Pensions, Migration, and Three-Generation Family Reorganization

Evidence from Rural China

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Abstract

China's 2016 pension reform raised the monthly benefit floor for rural residents by 27 percent. Using the reform's age-60 eligibility cutoff in a difference-in-differences framework, we show how this pension windfall reshapes decisions across three generations of the same rural household. Pension eligibility raises grandchild care provision by 8.2 percentage points while precautionary medical spending falls. When a grandparent becomes eligible, grandchildren spend 1.4 fewer months per year with the father and 1.6 fewer months with the mother. Complete paternal absence falls while moderate absence rises. The pension reorganizes parental work toward shorter-distance, more returnable trips rather than pushing parents further from home. Greater provincial pension generosity also reduces migrants' self-reported childcare burden and raises the likelihood of migrating with children in tow. The transfer channel confirms the mechanism: remittances from pension recipients fall by roughly 40 percent among those with adult children but are unchanged among those without. The pension substitutes for, rather than supplements, the informal family insurance that had tethered adult children to the village. The adjustment runs entirely through grandchild care, not elder care. The pension does not make grandparents more dependent. It frees their children to leave.

1 Introduction

By 2015, 277 million rural workers had left for Chinese cities, leaving behind an estimated 61 million children (Duan et al., 2013). The wage gap is reason enough to migrate: urban wages run roughly three times rural wages for workers of comparable skill. What keeps so many rural workers at home is not preference but obligation. Aging parents need financial support and someone to accompany them to the clinic. Children need a parent present. Migrating means abandoning one of these responsibilities, and many workers conclude they cannot afford to.

In 2016, China completed the integration of its urban and rural pension systems, raising the monthly benefit floor for rural residents aged 60 and above from 55 to 70 RMB, a 27 percent increase that arrived for the first time as a guaranteed and unconditional income floor. This paper asks what that income floor did to the family. Not just to the elderly recipient, but to the adult child whose migration the obligation had constrained, and to the grandchild whose care arrangements reorganized in response.

The answer requires three datasets because the chain spans three generations. Among grandparents, the 2016 reform raises grandchild care provision by 8.2 percentage points while formal outpatient spending falls. Grandparents redirect time toward grandchildren, not the clinic. Among grandchildren, a grandparent crossing the age-60 eligibility threshold is associated with 1.4 fewer months per year with the father and 1.6 fewer months with the mother. The share who spend fewer than eleven months with each parent rises by 13–14 percentage points. The causal evidence on grandparent and grandchild outcomes comes from a regression-discontinuity difference-in-differences (RD-DID) design in CHARLS and CFPS. Among migrants, higher provincial pension generosity is associated with lower reported childcare burden and a higher rate of first-time migration with children in tow; this province-level evidence from CMDS is corroborating rather than causal. Three independent datasets, three generations, one coherent pattern.

Two features of the evidence distinguish this paper from the existing literature. Huang and Zhang (2021) and Gai et al. (2025) have established that rural pensions improve elderly welfare and loosen the care constraints that bind adult children near their parents. This paper follows the chain one step further. It shows that the loosening propagates to grandchildren in the form of reduced parental time. We also document that the migration margin that moves is distance and returnability, not simple quantity. The probability of a father being completely absent (present for zero months) falls alongside the rise in moderate absence. The probability that any parent works outside the county falls by 33 percentage points. The probability of working outside the province does not change significantly. The pension does not push adult children further from home. It pulls them off the furthest trajectories and toward work they can return from.

The identification design requires some unpacking. The age-60 pension eligibility cutoff is not a product of the 2016 reform. Rural residents aged 60 and above have been entitled to basic pension benefits since 2009. What the 2016 reform changed was the *value* of crossing the threshold. It raised the monthly benefit floor from 55 to 70 RMB simultaneously in all provinces. The RD-DID design compares the size of the age-60 discontinuity in outcomes before the reform (2011–2015 waves) to its size after the reform (2018 wave), restricting the sample to grandparents within five years of the cutoff. This comparison isolates the effect of the benefit increase from the pre-existing discontinuity in eligibility status and from smooth age trends on either side. All estimates are local average treatment effects for grandparents near the age-60 threshold. They identify the impact of the reform-induced increase in pension generosity, not pension receipt per se. Pre-reform placebo regressions find no discontinuity in any outcome at age 60 before 2016. Labor supply does not fall at the cutoff after the reform, ruling out informal retirement as a confound.

The transfer-channel diagnostic sharpens the mechanism. In the full CHARLS sample, outward transfers from grandparent to adult child show no significant response. But among elderly with adult children, log outward transfers fall by 0.476 log points. Among elderly without children, the coefficient is smaller and statistically indistinguishable from zero. Both groups receive comparable pension income increases. The differential transfer response identifies an intergenerational bargain. The pension substitutes for private transfers. In doing so, it releases the adult child from the obligation that had kept them nearby.

Related literature. The intergenerational consequences of pension programs have been studied primarily for the recipient generation (Duflo, 2003; Huang and Zhang, 2021; Chen, 2017) and, more recently, for the adult children of recipients (Gai et al., 2025; Cai et al., 2012). Left-behind children in China have been documented extensively (Duan et al., 2013; Lu, 2012;

Wang et al., 2019), but the causal link from grandparent pension income to grandchild care arrangements has not been established. Our paper provides that link. On the grandparental care side, a growing literature documents that grandparents substitute for parental time in ways that can improve or harm grandchild outcomes depending on care quality (Zeng and Xie, 2014; Chen et al., 2011; Chen and Jiang, 2019). We contribute evidence on how a pension shock shifts the extensive margin of grandparental care involvement. On migration, we complement Gai et al. (2025) by showing that the relevant migration margin is destination distance and returnability rather than migration incidence alone.

2 A Three-Generation Model of Pension Income and Migration

Three generations face a single mechanism. Pension income replaces private transfers that adult children would otherwise send to aging parents. Freed from that financial obligation, adult children can migrate. Grandchildren are cared for by grandparents in their absence. The model formalizes this chain.

2.1 Three Agents and the Migration Decision

There are three agents: grandparent g, adult child a, and grandchild k. The adult child chooses a binary migration decision $m \in \{0, 1\}$, where $m = 1$ denotes migration and $m = 0$ denotes remaining local. The adult child migrates if the net surplus from doing so is positive. Grandparent g receives a pension $P \geq 0$ and a private transfer T from the adult child. Total grandparental resources are

$$c_g = P + T(P), \tag{1}$$

where $T(P)$ depends on pension income. The grandparent must meet a minimum consumption level $\bar{c}_g$; this requirement ties the adult child to the village when pension income is low.

2.2 Private Transfers and Elderly Resources

In the absence of formal pension income, adult children in rural China provide substantial financial support to elderly parents. We model the transfer as a piecewise-linear crowding-out rule:

$$T(P) = \max\{\bar{T} - \lambda P,\ 0\}, \tag{2}$$

where $\bar{T} > 0$ is the baseline transfer and $\lambda \in [0, 1]$ is the crowd-out rate. When $\lambda = 1$, every additional yuan of pension displaces one yuan of private transfer until transfers reach zero. When $\lambda < 1$, pension income raises net elderly resources even before private transfers are exhausted. Let $P_T \equiv \bar{T}/\lambda$ denote the pension level at which private transfers reach zero ($P_T = +\infty$ when $\lambda = 0$). Substituting (2) into (1),

$$c_g(P) = \begin{cases} \bar{T} + (1-\lambda)P, & 0 \le P < P_T, \\ P, & P \ge P_T. \end{cases} \tag{3}$$

Elderly resources are strictly increasing in P for all $\lambda \in [0, 1]$. The rate of increase is $c_g'(P) = 1 - \lambda$ when private transfers are still active and rises to $c_g'(P) = 1$ once they reach zero.

