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Do Private Household Transfers to the Elderly Respond to Public Pension Benefits? Evidence from Rural China[☆]

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Abstract. Aging populations in developing countries have spurred the introduction of public pension programs to preserve the standard of living for the elderly. The often-overlooked mechanism of intergenerational transfers, however, can dampen these intended policy effects, as adult children who make income contributions to their parents could adjust their behavior in response to changes in their parents' income. Exploiting a unique policy intervention in China, we examine using a difference-in-difference-in-differences (DDD) approach how a new pension program impacts inter vivos transfers. We show that pension benefits lower the propensity of adult children to transfer income to elderly parents in the context of a large middle-income country, and we also estimate a small crowd-out effect. Taken together, these estimates fit the pattern of previous research in high-income countries, although our estimates of the crowd-out effect are significantly smaller than previous studies in both middle- and high-income countries. (JEL D64, O15; O16; J14; J22; H55, R2)

Keywords: life cycle, retirement, pension, inter vivos transfers, middle-income countries, developing countries, China, crowd-out effect, aging

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1. Introduction

How family members respond to public transfers can play an important role in assessing the efficiency and welfare impact of redistributive policies on the welfare of the elderly. Of particular concern is the possibility that public transfers induce a reduction in private family transfers, thereby dampening the redistributive effect of public transfers and resulting in an overall reduction of available savings, and thus welfare.¹ This scenario is exceptionally relevant to developing countries and rapidly growing economies, such as China, where such crowd-out effects can affect millions of elderly people, many of whom live in poverty. China is a particularly compelling setting to examine intergenerational transfers because of the country's high age dependency ratio, with a large elderly cohort and relatively small working-age population, due to the only recently relaxed one-child policy (World Bank 2017). The World Bank (2001) estimates that expanding formal safety nets and public transfer programs in developing countries will likely displace private transfers by 20 to 91 percent. Previous empirical studies that examine the magnitude of crowd-out effects on inter vivos private transfers provide mixed evidence or some support for positive small crowd-out effects (Lucas and Stark 1985; Cox 1987; Cox and Rank 1992; Cox and Jakubson 1995; McGarry and Schoeni 1995; Altonji et al. 1997; Cox et al. 1998), although these studies are largely based on the experience of high-income countries. Evidence on the existence and magnitude of crowd-out effects in emerging and middle-income economies, such as China, is extremely limited.²

In this paper, we examine the impact of China's New Rural Pension Scheme (NRPS) on inter vivos private transfers (from children to parents and vice versa), particularly among adults in the age group of 60 years and above.³ The NRPS program was introduced in 2009 in response to rising demographic and old-age poverty concerns in the last decade (Liu and Sun 2016; Holzman, Robalino and Takayama 2009: 111–18).⁴ In 2007, approximately 11 percent of China's population was in the age group of 60 years and above, making up 21 percent of the world's elderly population (UN 2007). Similar to other developing countries, the Chinese

¹ Crowding-out refers to the phenomenon whereby public sector involvement (or spending) reduces private forms of spending. Feldstein and Liebman (2002) review the early literature on various forms of crowding-out.

² Cox et al. (2004) argue that the magnitude of crowd-out effects in low- and middle-income countries could be quite different from that detected in high-income countries because the latter have experienced a century of large public transfers.

³ In each case, we limit our discussion to interhousehold transfers, as the family transfer information collected by our survey source (China Health and Retirement Longitudinal Study) is only for parents and children not living together.

⁴ Feldstein and Liebman (2002) and Cutler and Johnson (2004) provide a historical overview of social pension programs in developed countries. Social pension programs are common in the developed world and are primarily aimed at providing old-age insurance and aiding consumption smoothing.

government faced additional pressing challenges: large rural and informal agricultural populations, high internal migration flows (Sabates-Wheeler and Koettl 2010), and weak institutions (Musalem and Ortiz 2011). The new program,⁵ a defined-contribution pension program, was made available to all rural residents who were 16 years of age or older. In this study, we examine the program's impacts on inter vivos transfers using various quasi-experimental techniques.^{6,7} We use a large panel dataset from which we focus on a cohort of individuals whose data on program participation and inter vivos transfers are rich. The eligibility of an individual for the program was based on his/her contributions and age. Our identification strategy relies on within-country variation in program implementation due to the staggered implementation of the policy across communities. We use this staggered implementation as a source of identifying variation, to detect the impact of this program on pension participation between those individuals living in newly integrated communities between 2011 and 2013 and those individuals who were not offered similar program benefits. We use a difference-in-difference-in-differences (DDD) empirical strategy to estimate the intent-to-treat (ITT) program's impact on access to pension benefits on individual-level outcomes. We also instrument the individual's program participation with a community variable related to the availability of the program. Finally, because of the potential right-censoring in our outcome variable, we use a censoring-data adjustment based on Cameron and Trivedi (2005).

Using data from 2009 to 2013 from the China Health and Retirement Longitudinal Study (CHARLS) and the China Health and Nutrition Survey (CHNS), we find a statistically significant impact of the availability of NRPS benefits on lowering the probability of the incidence of receiving a transfer from adult children by 7.4 percentage points and a lowering of the actual amount of transfers by 10.4 percent.^{8,9} We do not detect statistically significant evidence that the benefits' availability impacted transfers sent to children. The small amount of

⁵ Prior to the NRPS program, China had experience with other public pension programs, but these programs were largely decentralized. Vilela (2013) overviews China's experience with various pension programs prior to the introduction of the NRPS.

⁶ Using data from two Chinese provinces (Guizhou and Shandong), Chen et al. (2018) study the impact of the NRPS program and specifically examine whether higher income for elderly parents leads to a higher rate of independent living, whether parents have better access to healthcare, whether adult children reduce their own caregiving (for their parents) as a result of parental access to more healthcare, and whether the NRPS program results in changes to monetary and non-monetary transfers between adult children and parents who are NRPS beneficiaries. The study relies on a fuzzy regression discontinuity design, exploiting the fact that the NRPS program kicks in with benefits for individuals in rural areas when they reach the age of 60 years (the running variable in this regression discontinuity design (RDD)). Although the study relies on a much more limited sample from two provinces, its results suggest a behavioral response for NRPS participants: the study reports that transfers between grandparents and grandchildren decrease in both Shandong and Guizhou.

⁷ Nikolov and Adelman (2019) examine the impact of the NRPS program on physical health, mental health, social interactions, and mediating individual behavioral inputs.

⁸ Based on the average baseline transfer amount of 4242 yuan (approximately 692 USD in 2013).

⁹ Because of the right-censoring of the data for our main outcomes, our primary analysis relies on Tobit adjustment for the right-censoring.

evidence provided by previous studies in developing countries supports our findings that elderly pension benefits tend to crowd-out private transfers received.¹⁰ In addition, we find that the receipt of benefits had a significant effect on the intensive and extensive margins of private transfers. More specifically, NRPS benefit recipients were 10.4 percentage points (55.1 percent) less likely to receive transfers. And the receipt of benefits was associated with a decline in transfers of about 18.8 percent of the average transfer amount. Similarly, we did not find that the receipt of benefits affected private transfers sent to children. Finally, using Tobit analysis for the right-censored outcome measures, we investigate the magnitude of the crowd-out effect. We find a crowd-out effect of -0.03 (imprecisely estimated¹¹), suggesting that family members do offset their inter vivos transfers to elderly parents in response to pension benefits, but that substitution is extremely small in magnitude and much smaller than previously estimated using data from middle- and high-income countries.

Previous empirical studies have examined the substitution between the availability of public saving options and subsequent intergenerational transfers and have reported mixed results.¹² Studies that examine the relationship between public transfers and inter vivos transfers in the context of high-income countries (Cox 1987; Cox and Rank 1992; Lucas and Stark 1985; Cox et al. 1998; Cox and Jakubson 1995; McGarry and Schoeni 1995; Altonji et al. 1997) find evidence of substitution, with estimated impacts on the probability of transfers ranging from -0.01 to 0.015 (probability of decreased incidence of an inter vivos transfer).¹³ In contrast to these findings, studies conducted in low- and middle-income countries (Cox et al. 2004; Juarez 2009; Gerardi and Tsai 2014; Jung et al. 2016) also find evidence of substitution effects, but the estimated substitution response tends to be much higher for public programs in developing countries than those in high-income countries. Therefore, the potential welfare consequences of behavioral responses to newly introduced pension benefits in a large country are enormous.

¹⁰ See Cox et al. (2004), Juarez (2009), Gerardi and Tsai (2014), and Jung et al. (2016).

¹¹ We estimate a -0.03 effect size for the marginal effect (i.e., $E[T | T > 0]$), but the effect size estimate is imprecisely estimated because of large standard errors.

¹² Existing economic theory on the relationship between private and public transfers focuses on two important features guiding family-member exchanges—altruism and self-interested exchange (Cox 1987). These two features lead to important theoretical predictions. Assuming altruistic family transfers, the introduction of public transfer programs will offset private transfers (Becker 1974; Barro 1974). If exchange motives guide private transfers, public transfers will not necessarily undermine private transfers (Bernheim et al. 1986; Cox 1987; Cox and Jimenez 1990; Morduch 1999; Cox and Fafchamps 2007). However, which of these theoretical predictions play out in practice across various settings remains an empirical issue.

¹³ In the U.S., for example, the estimated decrease in the probability of private transfers is 0.002 (based on Altonji et al. 1997). The estimated impacts on the actual dollar amount decrease of private transfers received per dollar received in income ranges from 3 cents (McGarry and Schoeni 1995) to 13 cents (Altonji et al. 1997), suggesting that crowding out from government programs in the U.S. is negligible.

Furthermore, because of its rapidly aging population and a public saving option that has been recently introduced in its rural areas, China is a particularly compelling setting for this study.

This paper makes four important contributions to the existing empirical literature on inter vivos transfers and the possible crowd-out effects between various forms of saving. First, we contribute to the existing literature on intergenerational transfers in low- and middle-income countries, and we are among the first set of studies to shed light on how pension benefits influence inter vivos transfers in the context of a very large middle-income country.^{14,15,16,17} In this study, we focus on China, which is the world's most populated country. The setting of this study makes the consequences of our results important from a welfare standpoint.^{18,19} Worldwide, aging populations have prompted policy responses to alleviate old-age poverty. China's rural elderly have predominantly been without public assistance until the NRPS, making China a setting ripe to study the effects of an elderly pension program on private transfers. Thus, examining the potential behavioral response to the receipt of pension benefits in the context of the world's largest country has important implications for the design of public assistance programs in other highly populated and developing countries. Our second contribution relates to our program's estimated impact on the incidence of interhousehold transfers in response to pension benefits. All three studies that use data from middle-income countries do not explicitly test for impacts of these programs on the incidence of new transfers and instead focus on the crowd-out effect.²⁰ We find a very small (relative to previous studies from middle-income

¹⁴ Our study is closely related to a strand of literature (in high-income countries) that examines the substitution between private wealth and public pension provision in high-income countries. Using data from the U.S., Diamond and Hausman (1984), Hubbard (1986), and King and Dicks-Mireaux (1982) examine the degree of substitution between private wealth and public pensions. They find small offsets between the two. Similarly, Brugiavini (1987) and Jappelli (1995) obtain large estimates, using data from Italy, of the degree of substitutability between private wealth and public pension provision.

¹⁵ Related empirical literature, also in high-income countries, examines the substitution between various savings devices. Poterba, Venti, and Wise (1996) and Engen, Gale, and Scholz (1996) review the public finance literature in high-income countries. Some recent studies in this literature (e.g., Gelber 2011) present evidence that increases in IRA or 401(k) savings represent increases in total saving, whereas others (e.g., Benjamin (2003), Engelhardt and Kumar (2007), and Chetty et al. 2014) find that much of the increase in 401(k) savings represents substitution from other accounts. Although some of the differences between the results of these studies likely stem from differences in econometric assumptions, the variation that drives changes in contributions to 401(k)s could also explain the differences in results. For instance, increases in 401(k) contributions by employers may generate less crowd-out than tax incentives or programs that require active individual choice, an idea foreshadowed in early work by Cagan (1965) and Green (1981).

¹⁶ Juarez (2009), Jensen (2004), and Gibson, Olivia and Rozelle (2011) study the effect of public programs on private transfers in the context of other middle-income countries (respectively, Mexico, South Africa, and Vietnam).

¹⁷ Gerardi and Tsai (2014) and Jung et al. (2015) examine the effect of pension benefits on interhousehold allocation decisions, but in the context of a high-income country.

¹⁸ China's age dependency ratio has been rising rapidly in the 2000s, more so in rural areas, and by 2030 should resemble Japan's aging rate of the last 30 years (Cai et al. 2012).

