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Retirement and Social Capital: Evidence from Urban China

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Abstract

We study the effect of retirement on social capital in urban China. Using three waves of the China Family Panel Studies (2018–2022) and a fuzzy regression discontinuity design based on China's statutory retirement ages, we document that retirement does not raise or lower social capital uniformly but reallocates it. Retirement causes bridging social capital to fall and bonding social capital to rise. The reallocation is concentrated among men, whose pre-retirement networks are more heavily workplace-centered. As governments around the world postpone statutory retirement ages in response to population ageing, our estimates imply that pension reforms carry social externalities of opposing sign that fiscal accounting omits.

JEL classification

J26, Z13, I31, D91

Keywords

retirement, social capital, fuzzy regression discontinuity, bridging and bonding social capital

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1. Introduction

Social capital, the productive resources embedded in social networks (Bourdieu, 1986; Coleman, 1988), shapes access to credit (Guiso et al., 2004), population health (Kawachi et al., 1997), and economic growth (Knack and Keefer, 1997; Putnam, 2000). Despite these well-documented consequences, the determinants of social capital are far less understood. Most evidence points to deep historical and cultural roots, such as the slave trade (Nunn and Wantchekon, 2011), inherited civic traditions (Putnam et al., 1993), and long-lived political institutions (Guiso et al., 2016)— leaving open whether a single, discrete institutional event can reconfigure social capital within a person's lifetime. Retirement offers an unusual opportunity to answer it. It is among the most consequential life-course transitions in modern economies, and it simultaneously severs the institutional scaffolding of the workplace and releases a large amount of discretionary time that can be reinvested in social relationships.

Retirement changes not only how much people work but whom they meet — and it does so mechanically, as a direct consequence of the change in employment status. An employed person meets colleagues, clients, and business partners daily, simply by going to work; a retired person, by definition, no longer holds the job through which that contact occurred, and sees former colleagues only occasionally, at reunions or chance encounters. This loss of routine contact with unfamiliar others is part of what it means to be retired, just as working hours tied to the former job fall to zero at retirement.

Guiso et al. (2008) model trust as a prior that individuals update through repeated social interaction. Meeting people from outside one's immediate circle, such as colleagues, acquaintances, and strangers encountered through work, generates informative signals that revise generalized trust. Interactions confined to an already-familiar circle instead reinforce existing, particularized attachments rather than update beliefs about people at large. The workplace is a primary source of unplanned contact with unfamiliar others. Retirement removes that source of contact, which should erode bridging social capital. At the same time, retirement frees time that can be invested in bonding social capital: closer engagement with children and parents, who at this stage of the life course often require more intensive family support. The two shifts are not offsetting entries in a private welfare calculation. A decline in bridging capital weakens the willingness to trust and cooperate with people outside one's immediate circle, and thereby wider social cohesion, in a way that stronger family ties do not compensate for.

This paper examines how retirement affects social capital under China's statutory, age-based retirement system. We measure five components of social capital and classify them following Putnam's (2000) distinction between bridging and bonding capital: formal organizational participation, generalized trust, and trust in neighbors capture bridging capital; strong family ties and family caregiving capture bonding capital. This decomposition allows us to detect a reconfiguration that single-outcome studies cannot by construction. Our predictions follow a simple contact-and-time logic above: retirement should therefore not raise or lower total social capital uniformly but reallocate it from bridging to bonding.

Identifying the causal effect of retirement is challenging. Individuals with richer social capital may retire earlier, and unobserved traits such as health, preferences, and family structure may drive both retirement timing and social capital. We address this challenge by exploiting China's statutory retirement ages. Before the phased reform that took effect in January 2025, statutory retirement ages were enforced by law and generated discontinuities in the probability of retirement at age 60 for men, 55 for female cadres, and 50 for female blue-collar workers. We combine these thresholds with three waves of the China Family Panel Studies (CFPS) from 2018 to 2022 in a fuzzy regression discontinuity (FRD) design. Because some workers retire early and others continue working past the threshold, the first stage is below one, and we estimate the local average treatment effect for compliers, that is, workers who retire in direct response to reaching the statutory cutoff.

Our results show that retirement reallocates social capital rather than shifting it uniformly. Retirement reduces the probability of participating in a formal organization by 0.661 standard deviations and lowers generalized trust by 0.678 standard deviations. The effect on trust in neighbors is also negative but not statistically significant. On the family side, retirement raises the intensity of close family ties by 0.636 standard deviations and the frequency of family caregiving by 0.996 standard deviations. These results are robust to alternative bandwidths, polynomial orders, and retirement definitions, as well as to placebo cutoffs and covariate-continuity tests. The reconfiguration is concentrated among men, whose pre-retirement networks are more heavily workplace-centered: for men, strong ties, family caregiving, and generalized trust all shift significantly, while women show no statistically significant change on any dimension. There is also some variation by education, as participation in formal organizations contracts significantly among workers with middle school education or below, while strong ties and family caregiving strengthen significantly among those with high school education or above.

This paper makes two contributions. First, we add to a growing literature on the social consequences of retirement institutions, beyond their well-documented fiscal and labor-supply effects. The existing evidence is predominantly European or North American, relies largely on panel and instrumental-variable strategies suited to voluntary-retirement systems, and typically studies one outcome at a time (Fletcher, 2014; Van den Bogaard et al., 2014; Comi et al., 2022; Cusimano et al., 2025). We provide the first causal evidence covering both bridging and bonding dimensions under a mandatory retirement system, and we show that the net effect on any single dimension masks a systematic reallocation across dimensions. Second, we contribute to the literature on the determinants of social capital. In contrast to studies emphasizing persistent historical roots, we show that a discrete individual event dictated by an institution can produce a rapid and substantial reconfiguration within individual social capital. Institutional design can therefore shape social-capital trajectories at the population scale. This matters as China and other ageing societies redesign their retirement systems in response to population ageing. China itself began phasing in higher statutory retirement ages in January 2025, and our estimates identify a class of social externalities that the fiscal accounting of such reforms omits.

The remainder of the paper proceeds as follows. Section 2 reviews the related literature. Section 3 describes the institutional background. Section 4 presents the data and the empirical strategy. Section 5 reports the main results and robustness checks. Section 6 examines heterogeneity. Section 7 concludes with policy implications.

2. Related literature

This paper connects to three strands of research. The first studies the determinants of social capital. In economics, attention has focused on deep historical determinants such as the slave trade (Nunn and Wantchekon, 2011), inherited culture (Algan and Cahuc, 2010), and long-run institutional persistence (Guiso et al., 2016). A complementary line examines contemporary factors, including television (Olken, 2009), broadband internet (Bauernschuster et al., 2014; Geraci et al., 2022), corruption (Banerjee, 2016), and teaching practices (Algan et al., 2013). Recent work decomposes social capital into distinct dimensions with independent effects on economic outcomes (Durante et al., 2025). We contribute by identifying retirement as a discrete life-course event that rapidly and systematically reconfigures the structure of social capital.

