ONLINE APPENDIX

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ACCESS TO PENSIONS, OLD-AGE SUPPORT, AND CHILD INVESTMENT IN THE PEOPLE'S REPUBLIC OF CHINA

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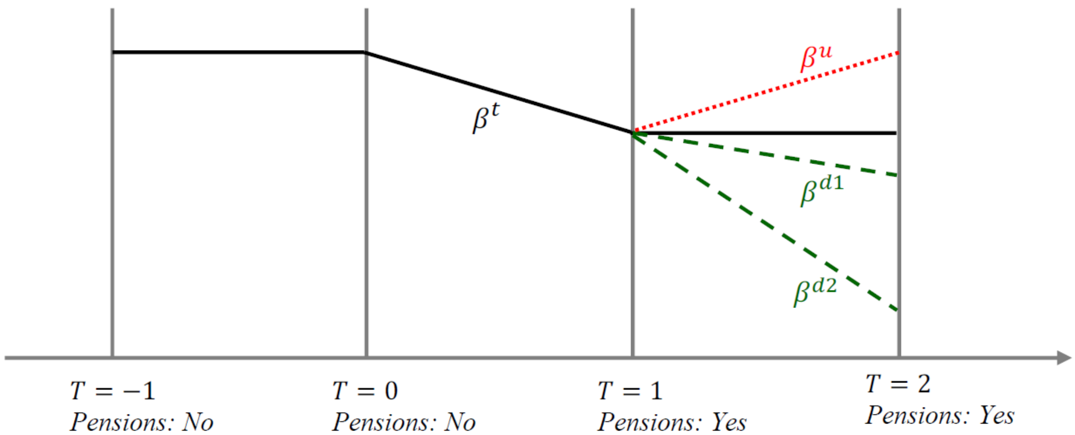
Note: ADB recognizes "China" as the People's Republic of China.

Appendix A: Figures and Tables

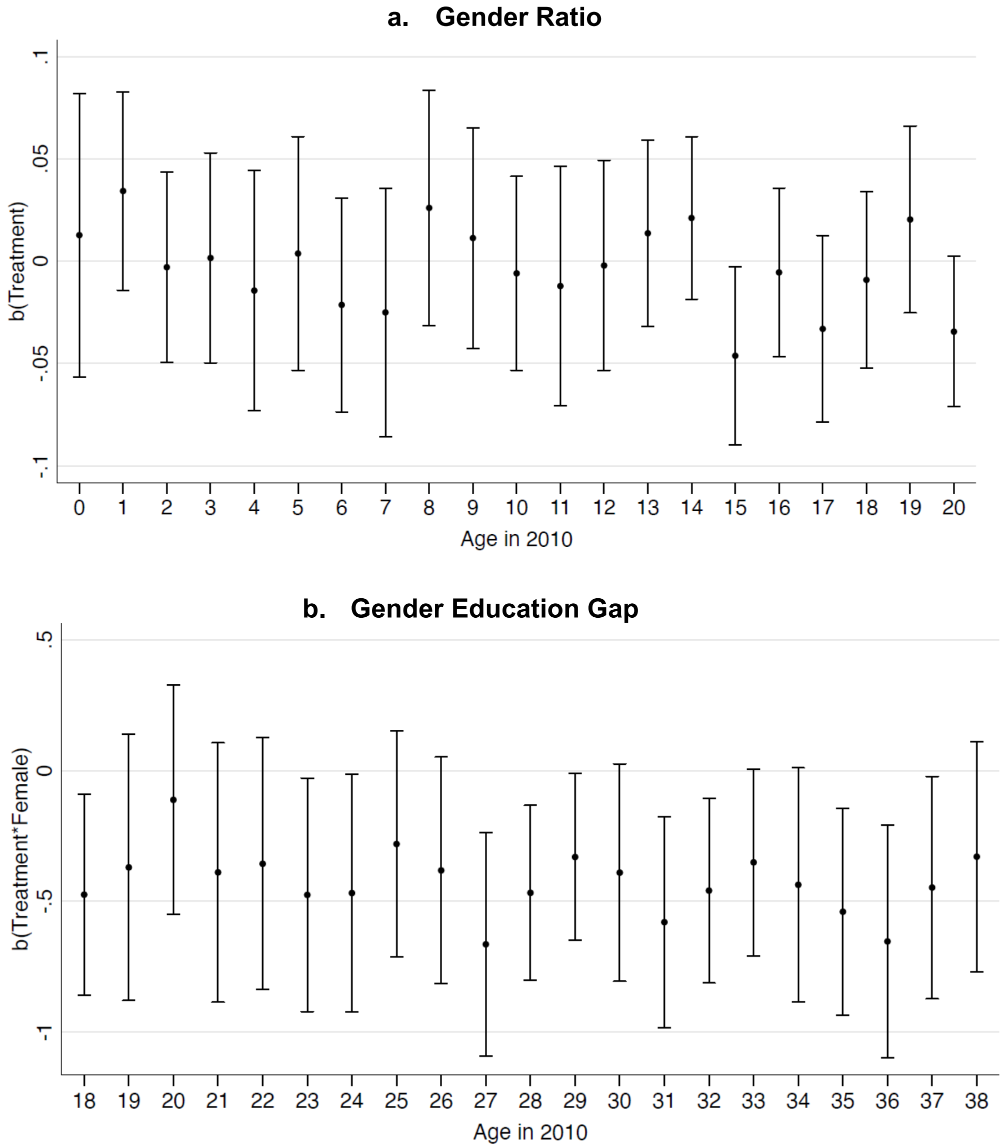
Figure A1: Dynamics of the Treatment Effect and Potential Biases of Our Estimations



Notes: When using counties in the Yes–Yes stage as the control group, our identifying assumption is that its trend is the same as in the No–No stage: being flat in our stylized example. Comparing the trends in the No–Yes stage and Yes–Yes stage therefore identifies the treatment effect of the pension program: β^t. However, as the figure shows, the actual trend in the Yes–Yes stage could be greater or smaller than zero. The trend remains negative when the pension program has a lasting effect: people gradually and consistently change their behavior in response to the treatment. In this case, our estimated treatment effect is biased toward zero (underestimated in magnitude), because the actual control group trend should be zero rather than negative. Note that this direction of bias is the same no matter if the pension program's effect increases (β^{d2}) or decreases (β^{d1}) in magnitude over time. By contrast, if people switch their behavior back toward the pre-treatment level in the Yes-Yes stage (β^u), our empirical strategy leads to overestimation of the treatment effect: we are comparing β^t to β^u, while the right comparison should be between β^t and zero. The directions of bias remain the same if the treatment effect is positive ($\beta^t > 0$): our estimated treatment effect is biased toward zero if the program has lasting and positive effect; our treatment effect is overestimated if the program has positive effect in the No-Yes stage but negative effect in the Yes–Yes stage.

Source: Authors.

Figure A2: Differences in Gender Ratio and Gender Education Gap between the Treatment and Control Group by Cohort



Notes: The graph plots the treatment-control differences in gender ratio and gender gap in years of schooling for different age cohorts. We use individual-level data from the 2010 Chinese Census and include only local rural residents for analysis. Each point estimate is derived from one regression. In Figure A2(a), we regress $Female$ (a dummy variable indicating the female gender) on $Treatment$. In Figure A2(b), we regress schooling years on $Female$ and its interaction term with $Treatment$, controlling for county fixed effects. All regressions cluster standard errors at the county level. Error bars indicate 95% confidence intervals.

Source: Authors.

Table A1: The Impact of the NRPS on Upward Transfers (Excluding Co-Resident Children)

	(1)	(2)	(3)	(4)	(5)	(6)
	Any Transfer		Transfer Wins. 1%		Transfer Wins. 5%	
Transfer from ...	Daughter	Son	Daughter	Son	Daughter	Son
Treatment × Post	0.004	-0.066*	28.5	-248.4***	20.3	-277.3***
	(0.057)	(0.040)	(59.0)	(90.3)	(62.5)	(86.3)
Observations	4,292	2,990	4,292	2,990	4,292	2,990
R-squared	0.218	0.193	0.199	0.165	0.177	0.154

NRPS = National Rural Pension Scheme.

