The Social Multiplier of Pension Reform

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Motivation

- In light of population aging, many countries are enacting **pension reforms**
- Retirement responses are crucial for fiscal and welfare impact of reforms
- Retirement decisions are made in **social context** (Atchley 1982)
  - Leisure complementarities
  - Complex, one-off decision $\rightarrow$ learn from others?
  - Social norms, e.g. ”normal” retirement age
- Potential implication: **social spillover** effects of pension reform
- So far, little evidence on the role of social networks for retirement (except spouses)

This Paper

Is individual retirement behavior influenced by social networks?

$\rightarrow$ What do retirement spillovers imply for the impact of pension reforms?

This Paper

- Estimate retirement spillovers in **families**, **neighborhoods**, and **workplaces**
- Setting: Netherlands
  - High-quality **admin data** on retirement behavior and social links of full population
  - Large-scale, **cohort-based pension reform** curbs early retirement
- Empirical approach: exploit exogenous variation in peers' behavior due to reform
  - Addresses econometric challenges in identifying peer effects (Manski 1993, Sacerdote 2014)
  - Main strategy: regression discontinuity design around cohort cutoff

Preview of Results

- **Significant retirement spillovers** between spouses, siblings, neighbors, and coworkers
- **Heterogeneity**:
  - Couples: large spillovers, mainly due to women reacting to husbands
  - Non-spousal peers: small average spillovers, but large between similar individuals
    $\rightarrow$ consistent with **homophily** in social interactions
- **Mechanisms**: evidence consistent with
  - Leisure complementarities
  - Social norms
- Policy implication: pension reform has **social multiplier** of
  - at least 1.40 on the average retirement age
  - at least 1.31 on fiscal effects

Literature

- **Individual** retirement behavior
  - Statutory retirement ages: Behaghel & Blau (2012), Staubli & Zweimüller (2013), Rabaté (2019), Deshpande et al. (2021), Seibold (2021), Gruber et al. (2022), Dolls & Krolage (2023), Lalive et al. (2023)
  - Dutch pension reforms: Lindeboom & Montizaan (2020), Rabaté et al. (2024)
- **Social** interactions in retirement behavior
  - Couples: Stancanelli & van Soest (2016), Lalive & Parrotta (2017), Selin (2017), Atalay et al. (2019), Bloemen et al. (2019), Johnsen et al. (2022), Nagore-Garcia & van Soest (2022), Garcia-Miralles & Leganza (2024)
  - Coworkers: Brown & Laschever (2012)
  - Spillovers onto younger workers & firms: Carta et al. (2021), Bianchi et al. (2023), Ferrari et al. (2023), Badalyan (2025)

Contributions:

1. Novel evidence of spillovers in key groups: siblings, neighbors, coworkers
2. Shed new light on mechanisms, in particular social norms
3. Quantify policy implications: social multiplier

Outline

I. Institutions and Data

II. Empirical Strategy

III. Main Results

IV. Mechanisms

V. Policy Implications

VI. Conclusion

Outline

The 2006 Dutch Pension Reform

- Employer-based pensions are an important pillar of Dutch pension system – large reform in 2006 ▸ details
- Reform package to curb early retirement via employer-based schemes:
  - Introduce actuarially fair benefit adjustment
  - Abolish special early retirement provisions, such as ”bridge” payments
  - Set minimum Early Retirement Age at 60
- Sharp cohort cutoff: only **January 1950 and younger** affected
- Causes discontinuous increase in average retirement age (Rabaté et al. 2024)

Note: NRA also gradually increases from 65 to 67 between 2012 and 2024 – but no change coincides with 1950 cutoff

Data

Use administrative data on the full Dutch population provided by Statistics Netherlands (CBS):

- Civil register data: family links, residential location
- Personal income tax data
- Employer-employee matched labor market data: firm, sector, monthly earnings

  $\rightarrow$ Merge across registers using unique personal ID

Period: 2003-2021

Main sample restrictions: cohorts 1946 to 1954, employed at least once after age 57

Sample size: 1,352,249

▸ Summary statistics

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Identifying Causal Retirement Spillovers

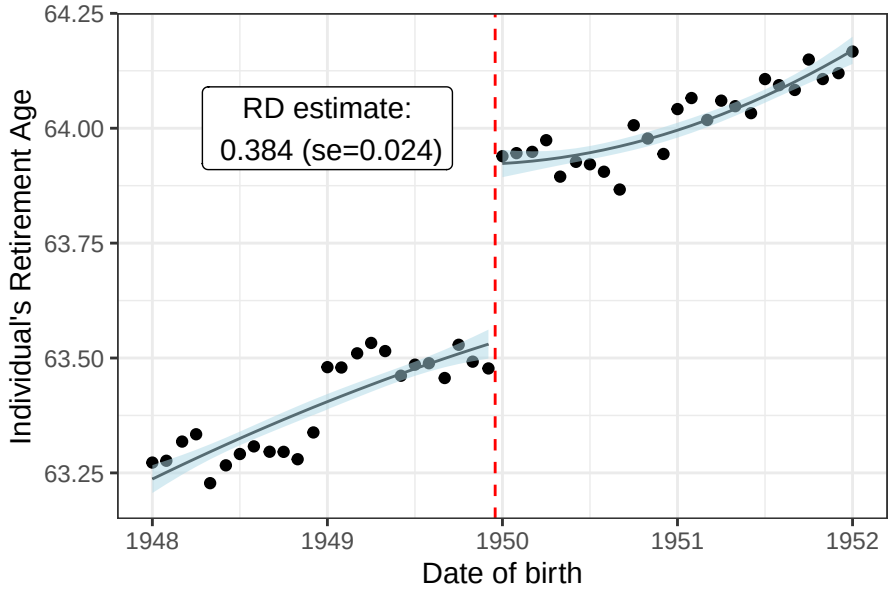
Approach: exploit **quasi-random variation** in peers' retirement due to pension reform

▸ identification problem

Two empirical strategies:

1. Two-stage regression discontinuity design exploiting reform exposure of a **single peer** (cf. Dahl et al. 2014)
   - First stage: estimate direct effect on peers' retirement age
   - Second stage: estimate spillover effect onto main individual
2. Instrumental variables approach leveraging exposure of **multiple peers** (later)

First Stage: Direct Effect of the 2006 Reform



extra

Regression Discontinuity Design (RDD): Spillover Effects

We estimate:

$$R_i = \beta_0 + \boldsymbol{\beta_1} \mathbb{1}(d_j \geq 1950) + h_l(d_j)\mathbb{1}(d_j < 1950) + h_r(d_j)\mathbb{1}(d_j \geq 1950) + e_i$$

where

- R_i: retirement age of **individual** i
- d_j: birth date of **peer** j
  - Spouse, sibling, neighbor, or coworker
- h_l, h_r: local functions of peer birth date
- β_1: RDD estimate of spillover effect

Identification assumptions: (1) no manipulation of birth dates ▸ full sample ▸ spouses ▸ siblings
(2) no sharp sorting by birth date around cutoff ▸ spouses ▸ siblings