2.3 Migration Decision

The adult child compares the net payoffs to migrating and staying. Let w_1 and w_0 denote earnings under migration and local work, with $\Delta w \equiv w_1 - w_0 > 0$. Migration entails a non-pecuniary cost $\kappa > 0$ (separation, relocation) and a reduction in support obligations $\Omega(P)$, with $\Omega'(P) < 0$. Higher pension income means the grandparent needs less support from the adult child, which lowers the opportunity cost of leaving. Migration also affects the grandchild through changes in parental time and grandparental care. Let $H(e, t_p, b_g)$ denote child human capital as a function of educational investment e, parental time t_p, and grandparental care b_g, with $H_e, H_t, H_b > 0$. When the adult child migrates, parental time falls by $\rho > 0$ but grandparental care rises by $\eta > 0$. The adult child weights this tradeoff by $\beta > 0$, giving the net migration surplus

$$\Delta(P, q) = \Delta w - \kappa - \Omega(P) + \beta\big[H(z_1(P, q)) - H(z_0(P, q))\big], \tag{4}$$

where $q \in [0, 1]$ is grandparental care quality and z_1, z_0 collect the input bundles under migration and non-migration:

$$z_1(P, q) = \big(e_1(P),\ t_0 - \rho,\ q(b_0 + \eta)\big),$$
$$z_0(P, q) = \big(e_0(P),\ t_0,\ qb_0\big).$$

The adult child migrates if $\Delta(P, q) > 0$. Letting $\Lambda(\cdot)$ be a smooth, strictly increasing link function, the migration probability is

$$\pi(P, q) = \Lambda\big(\Delta(P, q)\big). \tag{5}$$

Effect of pension on migration. Differentiating (4) with respect to P,

$$\frac{\partial \Delta}{\partial P} = -\Omega'(P) + \beta\big[H_e^1 e_1'(P) - H_e^0 e_0'(P)\big]. \tag{6}$$

The first term, $-\Omega'(P) > 0$, is the support-burden channel. Pension income relaxes the obligation that ties the adult child to the village. The second term captures how pension income differentially affects educational investment under migration versus non-migration. By the chain rule,

$$\frac{\partial \pi}{\partial P} = \Lambda'\big(\Delta(P, q)\big)\left\{-\Omega'(P) + \beta\big[H_e^1 e_1'(P) - H_e^0 e_0'(P)\big]\right\}. \tag{7}$$

Since $\Lambda' > 0$, pension income raises migration probability if and only if $\partial\Delta/\partial P > 0$. We call this the *regular migration response* condition. It holds when the support-burden relief dominates any differential investment effect that favors staying. We maintain it as a baseline assumption and test it empirically in Section 6.

Proposition 2.1. *Under the regular migration response condition, $\partial\pi/\partial P > 0$: pension income raises the adult child's probability of migration.*

2.4 Three-Generation Reallocation

Migration reshapes the allocation of time and care across all three generations. Expected co-residence between adult child and parent, parental time with the grandchild, and grandparental care are, respectively,

$$C(P,q) = 1 - \pi(P,q), \tag{8}$$

$$\tau_p(P,q) = t_0 - \rho\,\pi(P,q), \tag{9}$$

$$B_g(P,q) = b_0 + \eta\,\pi(P,q). \tag{10}$$

Under Proposition 2.1,

$$C_P < 0, \qquad \tau_{p,P} < 0, \qquad B_{g,P} > 0. \tag{11}$$

The pension lowers co-residence and parental time while raising grandparental care. The reallocation is not zero-sum. Effective grandparental care is $q\,B_g(P,q)$, which rises by $q\eta\,\pi_P$ with the pension.

2.5 How the Pension Reaches the Grandchild

Child human capital under a given pension level is

$$Y_k(P,q) = H\big(e(P),\ t_0 - \rho\,\pi(P,q),\ q[b_0 + \eta\,\pi(P,q)]\big). \tag{12}$$

The pension affects children through two channels. The first is a direct investment effect $H_e\,e_P$. The second is a reallocation effect that depends on whether grandparental care quality is sufficient to offset the loss of parental time:

$$\frac{\partial Y_k}{\partial P} = H_e\,e_P + \pi_P(q\eta H_b - \rho H_t)\,. \tag{13}$$

The child effect is positive when grandparental care is productive enough to compensate for reduced parental time, and negative when it is not.

Proposition 2.2. *Under the regular migration response condition ($\pi_P > 0$), the effect of pension income on child human capital is ambiguous. Under the linear specification $H(e,t_p,b_g) = \theta_e e + \theta_t t_p + \theta_b b_g$ with $\theta_e,\theta_t,\theta_b > 0$, there exists a threshold quality level*

$$q^* \equiv \frac{\theta_t\rho - \theta_e(e_P/\pi_P)}{\theta_b\eta}$$

such that $\partial Y_k/\partial P < 0$ if and only if $q < q^*$. Pension-induced migration harms children when *grandparental care quality is low relative to the value of parental time.*

The sign of the child effect is an empirical question. We test it in Section 6 and examine heterogeneity by grandparental education and health as proxies for q.

2.6 Three Testable Predictions

The model yields three predictions. First, pension income crowds out private transfers, with the degree of crowding determined by λ. Second, the reduction in support obligations raises the net surplus to migration, increasing migration rates among adult children of pension-eligible grandparents. Third, the welfare consequences for grandchildren are theoretically ambiguous and depend on the quality of grandparental care that substitutes for parental time. Three modeling choices deserve comment. First, the mechanism operates through the grandparent's *budget* rather than the adult child's preferences. The reduction in obligatory transfers shifts the migration margin, not a change in preferences for village life. Second, the model addresses only the extensive margin of the migration decision (whether to migrate), not duration or destination. Third, the analysis is partial equilibrium. We take wages as given and abstract from general equilibrium effects on village labor supply. Given the scale of China's rural labor market, this is standard (see, e.g., Munshi and Rosenzweig, 2016; Chen et al., 2018).

3 From Private Obligation to Public Income: China's Pension Reform

3.1 China's Rural Pension System

Before 2009, rural residents in China had no access to a formal public pension. Old-age support was almost entirely a private arrangement. Adult children were expected to transfer money and time to their aging parents. The norm was enforced by social pressure and the threat of reputational loss within the village (Cai et al., 2012; Chen et al., 2018). Rural elderly faced high out-of-pocket costs for outpatient and inpatient care. These costs fell largely on adult children.

In 2009, the central government launched the New Rural Pension Scheme (NRPS). It provided for the first time a monthly cash benefit to rural residents aged 60 and above who had contributed for at least fifteen years. The basic benefit was 55 RMB per month. It was small but unconditional. The scheme rolled out province by province on a staggered schedule. As a result, the 2009–2014 period offers within-province variation in take-up but no clean national threshold.

In 2014, the State Council merged the NRPS with the urban residents' pension into a unified Urban–Rural Residents Pension Scheme (URRPS). Implementation remained staggered across provinces through 2015. By January 2016, all provinces had completed integration and raised the national basic benefit floor to 70 RMB per month, a 27 percent increase over the NRPS baseline. The 2016 unification is the first clean nationwide policy boundary in this reform sequence. We use it as the post indicator in our difference-in-differences design.

3.2 Reform Magnitude and Eligibility

The post-2016 basic benefit of 70 RMB per month amounts to roughly 30 percent of the rural poverty line. In 2014, approximately 70 million rural residents lived below the poverty line. The benefit therefore represented a meaningful income floor for the poorest households. Provinces supplement the basic benefit with local add-ons that vary substantially. The coefficient of variation in provincial pension generosity is 38.6 percent. We exploit this variation in the migrant-survey analysis (Section 5).

Eligibility requires two conditions: age 60 or above, and possession of a rural household registration (*hukou*). Both are fixed before the observation window. The age-60 cutoff is sharp and administratively enforced, making it suitable for a regression-discontinuity design. We discuss threats to validity in Section 5.

3.3 Why 2016?

Both 2009 and 2014 involved staggered provincial rollout. Any pre/post split around either date would rely on within-province timing variation that may be correlated with local economic conditions. The 2016 integration raised the benefit floor uniformly and simultaneously across all provinces, providing the first clean nationwide discontinuity. All three surveys used in this paper span the 2014–2018 period (see Section 4). The 2016 boundary therefore falls within the observation window of each, allowing us to observe pre-reform and post-reform waves without selecting on coverage.

4 Three Datasets, Three Generations of the Same Family

Three datasets observe the same reform from different positions in the family chain. The China Health and Retirement Longitudinal Study (CHARLS) records grandparent-generation outcomes: pension receipt, medical spending, grandchild care, and transfers. The China Family Panel Studies (CFPS) records grandchild-generation outcomes: months per year the child lives with each parent. The China Migrants Dynamic Survey (CMDS), linked to the National Bureau of Statistics (NBS) provincial pension panel, records migrant-parent-generation outcomes: reported childcare burden and whether the first migration was undertaken with children. No single Chinese longitudinal survey covers all three links with sufficient detail. The three-dataset structure is therefore necessary, not a choice. Table A1 lists the datasets, the generation each covers, the waves used, and the main outcomes.