¹⁹ From 2013 to 2016, China experienced one of the largest changes in its age dependency ratio (2.16 percentage-point change), much larger than developing countries in the Middle East (0.56), East Asia and the Pacific (1.24), and Latin America (1.29). Over this period, China has been aging faster than the European Union (2.11) and the U.S. (1.33). This is based on the author's calculations using the age dependency ratio (%) provided by <https://data.worldbank.org/indicator/SP.POP.DPND>. The age dependency ratio is defined as the ratio of dependents, people younger than 15 years or older than 64 years, to the working-age population (those in the age group of 15–64 years).

²⁰ Although no studies that use data from middle-income countries specifically examine for impacts of these programs on the incidence of new transfers, some studies using data from high-income countries examine the issue. Using data from high-income countries, Gerardi and Tsai

countries) and statistically significant reduction in the incidence of inter vivos transfers to the elderly.²¹ Third, previous studies that rely on data from middle-income countries (Juarez 2009; Jensen 2004; Gibson, Olivia and Rozelle 2011) show a large crowd-out effect in response to pension benefits. Although our estimates are in line with the overall substitution pattern of the estimated crowd-out effect from middle-income countries, we find much smaller crowd-out effects (imprecisely estimated) than that of previous empirical studies using data from middle-income countries. Our fourth contribution relates to our large sample size when compared to studies from low- and middle-income countries. We rely on data from almost 12,000 households. Finally, we examine for heterogeneous effects, by household's income status, due to the introduction of the public program on the incidence and crowd-out effect of private transfers. Although we do not detect differential impacts when we formally test for equality of the crowd-out effect by household's income status, we do detect a larger decrease in the incidence of private transfers among high-income households.

This paper is organized as follows: Section 2 provides the background for rural pension programs in China and on the NRPS in particular. Section 3 presents a conceptual framework for intergenerational transfers. Section 4 presents a summary of the data and the study sample. Section 5 presents the identification strategy, and Section 6 discusses the results. Section 7 presents various robustness checks, and Section 8 concludes the paper.

2. China's New Rural Pension Scheme

2.1 Introduction of the NRPS Program

Over the last three decades, the Chinese government has gradually assumed the responsibility of alleviating poverty, particularly the risk of old-age poverty.²² To this end, the country introduced a rural pension scheme in 1986 by first piloting the rollout among rural residents before expanding coverage further in subsequent years. The financing of the program relied on individual voluntary contributions, with matching funds provided from the local government. From the nineties onward, the public pension system was based on pension

(2014), Jung et al. (2015), and Fan (2010) examine the program impacts on the incidence of interhousehold transfers and find large effects (i.e., reduction of probability by 0.4) relative to our estimates.

²¹ Cox and Jakubson (1995) and McGarry and Schoeni (1995) find a similar pattern, but much larger estimates in high-income countries.

²² Vilela (2013) reviews the history of China's pension policy since the establishment of the new People's Republic of China (PRC) in 1949 up to 2013, pointing out that the country's pension policy is still in transition, moving away from a historical focus on formal-sector workers to having an emphasis on universal coverage of formal and informal workers alike. The study outlines three distinct phases of the country's pension policy: the "Iron Rice Bowl" (1949–1978), formal sector pension reform and rural pension piloting (1978–2001), and a gear change in pension expansion (2003 to the present).

programs for the then-state enterprise employees and the then-called Basic Old Age Insurance Scheme (BOISE), to the introduction of an NRPS program in 2009 (Liu and Sun 2016). For the first time, compulsory coverage quotas for both urban and rural systems were included. By the end of 1998, two-thirds of the rural counties were covered, which translated to program coverage in 2,123 counties within 31 provinces (in mainland China). However, a combination of poor governance, unsound local operations, and inflationary pressures brought about by the Asian financial crisis in 1997 halted the expansion of the rural pension program. The program was scaled back in 1999 following concerns about its long-run sustainability in rural areas. Pension coverage fell from 80.25 million participants in 1998 (approximately 11 percent of the total rural population) to mid-50 million among rural participants in 2007.²³

The NRPS program was launched in 2009 and aimed to achieve full geographic coverage by 2020 (Dorfman et al. 2013; Cai et al. 2012). This program features a basic flat pension financed by the central government, individual contributions, and minimum matching funding by local governments. By the end of 2010, the program had expanded to 23 percent of rural communities²⁴, and by early 2012 had reached over 60 percent of the communities. Figure 1 shows the expansion of the program's coverage over time.

[Figure 1 about here]

Total program enrollment had grown to 326 million by the end of 2011 (Quan 2012), when over 50 percent of China's rural residents were covered. Four important factors likely account for this program's expansion. First is China's initial localized dedication to rural pension reform. Second, the country's high economic growth rate from 2009 to 2011 likely played an important role. A third factor was an increase in the demand for the basic monthly benefit in untargeted areas, generated by the initially unequal geographic coverage. Finally, rural pension expansion was a key element in domestic policy discourse in 2012; this political debate further intensified interest in the pension program.

²³ By the early 2000s, rural residents came to widely mistrust the pension's set-up, and the system failed to become more strongly established in rural areas. Furthermore, program participation favored wealthier regions, and poor provinces failed to make their matching contributions. However, the program witnessed a resurgence after 2003 as interest grew and individual participation rapidly increased. More than 300 communities and 25 provinces introduced program benefits by the end of 2008 (Dorfman et al. 2013). Reformist elites under the Hu-Wen Administration in the late 2000s sought to create a novel basic and non-contributory pension for individuals not covered by any social insurance program. A 2008 pilot project in the city of Baoji (Shaanxi province) created interest for a similar program on a nationwide scale. In the Baoji pilot, the local administration introduced a pension scheme covering rural residents that was funded through local tax revenues. In addition to the basic pension, residents were encouraged to participate in a separately funded pension plan, subsidized by the local administration. Hence, in Baoji rural individuals were covered by a two-part pension plan. This "Baoji model" (Qing 2009) served as a template for the new national program (NRPS) about to be introduced nationwide.

²⁴ Communities are small geographic units in rural China, similar to the U.S. survey townships, and typically a subunit within a county.

2.2 Program Eligibility and Benefits

The NRPS program is available to all rural residents who are 16 years of age or older and who have not enrolled in an urban pension program. The program was introduced in administrative districts determined to be rural under the *Hukou* registration system.²⁵ Program participation is voluntary, and those who contribute for at least 15 years are eligible to receive benefits upon reaching 60 years of age. Rural residents who are 60 years or older at the start of the program are eligible to receive the basic monthly benefit of 55 RMB if their children have already contributed to the pension scheme.²⁶ Individual benefits are calculated according to the “139 Rule.” The administrator takes the accumulated balance in an individual’s account and divides it by 139.²⁷

Individual contributions are voluntary and range annually from 100 to 500 Chinese RMB (approximately 15 to 77 USD). Based on the 2009 survey data, the mean participant contribution was 100 RMB (Dorfman et al. 2013). The local governments are required to match 30 RMB annually per contribution. Participants between the ages of 45 and 60 years, with less than 15 years of contributions, are encouraged to increase monthly payments to cover the absence of contribution over the work cycle. Rural residents who opt to participate in the program need to register at their local village government office, the lowest level of the government hierarchy. Payments are made at that office as a lump sum cash payment throughout the year. At the end of each contribution period (i.e., a month), each village-level government aggregates payments to it and transfers payment to the next level (a township-level government), which then transfers the contributions the next level (i.e., county). Although there have been some policy discussions for the funds to be managed at the province level, currently the rural pension funds are generally managed by county-level governments. In general, the contributions are deposited in banks and administrative expenses are paid by the local governments.

²⁵ The *Hukou* system is a governmental household registration system in China, launched in 1958 and still in place today (the system is called “*hujū*” but is commonly known by the name of the records—“*Hukou*”). The plan was implemented by the Chinese Communist Party as a classification system to keep a record of all Chinese, who are registered either as rural or urban citizens. The Chinese government tied all social benefits (e.g., healthcare, education, social security, working rights) to a person’s local government. The *Hukou* system effectively determines where a person is legally defined as a resident. If one is born into a rural *Hukou*, attempting to change to a more attractive residence or to an urban *Hukou* can be extremely difficult, if not impossible.

²⁶ The central government fully subsidizes the basic pension in Central and Western provinces and splits the cost with local governments in Eastern provinces (Cai, Giles, O’Keefe, and Wang 2012).

²⁷ For example, the amount of the NRPS pension is constructed as follows: the base of the amount is 55 plus the actual individual contribution divided by 139. The “139” is the average expected life expectancy (in months) at the age of 60 years. On average, the pension benefits of the NRPS pension program are approximately 70 RMB per month. Finally, the pension plan exhibits large regional disparities in terms of benefits. Although the central government has set a minimum of 55 RMB (approximately 9 USD) per month as a minimum benefit, the local administrations can supplement individual benefits depending on their fiscal capacity or the local cost of living. Therefore, the individual benefit levels can vary tremendously across regions.

3. Conceptual Framework

We provide a general framework that motivates how household members decide on inter vivos transfers.²⁸ Consider a simple model that assumes the following: (1) a parent and a child; (2) a utility normal good; (3) a parent with perfect information; (4) an individual good of monetary transfers; (5) a single period; (6) altruistic family members; and (7) exogenous income sources. The transfers are between two types of individuals: a donor (child) and a recipient (parent). The donor (e.g., an adult child) makes transfers F to a family recipient (e.g., the child's parent). The parent's utility is denoted as U_p and the adult child's utility is denoted as U_c . Private consumption by the parent and the adult child are denoted by C_p and C_c , respectively. The relationship between the utility functions of the adult child and the parent can be expressed as $U_c = U(C_c, V(C_p))$. The child chooses C_c to maximize his/her utility:

$$\max_{C_c} U(C_c, V(C_p)) \quad (1)$$

subject to

$$F \geq 0 \quad (2)$$

$$C_p = I_p + \tau_p + F \leq Y_p \quad (3)$$

$$C_c = I_c - \tau_c - F \leq Y_c \quad (4)$$

$U_c > 0$. U_v captures the intensity of the altruism (e.g., caring parameter) with $0 < U_v < 1$. We assume three income sources for the parent: financial support from adult children (F), net public welfare support (τ_p), and other types of income (I_p). C_p is the consumption of the parent and is given by constraint (3). The consumption function of the child includes the feature that requires him or her to subsidize net public transfers ($-\tau_p$). The adult child also has other sources of income (Y_c). The adult child's consumption function is C_c , as specified by (4).

Because the adult child's income taxes subsidize the transfer, the public welfare support (τ_p) received by the older people is de facto a net public transfer (τ_c). The implication of this relationship is that the total income of the family remains unchanged as $\tau_p = -\tau_c$. The child's

²⁸ Cai et al. (2006) theoretically model the extent to which private transfers respond to the failure of China's city-based pension schemes. The study models whether altruistically motivated private transfers insure retirees against low income in old age. Furthermore, it empirically focuses on the extent to which intergenerational altruism can make up for formal sources of support.

objective function including the transfers is $U_c = (I_c - \tau_c - F, V(I_c + \tau_p + F))$, with the child choosing $F \geq 0$ subject to the three aforementioned constraints.²⁹

The first-order condition (FOC) yields $-U_c + U_v U_p = 0$. Cox (1987) shows that the interior solution to the household maximization problem where household members choose F is: $\frac{\partial F}{\partial Y_c} - \frac{\partial F}{\partial Y_p} = 1$ (also known as the income-pooling result).

Two scenarios emerge from this framework. In the first scenario, the altruistic child can subsequently reduce a dollar of familial transfers previously given to his parent when he provides one dollar to support the public welfare system.³⁰ In this set-up, a complete crowding-out occurs—in other words, a dollar increase of public transfers to the parent will lead to a dollar decrease of private transfers from the child to the parent. In the second scenario, the adult child may reduce his support by less than the amount of public transfers received by his parent, if the adult child bears no specific tax burden for the new welfare program (*i.e.*, $\tau_p \neq -\tau_c$). In this case, the public transfers become windfall benefits for the family. A partial crowding-out occurs instead of a complete crowding-out.

4. Data

4.1 Survey Data

China Health and Retirement Longitudinal Study. Our primary data source is the CHARLS, from which we draw data on retirement status, pension program access and benefits, family transfers, as well as individual-level and community-level socioeconomic information. It is a nationally representative survey that samples individuals who are 45 years of age or older and their spouses. The survey collects data on demographic information, family structure, subjective and objective health status, healthcare use, pension and retirement, work, household wealth, income, and consumption. The sample consists of 17,708 individuals living in 10,287 households in 450 villages/urban communities in 150 counties across 28 of China's 31 provinces.³¹ Figure 2 is a map of the survey's coverage. Basic information on education, gender, age, household size, and

²⁹ Laferrère and Wolff (2006) overview carefully the rich tradition of various family transfer models based on what they call various “pillars” of the pure altruism model.

