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The Social Multiplier of Pension Reform

Abstract

We study the influence of family members, neighbors and coworkers on retirement behavior. To estimate causal retirement spillovers between individuals, we exploit a pension reform in the Netherlands that creates exogenous variation in peers' retirement ages, and we use administrative data on the full Dutch population. We find large spillovers in couples, primarily due to women reacting to their husband's retirement choices. Consistent with homophily in social interactions, the influence of the average sibling, neighbor and coworker is modest, but sizable spillovers emerge between similar individuals in these groups. Additional evidence suggests both leisure complementarities and the transmission of social norms as mechanisms behind retirement spillovers. Our findings imply that pension reforms have a large social multiplier, amplifying their overall impact on retirement behavior by 40%.

JEL-Codes: D910, H550, J260.

Keywords: retirement, pension reform, social networks, spillover, peer effects.

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1 Introduction

Population aging challenges the fiscal sustainability of public pension systems across the developed world. In OECD countries, the old-age dependency ratio is predicted to more than double over the next 50 years (OECD, 2023). To adapt social security systems to these demographic trends, many governments are enacting pension reforms with the goal of incentivizing later retirement. How individual retirement behavior responds to reforms is crucial for the fiscal balance of pension systems and their welfare impact.

In this paper, we investigate the role of social networks for retirement behavior. The view that social context matters for retirement decisions has long been prevalent in social sciences.[1] Conceptually, there are several reasons why one may expect workers' retirement behavior to be influenced by others around them. First, there could be leisure complementarities, such that individuals wish to retire together with their peers. Second, retirement is a complex, one-off decision where individuals may learn from others' choices. Third, social norms regarding "normal" retirement ages might play a role. These types of spillovers carry an important policy implication: Pension reforms can have a *social multiplier*, amplifying their impact on overall retirement behavior. Yet, so far there is little systematic quantitative evidence on social interactions in retirement, and what these imply for the effectiveness of pension reforms.[2]

We aim at filling this gap in the literature and study to what extent individual retirement behavior is influenced by others' choices in the context of a Dutch pension reform. We uncover significant retirement spillovers within families, neighborhoods, and workplaces. Additional evidence suggests that spillovers are driven both by leisure complementarities and by the transmission of social norms. Our findings imply a substantial social multiplier of pension reforms. Taken together, estimated spillover effects exacerbate the impact of a reform on the average retirement age by 40%, relative to its direct effect on individual behavior.

Quantifying causal retirement spillovers between individuals is challenging for two reasons. First, there are well-known econometric issues in identifying such peer effects, including correlated unobservables, endogenous group membership, and reflection problems (Manski, 1993; Sacerdote, 2014). Second, data requirements are very demanding: one needs information not only on retirement behavior, but also on the social linkages between individuals.

Our setting and data allow us to overcome these challenges. We exploit a pension reform in the Netherlands that generates large-scale, exogenous variation in peers' retirement behavior. The 2006 reform made early retirement via employer-based pension schemes much less financially attractive. The rule changes were implemented with a sharp cohort cutoff, affecting only workers born in January 1950 or later. The reform causes a large, discontinuous increase in average retirement ages of around five months among directly affected individuals. While many other pension

[1]For instance, classic sociological work by Atchley (1982) examines the social processes shaping retirement behavior over the course of modern history and ultimately characterizes retirement as a *social institution*.

[2]An exception is given by a number of studies examining interactions in the retirement behavior of spouses (see below).

reforms tend to be implemented more slowly and gradually, this sharp, immediate variation makes the Dutch setting ideally suited for our purposes.

We use high-quality administrative data provided by Statistics Netherlands (CBS), covering the universe of Dutch residents. Crucially, this data allows us to observe individuals' social linkages within families, neighborhoods, and workplaces. We merge various administrative registers at the individual level, including civil registration data, employer-employee matched labor market data, and income tax data. Taken together, the dataset we assemble contains detailed information on the retirement behavior of all workers linked to their spouses, siblings, neighbors and coworkers.

Our analysis relies on two complementary empirical strategies, both of which exploit exogenous variation in peers' retirement behavior around the cohort cutoff of the 2006 reform. The first strategy is a two-stage regression discontinuity design (RDD) similar to Dahl et al. (2014). The first stage is given by the direct impact of the reform on peers' retirement age. In the second stage, we estimate the causal spillover effect of peers' reform exposure onto the retirement age of other individuals. As a second empirical strategy, we use an instrumental variable (IV) approach exploiting variation in the fraction of an individual's peers treated by the reform. While this strategy relies on a somewhat stronger identification assumption than the RDD, it offers the advantage of utilizing the full variation in the reform exposure of peer groups. This is particularly useful for large groups, such as neighbor and coworker networks, where the IV approach provides substantially greater statistical power.