The second strand examines the effects of retirement on social outcomes, with mixed evidence drawn largely from Europe and North America. Some studies find that retirement reduces social capital: Börsch-Supan and Schuth (2014) document declining contact with non-family members in Europe, Patacchini and Engelhardt (2016) report reduced network size and density among U.S. retirees, especially for women and the highly educated, and Kauppi et al. (2021) conclude that Finnish public-sector employees lose roughly one peripheral contact around retirement while core ties remain intact. Others report positive effects: Cornwell et al. (2008) observe greater neighborhood socialization among older Americans, and Van den Bogaard et al. (2014) find increased instrumental support to children and parents and greater volunteering in the Netherlands. Comi et al. (2022) offer a nuanced picture in which retirement in Europe leaves overall network size unchanged but substitutes family ties for colleague ties. A third group finds no significant effect: Van Tilburg (2003) and Fletcher (2014) document high stability of networks and friendships, and Lim-Soh et al. (2024) find that most middle-aged and older Koreans maintain stable participation after retirement. We extend this literature by covering both bridging and bonding dimensions within a single design and by studying a non-Western, mandatory-retirement setting.

The third strand develops the economic theory of social capital and of aging more broadly. Posner (1995) interprets aging as a human-capital phenomenon: the fall in the opportunity cost of time at retirement, together with the truncation of the payback period on relationship-specific capital, governs which ties are formed and which are allowed to depreciate. Glaeser et al. (2002) formalize social capital as an investable asset with returns and depreciation, embedded in a life-cycle framework. Guiso et al. (2008) model trust as a prior updated through social contact, which explains why bridging and bonding capital respond differently to a change in whom one meets. We draw on this literature for the contact-and-time logic that motivates our empirical predictions.

3. Institutional background

China's public pension system determines both the timing and the terms of labor-market exit for urban workers through statutory retirement ages. Urban enterprise employees are covered by the Basic Old Age Insurance (BOAI), established in 1951; civil servants and public-sector employees were covered by a separate, more generous Public Employee Pension (PEP) until it was folded into BOAI in a 2015 reform (Fang and Feng, 2018). Rural and non-employed urban residents are covered by a separate, voluntary residents' pension, unified into a single national scheme in 2014 (Fang and Feng, 2018). Because mandatory retirement ages apply only to formal urban employment, our design exploits the BOAI/PEP employee schemes rather than the residents' pension.

The employee schemes date to the planned-economy era and were consolidated into their current form through a series of reforms. Individual employee contributions were introduced in the 1980s, and the system was unified in 1997 into a two-pillar structure combining employer-financed social pooling with employee-financed individual accounts (Feldstein, 1999; Feng et al., 2011; Zhao and Xu, 2002). For a worker with thirty-five years of contributions earning the local average wage, the BOAI replacement rate is approximately 59 percent, versus 80-90 percent under the more generous PEP before its 2015 merger into BOAI (Feng et al., 2020; Fang and Feng, 2018). By the end of 2023, basic pension insurance -- BOAI plus the residents' schemes introduced for rural and non-employed urban populations in 2009-2011 and unified in 2014 -- covered roughly 1.07 billion people, about 76 percent of the population (Giles et al., 2023).

The feature of the Chinese system most relevant for our design is its mandatory retirement-age policy. Statutory retirement ages, largely unchanged since the 1950s, are 60 for men, 55 for female cadres, and 50 for female blue-collar workers (Giles et al., 2023). The earlier ages for women were originally motivated by a desire to relieve women of physically demanding work in an era when household burdens fell disproportionately on them (Feng et al., 2020). Unlike pension-eligibility ages in many Western systems, which define when benefits may be claimed, China's retirement ages define when formal employment must end: upon reaching the statutory age, workers in the formal urban sector complete the retirement process, separate from their employer, and begin receiving pension benefits immediately (Feng et al., 2020; Lei and Liu, 2018).

This design produces a tight link between reaching the statutory age, exiting employment, and claiming benefits: there is no actuarial reward for delaying exit, and formal employment opportunities decline sharply after the mandatory age, although retirees may continue working informally without forfeiting benefits (Feng et al., 2020). Because retirement at the statutory age is quasi-mandatory rather than a choice responding to health shocks or preferences, the discontinuity is well suited to identifying the causal effects of retirement, and has been exploited in prior work on cognition and health behaviors (Lei and Liu, 2018; Feng et al., 2020). The system also faces mounting demographic pressure -- population aging and low fertility have intensified concerns over the pay-as-you-go pillar's long-run balance, and postponing statutory retirement ages is the centerpiece of the reform agenda, with a phased increase taking effect in January 2025 (Fang and Feng, 2018; Giles et al., 2023).

4. Data and empirical strategy

4.1. Data and sample

We use the 2018, 2020, and 2022 waves of the China Family Panel Studies (CFPS), a nationally representative longitudinal survey conducted by the Institute of Social Science Survey at Peking University. Because statutory retirement ages bind only for formally employed urban workers, and only insofar as retirement is the relevant channel through which they leave the workforce, we restrict the sample to respondents who satisfy three conditions within the same wave: (1) non-agricultural household registration (hukou); (2) a formal-sector employer (state-owned enterprise, government agency, public institution, social organization, or another formal work unit); and (3) they are currently employed or retired. The third condition excludes individuals who left the workforce for reasons unrelated to the statutory age rule -- informal employment, unemployment, or inactivity due to disability, family caregiving, or being "too old to work" -- since for them the age threshold is not the operative margin governing labor-market exit, and their absence from work cannot be attributed to the discontinuity we study. These three conditions together approximate, as closely as the data allow, the population of urban formal-sector workers who are eligible for the pension system and subject to its statutory age thresholds. We cannot observe true pension coverage directly, so hukou and employer type serve as the best available proxies for it; Section 4.5 discusses how this approximation, together with other sources of imperfect compliance, motivates a fuzzy rather than a sharp regression discontinuity design. The resulting sample includes 9,539 observations from 5,633 unique individuals. Of these observations, 590 (6.2%) are retired and 8,949 (93.8%) are employed. Of the individuals, 2,930 (52.0%) are observed in one wave, 1,500 (26.6%) in two, and 1,203 (21.4%) in all three. Standard errors are clustered at the individual level to account for within-person correlation. The running variable (age in months at the date of the interview) is non-missing for 9,265 observations. The effective sample in each RD estimation depends on the MSE-optimal bandwidth and ranges from roughly 2,000 to 3,100 observations across outcomes, as reported in the tables.

4.2. Measuring social capital

We define five measures of social capital, which we classify into two categories following Putnam (2000).