Notes: All regressions control for county fixed effects and province-year fixed effects. The dependent variable (DV) in columns (1)–(2) is the indicator for providing any upward transfers. The DV in columns (3)–(6) is the amount of upward transfers winsorized at the top 1% or 5%. Standard errors are in parentheses and clustered at the county level. $*p < .1$, $**p < .05$, $***p < .01$.

Source: Authors.

Table A2: The Impact of the NRPS on Transfers Received by Parents Aged below 60

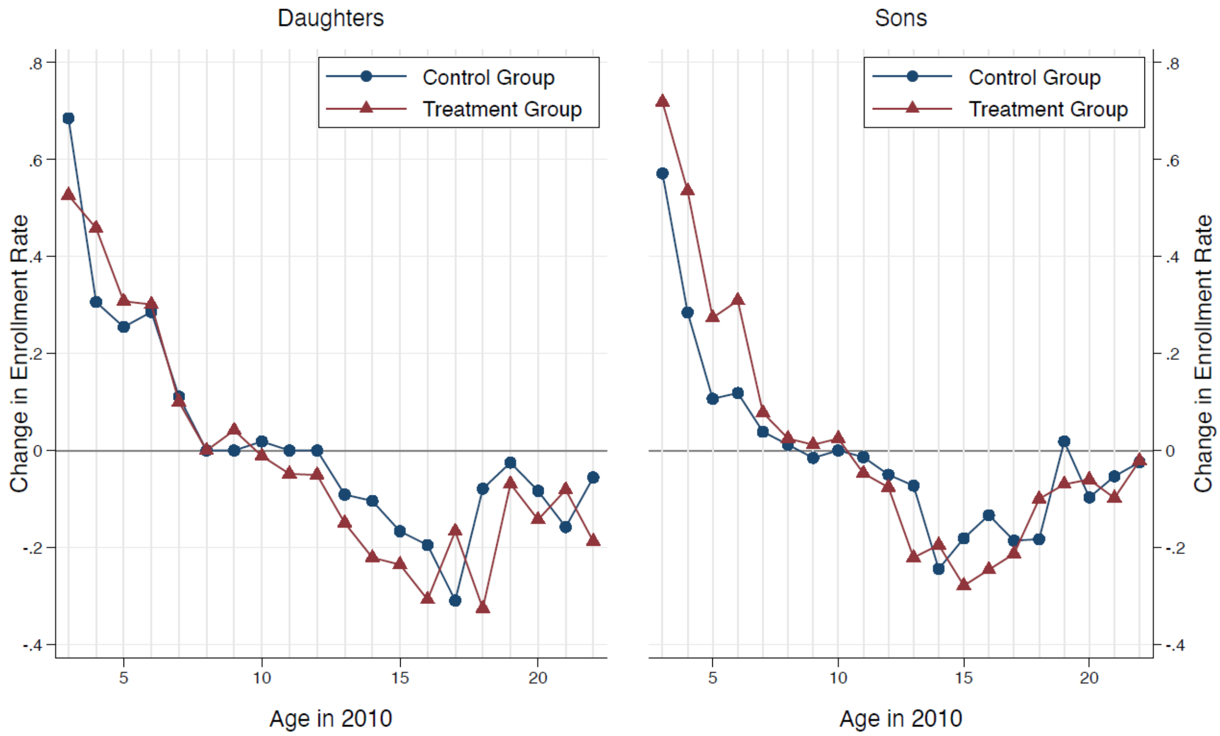
	(1) Any Transfer	(2) Any Net Transfer	(3) Transfer	(4) Net Transfer
Treatment × Post	-0.015	0.020	-245.4	1.1
	(0.050)	(0.040)	(179.3)	(205.6)
Observations	4,010	4,010	4,010	4,010
R-squared	0.295	0.256	0.179	0.117
Control baseline mean	0.328	0.308	560	324.2

NRPS = New Rural Pension Scheme.

Notes: The dependent variables (DVs) are the indicator for receiving any (net) transfers and the amount of (net) transfers. Control variables include county and province-year fixed effects, individual age, schooling years, gender, whether the spouse is alive, the number of children and male children, and the average age of children. *Control baseline mean* refers to the mean of the DV in the control group at baseline. Standard errors are in parentheses and clustered at the county level. $*p < .1$, $**p < .05$, $***p < .01$.

Source: Authors.

Figure A3: School Enrollment by Cohort and Treatment Status



Notes: The graph plots the change of school enrollment from 2010 to 2012 for each age cohort, in either the control group or treatment group. The left graph shows the change of enrollment for female children and the right graph shows the change of enrollment for male children.

Source: Authors.

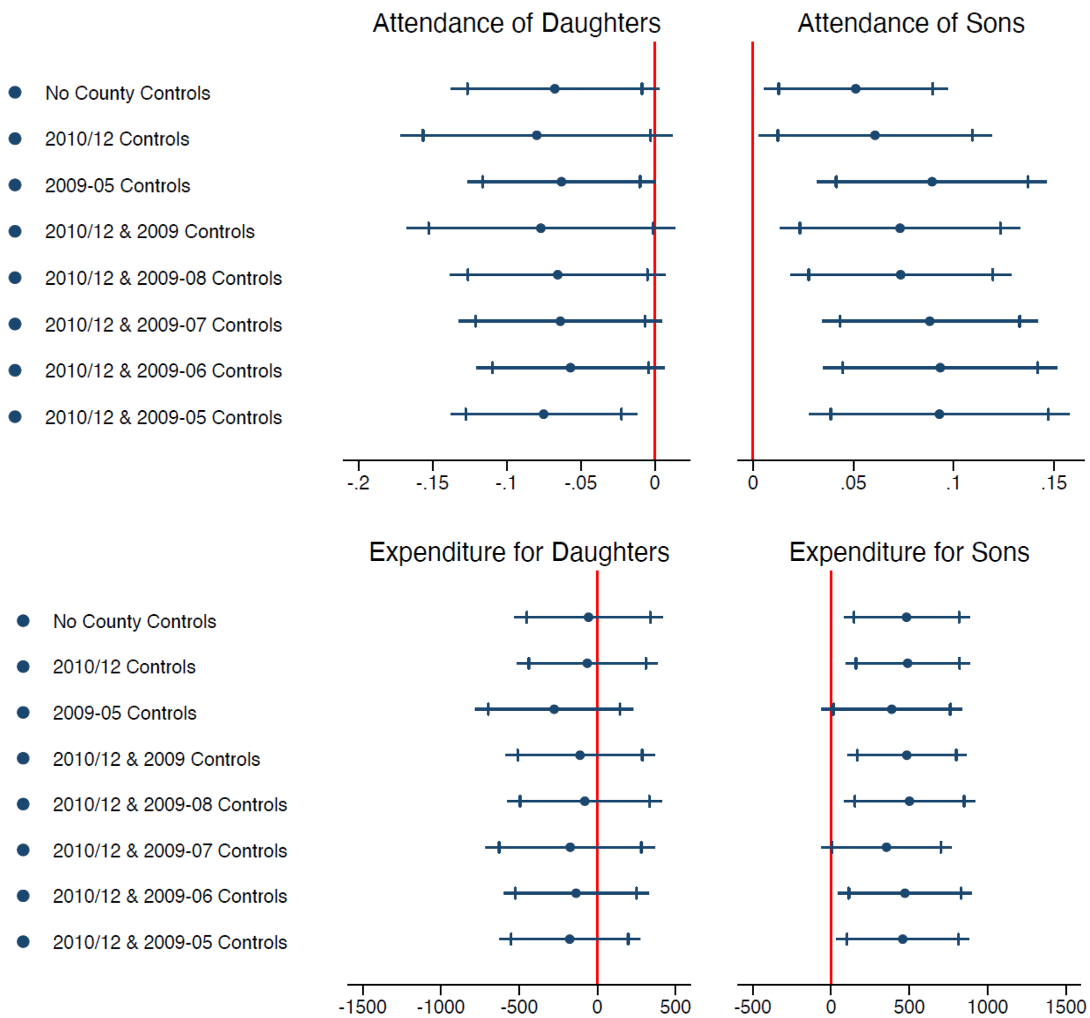
Table A3: Impact on Child Investment for the 5–22 Age Group