We begin by documenting retirement spillovers in couples. We find strong spillover effects of spouses' reform exposure. These are primarily concentrated among women who increase their retirement ages by 9.1 months per year of later retirement of their husband. Conversely, men exhibit a smaller and insignificant reaction to their wife's reform exposure. Further analyses reveal that this gender difference in spillover effects is neither driven by relative earnings nor by age differences within couples. Moving on to another type of family member, we examine siblings. On average, the impact of a sibling's reform exposure on individuals' retirement behavior is close to zero. However, significant spillovers emerge among "similar" sibling pairs who share socioeconomic characteristics such as gender, city of residence, sector of work, and pre-retirement earnings levels. For these sub-samples, we find spillover effects of up to 10.1 months.

Next, we investigate retirement spillovers among neighbors. When considering variation in the treatment exposure of all neighbors, we find small but statistically significant spillovers of 0.7 months per year of peers' later retirement. However, larger spillovers of up to 2.9 months occur when a neighbor with similar socioeonomic characteristics retires later. Our analysis of coworkers reveals analogous patterns. On average, spillover effects among all coworkers are modest at 1.0 months. But among the most similar coworkers, we find that individual's retirement age reacts by up to 5.9 months to a one-year retirement delay of the peer.

Hence, our main results indicate quantitatively important retirement spillovers between individuals and their closest peers. According to our estimates, spouses and similar siblings exert the largest influence on retirement decisions, followed by coworkers and neighbors. One particularly interesting pattern our analysis uncovers is that spillover effects between siblings, neighbors and coworkers increase strongly with the degree of similarity within pairs of individuals. This is consistent with the notion of *homophily* in social networks, that is the tendency of individuals to interact and form social ties with others who are similar to them (McPherson et al., 2001).

These estimates of total spillover effects are relevant from a policy perspective, as they govern the overall behavioral and fiscal externalities of pension reforms. Nevertheless, we also explore the mechanisms underlying retirement peer effects. First, we argue that information spillovers cannot explain our results. In principle, exposed peers' behavior could contain information about the post-reform retirement rules. However, we find that spillover effects are not driven by individuals for whom this information would be valuable. Instead, we show that leisure complementarities matter, especially for spouses. Spillover effects are particularly large among pairs who retire around the same time. But since the fraction of joint retirements is limited, leisure complementarities cannot account for the majority of spillovers.

Moreover, we provide evidence in favor of the transmission of social norms. In the Netherlands, there is a large concentration of retirements at the Normal Retirement Age (NRA), even though there is no financial incentive to retire at this age. Prior literature argues that this type behavior can be explained by the widespread perception of the NRA as a "normal" time to retire (Seibold, 2021). We first show that the 2006 reform increases the probability of retiring exactly at the NRA, as it pushes workers towards later retirement ages. We then demonstrate that this norm-related behavior is propagated via social spillovers: individuals become more likely to retire exactly at the NRA if their spouse, sibling, neighbor or coworker is induced to retire at the NRA by the reform.

Finally, we quantify the policy implications of our results. To calculate the social multiplier of pension reform, we aggregate the estimated spillover effects across all peer groups. We find a large social multiplier of 1.40. In other words, when a pension reform increases the average retirement age of a population by one year through its direct effect, social spillovers amplify its impact by an additional 0.40 years. This implies that individual-level analyses significantly underestimate the behavioral and fiscal effects of pension reforms. Ultimately, the social multiplier can substantially alter the welfare impact of pension reforms, for which total fiscal externalities arising from behavioral responses are a key factor (Kolsrud et al., 2024).

This paper contributes to a large literature on retirement behavior and pension reforms. Much of this literature focuses on individual retirement decisions and how they respond to pension policies, including financial retirement incentives (Brown, 2013; Manoli and Weber, 2016; Duggan et al., 2023) and statutory retirement ages (Behaghel and Blau, 2012; Staubli and Zweimüller, 2013; Rabaté, 2019; Deshpande et al., 2021; Seibold, 2021; Gruber et al., 2022; Dolls and Krolage, 2023; Lalive et al., 2023). The individual-level impact of Dutch pension reforms is analyzed by Lindeboom and Montizaan (2020) and Rabaté et al. (2023), among others. In contrast, less is known about social interactions in retirement behavior. Exceptions include several papers studying retirement spillovers between spouses (e.g. Johnsen et al., 2022; Garcia-Miralles and Leganza, 2024),[3] and Brown and Laschever (2012) who investigate retirement peer effects among a sample of Los Angeles high-school teachers.

We make several contributions to this literature. First, we provide novel evidence on retirement interactions across a comprehensive set of peer groups. Notably, this includes first-time evidence of retirement spillovers between siblings and neighbors, and our findings on coworkers constitute the first large-scale findings of retirement spillovers in workplaces. Second, we document the important role of homophily in shaping the magnitude of social spillovers within these groups, which has not received much attention in the literature so far. Third, our analysis sheds new light on the mechanisms behind retirement peer effects. In particular, we extend the recent literature on the effects of statutory retirement ages by demonstrating that behavioral responses to these ages are propagated through social norms. Fourth, we quantify the policy implications of social interactions in retirement behavior. Our social multiplier calculations suggest that the fiscal and welfare effects of pension reforms can be significantly altered by spillovers between individuals. To our knowledge, this factor is not taken into account in existing welfare analyses of retirement policies.