(1) Bridging social capital. Bridging capital connects individuals to people outside their immediate circle. We measure it with three indicators. *Formal participation* is a binary variable equal to 1 if the respondent belongs to at least one of the following organizations: the Communist Party, the Communist Youth League, a religious group, a trade union, or a self-employed workers' association. *Generalized trust* is the self-rated level of trust in strangers (CFPS: "On a scale of 0 to 10, how much do you trust strangers?"). *Trust in neighbors* is the self-rated level of trust in neighbors (CFPS: "On a scale of 0 to 10, how much do you trust your neighbors?"). We classify trust in neighbors as bridging rather than bonding capital because, in large, high-turnover Chinese cities, neighbors are frequently not people one grew up with or has known for long. Residential mobility and a large population of temporary migrants leave many urban neighborhoods with comparatively weak social cohesion (Wang et al., 2017), so trust in neighbors functions, for many urban residents, more like trust in unfamiliar others than like an extension of the family circle.

(2) Bonding social capital. Bonding capital reflects the strength of already-established, close relationships, typically with relatives and family members. *Strong ties* measure the intensity of family relationships: the number of relationships with children, father, and mother over the past six months reported as "close" or "very close." *Family caregiving* is the frequency (from 0, low, to 6, high) with which the respondent provides household chores, grandchild care, or personal care to children or parents over the past six months; we use the higher of the frequencies reported for children and parents.

4.3. Retirement status and the running variable

The core explanatory variable is retirement status, a binary indicator equal to 1 if the respondent is retired and 0 otherwise. A respondent is classified as retired if the reported reason for not working is retirement or being pensioned off, and as not retired if the official CFPS laborforce classification indicates current employment. Respondents who are neither employed nor retired for the specific reason of retirement (for example, unemployed and searching, or inactive because of disability, family caregiving, or being "too old to work") are excluded, as described above.

The running variable is normalized age: the individual's precise age in months minus the gender- and cadre-specific statutory retirement age, expressed in years. Precise age is computed from birth month and interview month, pooling the 2010–2022 CFPS waves to recover birth month for respondents missing it in a given wave. The statutory cutoff is 60 for men and, for women, either 55 or 50 depending on cadre status: women employed at any point during 2010–2022 by a government agency or public institution are assigned the 55-year cadre cutoff, and other women the 50-year worker cutoff. This normalization places men and women on a common scale referenced to their respective statutory cutoffs.

4.4. Control variables and descriptive statistics

Individual-level controls include gender, education (college or above as the reference group), marital status (widowed as the reference group), and chronic illness. Household-level controls include the log of per-capita household income and an indicator for family business assets. Age itself is not included as a separate control, since it is captured by the running variable. Table 1 reports descriptive statistics for the analysis sample.

As shown in Table 1, the retired group is considerably older on average (60.854 vs. 40.268 years), has a lower share of college-educated individuals (19.7% vs. 54.2%), and has a higher prevalence of chronic illness (24.1% vs. 11.2%). On the bridging outcomes, the retired group has lower means of formal participation (0.271 vs. 0.492) and generalized trust (2.432 vs. 2.989), but a higher mean of trust in neighbors (6.789 vs. 6.511). On the bonding outcomes, the retired group has a lower mean of strong ties (1.000 vs. 1.274) but a higher mean of family caregiving (2.482 vs. 1.557). Because the retired group is on average two decades older, these raw comparisons reflect broad life-cycle differences rather than the effect of retirement itself. The FRD estimates below isolate the local effect of crossing the statutory threshold and can differ in sign from the raw group means, as is the case for strong ties.

Table 1. Variable definitions and descriptive statistics

Variable Category	Variable Name	Definition	Retired Mean	Retired SD	Non-retired Mean	Non-retired SD
Bridging SC	Formal Part.	Member of ≥1 organization=1, else=0	0.271	0.445	0.492	0.500
Bridging SC	Generalized Trust	Trust in strangers (0-10)	2.432	2.254	2.989	2.190
Bridging SC	Trust in Neighbors	Trust in neighbors (0-10)	6.789	2.024	6.511	1.948
Bonding SC	Strong Ties	Intensity of close family relationships (children/parents)	1.000	0.801	1.274	0.801
Bonding SC	Family Caregiving	Frequency of helping family with housework/childcare (0-6)	2.482	2.758	1.557	2.242
Explanatory Variable	Retirement	Not working & primary reason is retirement=1, else=0	1.000	0.000	0.000	0.000
Running Variable Basis	Age	Respondent's actual age	60.854	6.392	40.268	10.971
Control Variables	Gender	Male=1, Female=0	0.566	0.496	0.559	0.497
	Education	Illiterate/Primary=1, else=0	0.122	0.328	0.058	0.234
		Middle/High School=1, else=0	0.681	0.466	0.400	0.490
	Marital Status	Married/cohabiting=1, else=0	0.898	0.303	0.801	0.399
		Single/divorced=1, else=0	0.042	0.202	0.188	0.391
	Chronic Illness	Has chronic illness=1, else=0	0.241	0.428	0.112	0.315
	Per-capita HH Income	Log of Per-capita household income	10.630	0.760	10.567	0.740
	Family Business Assets	Has family business assets=1, else=0	0.036	0.187	0.067	0.249

4.5. Empirical strategy

Although the statutory threshold is nominally mandatory for workers covered by the BOAI or PEP, compliance is imperfect for two reasons. First, our sample construction (Section 4.1) can only approximate the population actually covered by the mandatory-age rule: hukou registration and formal-sector employer status are observable proxies for BOAI/PEP coverage, not the coverage itself, and a substantial share of workers in nominally formal jobs lack a signed labor contract or employer-paid pension insurance in practice. If our sample corresponded exactly to the population for whom the statutory age is binding, the first stage would equal one: everyone would retire precisely at the cutoff. Second, even among covered workers, some continue to work past the threshold and others retire early. Both channels imply that crossing the statutory age raises the probability of retirement sharply but not deterministically. We therefore treat the threshold as an instrument for actual retirement status in a fuzzy regression discontinuity design. The FRD estimand is the ratio of the reduced-form discontinuity in the outcome to the first-stage discontinuity in treatment:

$$\tau_{FRD} = \frac{\lim_{\varepsilon\downarrow 0} E[S|a = 0 + \varepsilon] - \lim_{\varepsilon\uparrow 0} E[S|a = 0 + \varepsilon]}{\lim_{\varepsilon\downarrow 0} E[R|a = 0 + \varepsilon] - \lim_{\varepsilon\uparrow 0} E[R|a = 0 + \varepsilon]} \quad (1)$$

where *S* denotes social capital, *R* denotes the retirement dummy, and *a* denotes normalized age (male: a = age − 60; female cadres: a = age − 55; female workers: a = age − 50). τ_{FRD} represents the local average treatment effect (LATE) for compliers at the cutoff, defined as the average change in social capital for individuals who retire at the statutory age.

Valid FRD estimation relies on two assumptions (Lee and Lemieux, 2010). First, the probability of retirement must be discontinuous at the cutoff. Figure 1 plots the raw retirement rate by age and gender. For men, the rate jumps sharply at age 60; for women, it jumps at age 50, with a further, more gradual rise around age 55, consistent with the smaller subgroup of female cadres who face a 55-year cutoff (Zhang et al., 2018). Figure 2 confirms the discontinuity on the normalized running variable, plotting the RD relationship separately for men and women: both panels show a sharp jump in the retirement rate at the normalized cutoff of zero. Second, all predetermined variables must evolve continuously through the cutoff, so that no confounder jumps at the threshold. We test this assumption in Section 5.3.