³⁰ This type of outcome is known as the Ricardian Equivalence. According to this theory, when a fiscal policy is implemented, altruistic family members will redistribute resources among themselves to neutralize the effect of the policy.

³¹ Mainland China includes a total of 31 provinces; The CHARLS survey excludes the provinces of Tibet, Ningxia, and Hainan.

marital status was collected at the individual and household levels. The survey response rate was over 80 percent (94 percent in rural areas and 69 percent in urban areas).^{32,33}

Once the 450 primary sampling units (PSUs) were selected, age-eligible households were interviewed. The 2011 baseline wave interviewed 10,257 households with 18,245 respondents who were of 45 years of age and older.³⁴ The follow-up 2013 wave covered 10,979 households (or 19,666 respondents). The follow-up wave had a high response rate of 88.6 percent of the original participants and 89.6 percent of original households.³⁵ The 2013 CHARLS wave added 2053 new households with 3507 individuals. The 2016 harmonized CHARLS dataset merged all modules in the 2011 and 2013 waves.

Table 1 presents an overview of baseline characteristics (based on the CHARLS sample). Among the eligible sample of participants and nonparticipants, 70 and 69 percent, respectively, were employed in the baseline. About three-fourths of the sample worked in agriculture: 72 percent among participants and nonparticipants alike. The rural sample reported low levels of educational attainment—approximately 48 percent of participants and 46 percent of nonparticipants reported having completed at least the secondary level of education. The average nontransfer household income is 25,665 yuan, of which additional household member income makes up 35 percent. Although a balanced sample is not a necessary identifying assumption for our empirical strategy, our sample is balanced across most of the characteristics, including income, earnings, consumption, and hours worked. As a result, the differences between the two groups are likely to be stable over time and any changes in the treatment exposure are less likely to be associated with changes in the distribution of covariates. We formally test if the difference in means is statistically different from zero for each of the listed variables. It is reassuring that most of the variable means between treated and nontreated areas are statistically similar.

[Table 1 about here]

³² The sampling process occurred in three stages. First, all community-level units were stratified into eight regions, by rural and urban communities and by community/district gross domestic product (GDP) per capita. After this step, 150 communities were randomly chosen using probabilities proportional to size (PPS). Within the 150 communities, three primary sampling units (PSUs) were randomly selected using the same PPS method. Households were selected for an interview if a member of the household was 39 years of age or older. If the spouse of the main respondent was present, then the spouse was also selected for an interview. The individual and household surveys were administered between June 2011 and March 2012 at the respondents' homes using computer-assisted personal interviewing (CAPI) technology. The survey collected information on household income, expenditures, and assets.

³³ Household size in the CHARLS survey includes the number of household residents, including the respondent and/or his/her spouse.

³⁴ Initially, 19,081 households were sampled and 12,740 had age-eligible members, of which 10,257 responded.

³⁵ The number of tracked respondents in the follow-up wave was 16,159 (from the original 18,245 respondents); 9,185 of the original 10,257 households participated in the follow-up survey.

In the baseline, participants were more likely to receive transfers than nonparticipants. Around 43 percent of the participants were receiving transfers from children compared to 36 percent of nonparticipants. Both groups had around 16–18 percent of respondents who transferred money to their children. In monetary terms, participants gave, on average, 813 yuan and received 2051 yuan, whereas nonparticipants gave 765 yuan and received 1534 yuan.

China Health and Nutrition Survey. We additionally supplement the CHARLS data with survey data from the CHNS. We use auxiliary CHNS data on study outcomes because CHNS spans years prior to the program’s implementation, and it enables us to draw on outcome information prior to 2009 (i.e., the year of the program’s introduction) on family transfers, as well as individual-level and district-level socioeconomic information. Using CHNS survey data prior to the introduction of the NRPS enables us to test important identifying assumptions. The CHNS is a longitudinal survey that covered about 19,000 individuals in 15 provinces spanning 216 PSUs.³⁶ Figure 2 presents the coverage map for the CHNS survey. The CHNS dataset is one of the few panel datasets that has collected data on individuals every two or four years for several decades. It has collected a wide range of variables at the individual, household, and community levels, including income, employment, health and nutrition, consumption, water sources, sanitation, demographics, and access to improved roads.

[Figure 2 about here]

The survey was started in 1989 and covers the years 1989, 1991, 1993, 1997, 2000, 2004, 2006, 2009, and 2011, with the intent of providing data on how certain socioeconomic factors affect individuals’ health and nutrition. Subsequently, the CHNS modules include information on food choice, nutritional intake, health behaviors, physical activities, work activities, time usage, and nutritional status. The sample selection process is similar to the multi-stage random selection process used by the CHARLS. First, communities were stratified by the level of income, followed by a weighting scheme that selected four communities from each province (CHNS Research Team, 2010).³⁷ Individual respondents were asked about family transfers, that is, the amount of yuan received in the last year from children, parents, friends, and relatives.

³⁶ The survey covered the following provinces (also presented in Figure 2): Beijing, Chongqing, Guangxi, Guizhou, Heilongjiang, Henan, Hubei, Hunan, Jiangsu, Liaoning, Shaanxi, Shandong, Shanghai, Yunnan, and Zhejiang.

³⁷ From the 2004 survey onward, all questions related to individual activities, lifestyles, health status, demographic status, body shape, mass media exposure, etc., were added to the individual questionnaires. The individual questionnaires are split into two parts: one is for adults who were 18

4.2 Program Participation and Survey Measures of Interhousehold Transfers

To measure NRPS program participation, we rely on the CHARLS survey and its method of capturing individuals' NRPS program participation. In the CHARLS, respondents are specifically asked: "Do you participate in the New Rural Social Pension Insurance program?" Using this question, we can identify which individuals participated in this pension program. In addition, the questionnaire elicits information on the timing of NRPS benefits, as well as the monthly benefit amount (reported in yuan).³⁸ NRPS participants are classified as benefit recipients once they begin receiving income benefits by the age of 60 years or older. Individuals who were 60 years of age or older at the time of NRPS implementation can receive benefits if their living children participate in a pension scheme. Since the NRPS is a voluntary program, we classify nonparticipants as eligible individuals who choose not to participate in the pension program.

We construct an eligible sample based on the general program guidelines. Mainly, we drop observations with an urban *Hukou* status because these individuals are ineligible to participate in NRPS. For individuals living in eligible districts, we drop individuals in the baseline who are older than 60 years of age and without living children. We also drop urban pension participants with a rural *Hukou*.³⁹ In the eligible sample, we can directly observe NRPS participants and nonparticipants. Our eligible sample consists of 15,990 individuals from 429 communities in 121 cities across 28 provinces.

The CHARLS has comprehensive coverage of family transfers. The survey asks respondents to report transfers to/from children, parents, and other relatives. Because financial transfers can represent multiple aspects of family support, the questionnaire distinguishes among the types of support. Specifically, respondents report the amount of monetary and in-kind support given or received. These financial transfers between family members occur on a regular and non-regular basis. For regular transfers, the respondents report a monthly, quarterly, or a

years of age and older and the second is for children who were 18 years of age and younger. Children who were 6 years of age and older and all adults provided their time allocation on household and physical activities, as well as food and beverage consumption. Additional information was collected on smoking status, alcohol consumption, diet, and physical activity for adults and children who were 12 years and older. Adolescents who were 12 years and older and women who were 52 years and younger with children in the age group of 6–18 years living in the household were asked to answer additional questions related to mass media exposure. For adults who were 55 and older, daily living activities and memory test scores were provided.

³⁸ Since benefits are reported in monthly terms, we adjust the variable to an annual benefit amount.

³⁹ Both groups of individuals are not eligible to receive NRPS monthly benefits.

half-year interval. This information is used to construct the annual amount of transfers given/received between the respondent and the adult children, parents, and other relatives.⁴⁰

5. Empirical Strategy

5.1 Difference-in-Difference-in-Differences (DDD) Approach

Our identification strategy relies on within-country variation in pension policy implementation due to the staggered rollout of the policy across communities.⁴¹ We use this staggered implementation over the years 2011 and 2013 as a source of identifying variation to detect the program impacts from pension participation between individuals living in newly integrated communities and individuals who were not offered similar program benefits.

Our first estimation approach is a difference-in-difference-in-differences (DDD) strategy to estimate the impact of the program of having access to pension benefits on individual-level outcomes. We begin by constructing $OfferNRPS_{ct}$ for communities that offer the NRPS at time t .⁴² To estimate the program's intent-to-treat (ITT) effect, we use a two-way fixed-effect DDD model based on Hansen (2007) and Bertrand, Duflo, and Mullainathan (2004). However, because program benefits are only available to individuals who were 60 years of age and older, we interact the program offer with an age indicator, a binary indicator for whether an individual is 60 years or older, following the methodological approach in Katz (1996), Gruber (1994), and Rossin (2011). This approach is more efficient and leads to more precise estimates of the program effect.

$$(1) \quad T_{ict} = \beta_0 + \beta_1(OfferNRPS_{ct} \times Above60_{ict}) + \beta_2 Above60_{ict} \\ + \beta_3 \mathbf{X}_{ict} + \phi_c + \mu_t + \phi_c \times \mu_t + \varepsilon_{ict}$$

T_{ict} is the outcome in our analyses: the extensive and intensive margins of transfers to and from adult children. $Above60_{ict}$ is equal to 1 if the respondent is 60 years of age and older. β_1 in

⁴⁰ There are four types of family transfers that respondents report in the CHARLS. Monetary support can occur on a regular basis and may be used to cover living expenses, water, electricity, telephone expenses, loans, or rents. Non-regular monetary support can cover infrequent events such as festivals, birthdays, weddings, and funerals. However, in-kind transfers are material transfers in the form of food, clothing, etc. These can occur on a regular and irregular basis. The annual transfer amounts are constructed using a two-step process. First, the regular monetary and in-kind transfers are imputed based on the reported intervals. Second, we sum the imputed regular transfers and the non-regular transfers to get the transfer amount in the past year.

⁴¹ Nearly a quarter (23 percent) of program communities were covered by the end of 2010 and over 60 percent by 2012 (Dorfman et al. 2013). Figure 1 shows newly treated communities between 2011 and 2013.

⁴² Given data limitations, we construct $OfferNRPS_{ct}$ based on individual-level data. If no individuals indicate having NRPS at time t in community c , then $OfferNRPS_{ct}$ equals 0. If at least one person reports participating in NRPS, then $OfferNRPS_{ct}$ is set to 1.

equation (1) is the coefficient of interest (DDD estimator), which captures the estimate of the average effect of the program on eligible individuals who are 60 years of age and older. The coefficient captures the average effect of program’s availability on the outcomes of those above 60 years of age who live in a treated community, regardless of whether they decided to participate in the program—the ITT effect. X_{ict} is a vector of individual-level controls.⁴³ ϕ_c and μ_t are community-level and time fixed effects. Community-level fixed effects allow us to account for time-invariant characteristics that affect the likelihood of the program’s availability in the community. Time fixed effects account for community-specific characteristics that could directly influence the health-related outcomes. In addition, we use community-time fixed effects, $\phi_c \times \mu_t$, to control for community differences during the implementation of NRPS.⁴⁴

For specification (1) to yield unbiased estimates of the program’s impact, annual variation in the NRPS program offer across communities should be unrelated to any other observed or unobserved community-specific shocks. The DDD design we use relies on an identifying assumption (common trends assumption) that the important unmeasured variables are either time-invariant group attributes or time-varying factors that are group invariant. Together, these restrictions imply that the time series of outcomes in each group should differ by a fixed amount in every period and should exhibit a common set of period-specific changes. In other words, the identification assumption implies that treatment communities that provide NRPS program benefits would otherwise have changed in a manner similar, on average, to the control communities that did not provide NRPS program benefits. Even though the identification assumption cannot be tested directly, we can to some extent examine whether the two groups (treated and non-treated) exhibit parallel trends in the outcomes prior to 2009 when the program was introduced.

Even though the identification assumption cannot be directly tested, we can to some extent examine whether the two groups—treated areas that offered program benefits and non-treated areas that did not offer program benefits—exhibit parallel trends (i.e., the average change in the non-treated areas represents the counterfactual change in the treated areas if there were no treatment) in the study outcomes prior to 2011 (i.e., the first year for which we have data). The

⁴³ In each specification, we account for education, gender, household size, and marital status. Inter vivos transfers tend to be strongly related to household size and various socioeconomic characteristics of the recipient (McGarry and Schoeni 1995; McGarry 1999).