The remainder of this paper is organized as follows. Section 2 outlines context and data, Section 3 describes our empirical strategy, Section 4 presents the main results, Section 5 examines mechanisms behind spillover effects, Section 6 discusses policy implications, and finally Section 7 concludes.

2 Context and Data

2.1 The Dutch Pension System

The Dutch pension system consists of three pillars. The first pillar is given by public pay-as-you-go pensions, which are financed by social insurance contributions. Public pension benefits depend only on how long a person has been resident in the Netherlands. Individuals accumulate two percent of the full benefit amount per year of residence, up to the full amount which is paid to those with at least 50 years of residence. The full benefit amount is tied to the national monthly minimum wage ("social minimum").[4] Public pensions are automatically claimed at the Normal Retirement Age (NRA, or *AOW* age in Dutch). The NRA was set at 65 for decades, but has been increased to 67 in recent years. It is worth noting that many employment contracts end automatically at the NRA, such that firms and workers have to actively renew the contract in order to continue employment.

The second pillar are employer-based pension schemes. Participation in these schemes is mandatory for all employees. Most second-pillar plans are fully funded and organized at the

[3]Joint spousal retirement and its response to pension reforms is also studied by Zweimüller et al. (1996), Lalive and Parotta (2017), Selin (2017), Atalay et al. (2019) and Bloemen et al. (2019).

[4]A retired single individual receives 70% of the monthly minimum wage, whereas couples jointly receive 100% of the monthly minimum wage. The monthly minimum is set by the government and updated twice per year. As of July 2023, the monthly minimum wage is EUR 1995.

level of sectors, except a few cases where firms have their own schemes. Contribution payments are made by workers and firms, and benefit amounts are directly linked to contributions. Second-pillar pensions can be claimed from an Early Retirement Age (ERA), with some actuarial adjustment of monthly benefit amounts. Specific benefit and retirement rules, including the ERA, can differ across sectors. Employer-based pensions are an important component of old-age income in the Netherlands. The median individual receives a replacement rate from second-pillar pensions of 29% in addition to a replacement rate of 39% from public pensions (Knoef et al., 2017).

The third pillar of old-age support is given by voluntary private pension plans. Contributions to individual retirement savings accounts are encouraged by the government via a tax exemption of up to 1.875% of monthly income. Partly due to the well-established first- and second-pillar schemes, private pension savings play a less important role, contributing about 5% of overall pension income on average (Lindeboom and Montizaan, 2020).

2.2 The 2006 Reform

In the early 2000s, there were growing concerns about the sustainability of the Dutch pension system. At the time, early retirement was the norm in the Netherlands: around 80% of workers retired at the age of 62 or younger (Lindeboom and Montizaan, 2020). Early retirement was largely facilitated by employer-based pensions, which provide the only source of pension income before the NRA for most workers. These concerns stoked a major reform to second-pillar pension rules in 2006. The main goal of the 2006 reform was to incentivize later retirement by making existing early retirement provisions less generous. The reform thus comprised a bundle of rule changes to employer-based pensions. This included a cut in the level of early retirement benefits and the introduction of actuarially fair benefit adjustment, strengthening marginal financial incentives for later retirement. Moreover, specific early retirement provisions, such as "bridge" payments before the NRA, were abolished. Finally, a minimum ERA of 60 was legislated. The reform was announced at relatively short notice in July 2005, only a few months before taking effect.

Importantly for our empirical strategy, the 2006 reform was implemented with a sharp cohort cutoff. Only workers born on January 1, 1950 or later were affected, whereas older workers born below the cutoff could still retire under the generous pre-reform early retirement rules. Taken together, the bundle of reform measures caused a large increase in average retirement ages among directly affected workers (see Section 4.1).

Other Reforms. The most important other pension reform occurring during our analysis period is the increase of the NRA from 65 to 67, which is phased in between 2013 and 2024 (see Rabaté et al., 2023). The NRA gradually increases at a series of cutoff dates based on calendar years, none of which coincide with the January 1950 cohort cutoff of the 2006 reform. In addition, there were some further reforms uniformly affecting all individuals. This includes the introduction of tax-free retirement savings accounts in 2006 and a reform restricting eligibility for these in 2015. Moreover, tax credits for working at old age were introduced in 2012 and subsequently removed by 2018.

Since these reforms do not impact individuals differentially by cohort, they should not pose any concerns for our analysis of the 2006 reform.