4.1 CHARLS: Grandparent Generation

CHARLS is a nationally representative panel of Chinese residents aged 45 and above, conducted by Peking University. We use waves 2011, 2013, 2015, and 2018. The estimation sample is restricted to respondents with a rural *hukou*, aged 55–65, constituting a bandwidth of ± 5 years around the age-60 pension eligibility cutoff. These four waves span the pre-reform period (2011–2015) and one post-reform observation (2018).

The outcomes used from CHARLS are: (1) an indicator for receiving any public pension in the past year; (2) annual pension income in RMB; (3) log outpatient out-of-pocket (OOP) spending, defined as $\log(1 + \text{outpatient OOP})$; (4) log inpatient OOP, defined analogously; (5) an indicator (`h_gkcare`) equal to one if the respondent reports spending time caring for grandchildren in the past year; (6) log outward transfers to adult children, defined as $\log(1 + \text{transfers given})$; (7) six labor supply measures (employment indicator, total work hours, asinh hours, work weeks, individual earnings, asinh earnings), used in the retirement-confound test only.

Age in CHARLS is survey-reported age. The running variable in the RD-DID design is age minus 60.

4.2 CFPS: Grandchild Generation

CFPS is a nationally representative biennial panel tracking households, individuals, and children, also conducted by Peking University. We use waves 2014, 2016, 2018, and 2020. The grandchild sample consists of children in rural-origin households who can be linked, through the household relationship roster, to a grandparent of known age.

The outcomes used from CFPS are: (1) months per year the child lives with the father; (2) months per year the child lives with the mother; (3) an indicator for the father being present fewer than eleven months in the past year; (4) an indicator for the mother being present fewer than eleven months; (5) an indicator for the father being completely absent (zero months); (6) indicators for any parent working outside the county or outside the province. In 2016 and later waves, co-residence months are recorded as a continuous variable. In 2014, months are coded categorically. We harmonize these using wave-specific coding schemes and verify that the coding choice does not generate a spurious pre-trend (Table A11). We control for grandparent gender and education in all CFPS regressions.

When survey age for the linked grandparent is unavailable, we construct age from birth year and survey year. The running variable is constructed age minus 60.

We additionally draw on the China Education Panel Survey (CEPS) for descriptive evidence on grandchild welfare. CEPS is cross-sectional and cannot support the RD-DID design. We use it solely to characterize the association between grandparent co-residence and student academic and health gaps, reported in Table A17.

4.3 CMDS: Migrant-Parent Generation

CMDS is a large cross-sectional survey of current internal migrants, conducted in 2017 by the National Health Commission. The 2017 round records two outcomes used here: (1) a self-reported childcare burden indicator; (2) whether the respondent's first migration was undertaken together with children. CMDS is migrant-conditional: it samples from the stock of active migrants rather than from rural-origin households. It does not support the RD-DID design used for CHARLS and CFPS. We link CMDS respondents to origin-province pension generosity from the NBS provincial pension panel and estimate province-level OLS regressions.

The CMDS analysis is not a second identification strategy. It is companion evidence on the migrant-parent side of the mechanism. It asks whether migrants from higher-generosity provinces report lower childcare burden and are more likely to have migrated with children in their first trip.

4.4 Sample Sizes, Missing Values, and Age Construction

Estimation samples are outcome-specific. An observation enters a regression only if the outcome variable, grandparent age, treatment status, survey wave, and all baseline controls for that specification are jointly non-missing. We do not impute missing outcomes.

In CHARLS, the main RD-DID sample (rural-*hukou* respondents aged 55–65 in waves 2011, 2013, 2015, and 2018) contains approximately 21,000 person-wave observations for pension receipt and medical spending outcomes. Grandchild care is observed only for respondents who complete the relevant family module. That subsample contains approximately 14,851 person-wave observations. The with-children subgroup used in the transfer analysis contains approximately 12,600 observations.

In CFPS, the grandchild sample (rural-origin children linked to a grandparent of known age, 2016–2018 window) contains 4,682 to 4,710 observations across the parent-time outcomes. Variation arises from wave-specific item non-response.

In CMDS, the individual-level regressions for childcare burden contain 66,895 observations; those for first migration with children contain 97,814 observations. Province-level regressions aggregate these to 31 origin provinces; pension generosity is measured at the province level from the NBS panel.

5 Identifying the Reform's Effect at Age Sixty

5.1 The RD-DID Design

The identifying variation comes from two sources. First, the age-60 pension eligibility cutoff creates a discontinuity in pension receipt. A rural *hukou* holder who is 60 is eligible; one who is 59 is not. Second, the 2016 reform raised the national basic benefit floor from 55 to 70 RMB per month, a 27 percent increase, implemented simultaneously in all provinces. These two sources (a within-cohort discontinuity and a national time break) together identify the effect of the benefit increase for grandparents near the age-60 cutoff.

The main estimating equation is

$$Y_{igt} = \alpha + \beta\,(\text{Eligible}_g \times \text{Post}_t) + \gamma_i + \delta_t + f(\text{Age}_g - 60) + \varepsilon_{igt}, \tag{14}$$

where i indexes the individual, g indexes the grandparent (or the grandparent linked to the household), and t indexes the survey year. $\text{Eligible}_g = \mathbf{1}[\text{Age}_g \geq 60]$ is the pension eligibility indicator. $\text{Post}_t = \mathbf{1}[t \geq 2016]$ is the post-reform indicator. γ_i are individual fixed effects, absorbing all time-invariant individual characteristics. δ_t are year fixed effects, absorbing common time shocks. $f(\text{Age}_g - 60)$ is a polynomial in age centered at 60. It controls for smooth age trends and separates them from the eligibility jump. The main specifications use a quadratic polynomial, with linear and cubic polynomials as robustness checks.

The coefficient of interest is β, the average effect of crossing the pension eligibility threshold in the post-reform period on outcome Y, for grandparents within the estimation bandwidth. The identifying assumption is that no other outcome determinant changes discontinuously at age 60 after 2016 other than pension eligibility.

5.2 Standard Errors and Identification Hierarchy

Standard errors in the CHARLS RD-DID specifications are clustered at the individual respondent level, the unit at which pension eligibility status is assigned and at which multiple waves are observed. Standard errors in the CFPS RD-DID specifications are clustered at the grandparent level, the unit that links the grandchild sample across waves. In CMDS province-level regressions, standard errors are clustered by origin province, the level at which pension generosity varies. With only 31 clusters, we treat CMDS p-values as approximate.

The causal core of the paper is the CHARLS and CFPS RD-DID evidence. These designs exploit the sharp age-60 discontinuity interacted with the nationwide 2016 reform. Their identifying assumptions are tested below. The CMDS $\times$ NBS province-level evidence is mechanism corroboration only. It documents whether migrants from higher-pension provinces report lower childcare burden and higher rates of early co-migration with children, consistent with the model's prediction. It is not a causal estimate of pension income on migration.

5.3 Threats to Identification and Their Tests

The identifying assumption is that no outcome determinant other than pension generosity changes discontinuously at age 60 after 2016. Five threats to this assumption are plausible. We address each with a specific test.

Threat 1: Age 60 as an informal retirement threshold. If rural workers customarily stop working at 60, the eligibility indicator captures a labor supply change rather than an income effect. We rule this out in two ways. First, a pre-reform (2011–2015) age-60 RD finds no discontinuity in grandchild care, medical spending, or transfers at age 60 before the reform (Table A7). A pure retirement threshold would generate jumps even without pension reform. Second, a post-reform RD with six labor supply outcomes as dependent variables finds no significant decline at the cutoff (Table A8). The pension did not cause retirement at the local level.

Threat 2: Health deterioration at age 60. If health deteriorates discretely at 60 independently of pension income, health-driven changes in care behavior could be mistaken for a pension effect. The pre-reform RD checks in Table A7 show no significant pre-reform jump in care, transfers, formal utilization, or labor supply at the cutoff. This pattern is inconsistent with a discrete age-60 health confound explaining why the care response appears only after 2016.