Local average treatment effects τ_{FRD} can be estimated either parametrically or nonparametrically. We adopt the nonparametric local polynomial method (Cattaneo et al., 2017). The approach assumes local linearity in a small neighborhood around the cutoff and estimates causal effects via weighted least squares (WLS), reducing boundary bias and avoiding model misspecification.

The estimated LATE is:

$$\hat{\tau}_{FRD}(h) = \frac{\hat{\mu}_{S+(h)} - \hat{\mu}_{S-(h)}}{\hat{\mu}_{R+(h)} - \hat{\mu}_{R-(h)}} \tag{2}$$

In equation (2), $\hat{\mu}_{S+(h)}$ is the estimate of $\lim_{\varepsilon \downarrow 0} E[S|a = 0 + \varepsilon]$ from the numerator of equation (1), representing the local estimate of social capital S to the right of the cutoff within bandwidth h. Similarly, $\hat{\mu}_{S-(h)}$ is the estimate of $\lim_{\varepsilon \uparrow 0} E[S|a = 0 + \varepsilon]$ from the numerator of equation (1), representing the local estimate of S to the left of the cutoff within bandwidth h. Analogously, $\hat{\mu}_{R+(h)}$ and $\hat{\mu}_{R-(h)}$ are the estimates of the corresponding terms from the denominator of equation (1), representing the local estimates of retirement variable R to the right and left of the cutoff within bandwidth h, respectively.

Bandwidth selection involves a bias-variance tradeoff. Smaller bandwidths reduce bias but sacrifice efficiency due to smaller samples; larger bandwidths improve efficiency but increase bias. We use the MSE-optimal bandwidth h_{MSE} that minimizes the mean squared error of the RD estimator (Imbens and Kalyanaraman, 2012).

All estimates use local linear regression (p = 1) with MSE-optimal bandwidths (Calonico et al., 2014), applied to the full estimation sample without pre-restricting the age range. A quadratic specification (p = 2) is reported as a robustness check in Table 7, Panel B. RD plots (Figures 2–5) display binned sample averages fitted with the same local linear specification used in the estimation; Figure 2 displays observations within ±20 years of the cutoff, and Figures 3–5 within ±10 years.

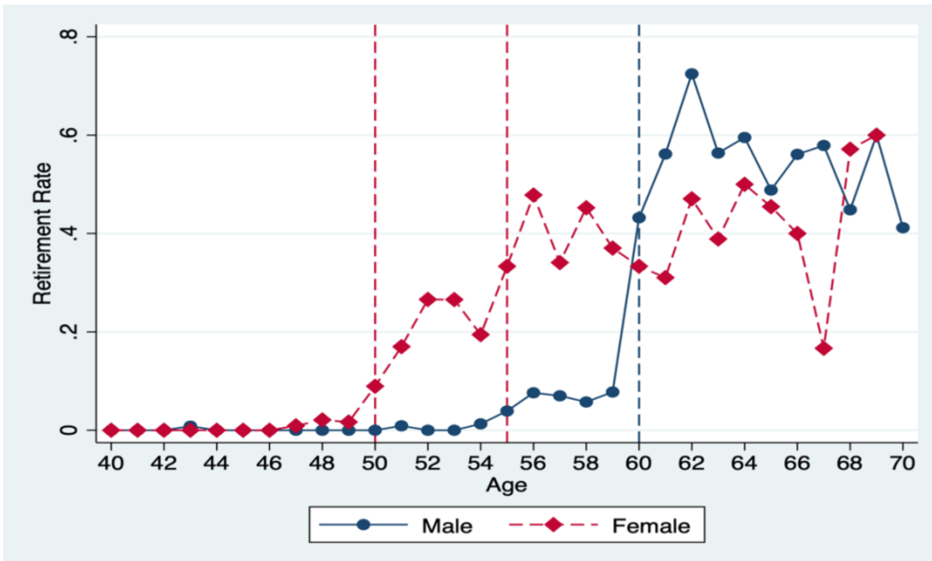

Figure 1. Retirement rates by age and gender

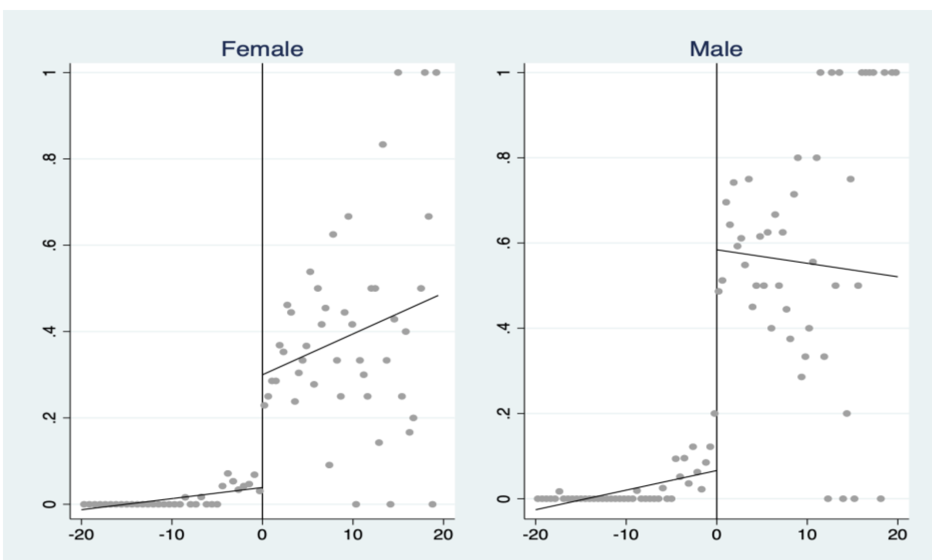

Figure 2. Retirement rate discontinuity on the normalized running variable, by gender

5. The effect of retirement on social capital

5.1. Retirement and bridging social capital

Figure 3 plots the three bridging outcomes, formal participation, generalized trust, and trust in neighbors, against normalized age. All three fitted lines drop discontinuously at the cutoff: formal participation falls from about 0.47 to 0.32, generalized trust from about 2.71 to 2.22, and trust in neighbors from about 6.91 to 6.62. Both fits in each panel use the same local linear specification as the baseline estimation, so the plotted discontinuities correspond directly to the point estimates reported below.