	(1)	(2)	(3)	(4)	(5)	(6)
	Enrollment 5%		Expenditures Wins. 1%		Expenditures Wins. 5%	
Investment in ...	Daughter	Son	Daughter	Son	Daughter	Son
Treatment × Post	-0.067***	0.085***	-240.8	504.9**	-184.2	443.3**
	(0.024)	(0.031)	(251.6)	(245.3)	(192.2)	(191.0)
Observations	3,056	3,383	3,056	3,383	3,056	3,383
R-squared	0.223	0.291	0.271	0.211	0.280	0.219
Wald Test: Gender Difference						
χ^2	19.07		6.99		19.30	
p-value	0.000		0.008		0.000	

Notes: All regressions control for county FEs, province-year FEs, child's age, parents' average age and schooling years, as well as county-level (pre-)trends (including log GDP, population, government revenues and expenditures). Standard errors are in parentheses and clustered at the county level. $*p < .1$, $**p < .05$, $***p < .01$.

Source: Authors.

Figure A4: Robustness Check—County Trends



Note: For both school attendance and education expenditure, we plot the estimated coefficient of the interaction term $Treatment \times Post$ for daughters and sons. The estimation model is the same as in Tables 7 and 8. Educational expenditure is the amount winsorized at the top 1 percentile. The error bars indicate 95% and 90% confidence intervals.

Source: Authors.

Table A4: Household Characteristics by Child Gender

Panel A: CHARLS Sample of Adult Children

	Daughters		Sons		Gender Difference
	Mean	SD	Mean	SD	*p*-val
Age	39.45	(5.56)	39.54	(5.42)	[0.567]
Schooling years	5.525	(3.34)	7.196	(2.83)	[0.000]
No. siblings	2.991	(1.46)	2.705	(1.37)	[0.000]
No. male siblings	1.511	(1.03)	1.389	(1.06)	[0.000]
No. female siblings	1.480	(1.21)	1.315	(1.12)	[0.000]
Father's schooling years	4.485	(4.00)	4.490	(3.91)	[0.969]
Mother's schooling years	1.689	(2.81)	1.893	(2.97)	[0.024]
Father's age	68.15	(6.36)	68.23	(6.34)	[0.696]
Mother's age	66.65	(6.83)	66.92	(6.89)	[0.219]
Live in same apartment/yard as parents	0.044	(0.21)	0.408	(0.49)	[0.000]
Live in same household as parents	0.026	(0.16)	0.282	(0.45)	[0.000]

Panel B: CFPS Sample of Children

	Daughters		Sons		Gender Difference
	Mean	SD	Mean	SD	*p*-val
Age	11.06	(5.12)	11.01	(5.43)	[0.762]
No. siblings	1.043	(0.81)	0.855	(0.80)	[0.000]
No. male siblings	0.629	(0.62)	0.357	(0.58)	[0.000]
No. ages 41–60 household members	0.957	(0.89)	0.971	(0.89)	[0.573]
No. ages 61–80 household members	0.427	(0.70)	0.434	(0.69)	[0.706]
No. ages > 81 household members	0.034	(0.20)	0.035	(0.19)	[0.954]
Father's age	38.29	(6.20)	38.72	(6.38)	[0.020]
Mother's age	36.62	(6.20)	37.04	(6.25)	[0.021]
Father's schooling years	6.743	(3.67)	6.749	(3.72)	[0.952]
Mother's schooling years	4.906	(4.08)	4.840	(4.04)	[0.579]

CFPS = China Family Panel Studies, CHARLS = China Health and Retirement Longitudinal Study, SD = standard deviation.

Notes: The table shows descriptive statistics of household characteristics for the sample of (adult) children in the CHARLS and CFPS data. We focus on the baseline pattern and only use data in the first wave—most characteristics remain stable across two waves. "No. ages 41-60 household members" means the number of household members aged 41 to 60, similarly for other age ranges.

Source: China Family Panel Studies and China Health and Retirement Longitudinal Study.

Table A5: Gender Differences in the Impact on Upward Transfers

	(1)	(2)	(3)	(4)
	Transfer		Net Transfer	
Treatment × Post	-148.4**	-168.4	-183.1***	-230.1**
	(57.8)	(116.0)	(55.7)	(111.2)
Treatment × Post × Female	137.1*	176.6*	166.3**	214.3**
	(74.5)	(96.7)	(74.0)	(95.0)
Treatment × Post × No. Sibling		-15.3		-13.9
		(32.5)		(30.8)
Treatment × Post × No. Male Sibling		34.2		44.5
		(48.0)		(44.9)
Treatment × Post × Same Household		60.6		57.6
		(179.0)		(174.1)
Treatment × Post × Same Apartment		82.4		111.3
		(181.5)		(177.5)
Treatment × Post × Both Parents Alive		-36.5		-39.9
		(89.1)		(83.3)
Observations	8,932	8,932	8,932	8,932
R-squared	0.110	0.134	0.099	0.119
Basic controls	Yes	Yes	Yes	Yes
Fully controlled	No	Yes	No	Yes

Notes: The table estimates the impact of the NRPS on upward transfers and how the impact varies with the adult child's gender, number of siblings and male siblings, whether the child lives in the same household or apartment as the parents, and whether both parents are alive. For each triple interaction term, all three variables and all pairwise interaction terms are also controlled for (unless already absorbed by the fixed effects). Columns (1) and (3) only include the basic controls: child age, schooling years, number of siblings and children, parents' average age and schooling years, and whether both parents are alive. Columns (2) and (4) further include all the new variables and interaction terms mentioned. All estimations also control for county fixed effects and province-by-year fixed effects. Standard errors are clustered at the county level. $*p < .1$, $**p < .05$, $***p < .01$. We do not show the results when using "Any Transfer" as the outcome, because we find no statistically significant gender gaps in the extensive-margin impact (both here and in the paper). We do not report the interactions between $Treatment \times Post$ and the age and schooling years of the father and mother, because these characteristics are not always observed. When flexibly controlling for whether they are observed and their values—and the corresponding interaction terms—we find similar results.

Source: Authors.

Table A6: Gender Differences in the Impact on Child Investment

	(1)	(2)	(3)	(4)
	Enrollment		Expenditure	
Panel A: All				
Treatment × Post	0.024	0.082**	358.1***	249.4
	(0.021)	(0.039)	(133.3)	(232.5)
Treatment × Post × Female	-0.061**	-0.069**	-360.9**	-415.3**
	(0.028)	(0.032)	(169.7)	(184.5)
Treatment × Post × No. Sibling		-0.010		-115.0
		(0.027)		(135.7)
Treatment × Post × No. Male Sibling		0.012		397.7**
		(0.032)		(172.5)
Treatment × Post × No. 41–60 HH Members		-0.023		90.1
		(0.018)		(113.5)
Treatment × Post × No. 61–80 HH Members		-0.043**		-29.2
		(0.019)		(127.8)
Treatment × Post × No. >80 HH Members		-0.018		-914.8*
		(0.102)		(469.0)
Panel B: Non-Compulsory Education				
Treatment × Post	0.050	0.170**	473.2**	660.2*
	(0.045)	(0.072)	(190.3)	(350.2)
Treatment × Post × Female	-0.117**	-0.138***	-657.5**	-590.6**
	(0.047)	(0.052)	(282.2)	(285.3)
Basic controls	Yes	Yes	Yes	Yes
Fully controlled	No	Yes	No	Yes

Notes: The table estimates the impact of the NRPS on child investment and how the impact varies with child gender and other household characteristics: the child's number of siblings and male siblings, the number of household members aged 41 to 60, 61 to 80, and above 80. For each triple interaction term, all three variables and all pairwise interaction terms are also controlled for (unless already absorbed by the fixed effects). The basic controls include the child's age and parents' average age and schooling years. The fully controlled regressions include all the individual-level and county-level characteristics specified in Table 7 and all the new variables and corresponding interaction terms. Panel A includes the full sample of children, and Panel B focuses on children in non-compulsory education ages (as defined in Figure 6). Standard errors are clustered at the county level. $*p < .1$, $**p < .05$, $***p < .01$. We do not report the interactions between $Treatment \times Post$ and the age and schooling years of the father and mother, because these characteristics are not always observed. When flexibly controlling for whether they are observed and their values—and the corresponding interaction terms—we find similar results.