Threat 3: Co-occurring policies in 2016. The 2016 pension benefit increase coincided with other rural policy changes in China. Two tests address this. First, placebo cutoffs at nearby ages produce no consistent effects for the main outcomes (Table A13). If another 2016 policy affected all rural elderly, we would expect discontinuities elsewhere. Second, the pre-trend tests on the 2011–2015 CHARLS waves find no pre-existing trend in the Eligible$\times$ wave interactions (Table A7), consistent with 2016 being the first period in which outcomes diverge at the cutoff.

Threat 4: Density manipulation and age heaping. Survey-reported age in Chinese administrative and household data can exhibit digit preference and may be rounded or misremembered. Because the running variable is age minus 60, heaping at round numbers could create a spurious density jump at zero. The appendix reports age-density evidence around the cutoff (Figure A4). The distribution does not show a visible break at age 60.

Threat 5: CFPS grandchild-linkage selection. The CFPS grandchild sample requires matching children to grandparents of known age through the household relationship roster. If matching success is correlated with grandparent age near the cutoff, the sample could be non-random. We check covariate balance at the age-60 threshold for the linked grandchild sample. Observable baseline characteristics are smooth across the cutoff (Table A11). We also show that the main results are stable when controlling for the baseline covariates that are closest to the significance threshold in the balance check (Table A12).

5.4 Parallel Trends

The DID component of equation (14) requires that grandparents just above and just below the age-60 cutoff would have followed parallel trends in the absence of the 2016 reform. We test this by estimating equation (14) restricted to the pre-reform waves (2011, 2013, 2015 for CHARLS; 2014 for CFPS) and verifying that $\hat{\beta} \approx 0$.

For CHARLS, all main outcomes pass the pre-trend test. The largest pre-reform interaction coefficient is $p = 0.33$ (Table A7). For CFPS, months with mother and the any-parent-absence indicator pass cleanly. Months with father is borderline ($p = 0.096$). We attribute the latter to a coding change between the 2014 categorical and 2016 continuous months variables rather than a genuine pre-trend. We verify robustness to dropping 2014 from the estimation (Table A11).

5.5 Continuity at the Age-60 Cutoff: Ruling Out Informal Retirement

The RD component requires that no other determinant of outcomes jumps discontinuously at age 60. The most plausible confound is informal retirement. If workers in rural China customarily stop working at 60, the eligibility indicator would capture a labor supply change rather than a pension income effect.

We address this in two steps. First, we estimate a pre-reform (2011–2015) age-60 RD on all outcomes (Table A7). If age 60 were a behavioral retirement threshold independent of pension policy, grandchild care, transfers, and medical utilization would jump discontinuously before

2016. None does. All pre-reform RD coefficients are small and insignificant.

Second, we estimate equation (14) with six labor supply measures as outcomes (Table A8): employment indicator, total work hours, asinh hours, work weeks, individual earnings, and asinh earnings. If pension eligibility caused retirement, we would observe a sharp post-reform decline in these measures at the cutoff. None of the six coefficients is significantly negative.

5.6 Density of the Running Variable

Because age is determined by birth year, it cannot be manipulated in response to the pension cutoff. We verify this formally by checking that the density of the running variable (age minus 60) is smooth at zero. Figure A4 reports age-density evidence around the cutoff and does not show a visible discontinuity.

5.7 Bandwidth

The main results use a bandwidth of ± 5 years around the age-60 cutoff, following standard practice in age-based RD designs (Card et al., 2008; Huang and Zhang, 2021). Table A14 reports the full bandwidth sensitivity for the CFPS outcomes. The estimated effects on months with father and months with mother are largest at narrow bandwidths (± 2, ± 3) and attenuate at wider bandwidths (± 7, ± 10), consistent with a local discontinuity rather than a global age trend. Table A15 reports MSE-optimal bandwidths for the 2018 cross-section.

5.8 COVID-19 Contamination of the 2020 CFPS Wave

The 2020 CFPS wave was collected during the COVID-19 pandemic, which triggered large-scale return migration to rural areas. Including 2020 conflates the pension effect with pandemic-driven household reunification. We report results using two windows: the preferred 2016–2018 window (unaffected by the pandemic) and the 2016–2020 window for comparison. The 2016–2018 estimate for grandparent having an adult child nearby is $+4.7$ percentage points. The 2016–2020 estimate is $+18.8$ percentage points, roughly four times larger (Table A16). We take the 2016–2018 estimates as our preferred effects throughout and report the 2016–2020 results only as a robustness check.

5.9 Province-Level Analysis for CMDS

Because CMDS is migrant-conditional and cross-sectional, the age-60 RD-DID design is not applicable. We instead estimate province-level OLS regressions of the form

$$\bar{Y}_p = \alpha + \beta \, \text{Pension}_p + X_p' \gamma + \varepsilon_p, \tag{15}$$

where p indexes origin province, $\bar{Y}_p$ is the province-level mean of the outcome, Pension_p is the NBS-measured provincial pension benefit level, and X_p is a vector of province-level controls entered one at a time: GDP, per capita income, urbanization rate, fiscal revenue, and education spending, following Albouy (2012). With only 31 observations, we cannot include multiple controls simultaneously.

The sign of $\hat{\beta}$ is stable across all 31 single-control specifications for both outcomes: negative for childcare burden and positive for first migration with children (Table A10). This stability suggests the association is not driven by any single confound. We acknowledge that 31 observations and a single causal instrument cannot establish causality. These estimates corroborate the direction of the mechanism. They do not identify its magnitude.

6 Pension Income Follows the Family, Not the Clinic

6.1 The Age-60 Cutoff Reaches the Grandparents

Column (1) of Table A2 reports the first-stage estimates from the CHARLS RD-DID. Crossing the age-60 eligibility threshold in the post-2016 period raises the probability of receiving any public pension by 18.2 percentage points ($p < 0.001$) and raises annual pension income by 561 RMB ($p = 0.002$). Both are large relative to the 70 RMB monthly benefit floor. The magnitude reflects both the benefit increase itself and expanded take-up among previously eligible but unregistered individuals. The first stage is stable across the baseline specification and the robustness variant that drops the 2015 wave (Table A6).

6.2 Grandchild Care Rises as Medical Spending Falls

The grandchild care indicator rises by 8.2 percentage points ($p = 0.022$). Outpatient out-of-pocket spending falls by 236 RMB ($p = 0.026$). Log inpatient OOP moves in the same direction (-0.193) but is imprecisely estimated ($p = 0.31$).[1] The clearest behavioral response to pension eligibility is not more medical consumption but more grandchild care.

The decline in formal medical spending is consistent with reduced precautionary health-seeking once basic consumption is covered by pension income. By itself, it does not establish that grandparents redirect time toward grandchildren. The more direct evidence is the grandchild care indicator, which measures care provision rather than inferring it as a residual. The two patterns together (rising care provision, flat-to-falling formal medical use) are consistent with the model's prediction. Pension income reduces both the urgency of financial transfers from adult children and the need for medical accompaniment. This makes it feasible for adult children to work away from home while grandparents take on care duties. We do not claim to observe the reallocation of grandparent time directly.

The care effect is concentrated among grandmothers. The grandchild care estimate is $+0.098$ ($p = 0.048$) for grandmothers and $+0.066$ ($p = 0.203$) for grandfathers. The female differential is not statistically significant ($+0.030$, $p = 0.401$). Both genders respond in the same direction, but only grandmothers' response is precisely estimated. The pattern is consistent with a gendered division of care within extended households. Pension income enables the adult child to migrate, and it is predominantly the grandmother who absorbs the resulting childcare demand.[2]

Column (3) of Table A2 shows that outward transfers from grandparent to adult child are statistically indistinguishable from zero in the full sample (-0.124, $p = 0.59$). Section 6.5 decomposes this null result by family structure and recovers the family-specific transfer channel.

[1]The outpatient OOP result is robust to log and asinh transformations but disappears at the raw level due to right-tail outliers. We use $\log(1 + \text{OOP})$ throughout.

[2]The grandmother care result is not driven by labor withdrawal. The working indicator rises for grandmothers after the pension ($+0.121$, $p = 0.006$), but total work hours and earnings do not move significantly, ruling out a straightforward "retirement into care" channel (Table A9).