⁴⁴ The inclusion of community-time fixed effects assumes that the unobserved community-level heterogeneity varies over time; this term can account for community-level time-varying effects related to variables that could in theory affect our outcomes. Therefore, our DDD estimate is net of community-specific time-varying characteristics.

CHARLS is a relatively new dataset that began in 2011 and serves as the baseline year in this study.

Although CHARLS does not have data prior to the NRPS intervention, we can use data from an alternative data source, the CHNS, to analyze data on pre-trends related to the identification assumption regarding parallel trends. The CHNS dataset is particularly suitable, as it covers the CHARLS sampling areas and elicits information from respondents regarding inter vivos transfers received from children. The primary challenge in this particular analysis of the identifying assumption (for potential pre-trends between “treated” and “non-treated” units) is that the community identifiers or the geographic-level variables do not match between the two surveys. Therefore, for this exercise, we had to re-define “treated” and “control” units at the province level (as opposed to the community level) in order to make use of geographic variables available in the CHNS survey and then proceed with the testing for the parallel trends assumption with CHNS data for the period 2004–2009, but at the province level. It is important to underscore that we define a treated province in the CHNS data for the period 2004–2009 based on the baseline data from the CHARLS for the percentage of NRPS-participating communities within a given province. Specifically, we define a “treated” province by using the percentage of communities/“community IDs” that indicated (in the CHARLS survey) that they participated in the NRPS program within a given province to capture the “treatment intensity” of a province and we then define the province’s “treatment status” with a binary definition, i.e., “treated” or “non-treated” province. Using this definition of a “treated” province, we can also proceed to data from the CHNS survey and to examine its data prior to the NRPS introduction for the purpose of a formal test of the common trends assumption. However, we rely on a threshold choice regarding the percentage of NRPS participating communities within a province to define a province with a “treatment” status.^{45,46} Our formal test of the common trends assumption follows the approach in Autor (2003). Using the CHNS survey data, prior to 2011,

⁴⁵ We define a province as “treated” if it had more than a 67 percent coverage rate based on the percentage of NRPS-participating communities within it at the survey baseline. We conducted additional sensitivity analyses based on the alternative choices regarding the coverage rate. In these additional sensitivity analyses, we varied the threshold choice so that we could impose lower and higher thresholds (e.g., 40 percent coverage, 50 percent coverage, and 70 percent coverage) to define a given province as a “treatment” or “control” province; the pattern of these sensitivity analyses remained robust to coverage rate choice, and we do not detect any evidence of violation of the parallel trends assumption based on alternative choices for the coverage rate within a province.

⁴⁶ The CHNS does not sample from the same communities/villages as the CHARLS, so we rely on our definition of treated and control provinces based on the CHARLS to test data in the CHNS.

on several private transfer variables from the 2004, 2006, and 2009 waves, we can estimate the following specification:

$$(2) \quad T_{ict} = \beta_0 + \beta_{-3}D_{ct} + \beta_{-1}D_{ct} + \phi_c + \mu_t + \varepsilon_{ict}$$

T_{ict} is the transfer outcome of interest, and ϕ_c and μ_t are community-level and time fixed effects. The coefficients on D_{ct} estimate the interaction of time-period dummy variables and the treatment indicator for the first pre-treatment period and the last pre-treatment period.⁴⁷ Based on the results presented in Appendix Table A.1, we fail to reject the null hypothesis that trends in the outcomes between treatment and control areas are the same, as β_{-3} and β_{-1} are insignificant.⁴⁸ This test addresses a possible identification threat and bolsters the validity of the underlying estimation approach.

5.2 Two-Stage Least Squares Approach

We further address the possibility that time-varying unobservable characteristics at the community level could bias estimates of β_1 in specification (1). To address any potential endogenous selection of program targeting (i.e., whether a community offers the NRPS program or not), we augment the DDD analysis by additionally instrumenting the program offer at the community level. Therefore, we re-estimate specification (1) using a combination of the DDD estimation and an instrumental variable strategy. The provision of pension benefits in a community may have been a function of its dependency ratio (or factors related to it) in 2009. Therefore, the estimate could potentially confound the effect of the program with mean reversion that would have taken place even in its absence. Thus, we use specifications that control for the interactions between cohort dummies and time periods. We use $OfferNRPS_{ct}$ to instrument for individual participation in the NRPS to augment the DDD estimation, similar to the approach employed by Nunn and Qian (2014). $OfferNRPS_{ct}$ equals 0 if no individuals participate in the NRPS and equals 1 if the community has at least one participant.⁴⁹ We estimate:

$$(3) \quad T_{ict} = \beta_0 + \beta_1(NRPS\widehat{Receipt}_{ict} \times Above60_{ict}) + \beta_2Above60_{ict} + \beta_3\mathbf{X}_{ict} + \phi_c + \mu_t + \phi_c \times \mu_t + \varepsilon_{ict}$$

⁴⁷ The omitted interaction is the second pre-treatment period.

⁴⁸ The results reported in Table 6 provide evidence that there was no statistical difference between the two groups in the period leading up to the implementation of the NRPS program.

⁴⁹ An assumption of this estimation approach is that the proposed instrument does not directly influence the outcome.

$\widehat{NRPSReceipt}_{ict}$ represents an individual's receipt of NRPS benefits and we instrument it with $OfferNRPS_{ct}$. X_{ict} is a vector of individual-level controls; ϕ_c , μ_t , and $\phi_c \times \mu_t$ are community-level, time, and community-time fixed effects, respectively.

5.3 Tobit Model Analysis

Our final analytical approach tackles a couple of data-related issues, both of which have implications for our estimates. The dependent variable in our analysis is self-reported household data on transfers to and from parents. However, since the actual question asked is if a transfer (and what amount of transfer) was made in the last 12 months, a couple of data issues could plague our analysis. First, household respondents might mis-specify small transfer amounts from their adult children throughout the year. For instance, it is possible that respondents reported small positive transfers as no transfers (i.e., zero transfers) because respondents forgot the actual amount of money being transferred to them because it was negligible and because it occurred in the past (i.e., right-censoring of the dependent variable). A second, and more important, econometric concern relates to the fact that the dependent variable was captured based on responses elicited for behavior that occurred in the previous 12 months. By the nature of the question (i.e., whether a transfer occurred in the previous 12 months and the actual amount of the transfer), numerous respondents could have actually made transfers to their elderly parents but simply might not have done so at the time the household survey was administered. This possibility is another example of right-censoring in our outcome variable and one that requires a different approach than the standard ordinary least squares (OLS) estimation. Estimating the effects using OLS or by using a two-stage least squares (2SLS) approach will produce inconsistent estimates due to data censoring (Cameron and Trivedi 2005).⁵⁰

To tackle the issue of a right-censored dependent variable, we estimate the effects using a Tobit maximum likelihood estimator (MLE), a method that addresses the two potential sources of right-censoring, as described above using the following specification:

$$(4) \quad T_{ict}^* = \beta_0 + \beta_1(OfferNRPS_{ct} \times Above60_{ict}) + \beta_2 Above60_{ict}$$

⁵⁰ Several other papers estimate the effect of income (or welfare income) on private transfers, using Tobit analysis alongside OLS for the analysis (Altonji et al. 1996; Raut and Tran 2005; Juarez 2009; Gerardi and Tsai 2014).

$$+ \beta_3 \mathbf{X}_{ict} + \phi_c + \mu_t + \phi_c \times \mu_t + \varepsilon_{ict}$$

where $\varepsilon_{ict} \sim \mathbf{N}(0, \sigma^2)$. We observe a latent version of transfers, T_{ict}^* , for positive amounts strictly greater than 0. The observed dependent variable is:

$$T_{ict} = \begin{cases} T_{ict}^* & \text{if } T_{ict}^* > 0 \\ 0 & \text{if } T_{ict}^* \leq 0 \end{cases}$$

We compare the marginal effect in specification (4) to the DDD estimation based on specification (1). We assume that the dependent variable is observed for all positive values of family transfers and is a linear function of the policy variable and a set of controls. We estimate the Tobit model using a two-way fixed effects model utilizing MLE.

The effect of interest is the marginal effect of the program on actual transfers, not the marginal effect on latent transfers.⁵¹ The marginal effect on latent transfers provides an estimate for uncensored, or desired, transfers. Our objective is to relate the Tobit marginal effects to the DDD estimator in specification (1).⁵² Even though the OLS estimate is inconsistent, it should still provide a reasonable approximation of the marginal effect on actual transfers in equation (4). Estimating specification (4) accounts for data censored at zero, and enables us to obtain the effect of interest: the marginal effect of the program on the expected value of actual transfers.⁵³

6. Results: NRPS Program Impacts

6.1 Impacts on Extensive and Intensive Margins

DDD Approach. In our analysis, we focus on the impacts of the NRPS on both the extensive (i.e., the impact on the *incidence* of inter vivos transfers) and intensive margins (i.e., the effect on the *amount* of inter vivos transfers) of transfers. We begin by estimating the intent-to-treat effects based on specification (1). Table 2, Panel A, reports the OLS estimates. We find that among NRPS beneficiaries, access to NRPS benefits leads to a statistically significant reduction of the likelihood that one receives transfers by 6.7 percentage points and lowers the average

⁵¹ The marginal effect on latent transfers is the ML estimated coefficient on β_1 in (4), $\left(\frac{\partial E[T^* | \text{Offer} \times \text{Above60}, \mathbf{X}]}{\partial \text{Offer} \times \text{Above60}} \right)$.

⁵² In equation (2), the coefficient of interest is: $\left(\frac{\partial E[T | \text{Offer} \times \text{Above60}, \mathbf{X}]}{\partial \text{Offer} \times \text{Above60}} \right)$.

⁵³ After we estimate the Tobit model in (4), we calculate the marginal effect on actual transfers conditional on them being positive transfers: $\left(\frac{\partial E[T | T > 0, \text{Offer} \times \text{Above60}, \mathbf{X}]}{\partial \text{Offer} \times \text{Above60}} \right) = \beta_1 \Phi(\omega)$, where $\omega = \frac{\beta_1 (\text{Offer} \times \text{Above60})}{\sigma}$.

transfer amount by 9.1 percent (although not significant at the conventional level). The negative estimated coefficients support an altruism model (outlined earlier), whereby an increase in pension income reduces private transfers from adult children. Because the OLS coefficients are likely inconsistent due to right-censoring in the dependent variable, we also report the ITT effects using a Tobit model.⁵⁴

[Table 2 about here]

Table 2, Panel B, summarizes the Tobit estimates. We focus on the marginal effects—the program effects on transfers for respondents who have positive transfers. The Tobit marginal effects are higher than the OLS (Panel A), as OLS does not properly account for observations censored at zero. Using the Tobit estimation, we find that access to program benefits had an even more pronounced effect on those with positive transfers. In Panel B, we find that among NRPS beneficiaries, access to NRPS benefits leads to a statistically significant reduction in the probability that one receives transfers from adult children by 7.4 percentage points, and lowered transfer amounts by 14 percent (compared to the average amount).⁵⁵ In the DDD approach (Panel A of Table 2 and Panel A of Table 3), we fail to detect evidence against the null of no statistically significant change in the probability of transfers to children; the Tobit-IV estimation (Panel B of Table 2 and Panel B of Table 3) bolsters the pattern of the results based on the DDD estimation but is not statistically significant at the conventional level ($p < 0.10$). These estimated negative coefficients support the pattern from recent studies on public pension and private transfers in developing countries (Cox et al. 2004; Juarez 2009; Gerardi and Tsai 2014; Jung et al. 2016), although we report estimates of effect sizes that are much lower than previous empirical estimates. Among the set of studies that use data from other middle-income countries (Juarez 2009; Gerardi and Tsai 2014; Cox, Eser, and Jimenez 1998), the estimated likelihood of the pension beneficiary receiving private transfers falls by approximately 0.48 to 0.55; therefore, our estimate is significantly smaller than previous estimates from other middle-income countries.⁵⁶ Cox et al. (2004) use data from low-income countries, but they do not explicitly test

⁵⁴ We also re-estimated the main OLS specifications, but based on the sample used in the Tobit estimations (the Tobit sample is smaller due to issues related to the convergence of the MLE function). Using comparable samples, we find that the pattern and statistical significance results remain consistent with the main results we report.

⁵⁵ In addition to the control variables reported in the analysis, we also estimated additional analyses where we additionally control for health and income measures, measured at the baseline. The pattern and statistical significance of these additional specifications with additional control variables are consistent with the results we report here.

⁵⁶ Using data from South Korea, Jung et al. (2016) estimate a decline of the incidence of transfers by -0.41 percentage points.

for the impact on the likelihood of subsequent intergenerational transfers. In sum, our estimated impacts on subsequent interhousehold transfers are about one-third of the previously estimated coefficients from other low- or middle-income countries.