2.3 Data

We use rich administrative data on the universe of Dutch residents provided by Statistics Netherlands (CBS). We construct our main dataset by merging a number of administrative registers at the individual level.[5] This includes civil registration data, personal income tax data, and employer-employee matched labor market data. We observe individual labor market states and incomes at the monthly level. Our main outcome of interest is a worker's retirement age, which we define as the age of last employment exit. For the analysis of retirement spillovers in families, we use information on household composition and family links from civil registers. Our analysis of workplaces is based on firm and sector of employment, which we observe in the labor market data. In order to define neighborhoods, we rely on residential location data at the level of fine-grained six-digit postcodes. Moreover, the administrative data contains a limited number of sociodemographic characteristics, including date of birth, gender, marital status and an indicator for foreign-born individuals.

Our analysis period is 2003 to 2021. We focus on cohorts 1946 to 1954, such that our sample includes a number of cohorts around the cutoff for whom we can observe a retirement age range between 57 and 67. As our main sample restriction, we require that individuals are employed for at least one month at or after the age of 57. This ensures that individuals are attached to the labor market, such that we can interpret their last employment exit as a retirement event. We generally assign peer group membership before retirement. This is important in order to minimize the number of endogenous, retirement-related "switches" between peer groups, such as moving to a new neighborhood around the time of retirement. Thus, we define neighborhoods based on place of residence at age 57 and couples as individuals who are married or in a registered civil partnership at age 57. Because of a change in firm identifier variables in the earlier years of data, we define coworker networks based on the firm where an individual is employed at age 59.

Table 1 shows summary statistics of our main sample, which comprises 1,352,249 individuals. 39% of the sample are female, 67% are married or in a civil partnership and 86% are Dutch-born. Annual labor income at age 57 is EUR 35.8k on average. The average retirement age is 63.8 years. 17% of individuals retire exactly at the NRA, 60% retire before the NRA, and the remaining 23% retire after the NRA. 56% of the sample are born in January 1950 or later, such that they are directly exposed to the 2006 reform. Within our sample, the median individual has one sibling, 37 neighbors (in 5-digit postcode areas) and 53 coworkers. The varying sizes of these different peer groups are important for how we estimate retirement spillovers later on. In addition, we note that the size distribution of firms is more skewed than that of other groups, as can be seen from the large difference between the median and the average number of coworkers. Nevertheless, family, neighborhood and workplace peers are quite similar in terms of cohort composition and average

[5] See Appendix B for a detailed description of the administrative registers we use.

retirement ages.

3 Empirical Strategy

Our primary objective is to estimate retirement spillovers in social networks. That is, we want to quantify the causal effect of peers' retirement behavior on the retirement behavior of an individual. To fix ideas, consider a classic linear-in-means model of peer effects in the context of retirement decisions:

$$R_i = \alpha + \beta \overline{R}_{-i} + X_i' \gamma_1 + \overline{X}'_{-i} \gamma_2 + \epsilon_i \tag{1}$$

where R_i is the retirement age of individual i, $\overline{R}_{-i}$ is the leave-out average retirement age of i's peers, and X_i and $\overline{X}_{-i}$ are background characteristics of the individual and their peers. The coefficient of interest is β, the causal effect of peers' retirement behavior on i's own retirement age.

It is well known from the literature on peer effects that identifying β is challenging. One set of challenges is given by "correlated effects" (Manski, 1993; Sacerdote, 2014). Namely, unobserved individual characteristics may be correlated with own retirement behavior and peer retirement behavior. This issue is especially likely to arise when individuals endogenously select into peer groups based on these characteristics. For instance, in our setting the selection of individuals into couples or workplaces may be related to unobserved retirement preferences. In addition, a reverse causality issue ("reflection problem") emerges in estimating equation (1) when individuals simultaneously influence each other's retirement behavior.

In order to address these crucial identification issues, our general approach is to exploit quasi-random variation in peers' retirement behavior generated by the 2006 pension reform. We use two complementary empirical strategies, a regression discontinuity design based on the reform exposure of a single peer, and an instrumental variable approach utilizing variation in the exposure of larger peer groups.

3.1 Regression Discontinuity Design

Our first empirical strategy is a regression discontinuity design (RDD) around the cohort cutoff of the reform. Building on Dahl et al. (2014) who use a similar strategy to estimate peer effects in parental leave take-up, we implement the RDD in two stages. In the first stage, we estimate the direct effect of the reform on the retirement behavior of individual j, who is a peer of individual i (i.e. their family member, neighbor, or coworker). In the second stage, we then estimate the spillover effect of j's reform-induced change in retirement behavior onto i's retirement behavior.

The first-stage estimation equation is given by

$$R_j = \alpha_0 + \alpha_1 \mathbb{1}(d_j \geq 1950) + f_l(d_j)\mathbb{1}(d_j < 1950) + f_r(d_j)\mathbb{1}(d_j \geq 1950) + \varepsilon_j \tag{2}$$

where R_j is the retirement age of individual j, d_j is j's birth date, the running variable of the RDD. $\mathbb{1}(d_j < 1950)$ is an indicator for being born in January 1950 or later, and f_l and f_r are functions of birth date to the left and the right of the cutoff, respectively. Our main RDD specifications parameterize f_l and f_r as quadratic functions estimated separately on both sides, using an estimation bandwidth of +/-24 months around the cutoff.

