} \neq 0$),

17. With $\rho > 0$, this omitted variable bias has the same sign as $\sigma_{\Delta D \Delta Y}$.

the latter bias term is an indirect result of such selection for it also leads to a biased estimate $\hat{\rho}_c$ for ρ as shown in equation (B3) in the Online Appendix, using which to account for the income effect will attribute part of the unaccounted income effect (i.e. $(\rho - \hat{\rho}_c)\Delta Y_j$) to ΔD_j when $\sigma_{\Delta D \Delta Y} \neq 0$.

Our strategy to eliminate the correlation bias in $\hat{\beta}_c$ from the multivariate regression of equation (9) is to apply a two-step estimation strategy, which follows the spirit of Angrist and Krueger (1994), Altonji, Elder, and Taber (2005), Das et al. (2013), and Van Kippersluis and Rietveld (2018). In the first step, we employ a subsample of non-send-down cohort of MZ twins born between 1962 and 1970 who were not exposed to the send-down treatment in order to estimate the income effect on transfer that is unaffected by the potential selection of the send-down choice in children's altruism. Given that $D_{1j} = D_{2j} = 0$ for all families in this sample, the term ΔD_j is eliminated from equation (9). A simple regression of Δt_j on ΔY_j yields an unbiased estimate of ρ if the difference in income between these MZ twins is uncorrelated with the difference in altruism. However, the income information is self-reported in our survey and is subject to measurement errors. Given that the income of the twins are highly correlated with each other, the measurement error problem is magnified in the within-twin income difference ΔY_j and can result in a severe attenuation bias in the estimated coefficient of the income effect on transfer. Nonetheless, from the survey, we not only have self-reported income Y_{ij} but also the cross-reported income by the co-twin, which is denoted by \tilde{Y}_{ij} .¹⁸ Therefore, following Ashenfelter and Krueger (1994), we employ an instrumental variable (IV) approach to use the difference in the cross-reported income $\Delta \tilde{Y}_j$ as an instrument for the difference in self-reported income ΔY_j to obtain an IV estimate of the income effect on transfer $\hat{\rho}_{iv}$ for this sample of non-send-down cohort of MZ twins that is not contaminated by either the selection of send-down choice in children's altruism or the measurement error in income.¹⁹

In the second step, we use the IV estimate $\hat{\rho}_{iv}$ obtained from the non-send-down cohort of MZ twins to net out the income effect for the send-down cohort of twins and regress the remaining within-twin difference in transfer $(\Delta t_j - \hat{\rho}_{iv} \Delta Y_j)$ on the difference in send-down experience (ΔD_j) as follows:

$$\Delta t_j - \hat{\rho}_{iv} \Delta Y_j = \beta \Delta D_j + \Delta \varepsilon_j, \quad (12)$$

18. The correlation coefficient between the self- and cross-reported incomes is 0.72 in our data.

19. Specifically, denoting Y_j^k twin k 's report of twin j 's income and assuming the classical measurement error, we use $Y_1^2 - Y_2^1$ as an instrument for $Y_1^1 - Y_2^2$. The identification assumption for this IV approach is that the measurement errors in $Y_1^2 - Y_2^1$ and $Y_1^1 - Y_2^2$ are uncorrelated. We call this estimator IV-1. However, the IV-1 estimate may still be biased if the measurement errors in $Y_1^2 - Y_2^1$ and $Y_1^1 - Y_2^2$ are correlated, which could happen if an individual-specific component of the measurement error exists in the reported income. For example, a twin who overreports his/her own income also tends to overreport his/her co-twin's income. To eliminate the individual-specific component of the measurement error in the estimation, an alternative approach is to use $Y_1^1 - Y_2^1$ as the regressor and $Y_1^2 - Y_2^2$ as the IV. We call this estimator IV-2. In this study, the two IV approaches yield similar estimates. Thus, we present only the results using IV-1 for ease of illustration.

where $\Delta\varepsilon_j = \Delta e_j + \Delta\zeta_j + \Delta\xi_j$ as defined for equation (9). Denote the estimate of the partitioned regression of equation (12) by $\hat{\beta}_p$, where subscript p indicates that the income effect has been partialled out by $\hat{\rho}_{iv}$ obtained from an auxiliary regression. When $\hat{\rho}_{iv}$ is a consistent estimate of ρ , the sampling error in $\hat{\rho}_{iv}$, or the measurement error in ΔY_j only leads to classical measurement error in the dependent variable $\Delta t_j - \hat{\rho}_{iv}\Delta Y_j$, which is uncorrelated with ΔD_j . Therefore, we have

$$plim\hat{\beta}_p = \beta + \frac{\sigma_{\Delta D\Delta e}}{\sigma_{\Delta D}^2}. \quad (13)$$

Although assuming that the within-MZ-twin difference in the observed determinant is orthogonal to the within-MZ-twin difference in the unobserved determinant, that is, $cov(\Delta Y_j, \Delta\delta_j) = 0$ among the non-send-down cohort of MZ twins in the context concerned here is a standard identification assumption imposed in the empirical twins literature (Rosenzweig and Wolpin 2000), one may still be concerned about potential violations of this assumption leading to a biased estimate $\hat{\rho}_{iv}$ for the income coefficient. Moreover, even if $\hat{\rho}_{iv}$ indeed yields an unbiased estimate of the income coefficient for the non-send-down cohort subsample used in the first-step estimation, the propensity to transfer to elderly parents may vary over the life cycle and therefore differ systematically between the send-down cohort and non-send-down cohort. To address both concerns, we employ income coefficient ρ^* taking values in a wide range of parameter space $[0, \bar{\rho}]$ in which we believe the true income coefficient falls, and estimate a series of partitioned regressions of $\Delta t_j - \rho^*\Delta Y_j$ on ΔD_j to check the sensitivity of estimates $\hat{\beta}_p$ to the values of ρ^* used to partial out the income effect. The results are shown in Figure 2 and discussed in Section 6.1.1. The sign of the partitioned estimate $\hat{\beta}_p$ —and for most cases, the statistical significance as well—is not sensitive to the value of the income coefficient used.

4.4. Comparison between the MZ and DZ Estimates

In the partitioned estimate $\hat{\beta}_p$ in equation (13), although the correlation bias term is eliminated, the selection bias term remains. Following prior twins studies (e.g. Li, Rosenzweig, and Zhang 2010), we consider in this subsection the sign of the selection bias term in equation (13) by comparing the MZ estimate $\hat{\beta}_{p,MZ}$ and the DZ estimate $\hat{\beta}_{p,DZ}$. Given that MZ twins share 100% of their genes whereas DZ twins share only 50% (Plomin et al. 2003), DZ twins are genetically far more different than MZ twins. To the extent that the genetic differences in endowment between the twins persist to adulthood, we would expect a greater variation in the adulthood outcomes between DZ twins than between MZ twins. Using the sample of the non-send-down cohort of twins not exposed to the rustication program, Table B2 in the Online Appendix shows that the within-twin differences in adult height, years of schooling, income, the incidence of making a positive transfer to parents, and the transfer amount are all greater (in terms of absolute values) between DZ twins than between MZ twins, suggesting that genetic differences in endowment in conception indeed persist to adulthood.