Source: Authors.

Table A7: Parents' Participation in the NRPS

	(1)	(2)	(3)	(4)
	All		Mother	Father
Panel A: CHARLS Sample of Parents				
Children's average age	0.000	0.000	-0.002	0.002
	(0.001)	(0.001)	(0.001)	(0.002)
Number of daughters	-0.003	-0.001	0.002	-0.005
	(0.005)	(0.005)	(0.005)	(0.006)
Number of sons	0.000	0.003	0.002	0.002
	(0.006)	(0.006)	(0.007)	(0.008)
Number of living-apart children	0.001	-0.003	-0.008	0.004
	(0.005)	(0.005)	(0.006)	(0.006)
log(parents' wealth)		0.003*	0.005***	0.001
		(0.002)	(0.002)	(0.002)
log(children's average income)		-0.003	-0.003	-0.004
		(0.003)	(0.003)	(0.003)
Panel B: CFPS Sample of Parents				
Children's average age	0.000	0.000	0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Number of sons	0.001	0.001	-0.016	0.016
	(0.008)	(0.008)	(0.010)	(0.010)
Number of daughters	0.001	0.001	0.001	0.007
	(0.007)	(0.007)	(0.009)	(0.008)
log(family income)		-0.004	-0.006	-0.003
		(0.004)	(0.005)	(0.004)

CFPS = China Family Panel Studies, CHARLS = China Health and Retirement Longitudinal Study.

Notes: The table tests whether parents' enrollment in the NRPS depends on the number and gender of children. The dependent variable in columns (1) and (2) is the average number of parents enrolled in the program, and the dependent variable in columns (3) and (4) is a dummy variable indicating whether the father or the mother is enrolled in the program. All estimations also control for parents' own age, schooling years, the number of household members in different age ranges (for Panel B), and county-by-year fixed effects. Standard errors are clustered at the county level. $*p < .1$, $**p < .05$, $***p < .01$.

Source: Authors.

Table A8: Impact of the NRPS on Co-Residence and Childcare Support

	(1)	(2)	(3)	(4)
	Panel A: Co-Residence			
	Same Household and Dependent		Same Household	
	Daughter	Son	Daughter	Son
Treatment × Post	-0.005	0.017	0.001	0.028
	(0.010)	(0.027)	(0.013)	(0.026)
	Panel B: Childcare			
	Any Childcare		Childcare Hours	
	Daughter	Son	Daughter	Son
Treatment × Post	0.009	0.011	-22.4	90.2
	(0.012)	(0.021)	(21.0)	(96.2)

NRPS = New Rural Pension Scheme.

Notes: The table shows the impact of the NRPS on adult children's co-residence with parents and the childcare support that parents provide for adult children, both using the CHARLS data. The dependent variable in Panel A is the indicator for living with parents in the same household, either sharing economics resources as one unit (dependent), or inclusive of both dependent and independent co-residence. In Panel B, the dependent variables are whether parents provide any childcare support and the hours of childcare provided for an adult child. All estimations control for province-by-year fixed effects, county fixed effects, parents' age and education, whether both parents are alive, the child's age, education level, the number of siblings, and the number of children. Standard errors are clustered at the county level. $*p < .1$, $** p < .05$, $***p < .01$.

Source: Authors.

Appendix B: Proof of Prediction 2

Prediction 2. Access to pensions has an ambiguous impact on child investment, due to (i) a positive effect of the windfall shock—"income effect (IE)," and (ii) a negative effect of the saving shock—"substitution effect (SE)."

Proof. As mentioned, the income effect is $\frac{\delta H^*}{\delta \bar{E}} = \frac{1}{\frac{(1+\alpha)w}{y} + \frac{xw}{R} + 1} \equiv \frac{1}{f(R)}$; the substitution effect is $\frac{\delta H^*}{\delta R} = -\frac{f\prime(R)}{f(R)^2}$. Because both x and y are positive, it is obvious to see that IE is positive. To prove that SE is negative, we only need to prove that $f'(R) > 0$.

First, we show that $\frac{x}{R}$ increases with R. The partial derivative of x/R with respect to R is:

$$\frac{\delta(x/R)}{\delta R} = \frac{(1+\gamma)g(R)}{[-R\tau(1-\tau)(1+\gamma)+\gamma w(1-\theta)(1-\tau)+w\theta\tau]^2 R^2} \tag{14}$$

where $g(R) = R^2\tau(1-\tau)[(1-\theta)(1-\tau)+\gamma\theta\tau] - 2R\theta\tau(1-\tau)(1-\theta)w(1+\gamma) + \theta(1-\theta)w^2[\gamma(1-\tau)(1-\theta)+\theta\tau]$.

Under the assumption that $\frac{\gamma\theta}{1-\theta} < \frac{R(1-\tau)-\theta w}{(1-\theta)w-R\tau} < \frac{\gamma(1-\tau)}{\tau}$, we only need to prove that $g(R)$ is positive in the range of $R \in (\frac{\omega\theta(1-\theta)(1+\gamma)}{(1-\tau)(1-\theta)+\gamma\theta\tau}, \frac{\omega\gamma(1-\theta\tau)+\omega\theta\tau}{\tau(1-\tau)(1+\gamma)})$. Because $g(R)$ is monotonically increasing with R in the range, we have

$$g(R) > g(R)|_{R=\frac{w\theta(1-\theta)(1+\gamma)}{(1-\tau)(1-\theta)+\gamma\theta\tau}}$$

$$= \frac{\gamma\theta(1-\theta)w^2}{(1-\tau)(1-\theta)+\gamma\theta\tau}(1-\tau-\theta)^2 \geq 0$$

Second, we show that x increases with R:

$$x'(R) = \frac{\gamma w(1-\theta-\tau)^2}{[-R\tau(1-\tau)(1+\gamma)+\gamma w(1-\theta)(1-\tau)+w\theta\tau]^2} > 0 \tag{15}$$

Because y decreases with x, $\frac{1}{y}$ increases with R. Taken together, we know that $f(R) = \frac{(1+\alpha)w}{y} + \frac{xw}{R} + 1$ increases with R.

Appendix C: Definition of Treatment Status—Details and Robustness

Definition of pension coverage. We use survey respondents' reported pension enrollment information to determine whether the New Rural Pension Scheme (NRPS) is available in a county in each wave. First, we prepare the analysis sample in both the China Health and Retirement Longitudinal Study (CHARLS) and the China Family Panel Studies (CFPS) following our selection criteria in terms of age, Hukou location and agricultural status, and the presence of (grand)parents or (grand)children (Section 3.1 has all the selection conditions). As Table C1 shows, the analysis sample has 123 counties in the CHARLS and 128 counties in the CFPS. Second, we retrieve all adult respondents with a local agricultural Hukou (not only those in our analysis sample) in these counties and document their participation in the NRPS in two waves.

Third, we define counties with at least five participants as being covered by the program and counties with less than five participants as not covered. To avoid the chance of small counties being classified as "not covered," we limit our analysis to counties with at least 20 adult respondents. This condition lowers the number of counties in the CFPS from 128 to 121 and drops only 46 observations in the analysis sample. For the CHARLS, this condition does not affect the sample. Figure C1 plots the distribution of the 123 CHARLS counties and 128 CFPS counties in our analysis sample by the number of adult respondents. Most counties in our sample have at least 50 adult respondents. We find that limiting our analysis to counties with at least 30 or 50 adult respondents does not change our results in a material way. Lastly, we merge the defined county-level pension availability with our analysis sample and get our final analysis sample—Table C1 summarizes the number of counties and (adult) children by the treatment status.