The coefficient α_1 in equation (2) captures the direct effect of the 2006 reform on j's retirement behavior. In order to identify this causal effect, we require the standard RDD identification assumptions. First, no other policy change should occur around the January 1950 cutoff. This is ensured by our institutional setting, where no other relevant pension reform is based on the same cohort cutoff. Second, the RDD requires that the running variable cannot be manipulated. While manipulation of birth dates is unlikely, we nonetheless conduct the usual test of the distribution of the running variable in Section 4.1.

In the second stage of the RDD strategy, we estimate spillover effects via the following equation:

$$R_i = \beta_0 + \beta_1 \mathbb{1}(d_j \geq 1950) + h_l(d_j)\mathbb{1}(d_j < 1950) + h_r(d_j)\mathbb{1}(d_j \geq 1950) + e_i \quad (3)$$

where R_i is the retirement age of individual i, and d_j is the birth date of another individual j, who is i's peer. As above, h_l and h_r are functions of birth date to the left and the right of the January 1950 cutoff, respectively.

The coefficient β_1 captures the causal spillover effect of individual j's reform exposure (and the resulting variation in their retirement behavior) on individual i's retirement age. Equation (3) thus yields an intention-to-treat (ITT) effect of j's reform exposure. In order to obtain a peer effect as defined in equation (1), i.e. the impact of j's retirement age on i's retirement age, we can scale the reduced-form spillover effect β_1 by the first-stage estimate α_1.[6] Like the first stage, estimating spillover effects via equation (3) requires no manipulation of birth dates. As an additional identification assumption, we need an exclusion restriction: j's reform exposure should affect i's retirement age only through j's retirement behavior. In particular, this requires the absence of sharp sorting into peer groups by birth date around the January 1950 cutoff. For instance, if there was a tendency to choose spouses born precisely in the same month, we may confound individuals' own reform exposure with that of the peer. We carefully test these identification assumptions for each peer group in Section 4.

3.2 Instrumental Variable Approach

The RDD strategy we describe above is well-suited to analyze the impact of the reform exposure of a single relevant peer. One challenge that arises for some of the groups we study is that there can be many potential peers, especially in neighborhoods and workplaces. In this case, implementing the RDD requires selecting a particular peer for each individual. As an alternative strategy, we develop an instrumental variable (IV) approach leveraging variation in the reform exposure of multiple peers.

[6] We follow Dahl et al. (2014) and implement the second-stage as an ITT specification. This requires fewer assumptions than a fully-fledged two-stage least squares (2SLS) estimation. For instance, 2SLS would additionally require a monotonicity assumption that the reform does not cause any individuals to retire earlier.

Our starting point is the linear-in-means model from equation (1), where an individual's retirement age is influenced by the average retirement behavior of multiple peers within a group. The 2006 reform generates exogenous variation in the average retirement age of peer groups, depending on their birth date composition around the cohort cutoff. Thus, we can utilize the reform cutoff to construct an IV for peers' average retirement age. Analogously to the RDD, we proceed in two stages. The first stage quantifies the direct effect of the reform on the average retirement behavior of individual i's peers, and the second stage estimates the spillover effect onto i's own retirement behavior.

In the first stage, we estimate

$$\overline{R}_{-i} = \phi_0 + \phi_1 P(d_{-i} \geq 1950) + X_i'\zeta + \nu_i \tag{4}$$

where $\overline{R}_{-i}$ is the leave-out average retirement age of i's peers, $P(d_{-i} \geq 1950)$ is the fraction of peers born above the January 1950 cutoff, and X_i is a vector of control variables. The share of peers exposed to the reform thus serves as the IV, and its first-stage effect on peers' average retirement age is captured by coefficient ϕ_1.

In the second stage, we estimate the spillover effect onto individual i's retirement behavior:

$$R_i = \delta_0 + \delta_1 P(d_{-i} \geq 1950) + X_i'\xi + u_i \tag{5}$$

Analogously to the RDD equation (3), we estimate the second stage as an ITT specification, that is we directly regress i's retirement age on the share of peers exposed to the reform. In order to scale spillover effects relative to the change in peers' average retirement age, we can divide the reduced-form effect δ_1 by the first-stage coefficient ϕ_1. This yields results equivalent to estimating equation (1) as a two-stage least squares specification, using $P(d_{-i} \geq 1950)$ as the IV for $\overline{R}_{-i}$.

The IV strategy rests on two identification assumptions. First, we require a sufficiently strong first stage, that is, the reform should induce significant variation in peers' average retirement behavior. As we show in the next section, estimating equation (4) indeed yields a highly significant first stage both among neighbors and coworkers. Second, we need an exclusion restriction: the fraction of peers exposed to the reform should only affect i's retirement age through peers' retirement behavior. In particular, this precludes any selection into groups based on peers' age composition around the January 1950 cutoff.