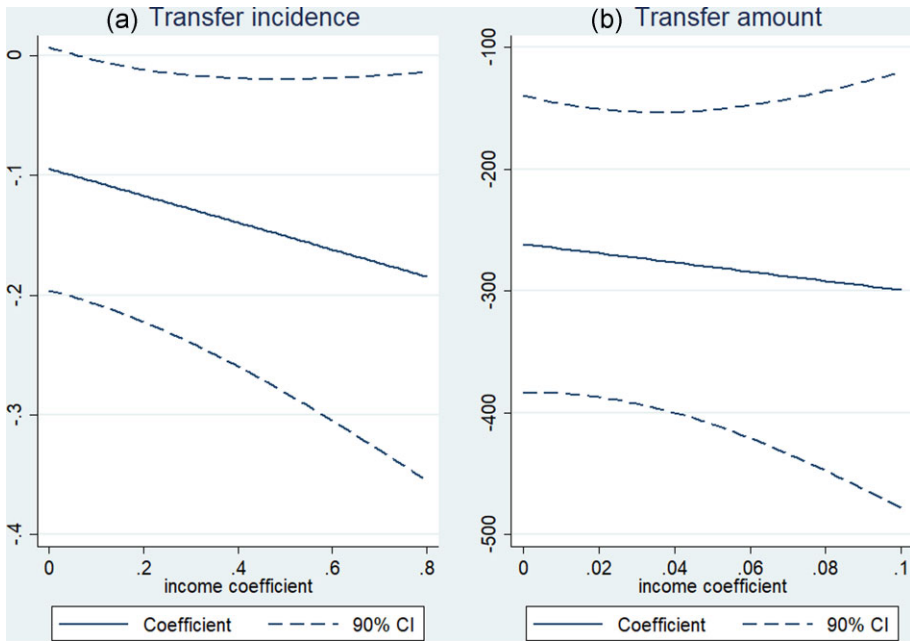


FIGURE 2. Sensitivity of the estimates of the send-down effect. The figure displays the results of a sensitivity analysis of the estimates $\hat{\beta}_p$ for transfer incidence (Panel (a)) and transfer amount (Panel (b)) with respect to different values of the income coefficient ρ used. The solid line is the coefficient on the rustication dummy, whereas the dash lines are the 90% confidence intervals.

Given the consistent empirical evidence demonstrated in Online Appendix Table B2, we would also expect the variation in the degree of altruism between DZ twins to be greater than that between MZ twins by the time of the send-down choice, that is, $\sigma_{\Delta e, DZ}^2 > \sigma_{\Delta e, MZ}^2$. Therefore, as long as the send-down selection in children’s altruism between DZ twins has the same sign as the selection between MZ twins yet is larger in magnitude, that is, $(\sigma_{\Delta D \Delta e, DZ} / \sigma_{\Delta D \Delta e, MZ}) > 1$, a comparison between $\hat{\beta}_{p, MZ}$ and $\hat{\beta}_{p, DZ}$ can sign the selection bias term.

To see this, take the difference between $\hat{\beta}_{p, DZ}$ and $\hat{\beta}_{p, MZ}$ from equation (13) yields:

$$\begin{aligned}
 plim \hat{\beta}_{p, DZ} - plim \hat{\beta}_{p, MZ} &= \frac{\sigma_{\Delta D \Delta e, DZ}}{\sigma_{\Delta D, DZ}^2} - \frac{\sigma_{\Delta D \Delta e, MZ}}{\sigma_{\Delta D, MZ}^2} \\
 &= \frac{\sigma_{\Delta D \Delta e, MZ}}{\sigma_{\Delta D, MZ}^2} \left(\frac{\sigma_{\Delta D \Delta e, DZ}}{\sigma_{\Delta D \Delta e, MZ}} \frac{\sigma_{\Delta D, MZ}^2}{\sigma_{\Delta D, DZ}^2} - 1 \right) \\
 &\approx \frac{\sigma_{\Delta D \Delta e, MZ}}{\sigma_{\Delta D, MZ}^2} \left(\frac{\sigma_{\Delta D \Delta e, DZ}}{\sigma_{\Delta D \Delta e, MZ}} - 1 \right). \tag{14}
 \end{aligned}$$

In the last line of equation (14), we use the property $\sigma_{\Delta D, MZ}^2 \approx \sigma_{\Delta D, DZ}^2$ as shown in the summary statistics in Table 1. Given that $\sigma_{\Delta e, DZ}^2 > \sigma_{\Delta e, MZ}^2$ and $\sigma_{\Delta D, DZ}^2 \approx \sigma_{\Delta D, DZ}^2$, a sufficient condition for $(\sigma_{\Delta D \Delta e, DZ} \sigma_{\Delta D \Delta e, MZ}) > 1$ is that the magnitude of the correlation coefficient between ΔD and Δe for DZ twins is no smaller than that for MZ twins (i.e. $(\rho_{\Delta D \Delta e, DZ} \rho_{\Delta D \Delta e, MZ}) \geq 1$), or in other words, the selection of parents' send-down choice in terms of children's altruism endowment at the time is equally or more sensitive to the larger variation in Δe between DZ twins than the smaller variation in Δe between MZ twins. With $(\sigma_{\Delta D \Delta e, DZ} \sigma_{\Delta D \Delta e, MZ}) > 1$, we have $\text{sign}(\text{plim} \hat{\beta}_{p, DZ} - \text{plim} \hat{\beta}_{p, MZ}) = \text{sign}(\sigma_{\Delta D \Delta e, MZ})$, that is, the sign of $(\hat{\beta}_{p, DZ} - \hat{\beta}_{p, MZ})$ identifies the sign of the selection in the send-down choice in children's altruism. Therefore, $\hat{\beta}_{p, DZ} > \hat{\beta}_{p, MZ}$ indicates a positive selection bias in both estimates, with $\hat{\beta}_{p, MZ}$ yielding a tighter upper bound for β , $\hat{\beta}_{p, DZ} < \hat{\beta}_{p, MZ}$ indicates a negative selection bias in both estimates with $\hat{\beta}_{p, MZ}$ yielding a tighter lower bound for β , and $\hat{\beta}_{p, MZ} = \hat{\beta}_{p, DZ}$ indicates no selection bias with $\hat{\beta}_{p, MZ}$ yielding a consistent estimate of β .

5. Main Results

In this section, we present our main empirical results on the effect of the send-down on children's transfer incidence (Section 5.1), transfer amount (Section 5.2), and transfer distribution (Section 5.3). In each subsection, we start by estimating the gross effect of the send-down while combining both the income and altruism channels and then employ various ways to control for the income channel to isolate the effect through the altruism channel.