2SLS and Tobit Estimates of the Program Impacts. Table 3 presents these results in Panel A, using the 2SLS approach, and Panel B, using IV-Tobit marginal effects. The instrumental variable approach has very high first-stage F-statistics (e.g., the F-statistics associated with the first stage are well above the usual rule of thumb of around 10).

[Table 3 about here]

The IV estimates reported in Table 3 provide additional evidence consistent with the pattern of results based on the DDD estimation procedure discussed earlier. As expected, the results based on the IV estimation report slightly higher magnitudes than the ITT effects. Panel A shows that the NRPS benefit recipients report a statistically significant reduction in receiving transfers from children by 9.3 percentage points (baseline mean of 55.1 percent). On the extensive margin, we find that, among program participants, the amount of the transfers received declined by around 13 percent of the average transfer amount. Similar to the result using the DDD estimation, we do not find evidence that the program actually lowered the probability of parents sending income transfers to their children.

Panel B reports the Tobit-IV estimates.⁵⁷ Once we account for the right-censoring of the outcome variable and instrument the individual decision to participate in the program, the results corroborate the overall pattern of program impacts (shown in Table 3, Panel B). However, we find a slightly more substantial effect size on the private transfer to beneficiaries. NRPS beneficiaries experienced a statistically significant reduction in the likelihood of receiving transfers by 10.4 percentage points (approximately 19 percent). The transfer amount received by the benefit recipients declined by 19 percent relative to the average transfer amount received. The effect sizes using the Tobit MLE procedure are slightly higher than the DDD-estimated impacts reported in Table 3. Although the Tobit effect size is somewhat higher than the DDD estimate in Table 3, our estimated coefficients are still considerably lower than the previous

⁵⁷ We used a control function approach to estimate the Tobit-IV, based on Rivers and Vuong (1988) and Wooldridge (2010). The approach proceeded in two steps. In the first step, we estimated the reduced-form regression of the endogenous variable on the instrument and controls using OLS. In the second step, we estimated a Tobit model using the MLE of the dependent variable on the endogenous variable, the OLS residual from first step and controls; we clustered standard errors at the community level; then, we calculated the marginal effects.

empirical estimates of the household response from other middle-income countries. Comparing our estimated effect size of the Tobit MLE procedure to the magnitude of estimates from other middle-income countries based on Juarez (2009), Gerardi and Tsai (2014), and Cox, Eser, and Jimenez (1998), our estimated decline in the likelihood of interhousehold transfers is only one-third of the previously estimated decline in the likelihood of inter vivos transfers. In summary, we find a considerably smaller empirical decline in the likelihood of inter vivos transfers in response to public benefits than in previously reported estimates in studies from other middle-income countries.

Drawing on the results presented in Tables 2 and 3, an important note on how these results relate to the theoretical framework presented earlier is warranted. The pure-altruist model presented in Section 3 posits a negative relationship between the recipient’s publicly funded (i.e., parent’s) income and private transfers from the donor (i.e., adult child). Tables 2 and 3 report results that are consistent with a pure-altruism model, as we detect negative program impacts on both the extensive and intensive margins of private transfers from adult children to their (beneficiary) parent.

6.2 The NRPS Crowd-Out Effect

Next, we investigate whether changes to individual income due to participation in the NRPS led to changes in total inter vivos transfers to the program beneficiary (i.e., the crowd-out effect). As highlighted earlier in the conceptual framework, when an individual receives NRPS benefits, his/her level of income increases, a change that could induce a large equilibrium response in private transfers depending on household preferences. In fact, our model suggests a quantitatively large crowd-out effect induced by the public program. We estimate the crowd-out effect using a 2SLS approach (similar to specification 4), using the following specification⁵⁸:

$$(5) \quad T_{ict} = \beta_0 + \beta_1 \widehat{Benefits}_{ict} + \beta_2 X_{ict} + \phi_c + \mu_t + \varepsilon_{ict}$$

where T_{ict} is the transfer variable of interest, X_{ict} is a vector of control variables, and β_1 is the crowd-out effect. The coefficient β_1 is denominated in yuan for yuan crowd-out, and not a measure of elasticity. We instrument $\widehat{Benefits}_{ict}$ (NRPS benefit amount in yuan) with $OfferNRPS_{ct}$ (an indicator equaling 1 if the community was offered access to NRPS at time t).

⁵⁸ This approach closely follows the approach for estimating the crowd-out effect in Juarez (2009) and Gerardi (2014), who also study interhousehold transfers in response to public programs (in Mexico and Taiwan, respectively).

Both transfers and NRPS benefits are in yuan terms and not in log terms. Therefore, we estimate a crowd-out measure and not an elasticity.⁵⁹

[Table 4 about here]

Table 4 reports the results on the estimated crowd-out effects in two panels (based on two different estimation approaches). Panel A reports the estimates based on the reduced-form approach without accounting for the right-censoring of the outcome variable. Alternatively, Panel B reports the Tobit-IV estimates; this estimate is the one we focus on. Panel B reports a small, negative but imprecisely estimated effect size. In comparison to the estimates of the crowd-out effect reported in other studies dealing with middle-income countries, this study's estimated crowd-out effect is significantly lower. For example, using data from Mexico, Juarez (2009) estimated a crowd-out effect of -0.86 . Jensen (2004) estimated an average crowd-out effect of -0.3 using data from South Africa. The absolute value of the estimated coefficient based on the Tobit-IV estimation (i.e., crowd-out effect of -0.03) is the lowest found among estimates from other middle-income countries, although the effect size is imprecisely estimated.⁶⁰

6.3 Heterogeneity of Program Impacts by Income Level

The amount of transfers could vary by the income level of the recipient. Therefore, we examine the impact of the program across two levels of income, measured at baseline in the CHARLS survey. Specifically, we focus on the magnitude of the crowd-out effect by the households' level of income (i.e., treatment heterogeneity). Treatment heterogeneity could also have important policy implications that can inform better program targeting. To this end, we estimate the local average treatment effects by the level of income and we repeat specifications (3) and (4) for two samples: for households below the poverty line and for households above the poverty line income threshold.⁶¹

⁵⁹ Some empirical papers estimate a log-log specification and then multiply their elasticity measures by a ratio of Y to X to derive a crowd-out effect, or the partial derivative of transfers. Their crowd-out effects vary by choice of mean, median, or quantile values for the ratio of Y to X. As noted in Payne (1998), the model specification matters. In a log-log linear specification, β_1 is the elasticity being estimated. The main assumption here is that the elasticity is constant, which is the opposite of the assumption made in Section 3—that the transfers-benefits gradient is -1 . Following previous literature, we do not use a log-log specification.

⁶⁰ Only one study—Cox, Hansen, and Jimenez (2004)—produced an estimate similar to ours, but it examines the response using data from a low-income country (Philippines).

⁶¹ We use the World Bank's individual income classification of poverty based on the 1.90 USD/day threshold. Based on the World Bank's Millennium Development Goals, China classifies the poverty standard as having a per capita income of 2,300 in 2010 yuan (World Bank, 2017). The standard is approximately equal to 1.90 USD per day in purchasing-power-parity terms using the 2010 yuan to U.S. conversion. In 2010, 1 USD was worth 3.31 yuan in purchasing-parity terms. This yields a conversion of 2296 yuan, annually.

[Table 5a about here] [Table 5b about here]

We report the results from the two specifications in Tables 5a and 5b, which report the estimates for the crowd-out effect. However, each table reports the results based on different definitions of the independent variable. In Table 5a, we employ a definition of the independent variable that is based on the amount of yuan received as an independent variable. In contrast, Table 5b reports the results based on a specification in which the independent variable is defined as a binary variable (e.g., whether a public benefit was reported, or not). Although the two tables use alternative definitions for the independent variable, both tables report crowd-out effect estimates in response to these alternative definitions of the independent variable. First, we focus our attention on the estimated marginal effects ($E[T | T > 0]$) based on the Tobit-IV estimation, reported in Panel B of Table 5a. As highlighted earlier, this particular estimation addresses the possibility that unobservable characteristics at the community level could bias the estimates of the coefficient for the crowd-out effect and the right-censoring in our outcome variable. The estimated coefficient, based on the 2SLS Tobit estimation (Panel A), is -0.37 for households living below the poverty line (reported in Table 5a, Column 1) and 0.04 for households living above the poverty line (reported in Table 5a, Column 2). These estimated effect sizes suggest that the estimated crowd-out effect is stronger (though we caution that these estimates are imprecisely estimated) for households living below the poverty line. Although this finding is consistent with previous estimates, the magnitude of the crowd-out effect is much smaller compared to the estimated effect sizes from data and interventions from middle-income countries. For example, Cox et al. (2004) examined crowd-out effects under an Indonesian pension program and found that the level of crowd-out is more severe for low-income households.⁶²

Table 5b reports the results for the heterogeneous treatment effects (by income level) for NRPS program participation, but they are based on specifications relying on a binary definition of the independent variable (i.e., receipt of NRPS benefits, or not). We estimated specifications (3) and (4) for respondents living at/below the poverty line and for those living above the poverty

⁶² Cai et al. (2006) found similar evidence in urban China for retired urban pensioners. They studied the question from a different angle and found that the responsiveness of transfers falls as income increases, suggesting a larger crowd-out effect for low-income households in urban China.

line. When we focus on the Tobit-IV estimated effect size, and consistent with the results from Table 5a, we find suggestive evidence that the likelihood of program beneficiaries receiving no inter vivos transfers decreases among households reporting baseline income below the poverty level (as reported in Table 5b, Column 1 for the marginal effect). When we turn our attention to Tobit-IV estimates for the sample reporting baseline income above the poverty line, the estimated effect size on the crowd-out effect was larger than the estimated crowd-out effect for the below-the-poverty-line sample (i.e., the marginal effect reported in Table 5b, Column 3). However, the estimated effect size is statistically significant.^{63,64} These estimates suggest a stronger behavioral response to the NRPS program for high-income households than that for low-income households. From a program targeting perspective, this implies that the policy may be welfare-enhancing for low-income beneficiaries.

7. Robustness Checks of Program Impacts

7.1 Falsification Tests

To test for potential spurious effects, we also perform a falsification exercise based on specifications (1) and (4) but we draw on data from an alternative group of individuals. Specifically, we draw on an alternative “placebo” sample of a group of individuals that, in theory, should not be affected by the program. The NRPS program is available to those individuals living in rural administrative districts who are not already enrolled in an urban pension scheme. In addition, one feature of the NRPS program is that age-eligible individuals can buy their way into the program if their children contribute on their behalf. In the main analysis, we used survey responses to remove urban pensioners and elderly individuals without children who live in rural administrative districts (rural *Hukou*). However, for this falsification exercise, we reconstructed a sample based only on these respondents. In other words, in the falsification exercise, we only included: (1) those individuals who live in rural areas but are obtaining benefits from an urban pension system; or (2) those individuals who are elderly and without children who happen to live in rural administrative districts and have no pension benefits

⁶³ For beneficiaries living below the poverty line, we still detect evidence that they receive fewer inter vivos transfers, but the estimated effect size is insignificant.

⁶⁴ We estimated these regressions using alternative income thresholds, and the results remain consistent. Tables using alternative income-status thresholds are reported in Online Appendix B.

from any other program (urban or rural). The objective of this falsification exercise is to examine whether the transfer outcomes for this “placebo” sample would differ between areas that had NRPS coverage and areas that did not. The placebo sample is not directly affected by the NRPS program in theory; therefore, this falsification exercise should yield non-significant results for NRPS program impacts on transfer outcomes.

Using this alternative sample, we then re-estimate specifications (1) and (4), as outlined in the main analysis, but the estimation is based only on this alternative sample. Appendix Table A.2 reports the results. Panel A (Columns 1 through 4) reports the results for the OLS estimation; Panel B reports the estimated program impacts among the alternative sample (Columns 1 through 4 report the marginal effects, $E[T | T>0]$). Appendix Table A.2 reports estimates of β_1 (from the OLS and Tobit specifications highlighted earlier). The estimates of the effect size are not significant for any of the study outcomes, as theoretically expected. Therefore, the falsification exercise further bolsters the validity of our DDD empirical design.⁶⁵

7.2 Placebo Outcome Tests

As an additional check for the plausibility of the results based on our main specification, we also re-estimate (1) and (4) on a set of outcomes for which no theoretical basis exists to detect program impacts (i.e., placebo outcomes). For this particular exercise, we choose outcomes with no plausible mechanism by which these “placebo outcomes” could have been caused by changes in NRPS program participation. Therefore, any significant correlation would be from random chance or an artifact of the research design that might also have given rise to our main results highlighted earlier. Specifically, we perform this additional falsification exercise with placebo outcomes: the likelihood of one’s nationality being Han and the number of daughters in the household. In Appendix Table A.3, we report the DDD results (for the ITT and TOT specifications) on the two placebo outcomes. Panel A reports the ITT results and Panel B reports the TOT results. Given the high number of observations in the sample for these two outcomes, the reported results fail to detect evidence of any program impacts on the two placebo outcomes.