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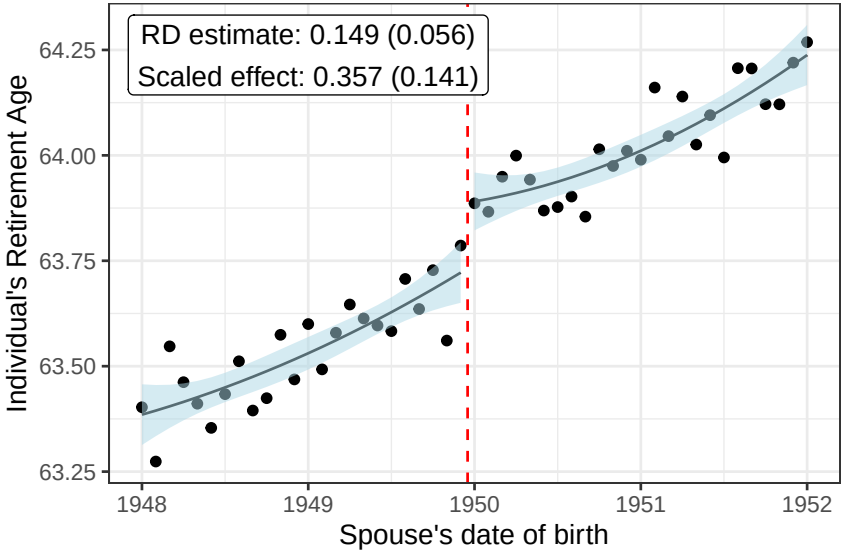
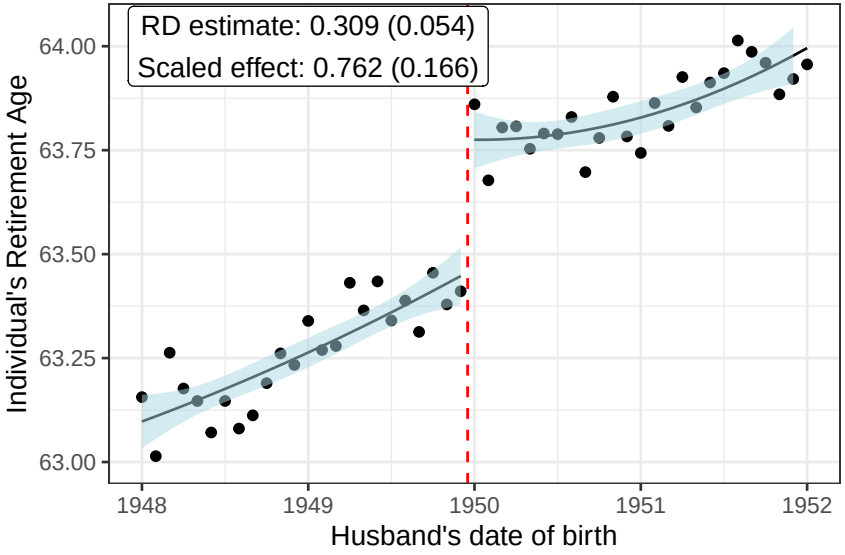
Spillovers in Couples



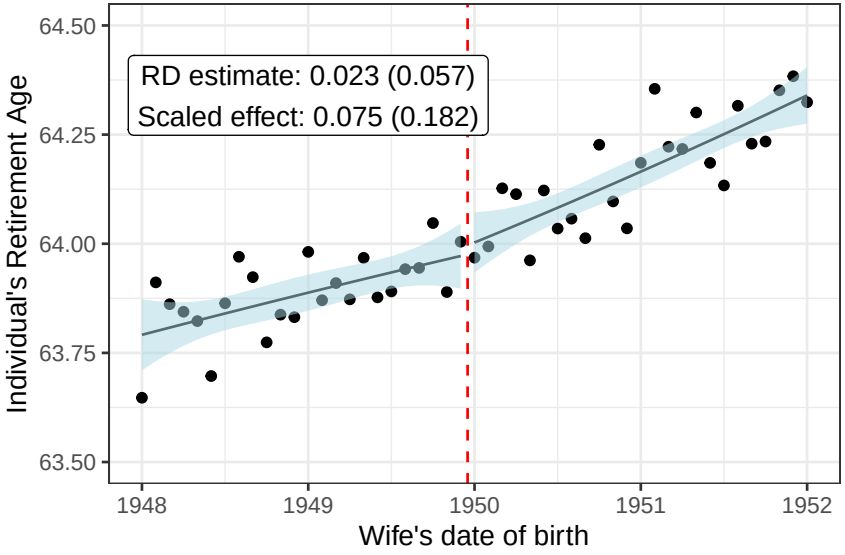
Spillovers in Couples

Effect on Women



Spillovers in Couples

Effect on Men

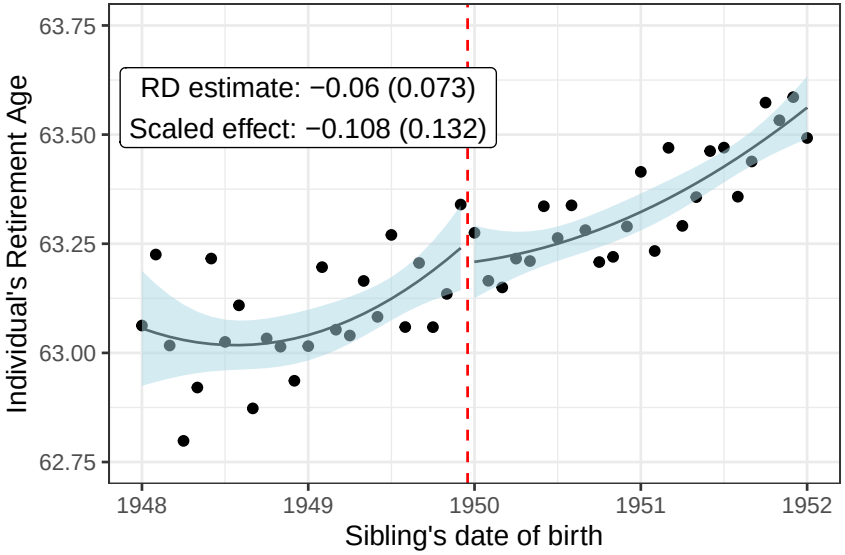


Spillovers in Couples

- Large retirement spillovers in couples
  - Average spillover $\approx$ 36% of direct reform effect
- Gender asymmetry persists after accounting for key confounders ▸ age ▸ earnings
- Findings in line with some prior studies (e.g. Johnsen et al. 2022, Garcia-Miralles & Leganza 2024)
- Couples provide a useful benchmark for other peer groups
  - Potential distinguishing feature: shared household budget constraint
    $\rightarrow$ empirically, not the main driver of spillovers (more on mechanisms later)