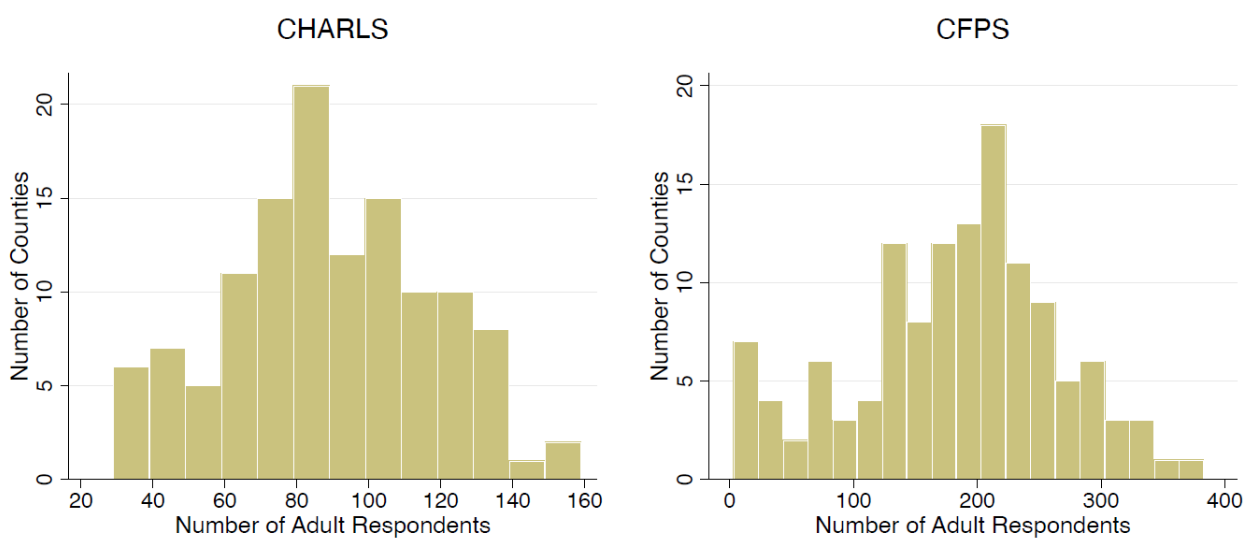
Table C1: Sample Size by Treatment Status

	(1)	(2)	(3)	(4)
	CHARLS		CFPS	
N(Counties) in the analysis sample	123		128	
N(Counties) with at least 20 adult respondents	123		121	
	Coverage Definition		Coverage Definition	
	Default	Strict	Default	Strict
N(Counties) in No–Yes group (treatment)	56	56	60	57
N(Counties) in Yes–Yes group	66	60	54	36
N(Counties) in No–No group	1	1	7	7
N(Children) in No–Yes group (treatment)	4,106	4,106	5,460	5,118
N(Children) in Yes–Yes group	4,826	4,594	3,448	2,508
N(Children) in No–No group	34	34	570	570

CFPS = China Family Panel Studies, CHARLS = China Health and Retirement Longitudinal Study.

Source: China Family Panel Studies and China Health and Retirement Longitudinal Study.

Figure C1: Distribution of Counties by the Number of Adult Respondents



CFPS = China Family Panel Studies, CHARLS = China Health and Retirement Longitudinal Study.

Source: China Family Panel Studies and China Health and Retirement Longitudinal Study.

Stricter definition of pension coverage. For robustness, we define pension availability in a stricter way than the default version described above: counties with at least 10 NRPS participants are defined as covered, and counties with less than five participants are defined as not covered. This definition further lowers the chance of misclassification but also lowers the sample size—as summarized in columns (2) and (4) of Table C1. Employing this strict definition of pension coverage, we analyze the effects of the NRPS on expectations about old-age support, upward transfers, and educational investment in Tables C2, C3, and C4. These results are very similar to the results estimated using the default definition of pension coverage.

Table C2: Impact on Old-age Support Expectations —Using Stricter Coverage Definition

	(1)	(2)	(3)	(4)	(5)	(6)
	All Parents		Parents aged < 60		Parents aged ≥ 60	
Rely on . . . for support	Children	Pensions	Children	Pensions	Children	Pensions
Treatment × Post	-0.071**	0.072***	-0.063*	0.043*	-0.076**	0.092***
	(0.031)	(0.024)	(0.034)	(0.024)	(0.038)	(0.032)
Observations	10,416	10,416	5,038	5,038	5,378	5,378
R-squared	0.080	0.090	0.097	0.110	0.103	0.122

Notes: All regressions control for county FEs, province-year FEs, individual age, schooling years, if the spouse is alive, number of children and male children, and average age of children. Standard errors are in parentheses and clustered at the county level. $*p < .1$, $**p < .05$, $***p < .01$.

Source: Authors.

Table C3: Impact on Upward Transfers—Using Stricter Coverage Definition

	(1)	(2)	(3)	(4)	(5)	(6)
	Any Transfer		Transfer Wins. 1%		Transfer Wins. 5%	
Transfer from ...	Daughter	Son	Daughter	Son	Daughter	Son
Treatment × Post	-0.007	-0.047	22.4	-222.5***	15.3	-251.4***
	(0.056)	(0.033)	(58.8)	(56.2)	(62.5)	(53.9)
Observations	4,310	4,390	4,310	4,390	4,310	4,390
R-squared	0.208	0.157	0.191	0.107	0.171	0.100
Wald Test: Gender Difference						
χ^2	0.693		9.088		10.41	
p-value	0.405		0.003		0.001	

Notes: All regressions control for county FEs, province-year FEs, child characteristics (gender, age, schooling years and number of siblings), and parents' characteristics (age, schooling years and if both alive). Standard errors are in parentheses and clustered at the county level. $*p < .1$, $**\ p < .05$, $***p < .01$.

Source: Authors.

Table C4: Impact on Educational Investment—Using Stricter Coverage Definition

	(1)	(2)	(3)	(4)	(5)	(6)
	Enrollment 5%		Expenditures Wins. 1%		Expenditures Wins.	
Investment in ...	Daughter	Son	Daughter	Son	Daughter	Son
Treatment × Post	-0.017	0.163***	195.7	747.8***	33.3	673.2***
	(0.031)	(0.047)	(486.9)	(254.0)	(337.7)	(182.4)
Observations	2,923	3,327	2,923	3,327	2,923	3,327
R-squared	0.115	0.138	0.264	0.221	0.276	0.232
Wald Test: Gender Difference						
χ^2	14.97		1.86		5.37	
p-value	0.000		0.173		0.020	

Notes: All regressions control for county FEs, province-year FEs, child's age, parents' average age and schooling years, as well as county-level (pre-)trends (including $\log GDP$, $\log population$, $\log government\ revenues$, and $\log expenditures$). Standard errors are in parentheses and clustered at the county level. $*p < .1$, $**\,p < .05$, $***p < .01$.

Source: Authors.

Cross-checking pension coverage. To further mitigate concerns about the misreporting of pension enrollment and the misclassification of pension coverage, we manually cross-checked the coverage status with the list of national pilot counties and online news reports on the introduction of the NRPS in each sample county. When there is misalignment between the timing of pilot county listing and our defined timing of pension coverage, we further check if online news reports are in conflict with our definition. If news reports are still misaligned with our definition, we flag the definition as possibly mistaken. Notice that we can only implement this cross-checking procedure for the CFPS data, because the names of CHARLS counties are not revealed. Also, online reports on county-level introduction of the program are rare and sometimes only suggestive. We find that out of the 121 CFPS counties, 7 could be misclassified using our default coverage definition. This means only 324 child observations (4%) out of the total 8,908 observations are possibly misclassified. Table C5 shows the estimated impact on educational investment after correcting for these potential misclassifications. The results are very similar, and we still find significant gender differences in the impact.