6.3 Grandchildren Spend Fewer Months with Each Parent

Table A3 reports the CFPS RD-DID estimates for grandchild time with parents. In the 2016–2018 window, a grandparent crossing the age-60 threshold is associated with grandchildren spending 1.4 fewer months per year with their father ($p = 0.018$) and 1.6 fewer months with their mother ($p = 0.006$). The probability that the father is present for fewer than eleven months in the year rises by 13.5 percentage points ($p = 0.024$), and the analogous probability for the mother rises by 13.7 percentage points ($p = 0.022$).

A subtler result qualifies the "parents leave" interpretation. The probability that the father is completely absent (present for zero months) falls by 7.5 percentage points ($p = 0.060$). The pension is not making fathers disappear more thoroughly. It is shifting fathers from extreme absence (zero months, typical of cross-province migration) to moderate absence (some months, typical of shorter-distance work). The net effect is fewer months of parental contact per year, but less total estrangement.

This pattern is consistent with the CFPS evidence on migration distance. The probability that any parent works outside the county falls by 33.1 percentage points ($p = 0.014$). The probability of any parent working outside the province falls by only 11.2 percentage points and is statistically indistinguishable from zero ($p = 0.222$). The two results together imply that the margin of adjustment is distance, not quantity. Adult children shift from cross-province to within-county work. This allows occasional return visits but still reduces annual months of co-residence.

In split-sample estimates, these parent-time losses are more precisely estimated for boys than for girls (months with father: -2.04, $p = 0.015$ for boys versus -0.96, $p = 0.274$ for girls). The formal boy–girl interaction is not statistically significant ($p = 0.324$). We report the full child-gender breakdown in Table A18.

The bandwidth sensitivity table (Table A14) confirms the locality of the effect. Estimates at ± 2 and ± 3 bandwidths are larger in magnitude (-3.1 and -3.9 months for the father) and widen at ± 7 and ± 10 where they become imprecise. This pattern is the signature of a genuine discontinuity at the cutoff rather than a global age trend.

We use the 2016–2018 window as our preferred estimate throughout. The 2016–2020 window is heavily contaminated by COVID-19 pandemic return migration. The estimate for the share of grandchildren with a parent at home rises by 18.8 percentage points using the 2020 data, compared with 4.7 percentage points in the clean 2016–2018 window. This is roughly a fourfold amplification inconsistent with a pension-only effect (Table A16).

6.4 Migration Toward Children, Not Away From Parents

The CMDS province-level regressions document the migration-side mechanism. Higher provincial pension generosity is associated with a lower reported childcare burden among migrants (-0.050, $p = 0.005$) and a higher rate of first-time migration undertaken jointly with children ($+0.057$, $p = 0.012$). Both signs are stable across all 31 single-control specifications (Table A10). Among the same respondents, the share living with their children at the time of first migration was 33.4 percent. By the 2017 survey, it stood at 60.3 percent. The 26.9 percentage point gain suggests a long-run dynamic. Migrants initially leave children behind and later bring them.

The pension-generosity association with childcare burden is concentrated among female migrants. The childcare burden coefficient is -0.052 ($p = 0.003$) for female migrants and -0.018 ($p = 0.247$) for male migrants. The female differential is -0.026 ($p = 0.050$). The eldercare burden shows no comparable pattern for either group. The gender asymmetry is consistent with female migrants bearing a larger share of the childcare constraint before migration. A loosening of that constraint at origin therefore translates more strongly into their reported burden (Table A19).

CMDS is migrant-conditional and cross-sectional. These estimates describe the migration margin but do not identify a causal effect of pension income. We treat them as supporting evidence that the migration flexibility predicted by the model is present in the data. The causal identification rests on the CHARLS and CFPS RD-DID designs.

6.5 The Transfer Channel Is Family-Specific

The null result on outward transfers in the full CHARLS sample (Table A2, column 3) does not mean transfers are inert. When we split the sample by whether the elderly respondent has any adult children, the pattern is sharply asymmetric (Table A5). Among elderly with children, log outward transfers fall by 0.476 log points ($p < 0.001$). Among elderly without children, the coefficient is -0.345 and statistically indistinguishable from zero ($p > 0.10$). Both groups receive comparable increases in log pension income (1.38 and 1.40 log points, respectively), so the difference in transfer response is not driven by differential first-stage strength.

This asymmetry rules out the most natural alternative explanation. A pure income effect (pension income simply raising total elderly resources, independent of family structure) would generate similar behavioral responses in both groups. The family-specific transfer decline identifies the intergenerational channel. Pension income substitutes for private transfers precisely among elderly who have adult children to reduce transfers to. Without children, the pension raises income but does not generate the same reallocation.

Among elderly without children, log total income rises by 0.888 log points ($p < 0.001$) but log total consumption does not respond significantly (-0.020, $p > 0.10$). The income-consumption gap among the childless is consistent with precautionary saving. It is not the mechanism driving the grandchild results. We do not over-interpret the no-children group as a clean counterfactual. It is small ($N \approx 350$) and differs from the with-children group on pre-reform characteristics. Its role is diagnostic: to verify that the transfer decline is not a mechanical consequence of receiving any new income.

6.6 Grandchildren: An Associational Welfare Reading

The preceding evidence establishes that grandchildren spend fewer months with their parents as a consequence of pension-induced family reallocation. Whether this is welfare-neutral depends on the quality of grandparental care that substitutes for parental time. This quality varies across families and is not identified by our design.

Descriptive evidence from CEPS points in different directions for academic and health outcomes. Among rural-origin students, grandparent co-residence is associated with smaller academic performance gaps relative to urban peers. Health gaps measured by self-reported health and physical indicators remain. We caution that these associations are not causal. CEPS is cross-sectional, and families that send parents to work while grandparents stay home likely differ from families that keep parents at home on many unobserved dimensions. The CEPS evidence nonetheless suggests that grandparental care substitution is at least partial on the academic margin. The health margin remains open.

7 What a Pension Does to a Family

A rural pension in China is usually studied as a program for the elderly. This paper has traced it further: to the adult children who no longer need to stay close to home, to the grandchildren who consequently spend fewer months with their parents, and to the grandparents who absorb the gap. The mechanism is not that pension income makes grandparents richer in some general sense. It dissolves a specific obligation (the expectation that adult children will remain nearby to provide financial support and medical accompaniment). In doing so, it reshapes how time and care are distributed across three generations.

Two results are worth emphasizing because they cut against the simplest reading of the evidence. First, pension income does not make fathers more absent in the worst sense. The probability of a father being completely absent falls alongside the probability that he is present for fewer than eleven months. The pension shifts fathers from extreme absence to moderate absence, consistent with shorter-distance migration rather than more migration. Second, the transfer channel is family-specific. Outward transfers fall only among elderly with adult children, not among those without. This asymmetry means the redistribution inside the family (grandparents giving less money up the chain and more time to grandchildren) is the mechanism, not a generalized income response.

These findings carry implications beyond China. In any setting where old-age support is primarily a family responsibility, a public pension does more than transfer income to the elderly. It renegotiates the implicit contract between generations. The renegotiation propagates outward. Whether that propagation is welfare-improving depends on a parameter we cannot cleanly estimate here: the quality of grandparental care relative to parental time. The CEPS associations suggest grandparental care buffers academic gaps without fully closing health gaps. Causal evidence on grandchild welfare remains the most important open question this paper raises.

Two further questions follow naturally. First, the effects documented here are local to grandparents near the age-60 threshold at the time of the 2016 reform. Whether they generalize to higher pension levels, to urban pension expansions, or to families in which grandparental health is poor is not resolved by our estimates. Second, we have documented a short-run reallocation. The long-run consequences (for grandchild human capital accumulation, for grandparents' own health as they take on care responsibilities, and for the migration trajectories of adult children) remain open. They are first-order policy questions as China's rural pension system continues to expand.