Even though the second identification assumption is somewhat stronger than its RDD analogue, there are several arguments in its support. First, we define reform exposure only within a narrow cohort window around the January 1950 cutoff, such that we use variation in the treatment status of peer groups with similar overall age structure. Second, as explained in Section 2.3, we generally define group membership before retirement, limiting potential selection into peer groups around the time of retirement. Third, in order to alleviate remaining concerns about sorting into peer groups based on age, we flexibly control for individuals' own birth date. This also controls for individuals' own reform exposure, and we additionally account for other pre-determined characteristics and fixed effects. Finally, we conduct placebo checks around artificial reform cutoffs in

Sections 4.3 and 4.4. We find no significant correlation between individual retirement ages and the share of peers born above placebo cohort cutoffs, supporting the validity of the exclusion restriction.

4 Main Results

4.1 Direct Effect of the 2006 Reform

The 2006 reform serves as our source of exogenous variation in peers' retirement behavior. Panel (a) of Figure 1 documents the discontinuous impact of the reform on the retirement age of directly affected individuals. The figure corresponds to the first-stage RDD specification from equation (2) run on the full sample. A sharp jump in retirement ages occurs precisely at the January 1950 cutoff. The RDD estimate implies a highly significant effect of the reform on the average retirement age of directly affected individuals of 0.38 years (4.6 months).

As discussed in Section 3.1, a key identification assumption behind the first-stage RDD estimation is no manipulation of the running variable. Appendix Figure A1 plots the distribution of birth dates in the full sample in order to test this assumption. The figures confirms that there is no discontinuous pattern in the density around January 1950, besides some seasonality in the number of births which occurs in each calendar year.

In order to provide additional information about the change in retirement patterns induced by the 2006 reform, Panel (b) of Figure 1 depicts the distribution of retirement ages among cohorts 1949 (the last year of birth below the reform cutoff) vs. 1950 (the first year above the cutoff). The figure shows that fewer individuals retire at ages 60 to 63 as a consequence of the reform. This is expected, since the reform makes early retirement less attractive. Instead, more individuals retire later among treated cohorts. One especially noticeable pattern is the strong increase in the density exactly at the NRA, which is 65.25 for most individuals born in 1950. This increase in the retirement spike at the NRA seems to emerge as a side effect of the 2006 reform, which generally incentivizes later retirement but does not provide any incentive to retire precisely at the NRA. Similar retirement bunching at the NRA in other settings has been attributed to behavioral factors (Seibold, 2021). We further analyze this pattern when we discuss the role of social norms in Section 5.3.

4.2 Retirement Spillovers in Families

We begin our analysis of retirement spillovers with families. We focus on spouses and siblings, the closest types of family members within the same generation. Peer groups defined by families are relatively small: couples always consist of two individuals, and the majority of individuals have at most one sibling within our sample. Thus, the RDD described in Section 3.1 is well-suited for studying spillovers in families.

4.2.1 Spillovers in Couples

Our analysis of couples focuses on pairs of individuals who are married or in a civil partnership, where both spouses satisfy our sample inclusion criteria.[7] Figure 2 illustrates estimated retirement spillovers in couples. Each panel provides a graphical depiction of equation (3), plotting individual retirement ages by the birth date of their spouse, the running variable. Panel (a) shows how women's retirement behavior is impacted by their husband's reform exposure. There is a sharp jump in women's average retirement ages at the husband's birth date cutoff in January 1950. The RDD estimate corresponds to a highly significant spillover effect of 0.31 years on women's own retirement ages when the husband is exposed to the reform. Conversely, Panel (b) shows no significant spillover effect onto men when their wife is exposed to the the reform. The point estimate of 0.02 is very small and not significantly different from zero. Thus, retirement spillovers within couples are almost exclusively driven by women reacting to their husband's retirement behavior.

Columns (1) and (2) of Table 2 show these RDD results in more detail. In Panel A, there is a large and significant first stage both among male and female spouses. Panel B displays the reduced-form spillover effects from Figure 2, and Panel C shows spillover effects scaled relative to the first stage. For women, the estimated spillover effect corresponds to a retirement age increase of 0.76 years (9.1 months) for each year the husband retires later as a result of the reform. For men, the scaled effect of 0.08 years remains insignificant.

Appendix Figure A2 shows some additional results for couples, suggesting that the stark gender difference in spillovers is not driven by differences in age or earnings. First, we split the sample by gender and age difference. We find that women who are younger than their husbands react most strongly to their husband's retirement age, while older women display small spillover effects. However, men's retirement choices never respond significantly to their wife, regardless of the age difference in the couple. Second, we estimate spillover effects by gender and relative earnings. Women always react strongly to their husband's retirement age, no matter whether they are the primary or secondary earner in the couple. Again, men are never significantly affected by their wife.