5.1. Transfer Incidence

Table 2 employs a dummy indicator for making a positive transfer to parents in the year prior to the survey as the dependent variable to examine the effect of rustication on a child's transfer behavior at the extensive margin. Panel A presents the FE estimates of the within-twin specification in equation (9) for MZ twins. Column (1) reports the unconditional estimate that ignores the within-twin difference in income. The point estimate indicates that the send-downs were 9.4 percentage points less likely to make a transfer to parents. Although not significant at conventional levels (p -value: 0.130), this coefficient can still reject the null hypothesis $\beta \geq 0$ at the 10% level in a one-tailed test. When the send-down choice within the MZ twin pairs is not selective in altruism endowment, this unconditional estimate would identify the gross effect of rustication on transfer incidence through both the income and altruism channels. In Appendix Table A.1, we employ a sample of non-send-down cohort of MZ twins to estimate the effect of income on children's transfer behavior toward parents. For transfer incidence, both the FE (column (1)) and FE-IV (column (2)) estimates—the

TABLE 2. Send-down and transfer incidence.

	Unconditional		Conditional	
	FE (1)	FE (2)	FE-IV (3)	FE, two-step (4)
Panel A: MZ twins				
Rustication	-0.094 [†] (0.062)	-0.099 [†] (0.062)	-0.115* (0.065)	-0.129* (0.068) [0.024]
Log income		0.040 (0.045)	0.183* (0.095)	
Observations	312	312	312	312
Families	156	156	156	156
Panel: DZ twins				
Rustication	-0.062 (0.14)	-0.066 (0.14)	-0.059 (0.14)	-0.124 (0.148) [0.240]
Log income		0.020 (0.105)	-0.016 (0.180)	
Observations	112	112	112	112
Families	56	56	56	56

Note: The sample is restricted to the send-down cohort of MZ and DZ twins. Panel A employs the MZ twins sample, whereas Panel B employs the DZ twins sample. In columns (1)–(3), the dependent variable is a dummy variable that equals to 1 if the adult child made a positive transfer to parents in the year prior to the survey year and equals to 0, otherwise. In column (4), the dependent variable is the residual equal to (transfer incidence $- \hat{\rho}_{iv}$ * log income), where the income coefficient $\hat{\rho}_{iv}$ is taken from column (2) of Appendix Table A.1. Robust standard errors are reported in parentheses. For column (4), enclosed in brackets are the p -values of the one-tailed test $\beta \geq 0$ calculated using a bootstrap procedure described in Online Appendix D that accounts for the estimation error in the income coefficient in the first stage. * $p < 0.1$, ** $p < 0.05$, and [†] $p < 0.15$.

latter of which uses the cross-reported income by the co-twin as an instrument—are positive and significant. Given that send-down experience is positively correlated with income (Panel (d) of Figure 1), the gross effect estimated in column (1) in Panel A of Table 2 herein yields an upper-bound estimate for the altruism channel effect (or a lower-bound estimate in magnitude for the adverse effect).

In columns (2)–(4) in Panel A of Table 2, we explore different ways to account for the income effect in order to isolate the effect through the altruism channel. In column (2), we control directly the logarithm of self-reported income. The estimated income coefficient is very small (0.040) and insignificant, and the conditional estimate $\hat{\beta}_c$ (-0.099) differs only slightly from the unconditional estimate $\hat{\beta}_u$ (-0.094). However, as discussed in Section 4.3, the measurement error problem in self-reported income is magnified in the FE estimation. To address this problem, we use the cross-reported income by the co-twin as an instrument for own reported income in column (3). The IV estimate of the income coefficient more than quadruples to 0.183 and also becomes statistically significant. Consequently, the IV estimate of the coefficient on the rustication dummy $\hat{\beta}_{iv}$ (-0.115) becomes larger in magnitude and statistically more significant (p -value: 0.076). Although the IV estimate of the income coefficient

in column (3) is not subject to attenuation bias due to measurement errors, such coefficient could still be biased if the send-down choice within the MZ twin pairs is correlated with the within-twin difference in degree of altruism at the time.²⁰ To address this potential concern, we use $\hat{\rho}_{iv}$ obtained from the subsample of the non-send-down cohort of MZ twins (column (2) of Appendix Table A.1) to net out the income effect on transfer incidence and then employ the residual as the dependent variable. The results of this two-step estimation are reported in column (4). The coefficient on the rustication dummy (-0.129) is very similar to that obtained from the IV estimate in column (3) (-0.115) and is also significant at the 10% level. It is worth noting that the standard error of the send-down coefficient from the two-step estimation in column (4) does not account for the variability in the income coefficient $\hat{\rho}_{iv}$ estimated in the first step. To address this problem, we adopt a bootstrap procedure, documented in detail in Online Appendix, by resampling with replacement the pooled dataset of the non-send-down cohort and send-down cohort such that the second-step estimation of the send-down coefficient $\hat{\beta}_p$ also reflects the variation in the income coefficient $\hat{\rho}_{iv}$ estimated in the first step. The p -value of the one-tailed test for $\beta \geq 0$ calculated by repeating the bootstrapped procedure 500 times is 0.024 (reported in the brackets in column (4)), showing that the estimated negative send-down coefficient (-0.129) remains significant after taking into account the variability associated with the income coefficient estimated in the first step. The persistent estimates of a negative coefficient on the rustication dummy under various ways of controlling for the income effect indicate that the send-downs indeed behave less altruistically in providing old-age support to parents compared with their non-send-down MZ co-twins.

Nonetheless, whether the lower degree of altruism of send-downs results from the send-down treatment (i.e. $\beta < 0$) or the selection of the send-down choice in altruism endowment (i.e. $\sigma_{\Delta D \Delta e} < 0$) remains unknown. As discussed in Section 4.4, the send-down selection in children's altruism, if any, tends to be more severe among DZ twins than MZ twins given that the former also differs in terms of genetic altruism endowment at birth. Therefore, if $\sigma_{\Delta D \Delta e}$ is indeed negative, then we expect $\hat{\beta}_{p,DZ} < \hat{\beta}_{p,MZ}$ when a consistent estimate of the income coefficient is used to net out the income effect in the two-step estimation. In Panel B of Table 2, we replicate the same estimations in Panel A for DZ twins. Given that the DZ twins sample is only about one-third the size of the MZ twins sample, the coefficients are much less precisely estimated in Panel B. Nonetheless, the within-DZ estimates of the coefficient on the rustication dummy are always negative, yet smaller in magnitude than the corresponding within-MZ estimates. Specifically, when the same income coefficient is used to account for the income effect in column (4), we find $\hat{\beta}_{p,DZ} > \hat{\beta}_{p,MZ}$, showing no evidence for any negative selection in the send-down choice in children's altruism endowment.

20. See equation (B3) in the Online Appendix.

TABLE 3. Send-down and transfer amount.