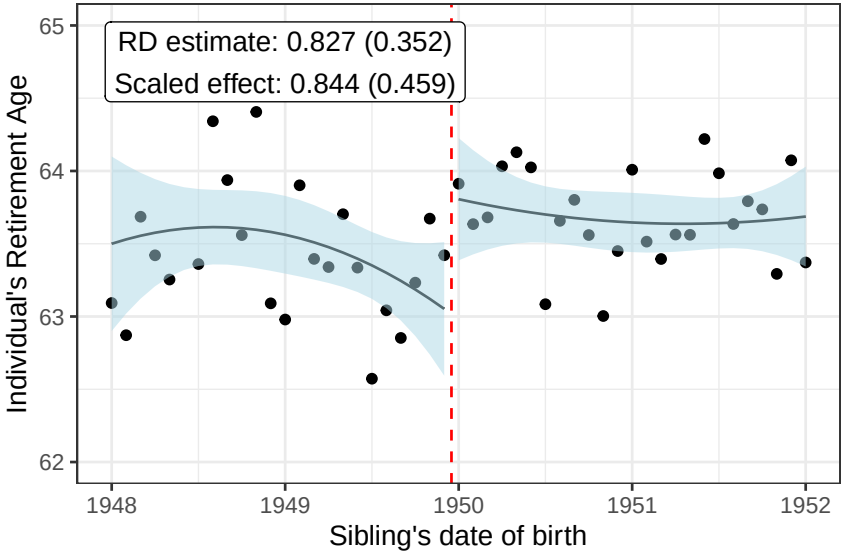
Spillovers between Siblings

All Siblings

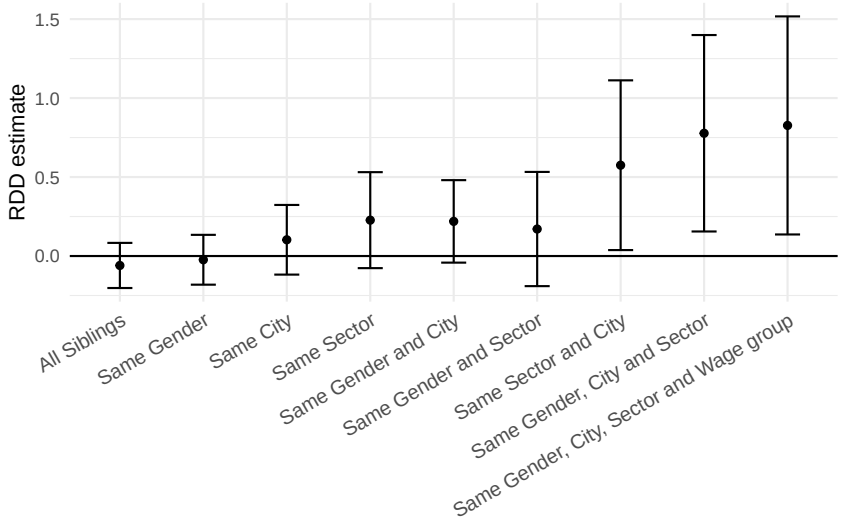


Spillovers between Siblings

Similar Siblings: Gender, City, Sector, Earnings



Siblings: Extended Results



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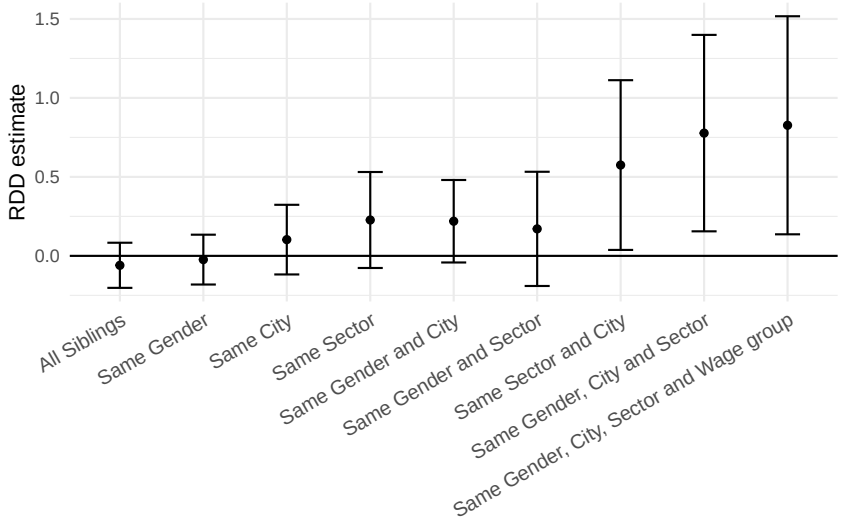


Peer influence increases with similarity
→ consistent with **homophily** in social interactions (McPherson et al. 2001)

scaled

Identification Checks and Robustness

- Density of running variable: no discontinuity in birth dates around 1950 cutoff
  ▸ full sample ▸ couples ▸ siblings
- Correlation between individual reform exposure and peer birth date: no sharp sorting
  ▸ couples ▸ siblings
- Placebo tests using artificial birth date cutoffs ▸ couples ▸ siblings
- RDD functional form: quadratic vs. linear ▸ link
- RDD bandwidth choice ▸ link

Estimating Spillovers between Neighbors and Coworkers

Issue: How to deal with **large groups**?

- Many potential peers in neighborhoods (median: 37) and firms (median: 53)

Two empirical approaches:

1. IV strategy: exploit full variation in neighbors'/coworkers' reform exposure
   $\rightarrow$ estimate **average** spillover effects
2. RDD: choose **similar** peers based on pre-determined observables
   - Neighbors: gender, migration status, earnings, sector of work ‣ result
   - Coworkers: gender, migration status, earnings, city of residence ‣ result

Instrumental Variable Approach

First stage (direct effect):

$$\overline{R}_{-i} = \phi_0 + \phi_1 P(d_{-i} \geq 1950) + X_i'\zeta + \nu_i$$

IV reduced form (spillover effect):

$$R_i = \delta_0 + \boldsymbol{\delta_1} P(d_{-i} \geq 1950) + X_i'\xi + u_i$$

where

- R_i: retirement age of **individual** i
- $\overline{R}_{-i}$: leave-out average retirement age of i's peers
- $P(d_{-i} \geq 1950)$: **fraction of peers** exposed to reform
- δ_1: reduced-form estimate of spillover effect

Identification assumption: no selection into groups based on fraction of exposed peers (conditional on observables X_i)

Spillovers between Neighbors: IV Approach

	(1)	(2)	(3)
	Main Results		Placebo
	First Stage: Neighbors' average retirement age	*Spillover:* Individual retirement age	*Spillover:* Individual retirement age
Neighbors' reform exposure (standardized)	0.114***	0.0066**	0.003
	(0.004)	(0.0031)	(0.003)
Observations	1,314,479	1,314,479	1,310,599
R-squared	0.267	0.060	0.060
F-statistic (first stage)	906.37		

Controls: 4-digit postcode FE, gender $\times$ month-by-year of birth FE. Standard errors clustered by 5-digit postcode in parantheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

$\rightarrow$ Scaled spillover effect: 0.059** (0.028)

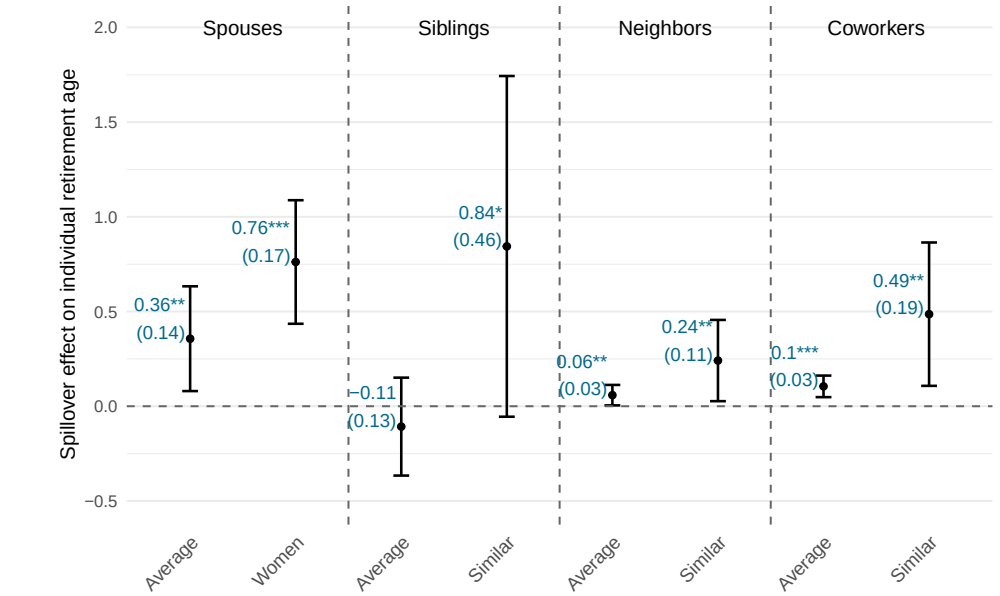
Spillovers between Coworkers: IV Approach

	(1)	(2)	(3)
	Main Results		Placebo
	First Stage: Coworkers' average retirement age	*Spillover:* Individual retirement age	*Spillover:* Individual retirement age
Coworkers' reform exposure (standardized)	0.212***	0.0222***	0.004
	(0.006)	(0.0061)	(0.005)
Observations	594,202	594,202	537,244
R-squared	0.178	0.078	0.079
F-statistic (first stage)	1262.10		

Controls: Sector FE, gender $\times$ month-by-year of birth FE. Standard errors clustered by firm in parantheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

$\rightarrow$ Scaled spillover effect: 0.105*** (0.029)

Main Results: Taking Stock



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Evidence on Mechanisms

1. **Leisure complementarity**
   - Empirical signature: retire at same time (e.g. Garcia-Miralles & Leganza, 2024)
   - **Result**: Joint retirements can explain some spillovers, especially in couples link
2. **Social norms**
   - Many workers retire exactly at *Normal Retirement Age (NRA)*, despite no financial incentive (Seibold, 2021)
   - **Result**: This norm-related behavior is propagated via social spillovers link

Evidence on Mechanisms

1. **Leisure complementarity**
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Other Potential Mechanisms