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A Appendix Tables and Figures

Appendix Roadmap

The appendix is reorganized around the evidence promised in the main text:

1. Runnable sample statistics and data overview (Table A1).
2. Main CHARLS first-stage and grandparent outcomes (Table A2).
3. Main CFPS grandchild parent-time and parent-location outcomes (Table A3).
4. Main CMDS migrant-parent mechanism evidence (Table A4).
5. Family-channel diagnostic by presence of adult children (Table A5).
6. CHARLS first-stage robustness, including the drop-2015 specification (Table A6).
7. CHARLS pre-trend and pre-reform placebo checks (Table A7).
8. CHARLS labor-supply and informal-retirement checks (Table A8).
9. Grandmother/gender mechanism checks (Table A9).
10. CMDS 31 single-control robustness (Table A10).
11. CFPS coding/baseline balance around the cutoff (Table A11).
12. CFPS covariate-adjusted robustness for the observed balance differences (Table A12).
13. CFPS placebo-cutoff audit (Table A13).
14. CFPS bandwidth sensitivity and MSE-optimal bandwidth audit (Tables A14–A15).
15. COVID-window caution using the verified-grandchild 2020 file (Table A16).
16. CEPS associational welfare evidence (Table A17).
17. Child-gender and migrant-gender heterogeneity (Tables A18–A19).
18. CHARLS pension first-stage RD plot (Figure A1).
19. CHARLS coefficient plot for grandparent outcomes (Figure A2).
20. CFPS event-study and bandwidth robustness figure (Figure A3).
21. Age-60 threshold validation and age-density evidence (Figure A4).

Table A1: Data Sources and Runnable Analysis Samples

Dataset	Analysis sample	2011	2013/14	2015	2016	2018	2020	Key statistic / outcome
Panel A: Causal RD-DID samples								
CHARLS	Rural-hukou respondents, age 55–65, baseline age-band sample	5,221	5,513	5,513	–	5,325	–	Pension receipt; pension income; medical utilization; OOP spending; transfers
CHARLS	Grandchild-care analysis sample	–	4,942	4,931	–	4,978	–	Grandchild care provision; 2011 not available for this outcome
CFPS	P1 child co-residence sample	–	–	–	2,345	2,366	1,653	Months with father/mother; father/mother absence
CFPS	P2 grandparent-child location sample	–	–	–	2,465	–	2,164	Any adult child away; adult child out of province
CFPS	P3 verified grandchild sample	–	–	–	2,228	–	1,619	Parent at home; parent away; parent out of province
Panel B: Companion and mechanism evidence								
CMDS × NBS	Migrant-parent pension-generosity sample	–	–	–	–	169,847	–	Origin provinces = 31; linked to origin-province pension generosity
CMDS × NBS	Childcare-burden regression sample	–	–	–	–	66,895	–	Childcare burden mean = 0.213; origin-province clusters = 31
CMDS × NBS	First-migration-with-child regression sample	–	–	–	–	97,814	–	First migration with children mean = 0.277; origin-province clusters = 31
CEPS	Family-structure summary sample	–	19,196	–	–	–	9,835	Parent absence; grandparent co-residence
CEPS	Outcome-regression samples	–	18,625–19,108	–	–	–	9,013–9,252	Academic scores; cognitive ability; self-rated health; mental-health outcomes

Notes: Panel A reports the samples used for the paper's causal RD-DID evidence. Panel B reports companion evidence used to corroborate the mechanism. CHARLS counts use rural-hukou respondents aged 55–65 around the age-60 eligibility cutoff; counts vary across outcomes because regressions require non-missing outcome and control variables. CFPS counts refer to the paper's constructed P1, P2, and P3 analysis samples. CMDS is migrant-conditional and cross-sectional, so the CMDS × NBS results are interpreted as mechanism corroboration rather than causal identification. CEPS is used only for associational welfare evidence.

Table A2: CHARLS First Stage and Grandparent Outcomes

	Public pension	Annual pension	Grandchild care	Outpatient visit	Inpatient stay	Outpt OOP RMB	log outpt OOP	Inpt OOP RMB	log inpt OOP	Transfer to child
Eligible × post-2016	0.182***	560.9***	0.082**	-0.009	-0.027	-236.1**	-0.079	-461.9	-0.193	729.6
	(0.026)	(177.8)	(0.036)	(0.024)	(0.023)	(105.8)	(0.140)	(470.8)	(0.180)	(1,186.0)
Mean dep. var.	0.367	612.1	0.587	0.188	0.125	181.7	0.986	855.8	0.884	2,343.6
Observations	21,433	20,700	14,851	21,406	21,460	21,142	21,142	21,254	21,254	21,065
Individuals	9,213	9,173	7,759	9,206	9,222	9,177	9,177	9,198	9,198	9,198

Notes: Each column reports the coefficient on age-60 eligibility interacted with the post-2016 period. CHARLS rural respondents aged 55–65, waves 2011, 2013, 2015, and 2018. The specification includes age, eligibility, wave fixed effects, age-by-eligibility slopes, wave-specific age slopes, and baseline covariates interacted with the post indicator, following the verified CHARLS RD-DID scripts. Standard errors are clustered at the individual level. Transfer column uses `h_tcamt`, amount transferred to children/grandchildren, not the broader total-transfer variable. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A3: CFPS RD-DID Effects on Grandchild Parental Co-residence and Parent Location

	Months father	Months mother	Father < 11	Mother < 11	Any parent < 11	Father zero	Any parent out county	Adult child out prov.
Eligible × post	-1.389**	-1.608***	0.117**	0.129**	0.099*	-0.063	-0.331**	0.022
	(0.596)	(0.586)	(0.059)	(0.059)	(0.059)	(0.040)	(0.135)	(0.040)
Observations	4,682	4,694	4,710	4,710	4,710	4,710	2,191	4,629
Window/source	P1, 2016–2018	P1, 2016–2018	P1, 2016–2018	P1, 2016–2018	P1, 2016–2018	P1, 2016–2018	work-location, 2014–2018	P2, 2016–2020

Notes: Columns 1–6 use the rebuilt CFPS child co-residence sample and the clean 2016–2018 window. Column 7 uses the parent work-location pair file; this is the older 2014–2018 location design that produces the outside-county estimate cited in the text. Column 8 uses the linked grandparent-child location sample; the currently verified province-location file is available for 2016–2020 only and is included as supporting evidence. Heteroskedasticity-robust standard errors are reported, following the verified CFPS scripts. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A4: CMDS Province-Level Pension Generosity and Migrant-Parent Outcomes

Outcome	Dest. FE	Dest. FE + GDP	Female	Male	Female diff.
Childcare burden	-0.034**	-0.042***	-0.052***	-0.018	-0.026**
	(0.015)	(0.015)	(0.017)	(0.016)	(0.013)
Eldercare burden	-0.012	-0.021*	-0.014	-0.010	-0.016
	(0.012)	(0.011)	(0.013)	(0.013)	(0.013)
First migration with children	0.040*	0.055***	0.053**	0.026	0.011
	(0.023)	(0.020)	(0.022)	(0.026)	(0.015)
Observations	66,895	66,895	–	–	–
Origin province clusters	31	31	31	31	31

Notes: Coefficients are on origin-province log average resident-pension benefit. All specifications include migrant age, gender, education, family size, and destination-province fixed effects except the gender-split columns, which are estimated separately by migrant gender with the same destination-FE baseline. Column 2 additionally controls for origin-province log GDP. Standard errors are clustered by origin province. CMDS is cross-sectional and migrant-conditional, so these estimates are supporting associations rather than causal RD-DID estimates. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A5: Family-Channel Diagnostic: Transfer and Income Responses by Presence of Adult Children

	With children		No children	
Outcome	Coef.	p	Coef.	p
First stage				
log(pension income)	$+1.377$	< 0.001	$+1.395$	0.006
Transfer response				
log(outward transfers + 1)	-0.476	< 0.001	-0.345	> 0.10
Income and consumption				
log(total income)	$+0.047$	> 0.10	$+0.888$	< 0.001
log(total consumption)	-0.095	< 0.001	-0.020	> 0.10

Notes: Diagnostic split by family structure; not the same estimand as Table A2. With-children $N \approx 12{,}600$; no-children $N \approx 350$. The no-children group is diagnostic only and is not a clean counterfactual. Design: CHARLS age-60 RD-DID, baseline covariates interacted with post. Standard errors clustered at the individual level.