Table C5: Impact on Educational Investment —Correcting Possible Misclassifications

	(1)	(2)	(3)	(4)	(5)	(6)
	Enrollment 5%		Expenditures Wins. 1%		Expenditures Wins.	
	Daughter	Son	Daughter	Son	Daughter	Son
Treatment × Post	-0.048	0.084**	3.0	469.9**	-39.3	361.7**
	(0.033)	(0.033)	(246.6)	(207.8)	(171.7)	(158.6)
Observations	3,459	3,879	3,459	3,879	3,459	3,879
R-squared	0.110	0.146	0.276	0.216	0.289	0.226
Wald Test: Gender Difference						
χ^2	13.63		3.43		4.50	
p-value	0.000		0.064		0.034	

Notes: All regressions control for county FEs, province-year FEs, child's age, parents' average age and schooling years, as well as county-level (pre-)trends (including $\log GDP$, $\log population$, $\log government\ revenues$, and $\log expenditures$). Standard errors are in parentheses and clustered at the county level. $*p < .1$, $**\ p < .05$, $***p < .01$.

Source: Authors.

"No-No" as the control group. As discussed in Section 3, using counties with access to the NRPS in both waves ("Yes-Yes") as the control group may bias the estimated treatment effect. We test if this bias is likely driving the results by using a small number of the CFPS counties without access to the program in both waves ("No-No") as the control group—the 7 counties listed in Table C1. Although the sample size is much smaller, we still find a similar pattern. As Panel A of Table C6 shows, parents lower investment in their daughters but raise investment in their sons. The gender difference also remains significant (*p*-value is 0.06 for enrollment and 0.05 for expenditures). Panel B uses "No-No" and "Yes-Yes" counties as the combined control group and shows similar but more precisely estimated results.

Table C6: The Impact of the NRPS on Educational Investment —Using Alternative Control Groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A: "No-No" as Control Group				Panel B: "No-No" & "Yes-Yes" Combined			
	Enrollment		Expenditures		Enrollment		Expenditures	
	Daughter	Son	Daughter	Son	Daughter	Son	Daughter	Son
Treatment x Post	-0.084*	0.078	-516.1	112.8	-0.069**	0.093***	-294.4	282.8**
	(0.042)	(0.091)	(341.7)	(205.1)	(0.030)	(0.033)	(187.6)	(141.5)
Observations	2,305	2,745	2,305	2,745	3,687	4,187	3,687	4,187
R-squared	0.138	0.161	0.301	0.226	0.134	0.158	0.297	0.230
Wald Test: Gender Difference								
x^2		3.44		3.79		29.33		9.35
p-value		0.064		0.051		0.000		0.002

Notes: Expenditures are winsorized at the top 5 percentiles. All regressions control for county FEs, province-year FEs, child's age, parents' average age and schooling years, as well as county-level (pre-)trends (including $\log GDP$, $\log population$, $\log government\ revenues$, and $\log expenditures$). Standard errors are in parentheses and clustered at the county level. $*p < .1$, $**\ p < .05$, $***p < .01$.

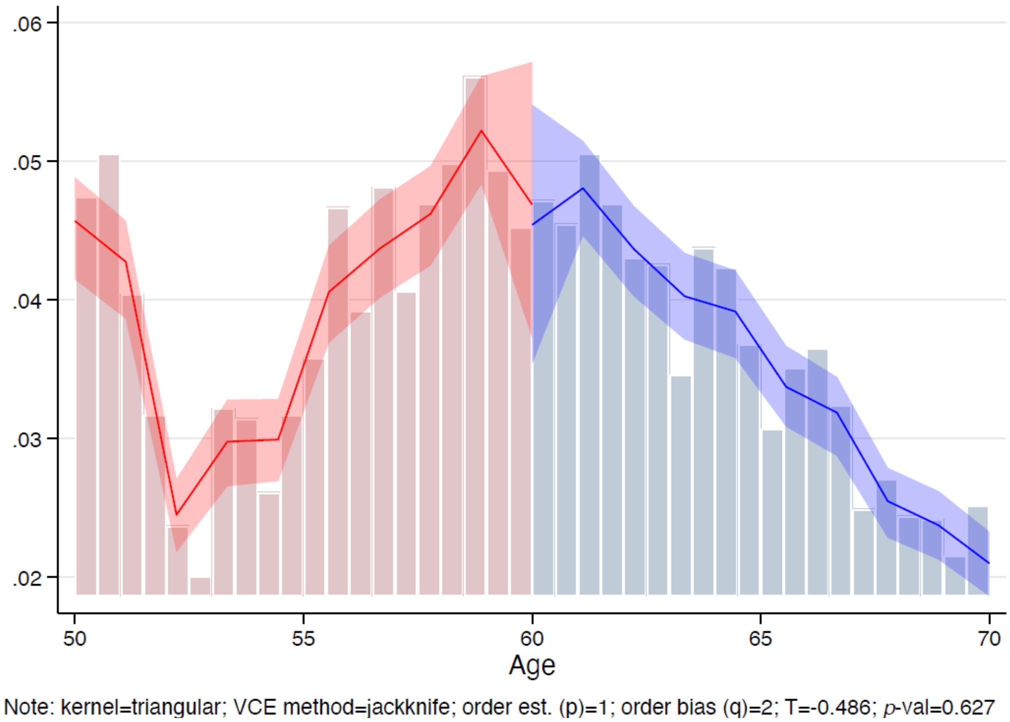
Source: Authors.

Appendix D: The Impact on Old-Age Support—RDD Results

As mentioned, the New Rural Pension Scheme (NRPS) creates a discontinuous access to pension payments: only participants aged above 60 receive pensions. This windfall shock of pension payments, as predicted by the model, may crowd out transfers from adult children to elderly parents. Therefore, we expect to see a discontinuous drop of transfers at the cutoff age. In Figure 4, we have already shown that age 60 is the de facto threshold for pension payments, but the participation rate in the program is lower than 100%. We therefore use the fuzzy regression discontinuity (FRD) design to estimate the treatment effect of pension payments on transfers.

To estimate the impact of pension incomes on old-age support, we focus on the sample of parents in 2013, when the NRPS is available in all counties. We first check if age, the running variable, is continuous around the cutoff age. Using the manipulation test proposed by Cattaneo et al. (2018), we find no evidence of a discontinuity in density around age 60 (Figure D1). Next, we examine whether the outcome variables exhibit a discontinuous change around the cutoff—the reduced form. Figure D2 plots the likelihood and amount of (net) transfers around the age 60, with 90% confidence intervals. We find that the likelihood of transfers seems continuous at age 60, while the amount of (net) transfers shows a downward jump at the cutoff.

Figure D1: Manipulation Test of the Running Variable (Age)



Notes: Use the manipulation test of Cattaneo et al. (2018). The testing results are robust to the choice of kernel functions, order of local-polynomial for estimation (p), and order of local-polynomial for bias correction (q).

Source: Authors.

Robust fuzzy regression discontinuity estimation. We present the local polynomial FRD estimations of treatment effects in Table D1, based on the approach of Calonico et al. (2014) and Calonico et al. (2019). In the FRD design, age is the running variable and the pension-receiving dummy indicates the treatment status. We use local linear regression $(p = 1)$, second-order local polynomial for bias correction, and choose different bandwidths for robustness. We present results using the triangular kernel function, but results using alternative kernel functions (the Epanechnikov or uniform function) are very similar. We control for gender, education level, whether the spouse is alive, the number of all children and male children, children's average age, and county fixed effects. We cluster standard errors at the county level.