	Unconditional		Conditional	
	FE (1)	FE (2)	FE-IV (3)	FE, two-step (4)
Panel A: MZ twins				
Rustication	-261.5*** (74.5)	-268.6*** (72.3)	-270.9*** (72.6)	-275.0*** (74.1) [0.006]
Income		0.019*** (0.006)	0.025*** (0.009)	
Observations	312	312	312	312
Families	156	156	156	156
Panel B: DZ twins				
Rustication	-109.1 (106.0)	-121.5 (102.1)	-129.3 (104.6)	-137.3 (108.9) [0.130]
Income		0.016** (0.007)	0.026* (0.015)	
Observations	112	112	112	112
Families	56	56	56	56

Note: The sample is restricted to the send-down cohort of MZ and DZ twins. Panel A employs the MZ twins sample, whereas Panel B employs the DZ twins sample. In columns (1)–(3), the dependent variable is the transfer amount. In column (4), the dependent variable is the residual that is equal to (transfer amount $- \hat{\rho}_{iv}$ * income), where the income coefficient $\hat{\rho}_{iv}$ is taken from column (4) of Appendix Table A.1. Robust standard errors are reported in parentheses. For column (4), enclosed in brackets are the p -values of the one-tailed test $\beta \geq 0$ calculated using a bootstrap procedure described in Online Appendix D that accounts for the estimation error in the income coefficient in the first stage. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

5.2. Transfer Amount

We now turn to the effect of rustication on the monetary amount of children's transfer to parents. Panel A of Table 3 presents the FE estimates for MZ twins. The unconditional estimate in column (1) indicates that on average the send-down treatment reduces a child's transfer amount to parents by 262 yuan, significant at the 1% level. In columns (2)–(4), different estimates of the propensity to transfer (i.e. income coefficient) are used to account for the income effect. The resulting conditional estimates are quantitatively similar to the unconditional estimate, yet slightly larger in magnitude. Specifically for column (4), the two-step estimation that nets out the income effect by using the income coefficient obtained from the subsample of the non-send-down cohort of MZ twins (column (4) of Appendix Table A.1) indicates that rustication reduces a child's transfer amount to parents by 275 yuan after accounting for the income effect, also significant at 1% level. The p -value of the bootstrapped one-tailed test for $\beta \geq 0$ (0.006) shows that the significance of the estimated send-down coefficient is robust to accounting for variability in the income coefficient estimated in the first step. Panel B of Table 3 reports the same estimates for DZ twins. Although still negative, these estimates (ranging from -137 to -109) are much smaller in magnitude compared with

TABLE 4. Send-down and transfer distribution, MZ twins.

Threshold value	Approximate percentile	Unconditional		Conditional	
		FE (1)	FE (2)	FE-IV (3)	FE, two-step (4)
>100	60th percentile	-0.094 [†] (0.064)	-0.100 [†] (0.065)	-0.110* (0.066)	-0.125* (0.069) [0.034]
>350	70th percentile	-0.151** (0.061)	-0.160*** (0.061)	-0.161*** (0.062)	-0.193*** (0.069) [0.006]
>600	80th percentile	-0.170*** (0.054)	-0.180*** (0.054)	-0.185*** (0.054)	-0.175*** (0.054) [0.008]
>1,000	90th percentile	-0.075** (0.034)	-0.078** (0.035)	-0.081** (0.035)	-0.081** (0.034) [0.034]

Note: The sample is restricted to the send-down cohort of MZ twins. In columns (1)–(3), the dependent variable is a dummy indicator for whether a child's transfer to parents exceeds the threshold value indicated in the row heading. In column (4), the dependent variable is the residual after netting out the income effect using the income coefficient $\hat{\rho}_{iv}$ estimated from the non-send-down cohort of MZ twins born 1962–1970. Each cell corresponds to a separate regression and reports the coefficient on the rustication dummy. Enclosed in parentheses are robust standard errors. For column (4), the p -values of the one-tailed test $\beta \geq 0$ calculated from a bootstrapping procedure described in Online Appendix D that accounts for the estimation error in the income coefficient in the first stage are also reported in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, and [†] $p < 0.15$.

the MZ estimates, thereby suggesting that the negative estimates for MZ twins in Panel A are indeed driven by the negative effect of rustication on children's transfer amount to parents rather than the adverse selection in altruism endowment in the send-down choice.

5.3. Distributional Treatment Effect

In this subsection, we examine the effect of rustication on the distribution of transfer to further understand its effect on transfer behavior. Specifically, we estimate the effect of rustication on the incidence that a child's transfer to parents exceeds a particular cutoff value c corresponding to a certain quantile of the transfer distribution (i.e. $\mathbf{1}(t_{ij} > c)$). Table 2 is a special case with $c = 0$, which is roughly the 45th percentile of the transfer distribution. In Table 4, we replicate the same estimations in Table 2 for MZ twins by using a set of different cutoff values 100, 350, 600, and 1,000, which corresponds to the 60th, 70th, 80th, and 90th percentiles of the transfer distribution, respectively. The point estimates of the send-down coefficient in Table 4 are negative in all specifications and are always significant at the 5% level when a cutoff value of 350 or higher is used. Moreover, the magnitude of the estimates increases along with the cutoff value until the highest decile is reached. The persistent negative estimates for all cutoff values in Table 4 demonstrate that the send-down treatment shifts the entire transfer distribution

leftward. For each cutoff value, regardless of how the income effect is accounted for, the conditional estimates are consistently larger in magnitude compared with the unconditional estimates. Nonetheless, their differences are always small quantitatively, thereby suggesting that at all levels of transfer, the majority of the effect of rustication goes through the altruism channel.

6. Additional Results

6.1. Robustness Checks

In this subsection, we check the robustness of our main empirical results with respect to the choice of the income coefficient, the permutation-based inference, and the exclusion of families with parents co-residing with one of the twins.

6.1.1. Accounting for the Income Effect. For our conditional estimates to identify the difference in the degree of altruism toward parents between rusticated and non-rusticated twins, it is critical to properly account for the effect of their income difference through the income channel. In Figure 2, we analyze the sensitivity of the two-step estimation results to the values of the income coefficient used. In Panel (a), we create 40 grids with a scale of 0.02 over the range of [0,0.8] for the coefficient on log income and then partial out the income effect on transfer incidence by using each value within this range.²¹ The estimates of $\hat{\beta}_p$ are consistently negative and decreasing in the income coefficient used, with the 90% confidence intervals always being below 0 except for very small values (0.06 or less). In Panel (b), we create 40 grids with a scale of 0.0025 over the range of [0,0.1] for the propensity to transfer to partial out the income effect on transfer amount, and then plot the estimate $\hat{\beta}_p$ against the value of the income coefficient used. The results are qualitatively the same as those shown in Panel (a). Taken together, the sensitivity analysis suggests that our conclusions that rustication reduces both children's transfer incidence and transfer amount are not sensitive to the income coefficient used to account for the income effect.

6.1.2. Permutation-Based Inference. To check the robustness of the statistical inference of our estimates obtained from a relatively small sample, we follow Heckman et al. (2010) and Campbell et al. (2014) by performing a permutation test where we compare our estimates of the send-down coefficient with the permutation distributions of the same coefficient obtained from resampled data wherein the send-down status is permuted to sustain the null hypothesis that the send-down has no effect on children's altruism toward parents. Specifically, the permutation test procedure is implemented as follows. First, we perform a stratified resampling of MZ twins with replacement

21. The upper-bounds of the income coefficients used in both panels Figure 2 exceed the upper limits of the 99.99% confidence intervals of the FE-IV estimates for the non-send-down cohort of MZ twins in Appendix Table A.1.