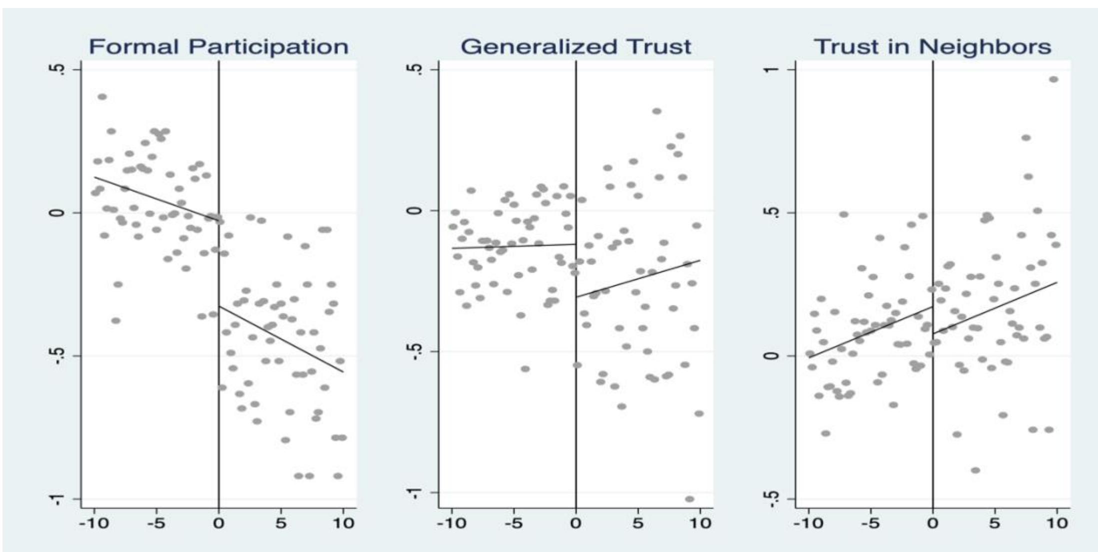

Figure 3. Normalized age and bridging social capital

Table 2 presents the estimates. In this case all outcomes are standardized within the estimation sample, so point estimates are directly comparable across outcomes in standard-deviation units. Retirement reduces the probability of participating in a formal organization by 0.661 standard deviations, significant at the 10% level (p = 0.080). This decline reflects the severing of ties to work-unit-affiliated organizations, such as trade unions and Party organizations, which are the primary channels of formal participation during employment: the workplace not only provides access to organizational membership but also sustains it, and once individuals leave their jobs these institutionalized pathways are cut off. Retirement also lowers generalized trust by 0.678 standard deviations, significant at the 5% level (p = 0.015). The workplace exposes individuals to people from diverse backgrounds, and repeated cross-group contact is a well-established driver of generalized trust (Guiso et al., 2008). By removing this contact, retirement erodes the foundation for trusting people at large.

Trust in neighbors moves in the same negative direction, though the estimate is smaller and not statistically significant (-0.200 standard deviations, p = 0.500). As discussed in Section 4.2, for many urban residents, neighbors are hardly an extension of the family circle (Wang et al., 2017).

Taken together, the bridging results point to a contraction of the social capital rooted in contact with people outside the family circle: participation in formal, work-linked organizations declines, generalized trust declines, and neighbor trust moves in the same direction. This pattern is consistent with the mechanism in Guiso et al. (2008): retirement removes the workplace as a source of everyday contact with unfamiliar others and cuts off the informative signals that sustain trust in people at large.

Table 2. Effects of retirement on bridging social capital

Dependent Variable	Formal Participation	Formal Participation	Generalized Trust	Generalized Trust	Trust in Neighbors	Trust in Neighbors
Bridging SC	First Stage	Second Stage	First Stage	Second Stage	First Stage	Second Stage
Retirement		-0.661* (0.321)		-0.678** (0.314)		-0.200 (0.318)
Statutory Retirement Age	0.299*** (0.039)		0.308*** (0.038)		0.302*** (0.039)	
Bandwidth	7.090	7.090	7.701	7.701	7.148	7.148
Observations	2363	2363	2512	2512	2352	2352
Controls	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F	57.35		66.61		60.20	

Notes: Standard errors are robust bias-corrected standard errors, clustered at the individual level, reported in parentheses; significance stars are based on robust bias-corrected p-values. *, **, and *** denote significance at the 10%, 5%, and 1% levels. Controls include gender, education, marital status, chronic illness, per-capita household income, and family business assets. Age itself is not a separate control, since it is captured by the running variable. All estimates use a triangular kernel and MSE-optimal bandwidths. Same hereafter.

5.2. Retirement and bonding social capital

Figure 4 plots the two bonding outcomes, strong ties and family caregiving, against normalized age. Both fitted lines rise discontinuously at the cutoff: the intensity of close family relationships jumps from about 0.75 to 0.93, and the frequency of family caregiving from about 1.3 to 2.2. The fits again use the baseline local linear specification, so the plotted jumps map directly into the estimates reported below.

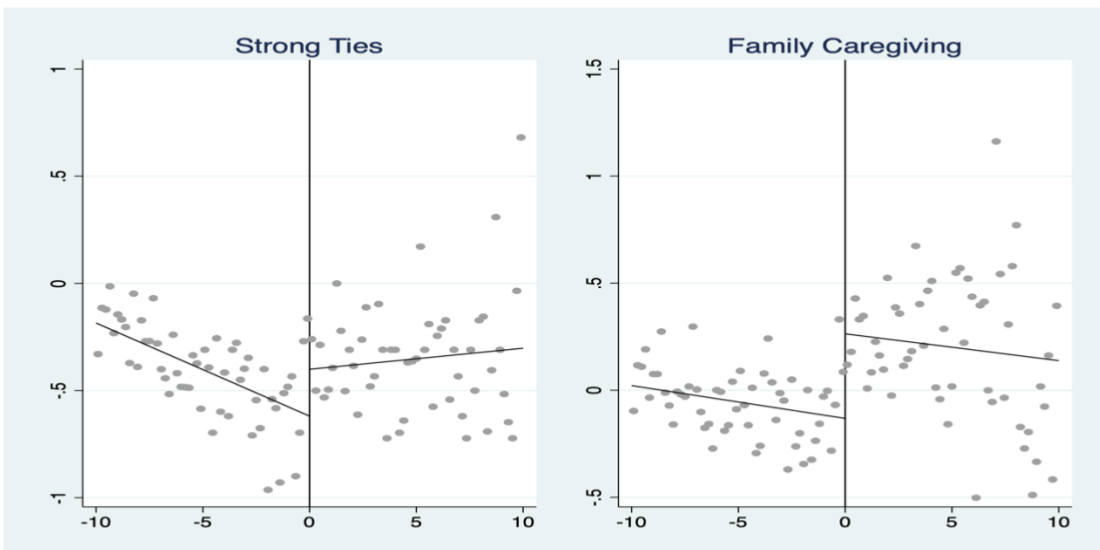

Figure 4. Normalized age and bonding social capital

Table 3 presents the estimates. Retirement increases the intensity of close family relationships by 0.636 standard deviations, significant at the 5% level (p = 0.018). With work no longer competing for time, individuals can invest more heavily in family relationships and deepen bonds with children, parents, and other core family members. This is also the stage of life at which aging parents are more likely to need old-age support and adult children more likely to need help with grandchildren, which makes the reallocation of time toward the family particularly salient.