⁶⁵ The estimations for the Tobit-IV estimation for the transfers-from-children outcome is based on 746 urban pensioners, so we caveat that our results for this falsification exercise are the best available results given the data available, and acknowledge the possibility of the estimations being statistically underpowered.

Because these outcomes were not supposed to be affected by the NRPS reform, this additional falsification exercise further bolsters the validity of our estimation approach.

8. Discussion and Conclusion

China is facing a rapidly aging population, a phenomenon that in the 2000s strained the country's defined-benefit pension system and the country's traditional family-based old-age support system. In 2009, three out of four Chinese workers had no pension coverage. Furthermore, demographic projections show that, by 2030, China will be home for approximately a quarter of the world's population who will be in the age cohort of 60 years and older. The number of Chinese individuals who will be 60 years of age and older will be larger than that of all the European countries combined: it will be equal to the elderly population of North America, Latin America and the Caribbean, Africa, and Oceania (UN 2010). The burden of caring for this large elderly cohort is additionally compounded by the small working-age cohort, a legacy of China's one-child policy. Given the size of its aging population, the Chinese government gradually assumed the responsibility for alleviating old-age poverty via pension reforms. Given the size of the country's population, the implications of reforms to China's old-age pension system can have tremendous consequences.

In this study, we explore the inter vivos behavioral response to China's new rural pension scheme. Specifically, we examine whether the fact that a parent receives pension benefits causes a reduction in private transfers from adult children and estimate the crowd-out effect of public pension benefits to the elderly. Previous studies that examine inter vivos transfers in developing and developed countries find a negative relationship and substantially large substitution between private transfers and the recipient's income. In this paper, we provide early estimates of how pension benefits influence inter vivos decisions in the context of a developing country. We estimate the impact of a public pension program in the context of the largest country in the world, with a population that is rapidly aging. Using a DDD strategy coupled with a Tobit 2SLS estimation, we find that the receipt of NRPS benefits reduces the likelihood that NRPS beneficiaries will receive private transfers from their children, even though the magnitude of reduction is quantitatively small. Our findings are consistent with previous empirical studies on this relationship, but we estimate a substantially smaller effect size (than previous studies in the

context of developing countries) of the private inter vivos transfer response from the children to the parents in response to the public program.

We also empirically estimate for the magnitude of the crowd-out effect among eligible individuals receiving benefits and we find some evidence of the crowding-out of private household transfers, but the estimated effect size is close to zero and statistically insignificant. In comparison to previous studies conducted in middle-income countries, our estimates on the NRPS program impact on both the incidence of inter vivos transfers and on the magnitude of the crowd-out effect are notably smaller. Overall, our estimates of the crowd-out effect, and in particular the smaller effect size in comparison with previous studies in developing countries, could have several explanations. One interpretation for the estimated effect size could be due to the fact that family ties are strong in China given the tradition of filial piety and close ties between parents and children. Another potential interpretation is that the magnitude of NRPS benefits is smaller in rural China than in many other developing countries. Therefore, relatively modest pension benefits in rural China may be unlikely to crowd-out the inter vivos motives of adult children to alleviate the poverty of their parents. A third potential explanation could be that the NRPS program is relatively recent, and the salience of NRPS benefits has not quite been fully recognized and factored into decisions on redistributive transfers among members of the same household.

These small program impacts on subsequent inter vivos transfers from household members imply that the introduction of pecuniary pension benefits can have additional welfare-improving consequences, contrary to the results based on previous empirical studies from other middle-income countries. Our findings have important welfare implications for the poorest households, in particular. One of the objectives of the NRPS program was to ensure the well-being of the aging population and to mitigate the risk of old-age poverty in rural areas, where the poverty rate is already very high. Although conceptually the welfare effects can be ambiguous *ex ante*, any empirical behavioral response and the tendency to further crowd-out familial transfers to the elderly may be of high concern in a program for which one of the objectives is to address old-age poverty. Yet, among the poorest households in our sample, we find no evidence to suggest that there is significant crowding-out of familial transfers to elderly parents as a result of increased NRPS benefits, and the estimated effect size in that subsample is smaller than the one based on the whole sample. Thus, among this demographic, any *ex ante* concerns regarding

disincentives for family transfers appear to be secondary, although understanding the mechanisms behind this interplay between poverty status and strength of the inter vivos transfers merits further research.

The findings reported in this paper are important, because we show that pension reform in the world's largest country could produce pecuniary benefits for program beneficiaries without spurring a negative behavioral response from household members, as suggested by studies conducted in middle- or high-income countries. This study has concentrated on estimating the behavioral response related to inter vivos transfers to beneficiaries of the NRPS program, launched in part to mitigate the risk of old-age poverty. However, this large public program also may have had additional broader impacts on the Chinese population, its economy, and on income inequality. How the NRPS program influenced these outcomes should be the subject of future work.

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Figures and Tables

(a) 2009-2010

(b) 2010-2013

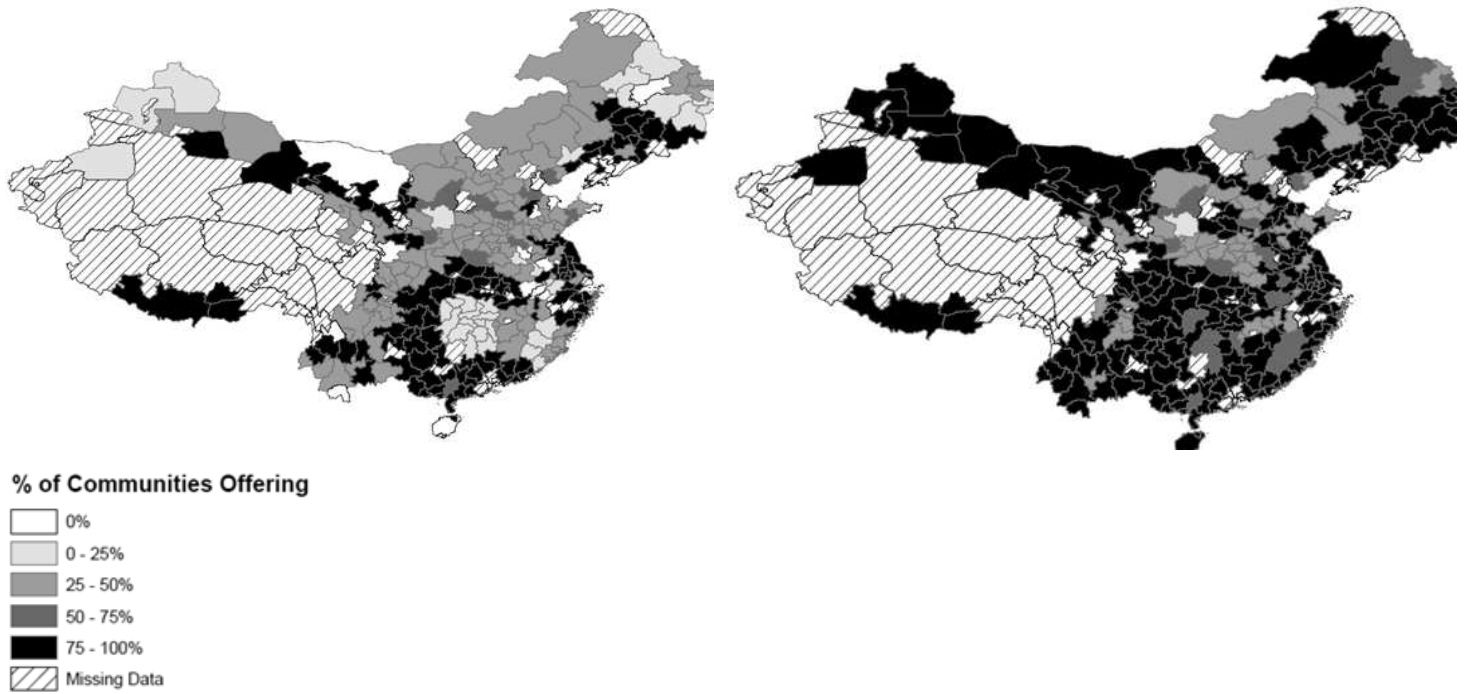


Fig 1. Geographic Implementation of NRPS.

Participating Provinces in China Health and Nutrition Survey

Sampled Communities in the China Health and Retirement Longitudinal Study

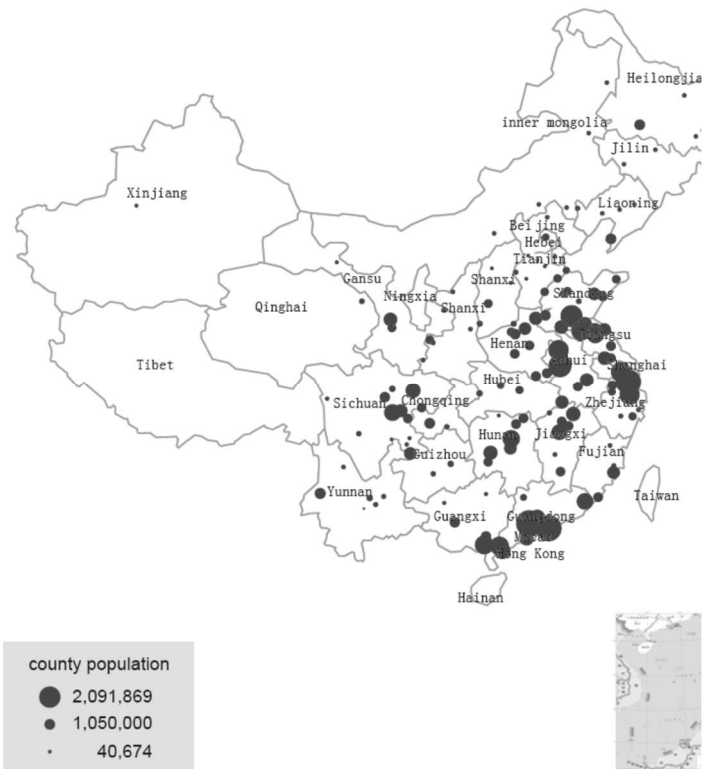
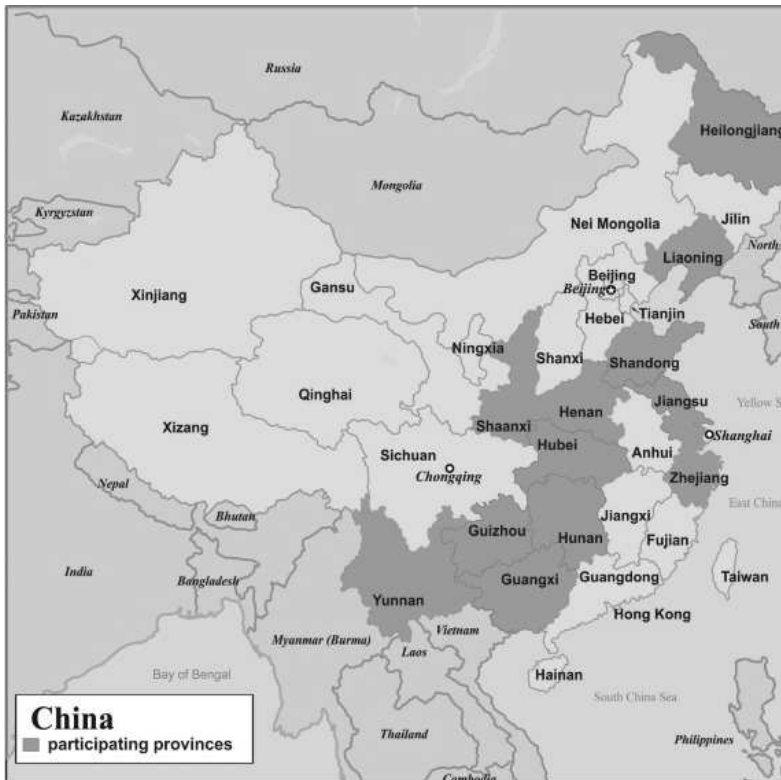


Fig 2. Coverage Maps. Based on CHARLS Research Team (2013) and CHNS Research Team (2015).

Table 1: Summary Statistics.