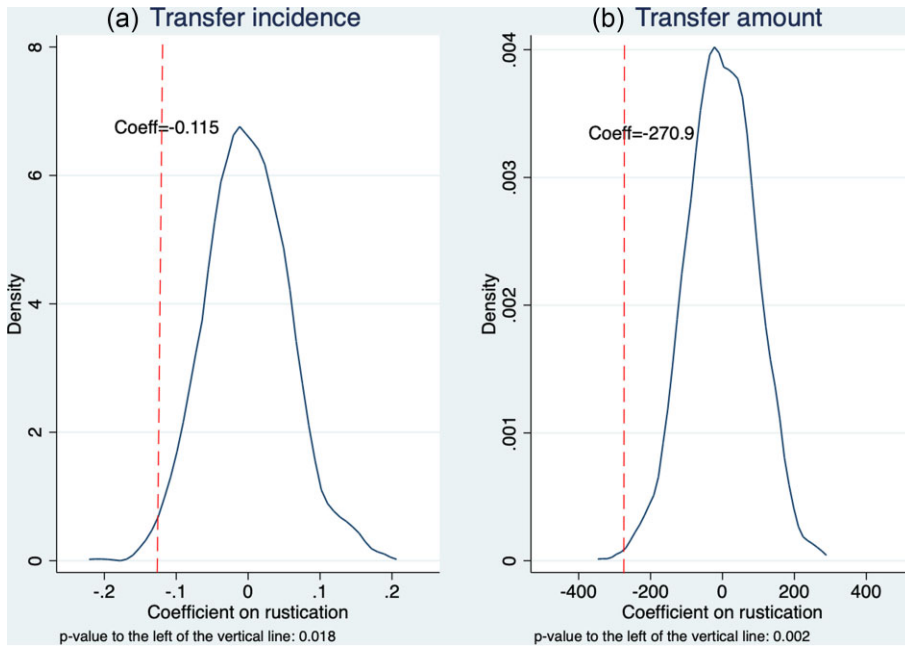


FIGURE 3. Permutation tests for estimates of send-down effect. The figure displays the kernel density curve of the bootstrapped IV estimates of the send-down coefficient on transfer incidence (Panel (a)) and transfer amount (Panel (b)) in a permutation analysis with 1,000 replications. The reference lines correspond to the IV estimate of the send-down coefficient in column (3) of Table 2 for Panel (a) and that in column (3) of Table 3 for Panel (b). The p -value displayed in each panel indicates the proportion of the estimates of the permutation analysis that falls to the left of the reference line.

by the number of rusticated twins in each family. Second, for each sampled family with one twin sent down and the other staying in the city, we reassign the send-down status randomly within the pair. Third, we replicate the IV estimates in column (3) of Tables 2 and 3 by using the resampled data with the permuted send-down status. Finally, we repeat the above steps 1,000 times and calculate the p -values for the bootstrapped coefficients is less than that obtained from the original sample. Figure 3 presents the results of this permutation test by comparing the IV estimates in column (3) of Tables 2 and 3 to the kernel density of the permutation distributions of the same coefficient. With p -values of 0.018 for transfer incidence (Panel (a)) and 0.002 for transfer amount (Panel (b)), our estimates remain significant under the permutation-based inference.

6.1.3. Co-Residence. Apart from financial transfers, adult children's altruism toward parents can also be reflected by their companionship behavior toward parents. In Table 5, we first investigate the effect of the send-down on two measures of children's companionship behavior: living in the same city as parents and co-residing with parents. Nearly three quarters (72%) of children in our sample live in the same city

TABLE 5. Send-down, co-residence and transfer behavior.

	Same city		Co-residence		Transfer incidence		Transfer amount	
	FE, unconditional (1)	FE, unconditional (2)	FE, unconditional (3)	FE, conditional (4)	FE-IV (5)	FE, unconditional (6)	FE, conditional (7)	FE-IV (8)
Rustication	0.000 (0.031)	-0.057 (0.049)	-0.143** (0.065)	-0.147** (0.065)	-0.155** (0.066)	-331.2* (88.1)	-341.0** (84.9)	-343.3** (85.2)
Log income				0.056 (0.047)	0.158 (0.100)			
Income							0.022*** (0.006)	0.026*** (0.010)
Observations	312	312	266	266	266	266	266	266
Families	156	156	133	133	133	133	133	133

Note: Columns (1) and (2) use the entire sample of the send-down cohort of MZ twins and employ the dummy indicator for living in the same city as parents and the dummy indicator for co-residing with parents as the dependent variable, respectively. Columns (3)-(8) restrict the sample to families with both twins not living with their parents. Columns (3)-(5) use transfer incidence as the dependent variable, and columns (6)-(8) employ transfer amount as the dependent variable. Robust standard errors are reported in parentheses. * $p < 0.10$ and ** $p > 0.05$.

as their parents. This probability is unaffected by children's send-down experience: The estimated send-down coefficient is *exactly* 0 in column (1) with the dummy indicator for living in the same city as parents as the dependent variable. For co-residence with parents, the point estimate of the send-down coefficient in column (2) indicates that the send-down reduces a child's probability to live with his/her parents by 5.7 percentage points. Given an average co-residence rate of about 10% in our sample, the relative magnitude of the reduction in co-residence is non-trivial and comparable with that of the reduction in transfer amount. Although the coefficient is statistically insignificant, its sign is consistent with that the send-down reduces children's altruism toward parents. One may be concerned that the difference in co-residence rates between send-downs and non-send-downs, though statistically insignificant, may contaminate our estimates of the effect of send-down on children's transfer to parents because children living with and without parents may show drastically different transfer behavior. To address this concern, we restrict to the subsample of MZ twin pairs with neither child co-residing with parents and redo our main estimations in columns (3)–(8) of Table 5. All estimates remain negative and significant for this subsample and are quantitatively larger than the estimates using the full sample of the send-down cohort of MZ twins in Tables 2 and 3.

6.2. Heterogeneity Analysis

In this subsection, we investigate the potential heterogeneity in the effects of the send-down on children's transfer behavior toward parents by child gender and send-down duration.

6.2.1. Child Gender. In Table 6, we allow the send-down effects on children's transfer behavior toward parents to vary by child gender and estimate separate send-down coefficients for the sons and daughters among the MZ twins. The estimated adverse effects of the send-down on transfers to parents are more salient for the daughters than for the sons. For the daughters, the estimated send-down coefficients are negative and significant at the 5% level for both transfer incidence (Panel A) and transfer amount (Panel B). For the sons, only the coefficients for transfer amount are negative and significant at the 10% level, whereas those for transfer incidence are indistinguishable from 0 both quantitatively and statistically. At the bottom of each panel, we also report the differences between the two gender-specific estimates of the send-down coefficient for every specification. For transfer incidence in Panel A, the differences are all significant at the 10% level, suggesting that the adverse effects of send-down on children's likelihood to transfer to parents found in Table 2 are entirely driven by the results for the daughters. However, for transfer amount in Panel B, although the point estimates of the gender-specific send-down coefficients indicate that the adverse effects on transfer amount are twice as large for the daughters than for the sons, the differences are never statistically significant. Overall, the results in Table 6 suggest that send-down experience adversely affects the daughters' transfer behavior at both the extensive and intensive margins but only adversely influences

TABLE 6. Gender-specific send-down effects on transfer behavior, MZ twins.