- **Information spillovers**
  - Peers may pass on information about post-reform retirement rules
  - **Result**: Spillover effects *not* driven by individuals subject to new rules ‣ link
- Couples: shared **household budget constraint**
  - Spousal labor supply may react to household pension wealth effect of reform
  - **Result**: Spillovers do *not* increase with impact on spouse's pension wealth ‣ link
- Coworkers: **labor demand side**
  - Firm labor demand may react to reform (Bianchi et al. 2023)
  - **Results**: ‣ link
    - Spillovers always positive, if anything *larger* for more substitutable workers
    - Strongest spillovers in small firms $\rightarrow$ interactions, not firm policies

Outline

Policy Implications

Key implication: social spillovers amplify impact of pension reform on retirement

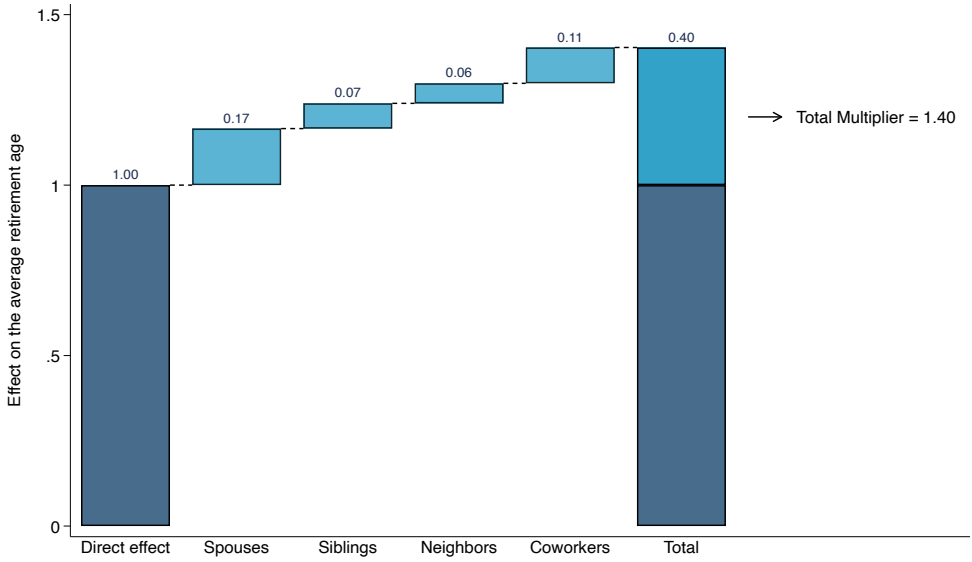
Calculate **social multiplier**:

$$M = 1 + \sum_k \overline{N}_{-i,k} \sum_{\theta \in \Theta_k} p_\theta \mu_\theta$$

where $\overline{N}_{-i,k}$: average number of peers in group k,
p_θ: fraction of type θ,
μ_θ: estimated spillover effect.

- Total effect of reform = $M \times$ direct effect (cf. Glaeser et al. 2003)
- Calculation likely yields lower bound
  - Additional unobserved peer groups
  - Cascading effects

Social Multiplier of Pension Reform



Social Multiplier of Pension Reform

	(1)	(2)	(3)	(4)	(5)	(6)
	Direct	Group-specific multiplier				Total
	effect	Spouses	Siblings	Neighbors	Coworkers	multiplier
Multiplier on:						
Average retirement age	1.000	0.166	0.074	0.059	0.105	1.404
Total earnings	1.000	0.083	0.069	0.060	0.105	1.317
Total fiscal impact	1.000	0.075	0.068	0.060	0.105	1.308

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Conclusion

- We document retirement spillovers in families, neighborhoods and workplaces
- Policy implication: spillovers amplify overall impact of pension reforms
- Find **social multiplier** of at least 1.40 on average retirement age, 1.31 on fiscal effects
  - Can substantially alter welfare impact of pension reforms (Hendren & Sprung-Keyser 2020, Kolsrud et al. 2024)
- Future work:
  - Data-driven exploration of heterogeneity/determinants of spillovers

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Appendix

The Dutch Pension System

Three pillars:

1. Public pay-as-you-go pensions $\rightarrow$ median replacement rate (RR) $\approx$ 39%
   - Pension benefits depend only on years of residence in the country
   - Automatic claiming at the Normal Retirement Age (NRA)
2. Employer-based, funded pension schemes $\rightarrow$ median RR $\approx$ 29%
   - Organized at sector level
   - Benefits depend on contributions (made by employee & employer)
   - Can claim from sectoral Early Retirement Age (ERA)
3. Subsidized private retirement savings (small)

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Summary Statistics

	(1) Mean	(2) S.D.	(3) Median
Female	0.39	0.49	0
Married or civil partnership	0.67	0.47	1
Dutch-born	0.86	0.35	1
Annual labor income (at age 57)	35,899	43,052	31,086
Year of birth	1950.61	2.62	1950.67
Birth date $\geq$ January 1950	0.56	0.50	1
Retirement age	63.80	3.14	64.42
Retiring at NRA	0.17	0.38	0
Retiring before NRA	0.60	0.49	1
Number of siblings	0.79	0.88	1
Siblings' year of birth	1950.12	1.49	1950.17
Siblings' retirement age	63.85	2.77	64.08
Number of neighbors	40.16	22.07	37.00
Neighbors' year of birth	1950.07	0.40	1950.06
Neighbors' retirement age	63.76	0.71	63.74
Number of coworkers	335.73	904.37	53.00
Coworkers' year of birth	1950.27	0.97	1950.22
Coworkers' retirement age	64.02	1.18	64.04
Observations	1,352,249		

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Estimating Causal Retirement Spillovers

- Standard model of peer effects (linear-in-means) :

$$R_i = \alpha + \beta \overline{R}_{-i} + X_i' \gamma_1 + \overline{X}_{-i}' \gamma_2 + \epsilon_i$$

  where R_i: retirement age of individual i
  $\overline{R}_{-i}$: peers' leave-out average retirement age
  X_i, $\overline{X}_{-i}$: background characteristics

- Goal: identify causal effect of peer behavior on own behavior β
- Central problem (Manski 1993, Sacerdote 2014): "correlated effects"
  - Unobserved individual characteristics correlated with peer behavior or peer characteristics
  - Key reason: endogenous selection into peer groups
  - In addition, simultaneity issue ("reflection problem")

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Regression Discontinuity Design (RDD): First Stage

We estimate:

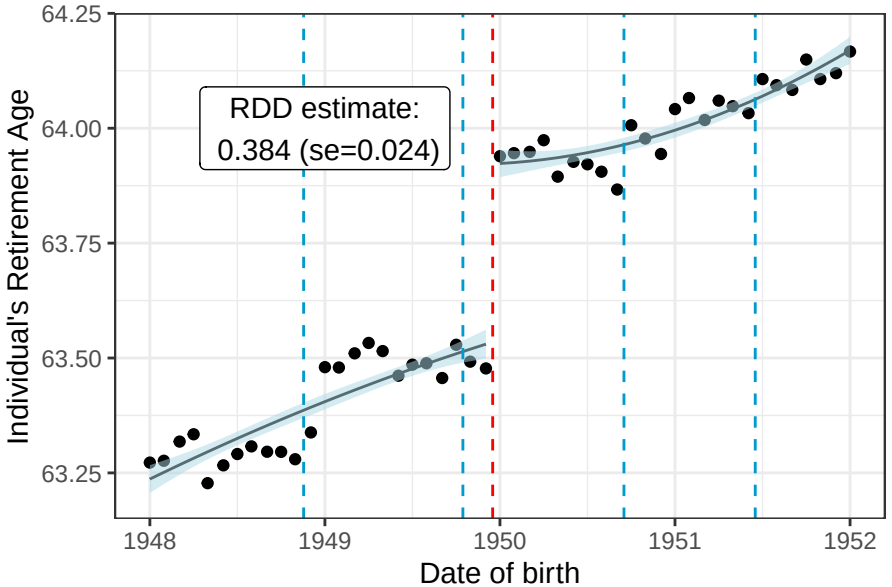
$$R_j = \alpha_0 + \boldsymbol{\alpha_1} \mathbb{1}(d_j \geq 1950) + f_l(d_j)\mathbb{1}(d_j < 1950) + f_r(d_j)\mathbb{1}(d_j \geq 1950) + \epsilon_j$$

where

- R_j: retirement age of individual j
- d_j: j's own birth date
- f_l, f_r: local functions of own birth date
- α_1: RDD estimate of direct reform effect