Table D1: The Impact of Pension Payments on Upward Transfers —Robust Fuzzy Regression Discontinuity Estimates

	Any Transfer	Any Net Transfer	Transfer	Net Transfer
Panel A: BW=10 (First-Stage Estimate: 0.381)				
RD Treatment Effect	**-0.047**	**-0.109**	**-389.2**	**-679.5**
Conventional S.E.	(0.063)	(0.069)	(371.1)	(428.4)
Robust *p*-value	0.428	0.019**	0.016**	0.002***
Panel B: BW=8 (First-Stage Estimate: 0.331)				
RD Treatment Effect	**-0.062**	**-0.154**	**-688.0**	**-1138.6**
Conventional S.E.	(0.078)	(0.086)	(465.5)	(544.0)
Robust *p*-value	0.621	0.032**	0.012**	0.002***
Panel C: BW=6 (First-Stage Estimate: 0.261)				
RD Treatment Effect	**-0.071**	**-0.208**	**-1236.1**	**-1811.1**
Conventional S.E.	(0.109)	(0.121)	(659.6)	(780.8)
Robust *p*-value	0.896	0.074*	0.023**	0.006***
Panel D: BW=4 (First-Stage Estimate: 0.152)				
RD Treatment Effect	**0.023**	**-0.268**	**-2244.4**	**-3173.1**
Conventional S.E.	(0.220)	(0.243)	(1405.0)	(1691.1)
Robust *p*-value	0.540	0.092*	0.060*	0.028**

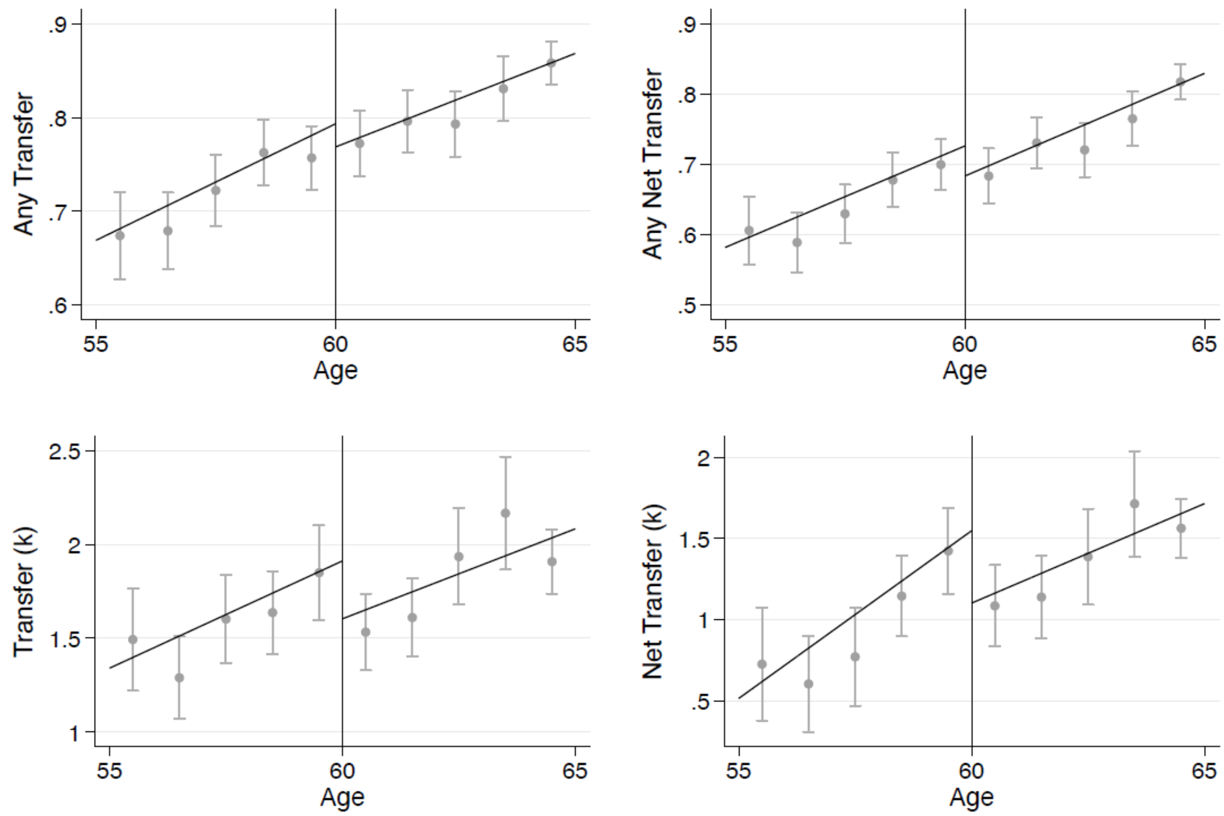
FRD = fuzzy regression discontinuity.

Notes: The table presents the FRD estimates of the impact of receiving pensions on upward transfers, for different choices of bandwidths. We present results using triangular kernel function; other kernel functions give similar results. For each panel of results, we show the first-stage estimates, treatment effect estimates, the conventional standard errors, and robust *p*- values. We control for county fixed effects and individual characteristics like gender, schooling years and whether the spouse is alive. Standard errors are clustered at county level. Significance level is based on the robust *p*-value. $*p < .1$, $**p < .05$, $***p < .01$.

Source: Authors.

In terms of the first stage, all specifications indicate that being older than age 60 increases the likelihood of receiving pensions (significant at the 5% or 1% level). For the likelihood of receiving any transfers from children, the estimated results are not statistically significant. For the likelihood of receiving net transfers, we find a negative effect, and the effect is more precisely estimated with larger bandwidths. In terms of the amount of (net) transfers, consistent with the pattern in Figure D2, we find a robust negative impact of pension payments (the *p*- values are mostly smaller than 0.5). Compared to the difference-in-differences (DID) results presented in Table 6, the FRD estimations display a similar pattern and suggest that the magnitude of the treatment effect decreases as the bandwidth increases.

Figure D2: Likelihood and Amount of Transfers around the Cutoff Age
(Reduced Form)



Notes: Regression discontinuity plot with 90% confidence intervals. Dependent variables in four graphs are respectively the indicator of receiving any transfers, any net transfers, the amount of transfers and net transfers.

Source: Authors.

Robustness and placebo test. In Table D2, we test if individual control variables are continuous around age 60. We do not find a significant jump at the cutoff for any of the covariates: gender, schooling years, schooling years, indicator of alive spouse, number of children, number of male children, and the average age of children. In Table D3, we conduct a placebo test for counties without access to the NRPS in 2011. We find no evidence for discontinuous changes in the likelihood or amount of transfers around age 60.

Table D2: Continuity of Covariates around the Cutoff Age

	Male	School Years	Spouse Alive	N (Children)	N (Male Children	Children Age
Kernel Function: Triangular						
Bandwidth (h)	4.211	6.123	5.119	4.337	4.868	5.800
RD Treatment Effect	**0.012**	**-0.406**	**-0.018**	**0.096**	**0.050**	**0.275**
Robust *p*-value	0.803	0.342	0.340	0.428	0.647	0.731
Kernel Function: Epanechnikov						
Bandwidth (h)	3.809	5.502	4.728	4.087	4.714	5.395
RD Treatment Effect	**0.002**	**-0.363**	**-0.016**	**0.100**	**0.047**	**0.309**
Robust *p*-value	0.973	0.420	0.413	0.397	0.648	0.653
Kernel Function: Uniform						
Bandwidth (h)	3.368	3.414	3.378	3.345	4.281	4.899
RD Treatment Effect	**-0.007**	**-0.001**	**-0.019**	**0.119**	**0.055**	**0.353**
Robust *p*-value	0.782	0.792	0.386	0.314	0.455	0.523

RD = regression discontinuity.

Notes: The table presents the robust regression discontinuity estimates of the impact of being aged above 60 on different covariates: gender, schooling years, indicator for alive spouse, number of children, number of male children, and the average age of children. We present results using triangular, Epanechnikov and uniform kernel functions. The bandwidths are MSE-optimal bandwidths. We control for county fixed effects and cluster standard errors at the county level.

Source: Authors.

Table D3: Placebo—Upward Transfers in Counties without the NRPS

	Any Transfer	Any Net Transfer	Transfer	Net Transfer
Kernel Function: Triangular				
Bandwidth (h)	5.58	6.58	5.96	5.10
RD Treatment Effect	-0.015	0.000	28.20	24.47
Robust *p*-value	0.727	0.943	0.784	0.848
Kernel Function: Epanechnikov				
Bandwidth (h)	4.87	5.47	6.08	4.94
RD Treatment Effect	-0.016	-0.005	19.57	17.90
Robust *p*-value	0.698	0.913	0.838	0.838
Kernel Function: Uniform				
Bandwidth (h)	4.17	4.20	5.06	4.96
RD Treatment Effect	0.001	0.003	-29.26	84.27
Robust *p*-value	0.962	0.966	0.929	0.608

NRPS = New Rural Pension Scheme, RD = regression discontinuity.