	Unconditional		Conditional	
	FE (1)	FE (2)	FE-IV (3)	FE, two-step (4)
Panel A: transfer incidence				
Rustication * Son	0.000 (0.083)	0.000 (0.083)	-0.003 (0.085)	-0.007 (0.092)
Rustication * Daughter	-0.208** (0.092)	-0.239** (0.093)	-0.264** (0.102)	-0.276*** (0.101)
Income * Son		0.008 (0.084)	0.147 (0.110)	
Income * Daughter		0.140* (0.053)	0.253 (0.182)	
Rustication: daughter-Son	-0.208* (0.124)	-0.239* (0.124)	-0.261* (0.133)	-0.269** (0.136)
Income: daughter-Son		0.132 (0.099)	0.105 (0.212)	
Panel B: transfer amount				
Rustication * Son	-181.7* (100.6)	-174.7* (97.4)	-174.3* (97.9)	-165.5* (99.6)
Rustication * Daughter	-357.9*** (110.5)	-397.2*** (107.3)	-413.0*** (109.6)	-407.3*** (109.5)
Income * Son		0.016** (0.007)	0.016 (0.011)	
Income * Daughter		0.028*** (0.011)	0.040** (0.016)	
Rustication: daughter-Son	-176.2 (149.4)	-222.6 (145.5)	-238.7 (146.9)	-241.8 (148.0)
Income: daughter-Son		0.013 (0.013)	0.023 (0.019)	
Observations	312	312	312	312
Families	156	156	156	156

Note: The sample is restricted to the send-down cohort of MZ twins. In Panel A, the dependent variable is the transfer incidence dummy for columns (1)–(3) and its residual that partials out the income effect using the income coefficient $\hat{\rho}_{iv}$ estimated from the non-send-down cohort of MZ twins born 1962–1970. In Panel B, the dependent variable is the transfer amount for columns (1)–(3) and the residual transfer amount that partials out the income effect using the income coefficient $\hat{\rho}_{iv}$ estimated from the non-send-down cohort of MZ twins born 1962–1970. Robust standard errors are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

the sons' intensive-margin transfer decisions. In addition, we compare the gender-specific income coefficients. Although the point estimates of the income coefficients are consistently larger for the women than for the men, the differences are statistically insignificant.

The finding on the gender-asymmetric effects of send-down experience on children's old-age support to parents does not come as a complete surprise. It has been widely documented that the women had more difficulties to adapt to the climatic and living conditions in villages, earned less work points, and also faced higher safety

TABLE 7. Send-down incidence, send-down years and transfer behavior, MZ twins.

	Unconditional		Conditional	
	FE (1)	FE (2)	FE-IV (3)	FE, two-step (4)
Panel A: transfer incidence				
Send-down years	-0.002 (0.017)	0.000 (0.017)	0.006 (0.018)	0.011 (0.018)
Send-down dummy	-0.090 (0.079)	-0.099 (0.080)	-0.133 [†] (0.085)	-0.161* (0.088)
Log income		0.040 (0.046)	0.188* (0.098)	
Panel B: transfer amount				
Send-down years	6.2 (20.1)	7.5 (19.5)	8.0 (19.6)	8.8 (19.9)
Send-down dummy	-279.8*** (95.4)	-290.9*** (92.7)	-294.8*** (93.2)	-301.0*** (94.9)
Income		0.019*** (0.006)	0.025*** (0.009)	
Observations	312	312	312	312
Families	156	156	156	156

Note: The sample is restricted to the send-down cohort of MZ twins. In Panel A, the dependent variable is the transfer incidence dummy for columns (1)–(3) and its residual that partials out the income effect using the income coefficient $\hat{\rho}_{iv}$ estimated from the non-send-down cohort of MZ twins born 1962–1970. In Panel B, the dependent variable is the transfer amount for columns (1)–(3) and the residual transfer amount that partials out the income effect using the income coefficient $\hat{\rho}_{iv}$ estimated from the non-send-down cohort of MZ twins born 1962–1970. Robust standard errors are reported in parentheses. * $p < 0.1$, *** $p < 0.01$, and [†] $p < 0.15$.

risks than the men (e.g. Liu 1998; Bonnin 2013). To the extent that the women suffered more both physically and morally from the exile and hardship of rural life than the men, the send-down experience could also exert a greater negative impact on the women's altruism toward their parents than the men's.

6.2.2. Send-Down Duration. In the main empirical analysis in Section 5, we focus on the extensive-margin differences in the send-down treatment between the rusticated and non-rusticated twins from the same family. However, in addition to the differences in the send-down treatment, the intensive-margin differences in years of rustication may also be an important determinant of children's altruism toward parents given that the send-down duration is associated with the “intensity” of exposure to hardship of rural life. In Table 7, we investigate this possible channel by incorporating both the send-down years and send-down treatment dummy in the family FE regressions. For both transfer incidence (Panel A) and transfer amount (Panel B), the coefficients of the send-down years are numerically close to 0 and statistically not significant at all, and the coefficients of the send-down treatment are largely unaffected and quantitatively indistinguishable from the corresponding estimates in Tables 2 and 3. Although the addition of send-down years in the FE regressions increases the standard errors of

the coefficients of the send-down treatment dummy, the two-step estimate for transfer incidence (column (4), Panel A) and all the estimates for transfer amount (columns (1)–(4), Panel B) remain statistically significant. Overall, we take the results of Table 7 as suggesting that the adverse effects of send-down experience on children's transfer behavior toward parents are driven primarily by their extensive-margin differences in the send-down treatment rather than intensive-margin differences in the send-down duration.

6.3. *Alternative Explanations*

Some alternative explanations can challenge our interpretation of the gap in the transfer behavior between rusticated and non-rusticated MZ twins as reflecting the adverse effect of the send-down on children's altruism toward parents. In Section 5, we rule out the possibility for the inferior transfer behavior of rusticated twins to be driven by their income disadvantage or the adverse selection in the send-down choice in children's altruism endowment. In this subsection, we discuss several other alternative explanations.

6.3.1. Spouse Characteristics and Transfers to Parents-in-Law. One alternative explanation for the inferior transfer behavior of rusticated twins is that send-down experience may lead to a disadvantage in the marriage market, thereby forcing these twins to marry spouses with poorer socioeconomic backgrounds, which would consequently reduce their total household resources. To assess this possibility, we investigate the effect of send-down experience on sorting in the marriage market, as measured by the spouse's education, employment status, and occupation.²² The results are in columns (1)–(3) of Appendix Table A.2: For all spousal characteristics examined, the estimated effect of rustication is very small and insignificant, showing no evidence that the send-down experience leads to poorer spouse characteristics. In columns (4)–(6) of Table A.2, we further examine whether one's own send-down experience affects the incidence for providing financial support to parents-in-law. The estimated send-down coefficients for transferring to parents-in-law are all very small and statistically insignificant, suggesting that the send-down experience only reduces one's *directed* altruism toward own parents but not that toward parents-in-law.