Identification assumption: no manipulation of birth dates around 1950 cutoff back

First Stage: Direct Effect of the 2006 Reform



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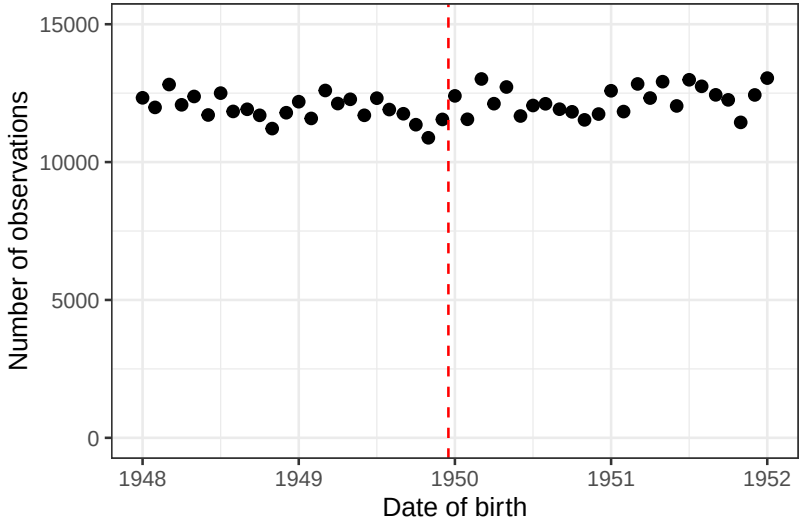
Distribution of Birth Dates: Full Sample



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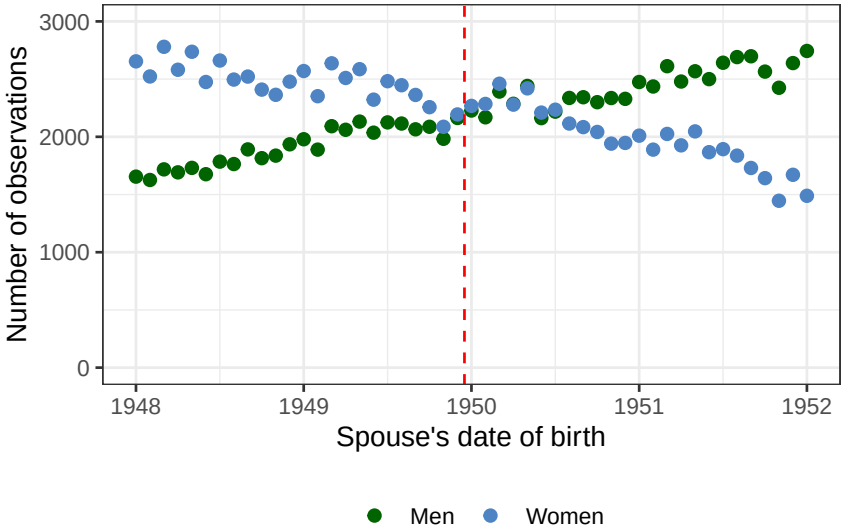
Distribution of Birth Dates: Couples



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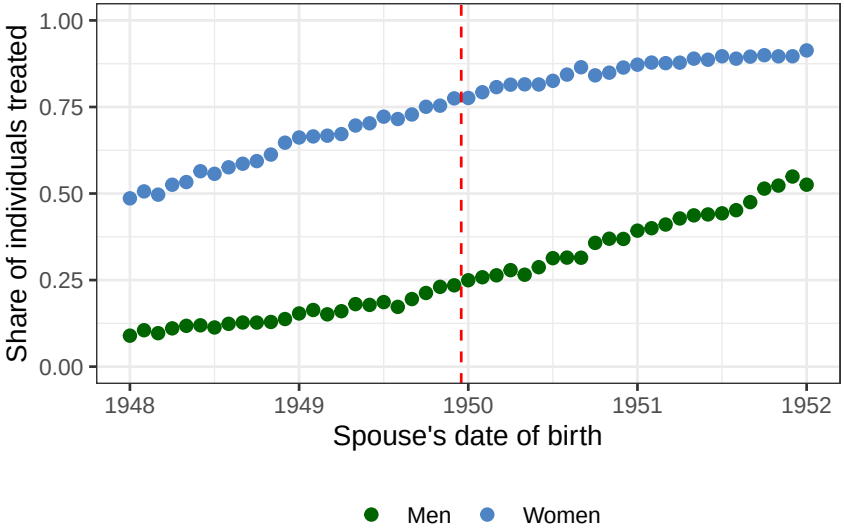
Reform Exposure Correlation: Couples



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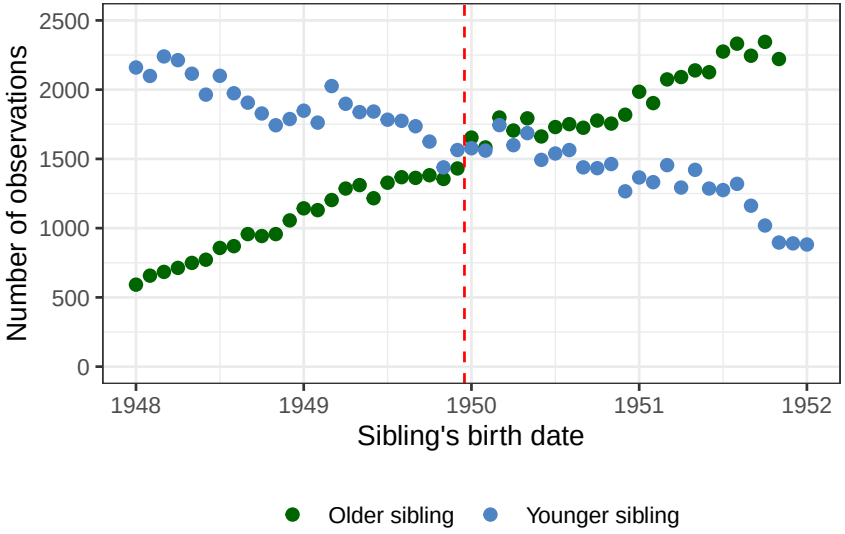
Distribution of Birth Dates: Siblings



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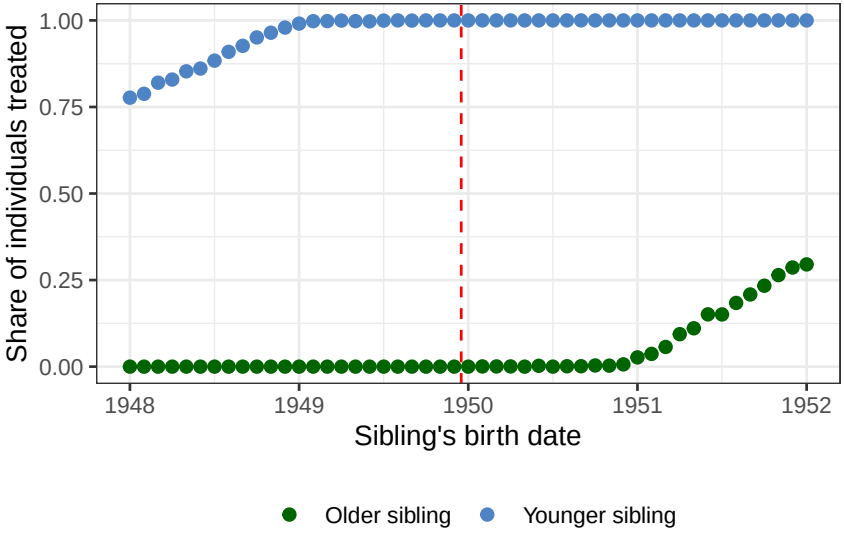
Reform Exposure Correlation: Siblings