Notes: The table presents the regression discontinuity estimates of the impact of being aged above 60 on upward transfers, for counties not covered by the NRPS in 2011. We present results using triangular, Epanechnikov and uniform kernel functions. Bandwidth is chosen by the MSE-optimal band- width selector. For each estimation, we show the selected bandwidth, treatment effect estimate and the robust *p*-value.

Source: Authors.

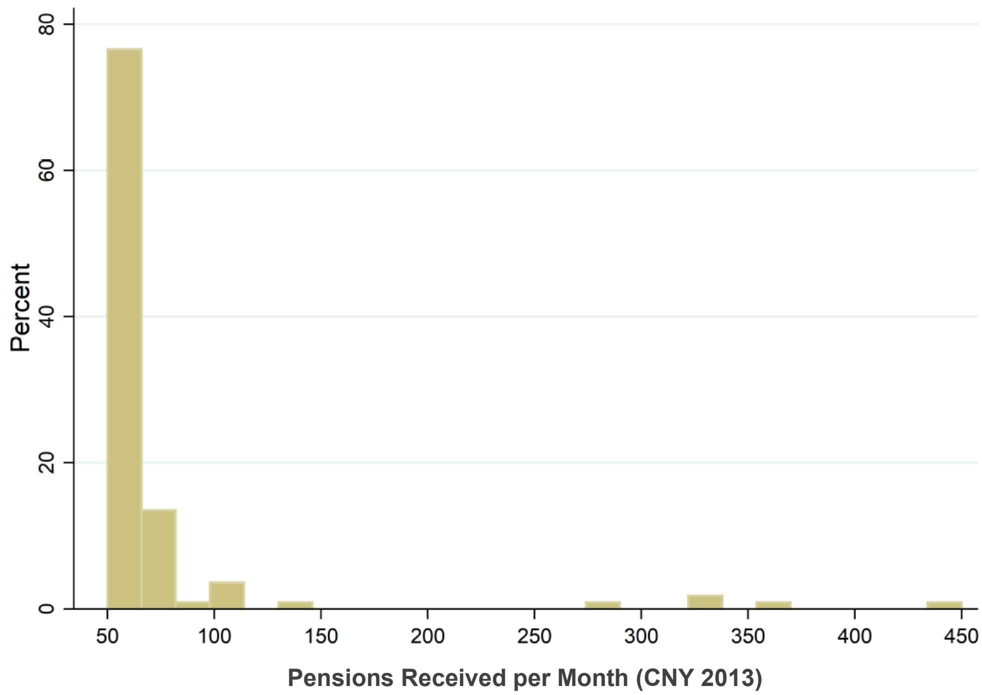
Appendix E. The New Rural Pension Scheme

E.1 Details of the New Rural Pension Scheme

The New Rural Pension Scheme (NRPS) of the People's Republic of China (PRC) was initiated in 2009, and different counties were gradually included in the program in several waves. Each year, a batch of counties in different provinces were included into the program. By 2013, universal coverage was achieved. As already mentioned, the NRPS creates two shocks: (i) a wealth windfall for the current old generation, and (ii) a better saving tool for working-age generations to prepare for their retirement. When the NRPS is implemented, participants older than age 60 immediately become eligible to claim a pension payment on a monthly basis. The payment is composed of two parts, funded by the central government and the local government. The central government provides a subsidy of CNY55 (about $8.50) per month, and the local government also provides a subsidy at different levels. In Figure E1, we show the distribution of county-level pension payment that elderly participants receive. The payment can go as high as CNY450 per month, but in most counties, participants receive the basic level: CNY55 per month or CNY660 per year (about $100). This amount is not negligible for a rural household in the PRC when this program was introduced. In 2010, the annual per capita disposable income of rural households was around $900. In our sample of China Family Panel Studies (CFPS) counties, the median annual rural disposable income per capita was only CNY5,200 (approximately $800).

Participants younger than age 60 must pay a fixed amount of premium every year, which is saved in the pension account. The premium ranges from CNY100 (about $15) up to CNY500. Besides individual premiums, the local government also provides a subsidy for participants. Both individual premiums and local government subsidies are saved in the pension account on a yearly basis, and receive accumulative interests until age 60. After turning 60, participants can withdraw 1/139 of the accumulated savings in their pension account each month, plus the basic pension payment (CNY55) funded by the central government.

Figure E1: Distribution of Monthly Pension Payment



Notes: We use reported amount of pension payment in the CHARLS 2013. We look at counties with at least 10 non-missing reported values (in total 111 counties). Then we derive the mode of all reported amounts at the county level, and plot the distribution of county-level pension payment.

Source: China Health and Retirement Longitudinal Study.

For low-income residents in rural PRC with a normal expected longevity, without access to high-return investment tools, the NRPS is beneficial. First, the premiums they save in the pension account is returned with interest. Second, they receive local government's subsidy, which is also saved in the pension account. Third, the central government will pay a basic pension unconditionally after age 60. In the next section, we present a simple example which calculates the rate of return in the scenario without pensions and the scenario with pensions. It is clear to see that the NRPS does raise the rate of return to savings.

E.2 The Rate of Return to Private Savings

In this section, we present a simplified example to calculate the rate of return to private savings before and after the NRPS. The main purpose is to convince the readership that the NRPS does increase the rate of return to savings, for low-income residents in rural PRC.

Consider a 45-year-old farmer named Bo, living in a village in the PRC. Bo grows some rice and vegetables, and raises some pigs and chicken. In a typical year, Bo has some small amount of cash left. Before the government launched the pension program, he usually goes to the bank in the nearest town and saves his cash there. His plan is to save $X = \text{CNY100}$ every year until age 60. Then from 60 to 75 (his expected longevity), he will withdraw $X(1+r)^{15}$ per year. Suppose the annual interest rate that the bank offers is $r = 2\%$. Then the total amount of returns to savings that Bo receives in his old age will be $W^s = 15X(1+r)^{15} = CNY2019$. And the aggregate annual rate of return from working age to old age is simply: $R^S = (W^S/15X)\frac{1}{15} = 1 + r = 1.02$.

Now, suppose the government introduces the pension program. Bo decides to save X in the pension account, rather than putting it in the bank, because he heard that money in the pension account also returns interests in the same rate as the bank. More specifically, his total savings in the pension account will be returned to him by $X\sum_{m=1}^{15}(1+r)^m/139) \times 12 \times 15 = CNY12{,}184$, which is about 5 times higher than $(W^P/15X)^{\frac{1}{15}} = 1.14$.

Consider another farmer, Wei. He is very similar to Bo, except that he lives in another county. In Wei's county, if he participates in the pension program, the local government also provides a subsidy to him ($U = CNY50$ per year). That subsidy also accumulates in the pension account until age 60, and returns to the participant by $U \sum_{m=1}^{15}(1+r)^m$/139 per month. That means Wei will receive a total income of $W^{PU} = [T + (X+U)\sum_{m=1}^{15}(1+r)^m/139] \times 12 \times 15 = CNY13{,}326$ in his old age, and the aggregate annual rate of return is equal to $R^{PU} = 1.16$.

Suppose Bo and Wei are wealthier and have more cash left each year. They can save $X = 1{,}000$ every year either in the bank or in the pension account. If they use the traditional bank saving, the aggregate rate of return doesn't change, still equal to $R^S = 1 + r = 1.02$. If they participate in the NRPS, they will receive more in their old age, respectively W^{P^t} = 32,742 and W^{PU^t} = 33,884. The aggregate rate of return of rich Bo and Wei is respectively R^{P^t} = 1.05 and R^{PU^t} = 1.06, lower than that of poor Bo and Wei, but still higher than the rate of return of savings in the bank.