Retirement also increases the frequency of family caregiving by 0.996 standard deviations, significant at the 1% level (p = 0.001); this is the most precisely estimated effect in the paper. The released time enables greater investment in household chores, childcare, and personal care for children and parents, consistent with Van den Bogaard et al. (2014). The two results reinforce each other: together they document a coherent family-centered shift in how retirees allocate both emotional attention and practical effort.

The bonding results thus document a clear strengthening of family-centered social capital, driven by already-existing relationships rather than by new social contact: retirees redirect their newly available time toward children and parents, both in the closeness of the relationship itself and in the practical support they provide.

Table 3. Effects of retirement on bonding social capital

Dependent Variable	Strong Ties		Family Caregiving	
Bonding SC	First Stage	Second Stage	First Stage	Second Stage
Retirement		0.636** (0.301)		0.996*** (0.324)
Statutory Retirement Age	0.310*** (0.037)		0.320*** (0.037)	
Bandwidth	8.379		9.837	
Observations	2615		2977	
Controls	Yes		Yes	
First-stage F	70.70		76.25	

5.3. Robustness checks

We conduct five robustness checks. First, we test the continuity assumption by verifying that the control variables used in the main specifications do not jump at the statutory retirement age (Lee and Lemieux, 2010). We treat each control variable as the dependent variable and estimate a sharp RD with the baseline specification, testing the exact variables entered as controls elsewhere in the paper (the education and marital-status indicators, not the underlying raw scales). As shown in Table 4, none of the eight control variables shows a statistically significant discontinuity at the cutoff, supporting the continuity assumption underlying the design.

Table 4. Continuity tests for control variables

	Running Variable: Normalized Age (Sharp RD)		
Control Variable	Coefficient	Bandwidth	Observations
Gender	-0.005 (0.048)	6.962	2717
Education (Illiterate/Primary)	0.032 (0.033)	7.922	3028
Education (Middle/High School)	0.018 (0.045)	7.135	2767
Marital Status (Married)	-0.015 (0.032)	7.335	2841
Marital Status (Single)	0.018 (0.026)	8.689	3240
Per-capita HH Income	-0.063 (0.065)	8.147	3019
Family Business Assets	0.000 (0.018)	8.682	3165
Chronic Illness	0.032 (0.035)	8.115	3069

Second, we apply the manipulation test of Cattaneo et al. (2020, 2021), a local polynomial density estimator building on McCrary (2008), to assess whether individuals can precisely manipulate their age around the cutoff. Manipulation would produce a discontinuous jump in the density of the running variable at the threshold and could confound the estimates (Shigeoka, 2014). Figure 5 presents the estimated density of normalized age on each side of the cutoff. At the cutoff itself the density is continuous: the levels on the two sides match closely and there is no visible jump or bunching. Formally, the manipulation test yields a robust test statistic of T = -0.413 (p = 0.680). As a complementary check, we also report binomial tests: for windows of increasing width around the cutoff, we count how many observations fall just to the left versus just to the right and test whether the split is 50/50, which would be violated if individuals were bunching just past the threshold. These tests are insignificant at every window width considered, from 0.083 to 0.833 in normalized-age units (p ranging from 0.25 to 1.00).

The density does decline steadily further to the right of the cutoff, but this is a feature of how the estimation sample is constructed rather than evidence of sorting: the sample includes only individuals who are either still employed or already retired (Section 4.1), so as age rises further past the statutory cutoff, progressively fewer individuals remain in this frame, which mechanically thins the right tail of the running variable. Because this decline is gradual and starts well past the cutoff itself, with no discontinuity exactly at zero, it does not indicate age manipulation.

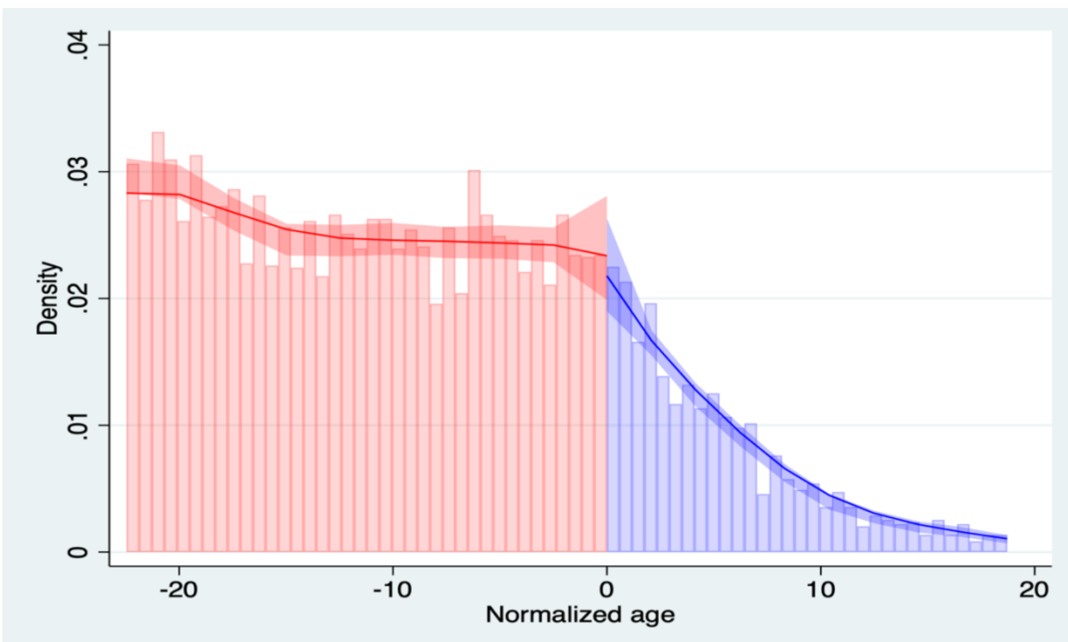

Figure 5. McCrary density test for normalized age

Third, we assess sensitivity to bandwidth selection by re-estimating the main specifications with a two-sided MSE-optimal bandwidth and with fixed bandwidths of h = 9 and h = 10. As shown in Table 5, family caregiving and generalized trust remain significant at the 1% to 5% level across all three choices. Strong ties and formal participation are significant under the two-sided

MSE-optimal and h = 10 bandwidths but not under h = 9 (p = 0.131 and p = 0.122). This does not mean the underlying effect vanishes at h = 9: the point estimates keep the same sign and a magnitude close to the other two bandwidths (strong ties 0.653–0.748; formal participation −0.661 to −0.748 across the three choices), so the loss of significance reflects reduced precision from the smaller effective sample at a narrower bandwidth, not a smaller effect. Trust in neighbors remains insignificant under every bandwidth, consistent with the baseline. The main conclusions are therefore not sensitive to the choice of bandwidth: point estimates are stable in sign and magnitude throughout, even where precision varies.