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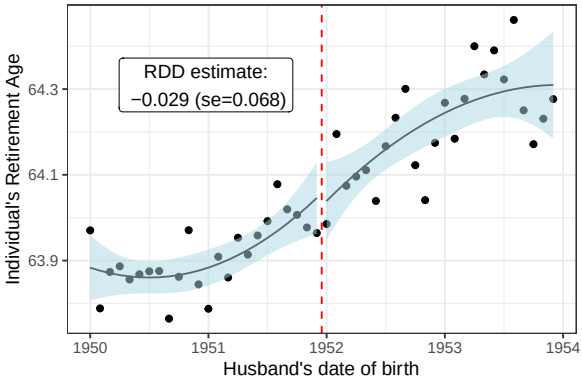
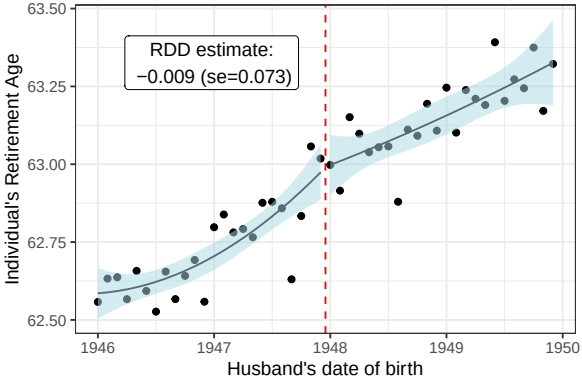
Placebo Results: Couples

(a) 1952 Placebo



(b) 1948 Placebo



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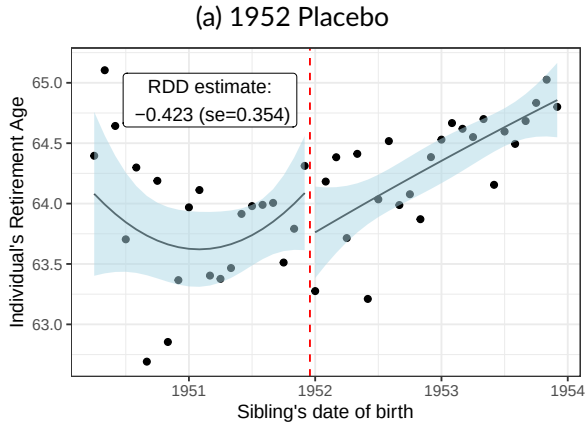
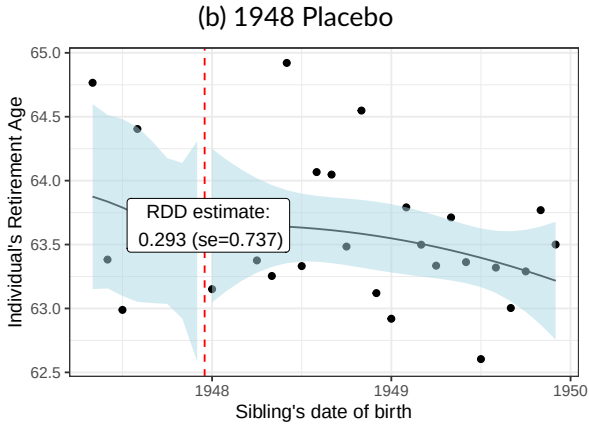
Placebo Results: Siblings





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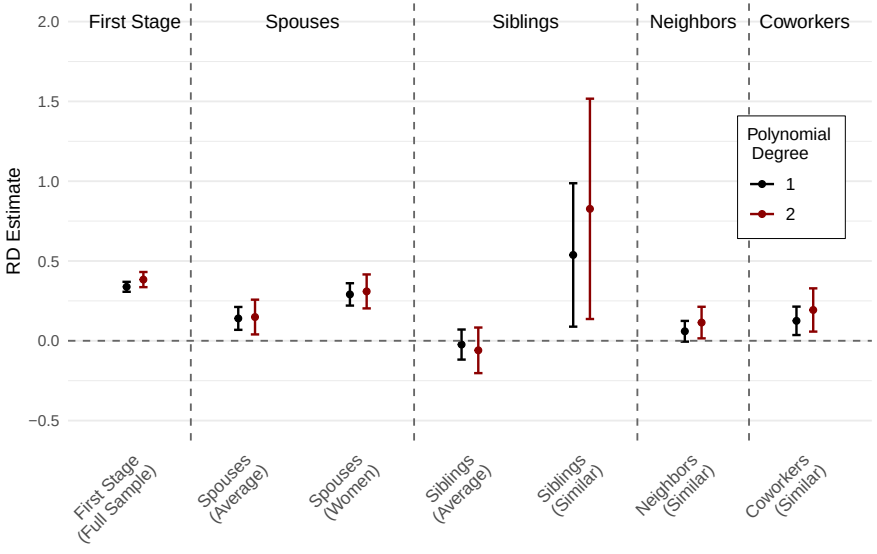
Robustness: Polynomial Degree



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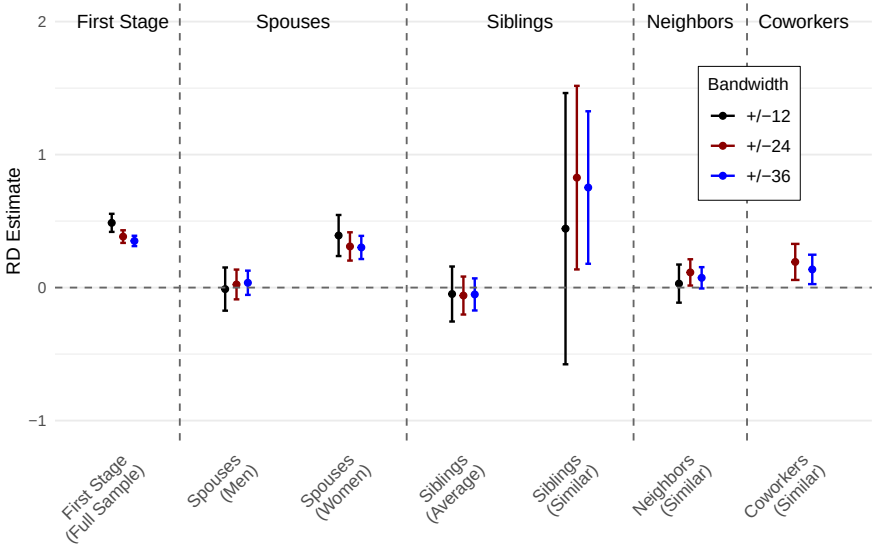
Robustness: Bandwidth



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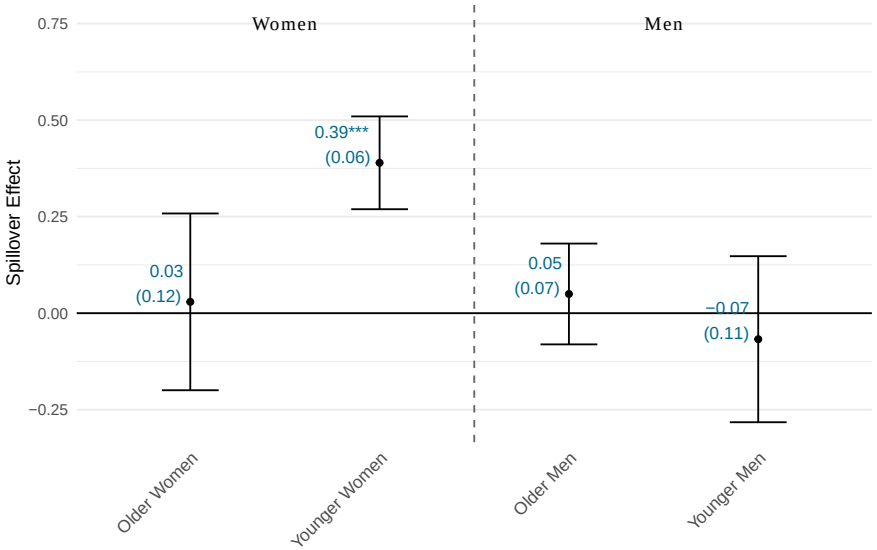
Couples: Results by Age Difference