	Baseline			p-value ^a
	Full Sample	Participants	Non-Participants	
<i>Demographics of Respondents</i>				
Respondent's Age	59.31 (10.01)	58.43 (9.68)	58.44 (10.24)	0.99
# of Household Residents	3.74 (1.87)	3.68 (1.78)	3.75 (1.88)	0.04
Percent Female	0.53 (0.50)	0.54 (0.50)	0.53 (0.50)	0.38
Percent Married	0.80 (0.40)	0.81 (0.39)	0.78 (0.41)	0.00
Percent With At Least Lower Secondary Education	0.48 (0.50)	0.48 (0.50)	0.46 (0.50)	0.10
Weekly Work Hours	45.45 (23.87)	47.26 (24.07)	46.89 (22.70)	0.50
Percent Currently Working	0.70 (0.46)	0.70 (0.46)	0.69 (0.46)	0.11
Percent Working in Agriculture	0.72 (0.45)	0.72 (0.45)	0.73 (0.45)	0.49
<i>Health Domains and Health Status</i>				
Percent with Difficulty Climbing Flight of Stairs	0.43 (0.49)	0.40 (0.49)	0.43 (0.50)	0.01
Mobility Index ^b	0.00 (1.37)	-0.06 (1.35)	0.00 (1.37)	0.02
Percent Rarely Feeling Happy	0.25 (0.43)	0.17 (0.38)	0.19 (0.39)	0.08
Affect Index ^b	0.00 (1.19)	-0.04 (1.17)	-0.01 (1.24)	0.27
Short Distance Vision is At Least Good	0.33 (0.47)	0.33 (0.47)	0.33 (0.47)	0.80
Vision Index ^c	0.00 (1.19)	-0.03 (1.19)	0.03 (1.21)	0.02
Percent with Difficulty Taking Bath	0.07 (0.26)	0.06 (0.23)	0.08 (0.27)	0.00
Self-Care Index ^b	0.00 (1.43)	-0.07 (1.34)	0.06 (1.55)	0.00
Percent with Difficulty Cleaning House	0.11 (0.31)	0.09 (0.29)	0.11 (0.31)	0.00
Usual Activities Index ^b	0.00 (1.47)	-0.06 (1.38)	0.03 (1.51)	0.00
Percent with Weekly Contact with Parents or In-Laws	0.27 (0.44)	0.30 (0.46)	0.28 (0.45)	0.13
Social Interactions Index ^c	0.00 (1.19)	0.15 (1.12)	0.01 (1.16)	0.00
Percent with At Least Good Self-Reported Health Status	0.25 (0.43)	0.27 (0.44)	0.26 (0.44)	0.23
<i>Health Behavior, Healthcare Utilization and Disease States</i>				
Percent Ever Smoked	0.41 (0.49)	0.40 (0.49)	0.40 (0.49)	0.98
Percent Smoking Now	0.25 (0.44)	0.29 (0.45)	0.30 (0.46)	0.39
Percent Consuming Alcohol in Past Year	0.33 (0.47)	0.33 (0.47)	0.33 (0.47)	0.74
Percent Drinking At Least Once a Week	0.18 (0.39)	0.16 (0.37)	0.16 (0.36)	0.28
Sleep Duration (hours per night)	6.28 (1.94)	6.40 (1.93)	6.32 (2.00)	0.07
Number of Doctor Visits	0.20 (0.40)	0.20 (0.40)	0.19 (0.39)	0.07
Number of Nights Stayed at the Hospital	0.11 (0.31)	0.10 (0.29)	0.09 (0.28)	0.06
Percent Ever Diagnosed with Diabetes	0.05 (0.22)	0.05 (0.22)	0.04 (0.20)	0.01
Percent Ever Diagnosed with Hypertension	0.25 (0.43)	0.25 (0.43)	0.22 (0.42)	0.01
Observations	28,034	10,011	3,680	

Notes: Standard deviations are reported in parenthesis. (a) We test the null hypothesis that the difference in participant and non-participant means is equal to 0. (b) Measured in terms of difficulty. Low (or Negative) values denote less difficulty and better mood/emotion. (c) Positively coded where Higher values denote better vision and increased social interaction.

Table 2: Intent-to-Treat (ITT) Estimates on Transfer Outcomes (DDD).

	Transfer From Children (Yes=1)	Transfer To Children (Yes=1)	Transfer From Children (Yuan)	Transfer To Children (Yuan)
	(1)	(2)	(3)	(4)
Panel A (OLS):				
Offered NRPS * Above60 ^a	-0.067*** (0.017)	-0.047* (0.021)	-385.925 (322.244)	-1.977 (263.450)
Baseline Mean	0.551	0.161	4242.603	2847.255
Controls	Yes	Yes	Yes	Yes
R-squared	0.340	0.193	0.102	0.083
Observations	11,562	11,540	11,537	11,534
Panel B (TOBIT) ^b :				
Marginal effect, E(T)	-0.129*** - (0.028)	-0.133** (0.066)	-1639.206*** (538.153)	-1761.077* (1025.290)
Marginal effect, E[T T>0]	-0.074*** (0.016)	-0.040** (0.020)	-576.194*** (187.4478)	-426.139* (245.3874)
Baseline Mean	0.551	0.161	4242.603	2847.255
Controls	Yes	Yes	Yes	Yes
Pseudo R-squared	0.173	0.094	0.013	0.017
Observations	9,210	8,231	9,173	8,225

Notes: (a) Policy instrument interacted with an indicator for being over 60 years old. (b) Tobit panel: Top row is the partial effect on latent dependent variable. Bottom row is the marginal effect on positive transfers (the effect of interest). Individual level controls: Age, Age Squared, Marital Status (=1 if Married), Gender (=1 if Female), Education Levels (Base Group is illiterate with no formal education), Household Size (# of Household Residents). All regressions are estimated with Community, Year and (Community X Year) fixed effects. Clustered standard errors at the community level reported in parenthesis. *p< 0.10, **p< 0.05, ***p< 0.01.

Table 3: Treatment-on-treated (TOT) Estimates on Transfer Outcomes (DDD).^a

	Transfer From Children (Yes=1)	Transfer To Children (Yes=1)	Transfer From Children (Yuan)	Transfer To Children (Yuan)
	(1)	(2)	(3)	(4)
Panel A (2SLS):				
NRPS Receipt* Above60 ^b	-0.093*** (0.023)	-0.065** (0.029)	-535.337 (447.702)	-2.748 (366.131)
Baseline Mean	0.551	0.161	4242.603	2847.255
Controls	Yes	Yes	Yes	Yes
F-Stat (First Stage)	537.74	535.35	536.23	534.87
R-squared	0.341	0.193	0.102	0.083
Observations	11,562	11,540	11,537	11,534
Panel B (TOBIT-IV) ^c :				
Marginal effect, E(T)	-0.180*** (0.028)	-0.183** (0.090)	-2276.892*** (745.359)	-2429.474* (1411.313)
Marginal effect, E[T T>0]	-0.104*** (0.022)	-0.055** (0.027)	-799.732*** (259.123)	-590.058* (340.079)
Baseline Mean	0.551	0.161	4242.603	2847.255
Controls	Yes	Yes	Yes	Yes
F-Stat (First Stage)	480.38	461.90	479.98	461.90
Pseudo R-squared	0.174	0.095	0.013	0.017
Observations	9,210	8,231	9,173	8,225

Notes: (a) The Treatment on the Treated (TOT) effects. (b) Endogenous NRPS Receipt is instrumented by a policy variable (Offer NRPS). (c) Tobit-IV panel: Top row is the partial effect on latent dependent variable. Bottom row is the marginal effect on positive transfers (the effect of interest). Individual level controls: Age, Age Squared, Marital Status (=1 if Married), Gender (=1 if Female), Education Levels (Base Group is illiterate with no formal education), Household Size (# of Household Residents). All regressions are estimated with Community, Year and (Community X Year) FE. Clustered standard errors at the community level reported in parenthesis. *p< 0.10, **p< 0.05, ***p< 0.01.

Table 4: Crowd-Out Effects Using Main Specification.^a

	Transfer From Children (Yuan)	Transfer To Children (Yuan)
	(1)	(2)
Panel A (2SLS):		
NRPS Benefit (Yuan) * Above60 ^b	-0.623 (0.512)	-0.524 (0.461)
Baseline Mean	4242.603	2847.255
Controls	Yes	Yes
F-Stat (First Stage)	34.49	34.70
R-squared	0.049	0.023
Observations	11,696	11,740
Panel B (TOBIT-IV) ^c :		
Marginal effect, E(T)	-0.084 (0.243)	1.010 (0.798)
Marginal effect, E[T T>0]	-0.030 (0.086)	0.238 (0.188)
Baseline Mean	4242.603	2847.255
Controls	Yes	Yes
F-Stat (First Stage)	45.24	45.14
Pseudo R-squared	0.014	0.016
Observations	11,717	11,760

Notes: (a) The crowd-out effect is estimated by regressing transfer amount on NRPS benefit amount interacted with age indicator. (b) Endogenous NRPS Benefit Amount instrumented by policy instrument (NRPS Offer). (c) Tobit- IV panel: Top row is the partial effect on latent dependent variable. Bottom row is the marginal effect on positive transfers (the effect of interest). Individual level controls: Age, Age Squared, Marital Status (=1 if Married), Gender (=1 if Female), Education Levels (Base Group is illiterate with no formal education, Household Size (# of Household Residents). All regressions are estimated with Community, Year and (Community X Year) fixed effects. Clustered standard errors at the community level reported in parenthesis. *p< 0.10, **p< 0.05, ***p< 0.01.

Table 5a: Heterogeneous Crowd-Out Effect by Pre-Transfer Income.^a

	Transfer From Children (Yuan)^b		Transfer To Children (Yuan)^c	
	Pre-Transfer	Pre-Transfer	Pre-Transfer	Pre-Transfer
	Income <= Poverty	Income > Poverty	Income <= Poverty	Income > Poverty
	Line	Line	Line	Line
	(1)	(2)	(3)	(4)
Panel A (2SLS):				
NRPS Receipt * Above60 ^d	0.410 (1.764)	-1.271* (0.743)	-1.235** (0.616)	-0.510 (0.797)
Baseline Mean	2372.711	2372.711	1425.112	1425.112
Controls	Yes	Yes	Yes	Yes
F-Stat (First Stage)	239.29	51.64	247.01	47.87
R-squared	0.159	0.058	0.080	0.030
Observations	3,467	5,552	3,483	5,569
Panel B (TOBIT-IV) ^e :				
Marginal effect, E(T)	-0.986 (1.357)	0.105 (0.222)	-0.126 (1.650)	1.175 (1.158)
Marginal effect, E[T T>0]	-0.374 (0.506)	0.038 (0.079)	-0.028 (0.365)	0.289 (0.285)
Baseline Mean	2372.711	2372.711	1425.112	1425.112
Controls	Yes	Yes	Yes	Yes
Pseudo R-squared	0.021	0.020	0.039	0.022
Observations	3,486	5,577	3,502	5,595

Notes: (a) Based on the World Bank's Millennium Development Goals, China classifies the poverty standard as per capita income of 2,300 in 2010 yuan (World Bank, 2017). (b) Transfer amount from children (reported in yuan). (c) Transfer amount to children (reported in yuan). (d) Endogenous NRPS benefit amount is instrumented by a policy variable (Offer NRPS). (e) Tobit-IV panel: Top row is the partial effect on latent dependent variable. Bottom row is the marginal effect on positive transfers (the effect of interest). Individual level controls: Age, Age Squared, Marital Status (=1 if Married), Gender (=1 if Female), Education Levels (Base Group is illiterate with no formal education), Household Size (# of Household Residents). All regressions are estimated with Community, Year and (Community X Year) fixed effects. Clustered standard errors at the community level reported in parenthesis. *p< 0.10, **p< 0.05, ***p< 0.01.