Table 5. Robustness checks: alternative bandwidths

Panel A:	Bridging SC	Bridging SC	Bridging SC	Bonding SC	Bonding SC
MSE-optimal (two-sided)	Formal Part.	Gen. Trust	Neighbor Trust	Strong Ties	Family Caregiving
Retirement	-0.661*	-0.683**	-0.218	0.748**	1.089***
	(0.337)	(0.339)	(0.353)	(0.348)	(0.368)
Bandwidth	8.931	8.152	9.356	9.435	10.158
	5.999	6.491	5.321	6.029	6.898
Observations	2677	2518	2707	2708	2905
Controls	Yes	Yes	Yes	Yes	Yes
First-stage F	50.51	56.54	46.74	50.29	57.98
Panel B:	Bridging SC	Bridging SC	Bridging SC	Bonding SC	Bonding SC
h=9	Formal Part.	Gen. Trust	Neighbor Trust	Strong Ties	Family Caregiving
Retirement	-0.710	-0.668**	-0.271	0.653	0.973**
	(0.350)	(0.362)	(0.348)	(0.350)	(0.397)
Bandwidth	9	9	9	9	9
Observations	2841	2820	2828	2744	2744
Controls	Yes	Yes	Yes	Yes	Yes
First-stage F	50.81	51.50	51.24	50.09	50.09
Panel C:	Bridging SC	Bridging SC	Bridging SC	Bonding SC	Bonding SC
h=10	Formal Part.	Gen. Trust	Neighbor Trust	Strong Ties	Family Caregiving
Retirement	-0.748*	-0.659**	-0.280	0.681*	1.002**
	(0.329)	(0.340)	(0.327)	(0.332)	(0.376)
Bandwidth	10	10	10	10	10
Observations	3117	3096	3104	3004	3004
Controls	Yes	Yes	Yes	Yes	Yes
First-stage F	57.11	57.82	57.59	56.32	56.32

Fourth, we conduct a placebo test to verify that the estimated effects are driven by the discontinuity at the statutory retirement age rather than by other age-related factors. If the statutory age is the sole driver, a false cutoff at another age should yield no significant effect on retirement status. We place the placebo cutoff five years below the true thresholds, at age 55 for men and age 45 for women (the true male cutoff is 60, the true female-worker cutoff is 50), and use a narrow, fixed bandwidth of 4 years for both the first-stage and reduced-form regressions. The bandwidth must be kept narrower than the five-year gap to the nearest true cutoff: with a wider window the estimation window itself reaches into the true statutory cutoff, and the local linear fit would mechanically attribute part of the real discontinuity to the false one.[1]

As shown in Table 6, the first stage is small and statistically indistinguishable from zero (0.015, p = 0.196). The reduced-form evidence is equally clean: none of the five outcomes is significant (formal participation, p = 0.229; generalized trust, p = 0.652; trust in neighbors, p = 0.816; strong ties, p = 0.163; family caregiving, p = 0.149).

Table 6. Placebo test

Cutoff: male 55, female 45	Retirement Status	Formal Part.	Gen. Trust	Neighbor Trust	Strong Ties	Family Caregiving
Coefficient	0.015	-0.112	-0.046	-0.023	0.134	0.140
	(0.012)	(0.093)	(0.102)	(0.097)	(0.096)	(0.097)
Bandwidth	4	4	4	4	4	4
Observations	1612	1722	1706	1712	1647	1647
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The explanatory variable is the placebo cutoff dummy (the discontinuity indicator at the false age threshold: male age 55, female age 45), not actual retirement. The "Retirement Status" column regresses the retirement indicator on the placebo cutoff dummy (the first stage); the remaining five columns regress each social-capital outcome directly on the same dummy (the reduced form). All six columns share the same specification -- fixed bandwidth h = 4, the same set of controls, and clustering at the individual level -- and differ only in the dependent variable. Standard errors here are conventional standard errors, clustered at the individual level, reported in

[1] A symmetric placebo cutoff five years above the true thresholds (age 65 for men, age 60 for women) is unfeasible because the sample has too few observations past those ages (only 279 men aged 65 or older and 220 women aged 60 or older) to estimate the model with useful precision at h = 4.

parentheses, and significance stars are based on conventional p-values

Fifth, we assess sensitivity to the definition of retirement and to the functional form, with results reported in Table 7.

Panel A adopts a broader definition of retirement. The baseline definition (Section 4.3) classifies a respondent as retired only if the reported reason for not working is retirement. The alternative definition pools five retirement-related indicators: (i) retirement is the reported reason for not working; (ii) the respondent has completed formal retirement or resignation procedures; (iii) the respondent has completed the procedures to start collecting a pension; (iv) the respondent currently receives a retirement pension; and (v) the respondent currently receives pension insurance. A respondent is classified as retired if any of the five holds, and anyone still employed is coded as not retired. Because it requires only one of five signals rather than the single reason-for-not-working question, this definition captures more of the true retired population and compensates for underreporting on the baseline question, at the cost of being less precise about the channel through which retirement is reported. The results are qualitatively unchanged from the baseline.

Panel B replaces the local linear estimator with a quadratic polynomial. Estimates have the same sign as the baseline and magnitudes are comparable, but their precision is smaller, reflecting the well-known efficiency loss of higher-order local polynomials in RD estimation.

Table 7. Robustness checks: alternative retirement definition and polynomial order

Panel A:	Bridging SC	Bridging SC	Bridging SC	Bonding SC	Bonding SC
Alternative Retirement Def.	Formal Part.	Gen. Trust	Neighbor Trust	Strong Ties	Family Caregiving
Retirement	-0.415*	-0.417**	-0.009	0.433**	0.719***
	(0.187)	(0.191)	(0.203)	(0.196)	(0.224)
Bandwidth	6.928	8.333	7.868	7.584	9.036
Observations	2632	2997	2897	2745	3109

Controls	Yes	Yes	Yes	Yes	Yes
First-stage F	129.94	153.95	129.88	136.60	139.17
Panel B:	Bridging SC	Bridging SC	Bridging SC	Bonding SC	Bonding SC
Quadratic Polynomial	Formal Part.	Gen. Trust	Neighbor Trust	Strong Ties	Family Caregiving
Retirement	-0.503	-0.747	-0.092	0.515	0.928*
	(0.466)	(0.517)	(0.485)	(0.435)	(0.509)
Bandwidth	9.052	8.148	8.211	9.856	9.342
Observations	2861	2619	2645	2977	2852
Controls	Yes	Yes	Yes	Yes	Yes
First-stage F	27.87	26.03	26.34	31.49	30.06

6. Heterogeneity

The reallocation need not be uniform across groups. Workers differ in how heavily their pre-retirement networks depend on the workplace, so the same shock can produce shifts of very different size. We examine heterogeneity by gender and by education.

6.1. Gender

Table 8 reveals substantial gender heterogeneity. None of the five outcomes shows a statistically significant effect for women. If anything, both bridging and bonding social capital decline. For men, the effects on bonding social capital are stronger: strong ties rise by 1.710 standard deviations ($p < 0.001$), family caregiving rises by 1.581 standard deviations ($p = 0.004$). Generalized trust falls by 0.724 standard deviations ($p = 0.044$).