We conduct a number of checks in order to test the validity of the identification assumptions behind our RDD. Appendix Figure A4 shows the distribution of birth dates in the male and female spouse samples we use to obtain the results in Figure 2. As in the full sample, the density evolves smoothly through the 1950 cutoff, besides some seasonality patterns that are present in each year. Furthermore, Appendix Figure A5 tests for sorting on birth dates within couples. As discussed in Section 3.1, sharp sorting by month of birth would present a problem for our identification strategy, as this may lead us to confound individuals' own reform exposure with that of the spouse. We thus plot an indicator for own birth date being above the January 1950 cutoff by the spouse's birth date, the running variable. As one may expect, there exists a general positive age correlation within couples. However, crucially, the figure shows that own reform exposure evolves smoothly around the cutoff. In other words, sorting is not sufficiently sharp to create any issues for the RDD.

[7] Throughout this paper, we use the term "spouses" to refer to couples who are either married or in a civil partnership.

In order to further validate our empirical strategy, Appendix Figure A7 presents placebo RDD results around artificial cohort cutoffs. We implement placebo specifications around two placebo cutoffs, one in January 1948 (using only pre-reform data) and one in January 1952 (using only post-reform data).[8] Apart from varying the birth date cutoff, all specification choices are the same as in our main estimation. We find small and insignificant spillover effects both for women and for men around the two placebo cutoffs. These null effects suggest that there are no other sources of correlation between individual retirement patterns and spouses' birth dates that would bias our main results.

4.2.2 Spillovers between Siblings

Next, we investigate retirement spillovers between siblings. In order to implement the RDD, we focus on individuals who have one sibling within our sample. We exclude twins and other multiple births from the analysis, since own reform exposure and siblings' reform exposure perfectly coincide for these pairs, which would violate the exclusion restriction.

Figure 3 displays our main results for siblings. Panel (a) considers all siblings pairs and plots individual retirement ages by the running variable, the sibling's birth date. Estimated retirement spillovers among all sibling pairs are small and insignificant. However, we find strong spillovers among siblings who are similar to each other, i.e. who share additional socioeconomic characteristics. Panel (b) shows our preferred specification focusing on siblings who have the same gender, live in the same city, work in the same 2-digit sector, and have similar earnings. Note that all characteristics are measured before retirement (at age 57). For this group, the RDD estimate is large and significant. Panel (c) shows a range of estimates for subsamples of siblings who share varying sets of characteristics. We find that spillover effects increase almost monotonically with the degree of similarity between siblings. For the most similar sibling pairs sharing a combination of two or more additional characteristics, a sibling's reform exposure increases individual retirement ages by at least 0.24 years.

Columns (3) and (4) of Table 2 show full results from our preferred RDD specifications for siblings. Both among all siblings and among similar pairs, we find a highly significant first stage. The first-stage point estimate is particularly large among similar siblings, but it is also more noisily estimated due to the relatively small size of this subsample. The reduced-form RDD estimate among similar siblings translates into a scaled retirement age spillover of 0.84 years (10.1 months) for each year the sibling retires later. This suggests that the impact of similar siblings on individuals' retirement decisions is comparable to spillover effects between spouses.

Note that the results presented in Figure 3 and Table 2 consider the older sibling as the main individual, as we generally find stronger spillover effects for this group. Appendix Figure A3 shows analogous results for younger siblings. While the magnitude of spillover effects tends to

[8] No other reform occurs at the 1952 placebo cutoff. At the 1948 cutoff, a one-month increase in the NRA occurs, which may cause a small direct effect on retirement ages. As Appendix Figure A6 shows, our RDD specifications do not yield any significant first-stage effect around either placebo cutoff.

be smaller, the same qualitative patterns emerge for younger siblings. In particular, spillovers also increase in the degree of similarity within pairs.

Our findings for siblings are consistent with the notion of *homophily* in social networks, that is the tendency of individuals to form closer social ties with others who are similar to them. A large body of interdisciplinary evidence suggests that homophily plays an important role in shaping social interactions across many domains (McPherson et al., 2001). In the context of retirement, there are at least two potential reasons why the degree of similarity between siblings or other peers could affect the magnitude of social spillovers. First, homophily implies that similar individuals have closer relationships and interact more frequently, increasing the likelihood of discussing and observing each other's retirement choices. Second, individuals may be more strongly influenced by the retirement behavior of similar individuals, as they might view those as more relevant to their own situation.

Finally, we implement validation checks for the sibling samples. Appendix Figure A4 shows that the distribution of birth dates evolves smoothly through the cutoff among siblings. Appendix Figure A5 tests for sorting on birth dates. As expected, once we exclude multiple births, there is no discontinuity in own treatment exposure at the sibling's birth date cutoff. Finally, Appendix Figure A7 includes placebo RDD results for siblings. All placebo estimates around the artificial 1948 and 1952 cutoffs are small and insignificant, further supporting the identification assumptions behind the RDD.