Table 5b: Heterogeneous Crowd-Out Effect by Pre-Transfer Income.^a

	Pre-Transfer Income ≤ Poverty Line		Pre-Transfer Income > Poverty Line	
	Transfer From Children (Yes=1)^b	Transfer From Children (Yuan)^c	Transfer From Children (Yes=1)^b	Transfer From Children (Yuan)^c
	(1)	(2)	(3)	(4)
Panel A (2SLS):				
NRPS Receipt* Above60 ^d	-0.017 (0.049)	95.701 (1242.683)	-0.101*** (0.036)	-955.016 (660.895)
Baseline Mean	0.475	2160.544	0.333	1480.863
Controls	Yes	Yes	Yes	Yes
F-Stat (First Stage)	653.74	640.80	519.93	519.38
R-squared	0.248	0.083	0.253	0.060
Observations	2,956	2,948	5,208	5,200
Panel B (TOBIT-IV) ^e :				
Marginal effect, E(T)	-0.047 (0.077)	-225.859 (1727.011)	-0.208*** (0.057)	-2797.418*** -934.572
Marginal effect, E[T T>0]	-0.029 (0.048)	-83.485 (635.447)	-0.112*** (0.031)	-973.214*** (323.632)
Baseline Mean	0.475	2160.544	0.333	1480.863
Controls	Yes	Yes	Yes	Yes
F-Stat (First Stage)	808.47	808.47	619.69	619.69
Pseudo R-squared	0.183	0.016	0.169	0.016
Observations	2,956	2,948	5,208	5,200

Notes: (a) Based on the World Bank's Millennium Development Goals, China classifies the poverty standard as per capita income of 2,300 in 2010 yuan (World Bank, 2017). (b) Transfer amount from children (=1 if Yes). (c) Transfer amount to children (reported in yuan). (d) Endogenous NRPS benefit receipt (=1 if began receiving benefits) is instrumented by a binary variable (Offer NRPS). (e) Tobit-IV panel: Top row is the partial effect on latent dependent variable. Bottom row is the marginal effect on positive transfers (the effect of interest). Individual level controls: Age, Age Squared, Marital Status (=1 if Married), Gender (=1 if Female), Education Levels (Base Group is illiterate with no formal education), Household Size (# of Household Residents). All regressions are estimated with Community, Year and (Community X Year) fixed effects. Clustered standard errors at the community level reported in parenthesis. *p< 0.10, **p< 0.05, ***p< 0.01.

Online Appendix

A. Robustness Checks

Table A.1: Test of Common Trends Using CHNS Data.

	Transfer From Children (Yuan)	In-Kind Transfer From Children (Yes=1)	In-Kind Transfer From Children (Yuan)
	(1)	(2)	(3)
Treatment * 2004	-697.621 (692.830)	0.042 (0.086)	83.105 (104.600)
Treatment * 2009	-806.916 (645.392)	0.112 (0.085)	29.249 (106.388)
R-Squared Adj	0.161	0.074	0.151
Year FE	Yes	Yes	Yes
Community FE	Yes	Yes	Yes
Observations	7,032	18,465	9,544

Notes: Base year is 2006. Columns 1-3 are estimated using Ordinary Least Squares (OLS) with Community and Year FE. Clustered standard errors at the community level reported in parenthesis. *p< 0.10, **p< 0.05, ***p< 0.01. Source: CHNS 2004, 2006 and 2009 waves.

Table A.2: Falsification Test Using Placebo Sample^a.

Panel A (OLS):	Transfer From	Transfer To	Transfer From	Transfer To
	Children	Children	Children	Children
	(Yes=1)	(Yes=1)	(Yuan)	(Yuan)
	(1)	(2)	(3)	(4)
Offered NRPS * Above60 ^b	0.051 (0.074)	0.009 (0.082)	127.572 (1330.410)	1533.332 (1815.857)
Baseline Mean	0.233	0.214	7,576.844	16,503.690
Controls	Yes	Yes	Yes	Yes
R-squared	0.553	0.482	0.297	0.357
Observations	685	685	685	685
Panel B (TOBIT) ^c :				
	(1)	(2)	(3)	(4)
Marginal effect, $E(T)$	-0.210 (0.163)	0.070 (0.259)	-5075.081 (3577.173)	-2252.211 (6663.510)
Marginal effect, $E[T T>0]$	-0.053 (0.040)	0.014 (0.053)	-944.095 (634.858)	372.087 (1111.883)
Baseline Mean	0.233	0.214	7,576.844	16,503.690
Controls	Yes	Yes	Yes	Yes
R-squared	0.445	0.365	0.086	0.070
Observations	875	875	875	875

Notes: (a) The placebo sample comprises two groups that in theory should not be affected by the NRPS program: the first group is pensioners who live in rural areas but receive benefits from an urban pension program; the second group is elderly individuals who are ages 60 and above and without any formal pension benefits. (b) Our DDD coefficient: Policy instrument interacted with an indicator for being over 60 years old. A significant coefficient would imply the presence of treatment effects for the “placebo” group in treated communities. (c) Tobit panel: Top row is the partial effect on latent dependent variable. Bottom row is the marginal effect on positive transfers (the effect of interest). Individual level controls: Age, Age Squared, Marital Status (=1 if Married), Gender (=1 if Female), Education Levels (Base Group is illiterate with no formal education), Household Size (# of Household Residents). All regressions are estimated with Community, Year and (Community X Year) FE. Clustered standard errors at the community level reported in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Falsification Test Using Placebo Outcomes.

	Nationality (Han =1)	Daughters (#)
	(1)	(2)
Panel A (ITT):		
Offered NRPS * Above60 ^a	0.013 (0.008)	0.006 (0.044)
Baseline Mean	0.920	1.299
Controls	Yes	Yes
R-squared	0.636	0.147
Observations	12,107	12,107
Panel B (TOT):		
NRPS Receipt * Above60 ^b	0.018 (0.011)	0.082 (0.060)
Baseline Mean	0.920	1.299
Controls	Yes	Yes
F-Stat (First Stage)	797.19	797.19
R-squared	0.636	0.147
Observations	12,107	12,107

Notes: (a) Our ITT coefficient: Policy instrument interacted with an indicator for being over 60 years old. The control group becomes individuals under the Age of 60 living in eligible communities that didn't offer NRPS between '11 and '13. (b) Individual participation instrumented with the policy variable. Individual level controls: Age, Age Squared, Marital Status (=1 if Married), Gender (=1 if Female), Education Levels (Base Group is illiterate with no formal education), Household Size (# of Household Residents). Panel A is estimated using Ordinary Least Squares (OLS) with Community, Year and Community*Year FE. Panel B is estimated using Two-Stage Least Squares (2SLS) with Community, Year and Community*Year FE. Clustered standard errors at the community level reported in parenthesis. *p< 0.10, **p< 0.05, ***p< 0.01.

B. Additional Robustness Checks

Table B.1: Heterogeneous Treatment Effects Using 1.5x the Poverty Line.^a

Panel A (ITT):	Pre-Transfer Income $\leq 1.5 \times$ Poverty Line		Pre-Transfer Income $> 1.5 \times$ Poverty Line	
	Transfer From Children (Yes=1)	Transfer From Children (Yuan)	Transfer From Children (Yes=1)	Transfer From Children (Yuan)
	(1)	(2)	(3)	(4)
OLS				
Offered NRPS* Above60 ^b	-0.029 (0.030)	-174.960 (665.317)	-0.068** (0.029)	-618.438 (539.373)
Baseline Mean	0.464	2041.368	0.319	1477.774
Controls	Yes	Yes	Yes	Yes
R-squared	0.259	0.085	0.248	0.057
Observations	3,635	3,627	4,449	4,441
Tobit^c				
Marginal effect, E(T)	-0.063 (0.046)	-672.061 (971.646)	-0.143*** (-0.048)	-1959.077** (-786.649)
Marginal effect, E[T T>0]	-0.04 (0.029)	-250.749 (355.5855)	-0.073*** (0.024)	-662.442** (263.164)
Baseline Mean	0.464	2041.368	0.319	1477.774
Controls	Yes	Yes	Yes	Yes
Pseudo R-squared	0.188	0.016	0.170	0.017
Observations	3,652	3,644	4,450	4,442
Panel B (TOT):				
2SLS				
NRPS Receipt* Above60 ^d	-0.042 (0.043)	-254.591 (968.908)	-0.094** (0.041)	-859.657 (749.976)
Baseline Mean	0.464	2041.368	0.319	1477.774
Controls	Yes	Yes	Yes	Yes
F-Stat (First Stage)	.	.	419.86	420.41
R-squared	0.260	0.085	0.246	0.057
Observations	3,635	3,627	4,449	4,441
Tobit-IV^e				
Marginal effect, E(T)	-0.093 (0.066)	-979.501 (1396.671)	-0.199*** (0.066)	-2719.265** (1097.652)
Marginal effect, E[T T>0]	-0.058 (0.041)	-363.368 (505.541)	-0.103*** (0.035)	-928.655** (374.146)
Baseline Mean	0.319	1477.774	0.319	1477.774
Controls	Yes	Yes	Yes	Yes
F-Stat (First Stage)	.	.	546.98	546.98
Pseudo R-squared	0.187	0.015	0.170	0.017
Observations	3,635	3,627	4,449	4,441

Notes: a) Based on the World Bank's Income Classification for Millennium Development Goals, China classifies the poverty standard as per capita income of 2,300 in 2010 yuan (World Bank, 2017). (b) Policy instrument interacted with an indicator for being over 60 years old. (c) Tobit panel: Top row is the partial effect on latent dependent variable. Bottom row is the marginal effect on positive transfers (the effect of interest). (d) Endogenous NRPS Receipt is instrumented by a policy variable (Offer NRPS). (e) Tobit-IV panel: Top row is the partial effect on latent dependent variable. Bottom row is the marginal effect on positive transfers (the effect of interest). Individual level controls: Age, Age Squared, Marital Status (=1 if Married), Gender (=1 if Female), Education Levels (Base Group is illiterate with no formal education), Household Size (# of Household Residents). All regressions are estimated with Community, Year and (Community X Year) fixed effects. Clustered standard errors at the community level reported in parenthesis. *p<0.10, **p<0.05, ***p<0.01.

Table B.2: Heterogeneous Treatment Effects Using 2x the Poverty Line.^a

Panel A (ITT):	Pre-Transfer Income $\leq 2 \times$ Poverty Line		Pre-Transfer Income $> 2 \times$ Poverty Line	
	Transfer From Children (Yes=1)	Transfer From Children (Yuan)	Transfer From Children (Yes=1)	Transfer From Children (Yuan)
	(1)	(2)	(3)	(4)
OLS				
Offered NRPS * Above60 ^b	-0.042 (0.025)	-441.830 (565.126)	-0.067** (0.031)	-405.015 (622.075)
Baseline Mean	0.450	1997.352	0.313	1440.244
Controls	Yes	Yes	Yes	Yes
R-squared	0.255	0.061	0.266	0.058
Observations	4,240	4,227	3,766	3,763
Tobit^c				
Marginal effect, E(T)	-0.077* (0.040)	-1034.035 (844.277)	-0.151*** (0.051)	-1893.169** (880.318)
Marginal effect, E[T T>0]	-0.048* (0.025)	-386.796 (305.828)	-0.076*** (0.025)	-630.324** (289.924)
Baseline Mean	0.450	1997.352	0.313	1440.244
Controls	Yes	Yes	Yes	Yes
Pseudo R-squared	0.181	0.014	0.187	0.019
Observations	4,258	4,245	3,771	3,768
Panel B (TOT):				
2SLS				
NRPS Receipt* Above60 ^d	-0.061 (0.043)	-664.727 (826.728)	-0.093** (0.043)	-563.317 (865.708)
Baseline Mean	0.450	1997.352	0.313	1440.244
Controls	Yes	Yes	Yes	Yes
F-Stat (First Stage)	.	.	362.43	362.03
R-squared	0.256	0.060	0.264	0.058
Observations	4,240	4,227	3,766	3,763
Tobit-IV^e				
Marginal effect, E(T)	-0.113** (0.057)	-1507.860 (1223.803)	-0.211*** (0.071)	-2636.567** (1230.315)
Marginal effect, E[T T>0]	-0.070** (0.036)	-559.160 (436.688)	-0.107*** (0.036)	-886.679** (413.069)
Baseline Mean	0.313	1440.244	0.313	1440.244
Controls	Yes	Yes	Yes	Yes
F-Stat (First Stage)	.	.	489.59	489.59
Pseudo R-squared	0.180	0.014	0.186	0.019
Observations	4,240	4,227	3,766	3,763

Notes: a) Based on the World Bank's Income Classification for Millennium Development Goals, China classifies the poverty standard as per capita income of 2,300 in 2010 yuan (World Bank, 2017). (b) Policy instrument interacted with an indicator for being over 60 years old. (c) Tobit panel: Top row is the partial effect on latent dependent variable. Bottom row is the marginal effect on positive transfers (the effect of interest). (d) Endogenous NRPS Receipt is instrumented by a policy variable (Offer NRPS). (e) Tobit-IV panel: Top row is the partial effect on latent dependent variable. Bottom row is the marginal effect on positive transfers (the effect of interest). Individual level controls: Age, Age Squared, Marital Status (=1 if Married), Gender (=1 if Female), Education Levels (Base Group is illiterate with no formal education), Household Size (# of Household Residents). All regressions are estimated with Community, Year and (Community X Year) fixed effects. Clustered standard errors at the community level reported in parenthesis. *p< 0.10, **p< 0.05, ***p< 0.01.