6.3.2. Reciprocity. About a quarter of the children in our sample reported that they had received transfers or gifts from their parents in the previous year, with the gift-receiving children also reporting the estimated monetary value of these gifts. Therefore, the inferior transfer behavior of send-downs can reflect reciprocity instead of weaker altruism if parents had treated them less favorably than their non-send-down twin siblings in inter-vivos transfers. To eliminate the role of reciprocity in a child's transfer

22. Specifically, we employ a dependent variable for the spouse's schooling years, a dummy indicator for the spouse's employment status (1 for working and 0, otherwise), and a dummy indicator for whether the spouse has a professional or managerial job in columns (1)–(3) of Appendix Table A.2, respectively.

to parents, we subtract parents' transfer to the child to measure the child's net transfer to parents and employ in columns (7)–(9) of Appendix Table A.2 a dummy indicator for making a positive net transfer to parents as the dependent variable. The estimates for the send-down effect on net transfer incidence are always negative, statistically significant, and quantitatively larger in magnitude than the estimates on transfer incidence given in Table 2, thereby suggesting that, if anything, the rusticated twins were treated more favorably in parents' inter-vivos transfers compared with their non-rusticated counterparts.²³

6.3.3. Exchange Motive. Even if there is no differential parental treatment in contemporaneous transfers between rusticated and non-rusticated children, the within-twin difference in transfer behavior could still reflect the difference in the exchange motives of the twins. For example, if the send-down experience made a child less strategic (i.e. reduced the expected utility from potential future rewards from parents induced by the act of giving), then the rusticated child may also transfer less to parents. In a seminal paper, Bernheim, Schleifer, and Summers (1985) employ the U.S. Longitudinal Retirement History Survey to estimate a model of strategic bequests and find that a child's attention to parents is strongly positively correlated with parents' bequeathable wealth in multiple-child families.²⁴ If that is also the case for our context, we would expect the difference in the expected gains from the exchange to matter more for the children from families with higher socioeconomic status because they have a larger stake from their parents' future inter-vivos transfers or bequests. To investigate the validity of this alternative explanation, we conduct a falsification test that examines the heterogeneity in the effect of send-down on children's transfer by parents' educational attainment, which serves as a proxy for parents' socioeconomic status.²⁵ Specifically, in Panels A–C of Appendix Table A.3, we estimate the interactive effect of the send-down with father, mother, and number of parents having a high-school degree or above, respectively. In all specifications, the estimated coefficient on the interactive term is quantitatively quite small and statistically not significant at all, thereby showing little evidence for any heterogeneity in the effect of send-down on the children's transfer decision by parents' socioeconomic status.

7. Conclusion

In this paper, we combine a unique policy experiment—the send-down movement during China's Cultural Revolution—and twins data to exploit the distinction in the

23. The finding that rusticated twins were treated more favorably in parents' inter-vivos transfers is consistent with that of Li, Rosenzweig, and Zhang (2010) on parents' wedding gifts.

24. They find little evidence for a positive correlation between a child's attention and his/her parents' bequeathable wealth in single-child families and attribute it to the lack of a credible threat for parents to disinherit a child who ignored them in single-child families.

25. Direct measures of parents' wealth are not available as the CATS did not collect information on parents' wealth or income.

send-down experience of split MZ twin pairs to identify the causal impact of the send-down experience on children's old age support toward parents. We find that among MZ twins who share the same genetic endowment and rearing environment, the send-downs were less likely to make a financial transfer to parents in adulthood and, even when they did, tended to transfer less. The gap in the transfer behavior between the rusticated and non-rusticated MZ twins is not caused by the effect of rustication on income because, if anything, the send-downs actually earned more than their non-send-down counterparts. We also do not find any evidence for the adverse selection in the send-down choice in children's altruism endowment, less favorable parental treatment in inter-vivos transfers to the send-downs, or difference in the exchange motives of send-downs and non-send-downs.

Therefore, we attribute the inferior transfer behavior of rusticated twins to the adverse effect of childhood send-down experience on willingness for giving to parents in adulthood. This can be because either separation from their families in adolescent years lowered these send-downs' valuation of parents' welfare (i.e. pure altruism), or the hardship of rural life reduced the warm glow of giving of these send-downs (i.e. impure altruism), or a combination of both effects. We combine pure and impure altruism in our interpretation of the within-twin difference in transfer behavior to parents not only because these two are empirically indistinguishable in the within-twin comparison, but also because instilling either form of altruism can effectively foster children's willingness to provide old-age support to parents.

With only a single cross section pertaining to a particular historical context, our results can shed little light on the life-cycle effects of the send-down treatment on children's transfers to parents. Nonetheless, our finding that children's old-age support to their parents at the time of the survey is affected by their early-life experiences provides empirical support for the roles of endogenous parenting practices and school curricula in fostering children's altruism and old-age support to parents, which have important implications for macro and micro policies. From the macro perspective, the government could develop relevant school curricula²⁶ or promote certain parenting styles to foster children's altruism toward parents to achieve an optimal level of old-age support provision within the family, which could largely reduce the pressure for social security provision to adapt to the ageing society. From the micro perspective, fostering children's altruism toward parents will affect both the parents' human capital investment in their children and the adult children's old-age support to elderly parents, as evidenced in two recent studies (Yi 2019; Guo and Zhang 2020). Our study provides another piece of empirical evidence for promoting parental investment in children's altruistic education to achieve optimal intra-household resources allocation.

26. For example, primary schools can educate children to be altruistic toward their parents by selecting articles motivating filial piety for inclusion in textbooks.

Appendix A: Extended Empirical Model with Cross-Twin Effects

In this Appendix Section, we extend the empirical model presented in Section 4.1 to allow cross-twin effects in the determination of transfer to parents. That is, an adult child's transfer to parents is determined not only by own income and altruism toward parent, but also by the co-twin's income and altruism. With this extension, the transfer equations for the twin pair take the following form:

$$t_{1j} = \rho_1 Y_{1j} + \rho_2 Y_{2j} + \delta_{1j} + \kappa \delta_{2j} + \mu_j + \xi_{1j} \quad (\text{A.1a})$$

and

$$t_{2j} = \rho_1 Y_{2j} + \rho_2 Y_{1j} + \delta_{2j} + \kappa \delta_{1j} + \mu_j + \xi_{2j}, \quad (\text{A.1b})$$

where ρ_1 denotes own income effect, ρ_2 denotes cross income effect, κ denotes cross altruism effect, and own altruism effect is normalized to 1. Taking the difference between (A.1a) and (A.1b) yields the following first-difference equation:

$$\Delta t_j = (\rho_1 - \rho_2) \Delta Y_j + (1 - \kappa) \Delta \delta_j + \Delta \xi_j. \quad (\text{A.2})$$

Next, we also allow for the existence of the cross-twin effect of send-down experience on altruism. That is, a child's degree of altruism in adulthood depends not only on own send-down experience, but also on the co-twin's send-down experience. For example, when one of the twins was sent to the countryside while the other remained in the city, the latter twin may feel guilt and develop stronger warm-glow incentives to support the family thereafter, that is, a positive cross effect of the co-twin's send-down experience on own altruism. With the presence of both own and cross effects of send-down experience on altruism, we can relate a child's altruism in

TABLE A.1. Income and transfer, non-send-down cohort of MZ twins born 1962–1970.