This pattern reflects men's deeper dependence on the workplace as the organizing institution of their social lives. During employment, men's networks are built around work units and encompass organizational membership and the cross-group contact that underpins generalized trust (Comi et al., 2022). Retirement therefore removes a larger share of men's networks and triggers a correspondingly larger reallocation toward family ties. Women's portfolios, already more family- and community-oriented during working life (Cusimano et al., 2025), show no statistically detectable shift, consistent with a dual work-and-family structure that retirement does not visibly disrupt and echoing Patacchini and Engelhardt's (2016) evidence of gender-differentiated retirement effects.

Table 8. Heterogeneity by gender

Panel A:	Bridging SC	Bridging SC	Bridging SC	Bonding SC	Bonding SC
Female	Formal Part.	Gen. Trust	Neighbor Trust	Strong Ties	Family Caregiving
Retirement	-0.995 (0.642)	-0.725 (0.671)	-0.291 (0.661)	-0.977 (0.660)	-0.035 (0.620)
Bandwidth	7.021	7.125	6.482	6.564	7.092
Observations	1211	1217	1125	1105	1174
Controls	Yes	Yes	Yes	Yes	Yes
First-stage F	17.59	18.52	17.08	16.79	18.34
Panel B:	Bridging SC	Bridging SC	Bridging SC	Bonding SC	Bonding SC
Male	Formal Part.	Gen. Trust	Neighbor Trust	Strong Ties	Family Caregiving
Retirement	-0.378 (0.403)	-0.724** (0.423)	-0.048 (0.405)	1.710*** (0.440)	1.581*** (0.569)
Bandwidth	6.107	5.458	5.886	5.755	5.383
Observations	1008	862	947	925	854
Controls	Yes	Yes	Yes	Yes	Yes
First-stage F	32.47	31.42	33.07	29.83	27.69

Notes: Formal Part. = formal participation; Gen. Trust = generalized trust; Neighbor Trust = trust in neighbors; Family Caregiving = frequency of family caregiving. Same hereafter.

6.2. Education

Table 9 documents heterogeneity by education. The pattern is broadly similar to the pooled results for both groups: retirement is associated with lower bridging social capital and higher bonding social capital. There is some evidence, however, that the increase in bonding social capital is stronger among the more educated: strong ties rise by 0.888 standard deviations ($p = 0.013$) and family caregiving by 0.998 standard deviations ($p = 0.019$) for individuals with high school education or above, while neither effect reaches conventional significance among those with middle school education or below. A plausible explanation is that more educated workers are more intensely engaged in their jobs before retirement, so leaving work releases comparatively more time and attention that can then be redirected toward family relationships.

Table 9. Heterogeneity by education

Panel A:	Bridging SC	Bridging SC	Bridging SC	Bonding SC	Bonding SC
High School or Above	Formal Part.	Gen. Trust	Neighbor Trust	Strong Ties	Family Caregiving
Retirement	-0.315 (0.439)	-0.709 (0.445)	-0.417 (0.366)	0.888** (0.390)	0.998** (0.423)
Bandwidth	6.039	6.009	7.966	8.720	9.477
Observations	1161	1156	1469	1527	1648
Controls	Yes	Yes	Yes	Yes	Yes
First-stage F	28.29	29.36	38.05	40.91	42.62
Panel B:	Bridging SC	Bridging SC	Bridging SC	Bonding SC	Bonding SC
Middle School or Below	Formal Part.	Gen. Trust	Neighbor Trust	Strong Ties	Family Caregiving
Retirement	-0.932* (0.501)	-0.665 (0.557)	-0.001 (0.625)	0.270 (0.486)	0.965 (0.608)
Bandwidth	10.288	9.605	7.075	12.019	8.744
Observations	1364	1284	1012	1455	1166
Controls	Yes	Yes	Yes	Yes	Yes
First-stage F	29.01	27.17	20.74	32.99	24.32

7. Conclusions

This paper uses three waves of the CFPS and a fuzzy regression discontinuity design based on China's statutory retirement ages to identify the causal effect of retirement on social capital. The central finding is a systematic reallocation rather than a uniform change: bridging capital, formal participation and generalized trust, declines, while bonding capital, strong family ties and family caregiving, strengthens. This extends the European evidence of Comi et al. (2022) on network composition to a statutory, age-based retirement system, and the directional consistency of trust in neighbors with the other bridging outcomes, despite its insignificance on its own, is in line with the stability that Van Tilburg (2003) and Fletcher (2014) document for some dimensions of social capital.

The result maps directly onto Putnam's (2000) distinction between bridging and bonding social capital: retirement converts bridging into bonding capital, consistent with the trust-updating mechanism of Guiso et al. (2008). The welfare implications are not a private wash between two substitute forms of capital. Stronger family bonds provide valuable emotional and instrumental support in old age, but a decline in generalized trust, civic participation, and diverse contact makes retirees less open to, and less cooperative with, people outside their immediate circle, with consequences for public-goods provision and social cohesion that extend beyond the retiree's own household (Knack and Keefer, 1997; Putnam et al., 1993). The pattern parallels the finding of Geraci et al. (2022) that fast internet in the UK crowded out participation in bridging (Putnam-type) organizations while leaving bonding ties intact: in both cases, a shock to contact patterns moves the bridging and bonding components in opposite directions, suggesting a feature of social-capital adjustment.

The heterogeneity results reinforce this picture. The reallocation is concentrated among men, whose pre-retirement social capital was more workplace-centered, while women's already family- and community-oriented portfolios show no detectable shift.

Our results bear directly on the policy debate over postponing retirement. China began raising its statutory retirement ages in January 2025, and most ageing societies are moving in the same direction. The standard accounting of such reforms weighs fiscal savings against labor-market and health effects. Our estimates add a social margin to this calculus, and it cuts both ways. Later retirement keeps workers in the workplace, the environment that sustains formal participation and generalized trust, so postponement may slow the erosion of bridging capital that we document at the current thresholds. At the same time, later retirement postpones the release of time that retirees redirect to their families: the largest and most precisely estimated effect in this paper is the increase in caregiving for children, grandchildren, and parents. In a society where families remain a principal provider of childcare and old-age support, delaying retirement therefore also delays a sizable supply of informal care, and this delay will be felt most by the groups for whom the reallocation is largest, men and formal-sector employees near the statutory thresholds. A full welfare assessment of retirement-age reform should count both sides of this social ledger alongside the fiscal ones.

More broadly, our findings show that social capital, often portrayed as the slow-moving residue of history, responds quickly and systematically to a discrete individual event triggered by a policy rule. Retirement does not simply raise or lower social capital; it converts bridging capital into bonding capital. For the design of active-ageing policy, this reframes the question from how to prevent post-retirement decline to how to sustain retirees' connections beyond the family, for instance by keeping organizational memberships open to retirees rather than tying them to employment, while supporting the family caregiving that retirees evidently supply.

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