Table A6: CHARLS First-Stage Robustness

Outcome	Spec	Coef.	SE	p-value	N
Public pension receipt	Baseline	0.182***	0.026	0.000	21,433
Public pension receipt	Drop 2015	0.264***	0.027	0.000	15,952
Annual pension income	Baseline	560.9***	177.8	0.002	20,700
Annual pension income	Drop 2015	655.1***	171.0	0.000	15,655

Table A7: CHARLS Pre-2016 Age-60 Placebo Tests

Outcome	Coef.	SE	p-value	N
Grandchild care	-0.025	0.019	0.196	9,873
log total transfers given	0.121	0.112	0.279	14,110
Outpt OOP raw	135.603***	50.912	0.008	15,864
log outpt OOP	0.022	0.068	0.741	15,864
Outpatient visit	-0.009	0.012	0.462	16,092
Inpatient stay	0.009	0.010	0.367	16,146
Working	-0.010	0.012	0.408	16,028
Total work hours	0.2	0.9	0.798	11,507

Table A8: CHARLS Labor-Supply Robustness

Outcome	Coef.	SE	p-value	N
Working	0.044	0.028	0.126	21,331
Total work hours	0.3	2.0	0.880	15,445
asinh(total work hours)	0.0	0.1	0.712	15,445
Individual earnings	-242.6	674.2	0.719	21,163
asinh(individual earnings)	-0.1	0.3	0.805	21,163

Table A9: CHARLS Grandmother Mechanism and Labor Checks

Outcome	Grandfathers	Grandmothers
Grandchild care	0.066	0.098**
Working	-0.041	0.121***
Total work hours	2.5	-2.3
asinh(total work hours)	0.1	-0.1
Individual earnings	-578.8	-136.6
asinh(individual earnings)	-0.3	0.1

Table A10: CMDS Single-Control Robustness Summary

Outcome	Min coef.	Max coef.	p<0.10 specs	Specs
Childcare burden	-0.079	-0.007	21	30
Eldercare burden	-0.059	-0.005	10	30
First migration with children	0.005	0.104	19	30

Table A11: CFPS Baseline Balance at the Age-60 Cutoff

Variable	Mean	RD coef.	SE	p-value
child_age	6.791	0.461	0.294	0.117
child_female	0.468	-0.026	0.040	0.516
older_female	0.526	0.087**	0.039	0.027
older_edu_years	4.620	0.871**	0.368	0.018
wb401	6.960	0.423	0.396	0.285
wb402	8.325	0.641	0.399	0.108
father_lt11	0.558	-0.045	0.039	0.250
mother_lt11	0.397	-0.043	0.040	0.282

Table A12: CFPS Covariate-Adjusted Robustness for Grandchild Outcomes

Outcome	Baseline	Add grandparent sex/education	N
Months with father	-1.389**	-1.548**	4,472
Months with mother	-1.608***	-1.783***	4,484
Father $<$ 11 months	0.117**	0.126**	4,499
Mother $<$ 11 months	0.129**	0.142**	4,499
Any parent $<$ 11 months	0.099*	0.109*	4,499
Father zero months	-0.063	-0.058	4,499

Notes: The baseline column matches the P1 2016–2018 co-residence estimates in Table A3. The adjusted column adds grandparent sex and education to the child age and child sex controls. The check responds to the baseline-balance differences in grandparent sex and education reported in Table A11. Heteroskedasticity-robust standard errors are used in the verified CFPS scripts. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A13: CFPS Placebo Cutoffs Around Age 60

Fake cutoff	Months with father	Months with mother	Father $<$ 11 months	Mother $<$ 11 months
54	1.720*	1.247	-0.269***	-0.140
56	-0.716	-2.061*	0.046	0.173
64	-0.653	-1.812*	0.133	0.177*
66	2.849**	2.279**	-0.308***	-0.176

Notes: Each row re-estimates the P1 CFPS RD-DID using a fake cutoff and a ±2-year window around that fake cutoff. The placebo audit is not uniformly quiet, so the CFPS evidence should be read together with the true-cutoff first stage, covariate adjustment, bandwidth sensitivity, and CHARLS mechanism evidence. Heteroskedasticity-robust standard errors are used in the verified CFPS scripts. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A14: CFPS Bandwidth Sensitivity

Outcome	BW 2	BW 3	BW 5	BW 7	BW 10
Months with father	-3.058***	-3.934***	-1.389**	-0.646	0.029
Months with mother	-2.890***	-4.139***	-1.608***	-1.046**	-0.410
Father $<$ 11 months	0.274**	0.355***	0.117**	0.072	0.001
Mother $<$ 11 months	0.247**	0.356***	0.129**	0.101**	0.035
Father zero months	-0.019	0.007	-0.063	-0.052	-0.055**

Table A15: CFPS MSE-Optimal RD Bandwidths in the 2018 Cross Section

Outcome	MSE-optimal bandwidth
Months with father	3.024
Months with mother	3.411
Father $<$ 11 months	2.947
Mother $<$ 11 months	3.473

Notes: Bandwidths are from the verified CFPS rdrobust audit for the 2018 cross-sectional RD. The values support reporting the main CFPS estimates in narrow local windows and interpreting wider bandwidths as sensitivity checks rather than as the preferred specification.

Table A16: CFPS Verified-Grandchild 2020 Window and COVID Caution

Specification	Outcome	Estimate	SE	N
RD-DID, 2016–2020	Parent at home	0.188***	0.060	3,846
RD-DID, 2016–2020	Parent away	-0.188***	0.060	3,846
RD-DID, 2016–2020	Parent out of province	-0.022	0.040	3,846
Cross-sectional RD, 2016	Parent at home	-0.043	0.038	2,227
Cross-sectional RD, 2020	Parent at home	0.146***	0.047	1,619
Cross-sectional RD, 2020	Parent out of province	-0.051*	0.028	1,619

Notes: The verified-grandchild file links children to parent-location outcomes in 2016 and 2020. Because the 2020 wave was collected during the COVID period, these estimates are reported as medium-run descriptive evidence rather than as the preferred pension window. The preferred CFPS co-residence estimates use the 2016–2018 P1 window in Table A3. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A17: CEPS Associations: Family Structure and Child Welfare

Outcome	Father absent	Mother absent	Grandparent co-residence
Cognitive ability	-0.043 (0.027)	-0.063** (0.028)	0.082*** (0.019)
Chinese score	-0.009 (0.021)	-0.075*** (0.020)	0.041*** (0.014)
Math score	-0.002 (0.019)	-0.070*** (0.024)	0.033** (0.014)
English score	-0.019 (0.016)	0.001 (0.021)	0.023** (0.011)
Self-rated health	-0.124*** (0.033)	-0.048 (0.037)	-0.003 (0.021)
Not depressed	-0.110*** (0.035)	0.004 (0.038)	0.004 (0.024)
Not unhappy	-0.084** (0.036)	-0.026 (0.040)	0.021 (0.021)

Notes: CEPS associations only; not causal. Baseline controls included as in the CEPS output file. Standard errors in parentheses.

Table A18: CFPS Child-Gender Heterogeneity

Outcome	Boys	Girls
Months with father	-2.037**	-0.964
Months with mother	-2.153***	-1.349
Father <11 months	0.165**	0.077
Mother <11 months	0.140*	0.140
Father zero months	-0.112**	-0.003

Table A19: CMDS Migrant-Gender Heterogeneity

Outcome	Female	Male	Female diff.
Childcare burden	-0.052***	-0.018	-0.026**
Eldercare burden	-0.014	-0.010	-0.016
First migration with children	0.053**	0.026	0.011