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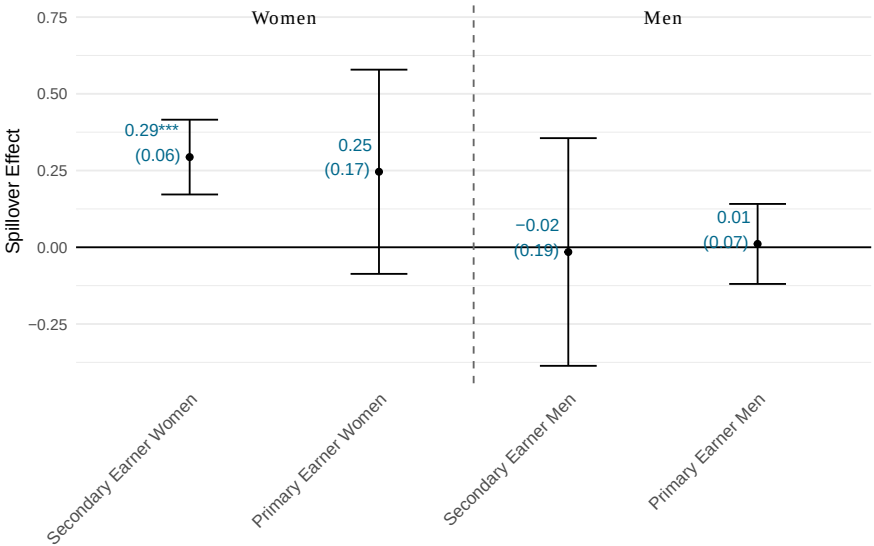
Couples: Results by Relative Earnings



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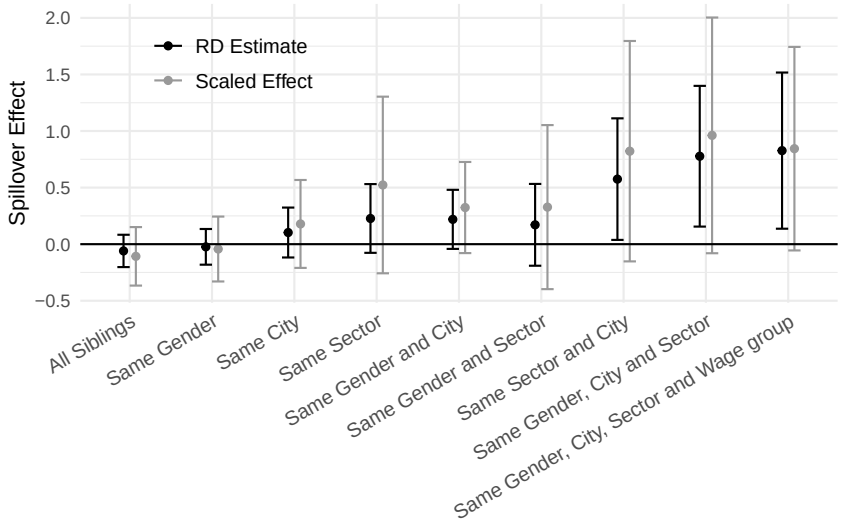
Siblings: Extended Results



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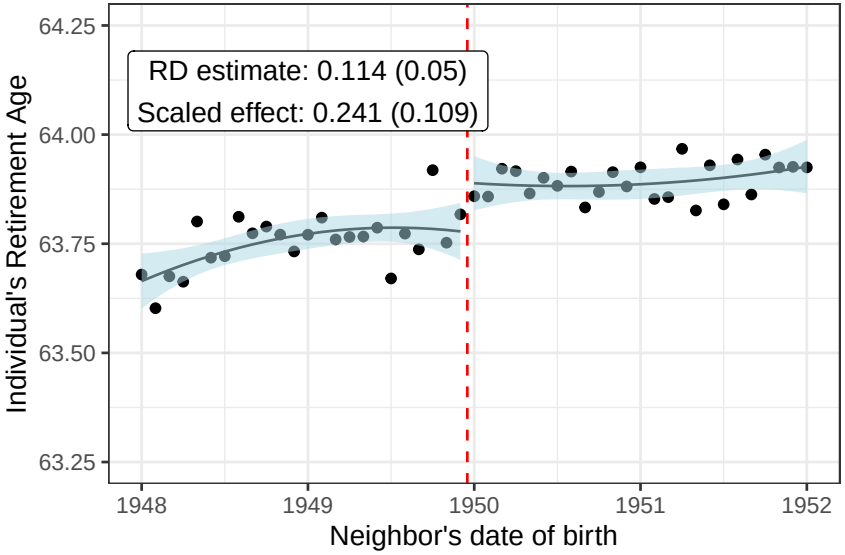
Spillovers between Neighbors

RDD: Similar Neighbors



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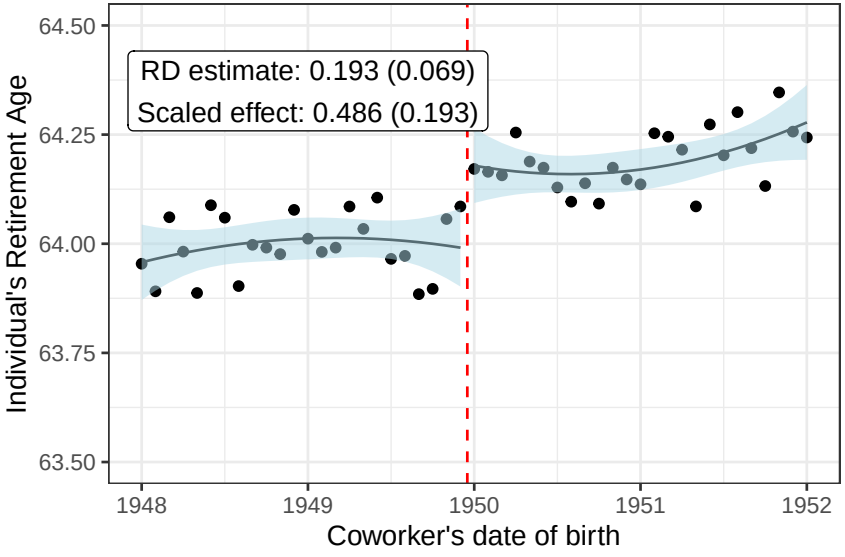
Spillovers between Coworkers

RDD: Similar Coworkers



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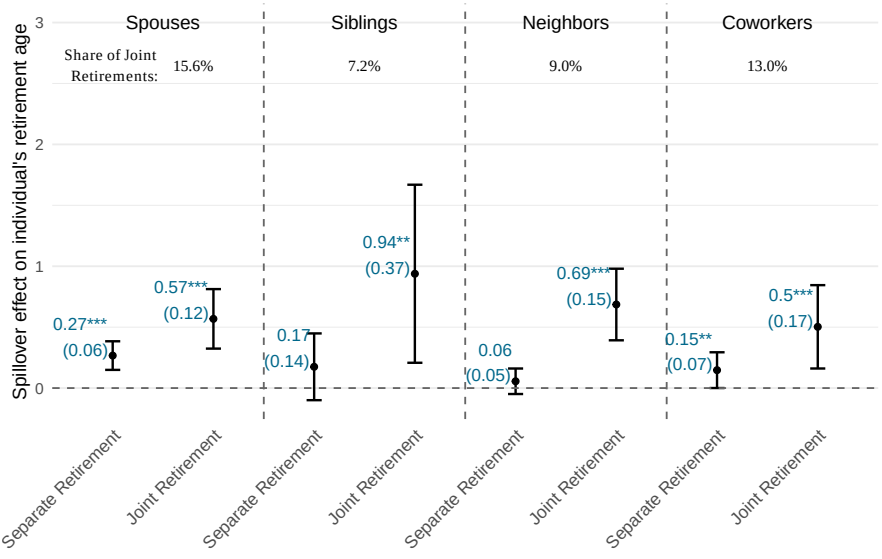
The Role of Joint Retirement