	Transfer incidence		Transfer amount	
	FE (1)	FE-IV (2)	FE (3)	FE-IV (4)
Log income	0.147** (0.058)	0.307** (0.121)		
	[-0.087, 0.381]		[-0.165, 0.779]	
Income			0.026*** (0.008)	0.036*** (0.013)
			[-0.006, 0.057]	
Observations	282	282	282	282
Families	141	141	141	141

Note: The sample is restricted to the non-send-down cohort of MZ twins born between 1962 and 1970. Columns (1) and (2) regress transfer incidence on log income, and columns (3) and (4) regress transfer amount on income. The even columns employ the cross-reported income by the co-twin as an instrument for own reported income. Robust standard errors are reported in parentheses. The 99.99% confidence intervals are reported in bracket. ** $p < 0.05$ and *** $p < 0.01$.

TABLE A.2. Spouse characteristics, transfer to parents-in-law and net transfer to parents, MZ twins.

	Spouse characteristics			Transfer incidence to parents-in-law			Net transfer incidence to parents		
	Schooling (1)	Working (2)	Occupation (3)	FE, unconditional (4)	FE, conditional (5)	FE-IV (6)	FE, unconditional (7)	FE, conditional (8)	FE-IV (9)
Rustication	0.333 (0.427)	-0.044 (0.082)	-0.044 (0.038)	0.044 (0.082)	0.048 (0.083)	0.035 (0.086)	-0.151** (0.058)	-0.156*** (0.058)	-0.164*** (0.060)
Log income					-0.059 (0.059)	0.144 (0.127)		0.044 (0.042)	0.119 (0.088)
Observations	276	276	276	276	276	276	312	312	312
Families	138	138	138	138	138	138	156	156	156

Note: For Columns (1)–(6), the sample used is the send-down cohort of MZ twin pairs with both twins married. The dependent variable is the spouse’s schooling years for column (1), a dummy indicator for the spouse’s employment status (1 for working and 0, otherwise) for column (2), a dummy indicator for whether the spouse has a professional or managerial job for column (3), and a dummy indicator for making a positive transfer to parents-in-law for columns (4)–(6). For columns (7)–(9), the sample used is all the send-down cohort of MZ twin pairs and the dependent variable is a dummy indicator for making a positive net transfer to parents. Robust standard errors are reported in parentheses. ** $p < 0.05$ and *** $p > 0.01$.

TABLE A.3. Send-down, parental education and transfer behavior, MZ twins.

	Transfer incidence				Transfer amount			
	Unconditional		Conditional		Unconditional		Conditional	
	FE (1)	FE (2)	FE-IV (3)	FE, two-step (4)	FE (5)	FE (6)	FE-IV (7)	FE, two-step (8)
Panel A: father having a high school degree or above								
Rustication	-0.097 (0.076)	-0.100 (0.076)	-0.121 [†] (0.082)	-0.130 [†] (0.086)	-186.1** (81.7)	-186.9** (80.6)	-187.2** (80.7)	-188.5** (85.4)
Father above high school * Rustication	0.020 (0.139)	0.020 (0.139)	0.021 (0.149)	0.021 (0.158)	120.7 (150.3)	101.1 (148.6)	95.6 (149.3)	63.4 (157.1)
Panel B: mother having a high school degree or above								
Rustication	-0.083 (0.070)	-0.086 (0.070)	-0.110 (0.076)	-0.119 [†] (0.079)	-158.9** (76.0)	-161.8** (74.9)	-162.7** (75.1)	-167.3** (79.3)
Mother above high school * Rustication	-0.042 (0.164)	-0.040 (0.165)	-0.030 (0.177)	-0.026 (0.186)	46.4 (178.2)	25.5 (176.0)	19.5 (176.7)	-13.6 (186.0)
Panel C: number of parents having a high school degree or above								
Rustication	-0.089 (0.074)	-0.092 (0.075)	-0.115 (0.081)	-0.124 [†] (0.084)	-175.4** (80.4)	-176.5** (79.3)	-176.8** (79.4)	-178.5** (84.0)
Number of parents above high school * Rustication	-0.003 (0.081)	-0.003 (0.081)	0.000 (0.087)	0.001 (0.092)	-52.2 (87.7)	-40.5 (86.7)	-37.3 (87.1)	-18.2 (91.6)
Observations	260	260	260	260	260	260	260	260
Families	130	130	130	130	130	130	130	130

Note: The sample is restricted to the send-down cohort of MZ twins. The dependent variable is the transfer incidence dummy for columns (1)–(3), its residual that partials out the income effect using the income coefficient $\hat{\rho}_{iv}$ estimated from the non-send-down cohort of MZ twins born 1962–1970 for column (4), the transfer amount for columns (5)–(7), and the residual transfer amount that partials out the income effect using the income coefficient $\hat{\rho}_{iv}$ estimated from the non-send-down cohort of MZ twins born 1962–1970 for column (8). The regressions in columns (2) and (3) control for log income and those in columns (6) and (7) control for income level. Robust standard errors are reported in parentheses. ** $p < 0.05$ and [†] $p < 0.15$.

adulthood to both own and co-twin's send-down experience as follows:

$$\delta_{1j} = \beta_1 D_{1j} + \beta_2 D_{2j} + e_{1j} + \zeta_{2j} \quad (\text{A.3a})$$

and

$$\delta_{2j} = \beta_1 D_{2j} + \beta_2 D_{1j} + e_{2j} + \zeta_{2j}, \quad (\text{A.3b})$$

where β_1 denotes own effect of send-down on altruism and β_2 denotes cross effect of send-down on altruism, the latter of which is assumed to be 0 in equation (8). Taking the difference between (A.3a) and (A.3b) yields

$$\Delta\delta_j = (\beta_1 - \beta_2)\Delta D_j + \Delta e_j + \Delta\zeta_j. \quad (\text{A.4})$$

Substituting equation (A.4) for $\Delta\delta_j$ in equation (A.2) yields the main specification of the extended empirical model:

$$\Delta t_j = (\rho_1 - \rho_2)\Delta Y_j + (1 - \kappa)(\beta_1 - \beta_2)\Delta D_j + \Delta\varepsilon_j, \quad (\text{A.5})$$

where $\Delta\varepsilon_j = (1 - \kappa)(\Delta e_j + \Delta\zeta_j) + \Delta\xi_j$.

Note that equation (A.5) has the same empirical specification as equation (9) but employ different notations for the coefficients on ΔY and ΔD . When no cross-twin effects exist, that is, $\rho_2, \beta_2, \kappa = 0$, the two equations are identical with $\rho = \rho_1$ and $\beta = \beta_1$. However, when cross-twin effects exist, the interpretations of the coefficients on ΔY and ΔD in equation (A.5) are different from equation (9). Specifically, the coefficient of ΔY represents *net* effect (i.e. own minus cross effect) of income on transfer to parents given altruism, and the coefficient of ΔD represents *net* effect of send-down on transfer to parents through the altruism channel given income, the latter of which is the product of the difference between own and cross effects of send-down on altruism ($\beta_1 - \beta_2$) and the difference between own and cross effects of altruism on transfer to parents ($1 - \kappa$) given income.

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Supplementary Material

Supplementary data are available at [JEEA](https://academic.oup.com/jeea/article/21/4/1647/7049967) online.