Appendix Figures

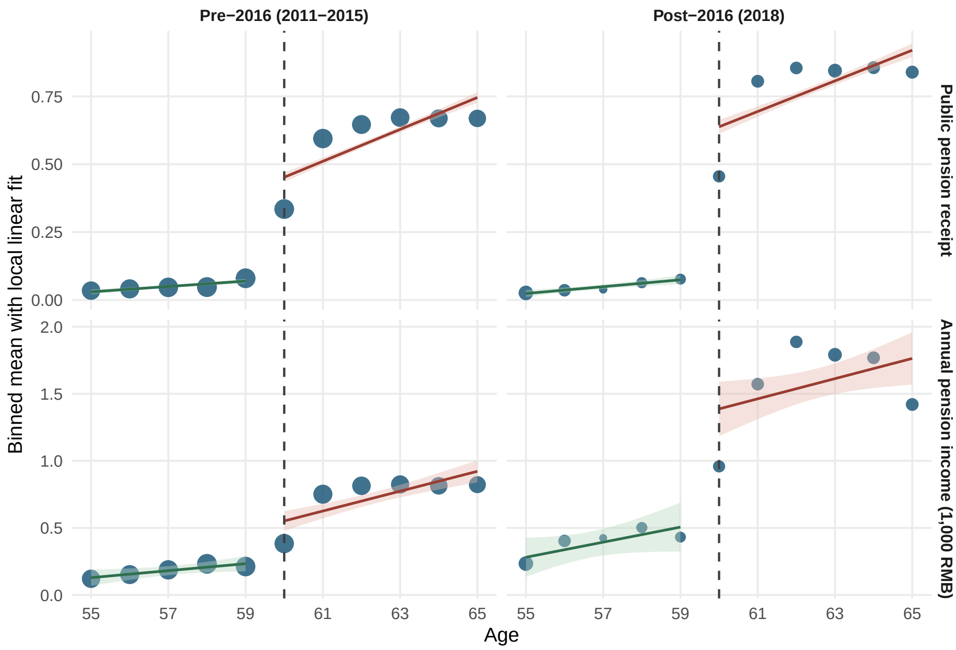

Figure A1: Pension receipt at the age−60 cutoff, before and after 2016. *Notes:* Each panel plots the mean pension receipt rate by single year of age, with a local linear fit on each side of the cutoff. The left panel uses pre−reform waves (2011, 2013, 2015); the right panel uses the 2018 wave. Rural respondents only, CHARLS.

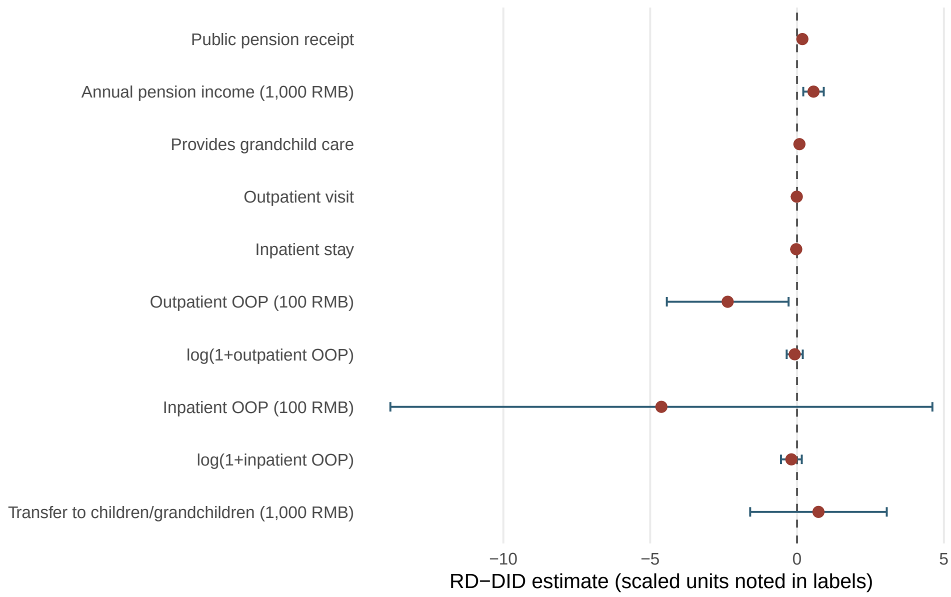

Figure A2: CHARLS RD-DID estimates: pension eligibility effects on grandparent outcomes. *Notes:* Each row plots the point estimate and 95% confidence interval from equation (14). Rural respondents aged 50–70, CHARLS waves 2011, 2013, 2015, 2018. Bandwidth ±5 years. Standard errors clustered at the individual level.

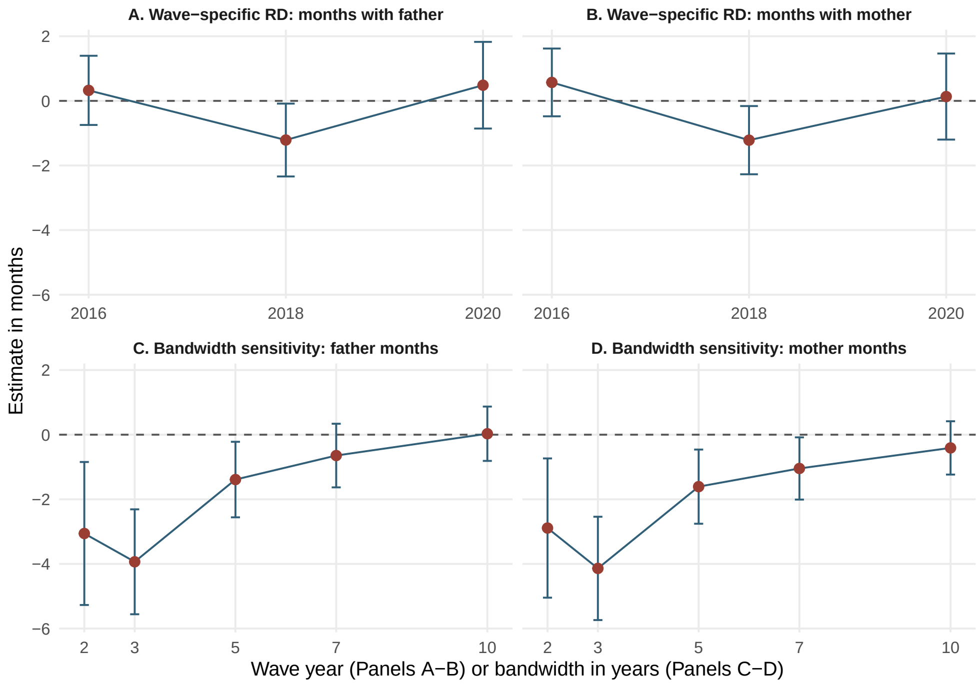

Figure A3: Grandchild time with parents: event-study and bandwidth robustness. *Notes:* Left panel shows annual RD estimates of months with father relative to 2016 (reform year), using CFPS waves 2014, 2016, 2018. Right panel plots the RD-DID coefficient on months with father and mother at bandwidths ± 2 through ± 10 years around the age-60 cutoff. CFPS rural-origin grandchildren sample. Standard errors clustered at the grandparent level.

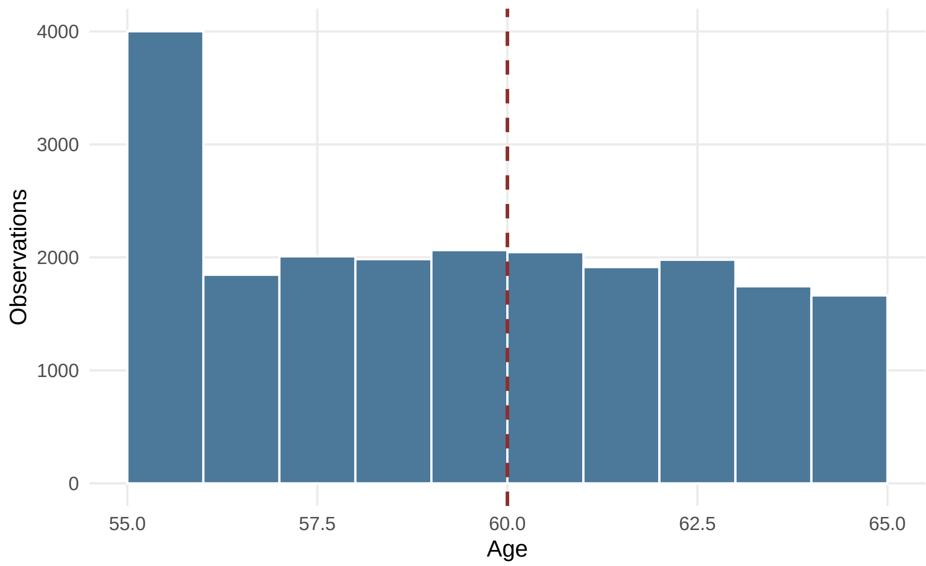

Figure A4: Age distribution around the age-60 cutoff in CHARLS. *Notes:* Histogram uses rural CHARLS respondents in the age-55 to age-65 bandwidth used for the main RD-DID analysis. The dashed line marks the pension eligibility cutoff.