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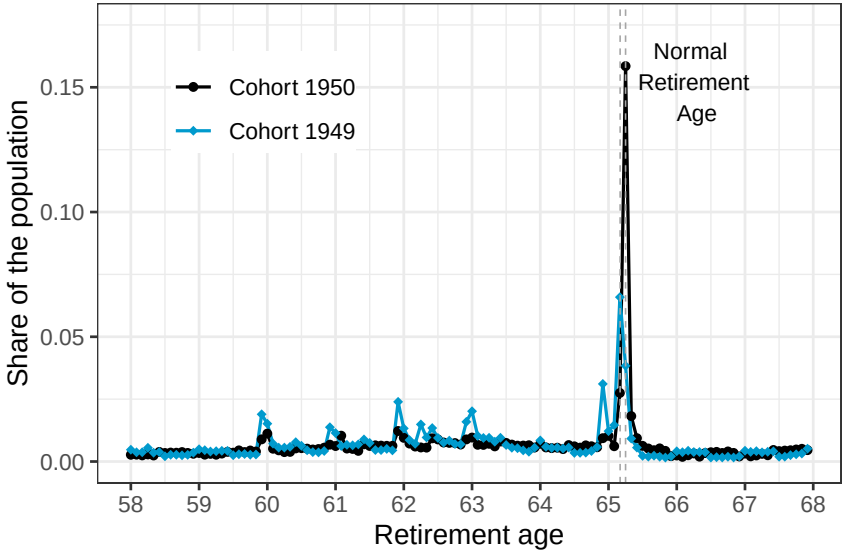
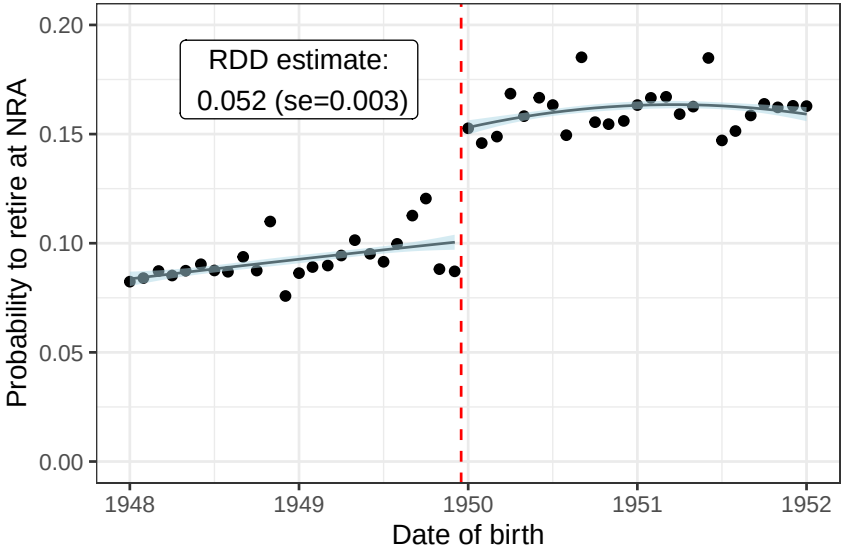
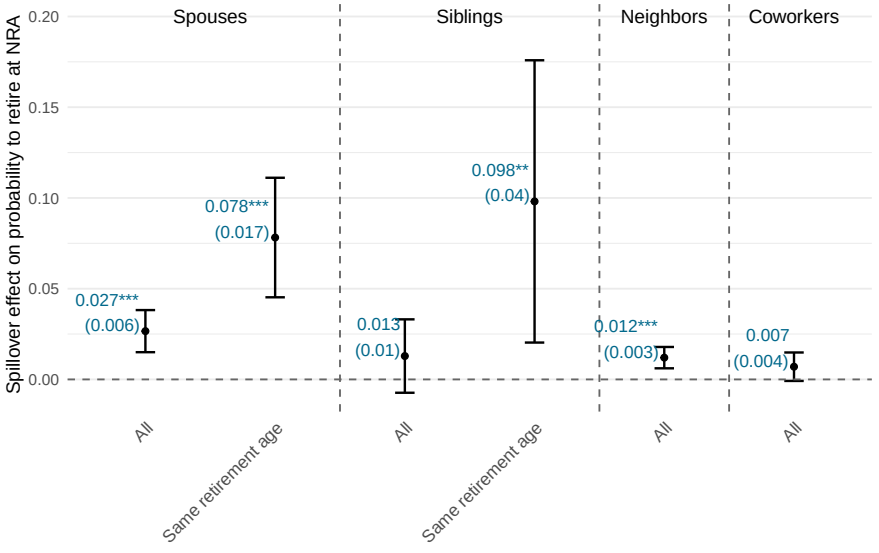
The 2006 Reform and Bunching at the NRA



Bunching at the NRA: First Stage



Bunching at the NRA: Spillover Effects



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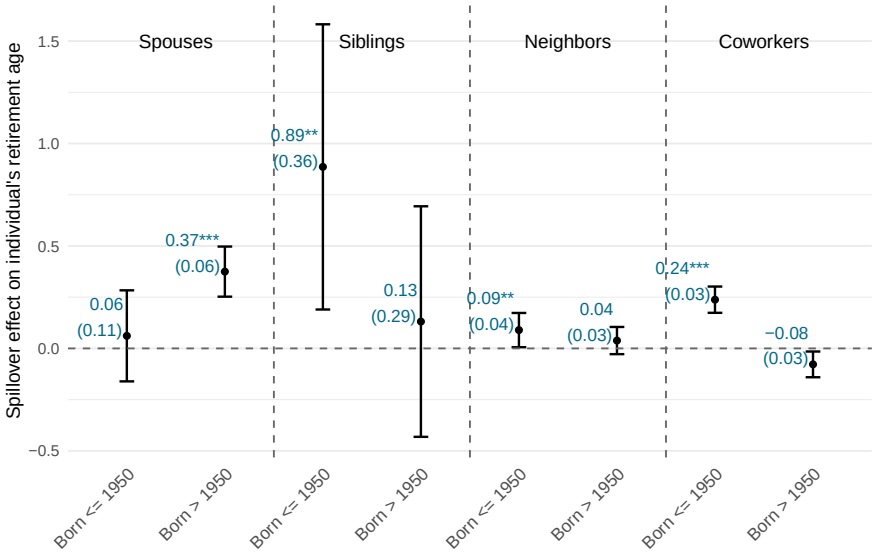
Spillover Effects by Own Reform Exposure



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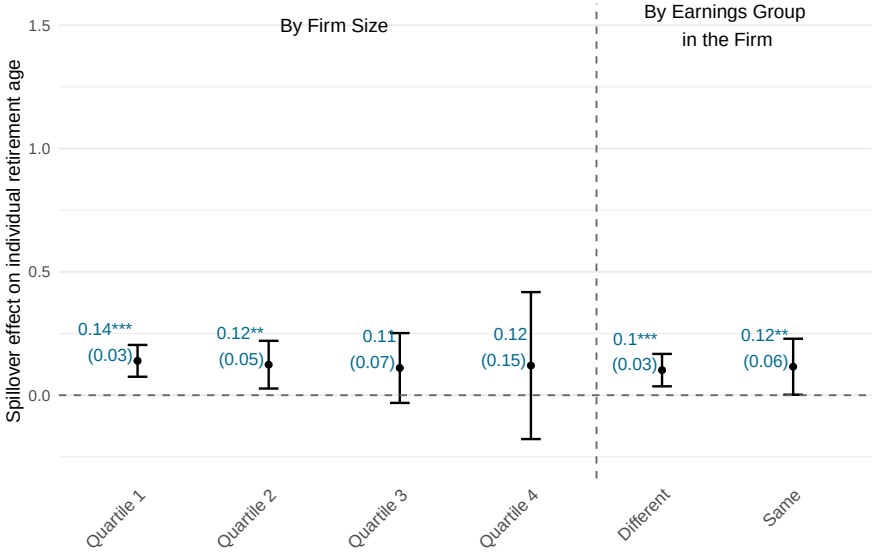
Evidence on the Role of Firms



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