4.3 Retirement Spillovers in Neighborhoods

Neighborhoods are an important part of individuals' social networks, and there is evidence that neighbors influence a number of economic decisions and outcomes (e.g. Bayer et al., 2008; Chetty et al., 2013; Chetty and Hendren, 2018; Barrios-Fernandez, 2022). For our analysis of retirement spillovers between neighbors, a key consideration is that neighborhoods constitute large peer groups. Within our sample, the median individual has 37 potential peers in neighborhoods defined by 5-digit postcodes. We follow two empirical strategies to address this challenge. First, we employ an RDD similar to our approach for families, using specific neighbors as peers. Our second strategy is the IV approach outlined in Section 3.2, which exploits the full variation in the treatment exposure of neighborhoods.

In order to implement the RDD, we choose particular neighbors to serve as peers for each individual. This approach is motivated by our analysis of siblings, where we found an important role for homophily in retirement interactions. Thus, we try to identify similar neighbors with whom individuals are more likely to have closer ties and frequent interactions. Concretely, we match individuals to their most similar neighborhood peer based on a number of socioeconomic characteristics, including 2-digit sector of work, an indicator for being foreign-born, earnings (below vs. above median), and gender. As before, we measure these variables at age 57. We then construct similar-peer samples focusing on unique matches between individuals and peers who share the maximum number of characteristics. In order to increase sample size, we consider both our main neighborhood definition based on 5-digit postcodes and smaller areas defined by 6-digit postcodes to search for matches.[9]

Figure 4 shows results from the RDD strategy. Panel (a) shows a benchmark specification where peers are selected out of all neighbors in 6-digit postcode areas, without limiting the set of peers to similar individuals. Note that as we keep only unique matches for the RDD, this specification effectively focuses on cases where individuals have a single immediate neighbor among the cohorts in our sample. The corresponding RDD estimate is close to zero and insignificant. Panel (b) shows results from our preferred specification using neighborhood, sector of work and the foreign-born indicator to match individuals with similar peers. In this sample, we find a sizable and significant spillover effect of the neighbor's reform exposure of 0.11 years. The effect in Panel (b) is not only larger, but also more precisely estimated than in Panel (a). This occurs because in large groups such as neighborhoods, the number of unique pairs increases when using more variables to match individuals and peers, up to a point.[10]

Columns (5) and (6) of Table 2 show our preferred RDD specifications in more detail. There is a strong first-stage effect of the 2006 reform on neighborhood peers' retirement ages, both in the all-neighbor and similar-neighbor samples. Scaled relative to the first stage, the spillover effect exerted by similar neighbors corresponds to 0.24 years (2.9 months) per year of later of retirement of the peer. In addition, Appendix Figure A8 displays a range of RDD estimates using varying sets of characteristics in the matching procedure. Point estimates tend to increase in magnitude and precision as the number of matching characteristics increases. Estimated spillovers appear to decrease somewhat when including gender and earnings as additional characteristics, though. Finally, we again implement the usual set of validation checks for these RDD specifications. In Appendix Figures A4 and A5, both the density and individuals' own treatment status evolve smoothly through the reform cutoff in our neighborhood samples. Appendix Figure A7 shows that placebo specifications around artificial birth date cutoffs yield no significant RDD estimates.

Our RDD results demonstrate that similar neighbors matter for individuals' retirement behavior. To complement these findings, we implement the IV strategy, which allows us to uncover average spillover effects between neighbors. Table 3 shows IV results for neighbors. Column (1) begins with the first-stage estimation based on equation (4). We find a large and highly significant impact of the fraction of neighbors exposed to the reform on the leave-out average retirement age in the neighborhood. The point estimate of 1.0 suggests that moving the share of treated neighbors from zero to one corresponds to a one-year increase in peers' average retirement age. Column (2) shows retirement spillovers estimated according to equation (5). We find that neighbors' reform exposure causes a small but precisely estimated 0.06 year (0.7 months) increase in individual retirement ages. In addition, Column (7) shows a placebo IV specification around an artificial cohort

[9] We first search within 6-digit postcode neighborhoods and in case no peer sharing the respective set of characteristics can be found in this small area, we search within larger 5-digit postcode areas.

[10] In small peer groups such as siblings, the opposite pattern arises: sample size decreases when focusing on pairs sharing more characteristics. Thus, standard errors increase when considering more similar siblings in Figure 3.

cutoff in January 1948.[11] The placebo estimation yields no significant effect on retirement ages. This suggests that there is no general correlation between individual retirement ages and neighbor's cohort composition that would bias our main estimates, supporting the exclusion restriction required for the IV strategy.

4.4 Retirement Spillovers in Workplaces

Several studies find that coworkers play an important role in shaping individual labor supply and career choices (e.g. Falk and Ichino, 2006; Mas and Moretti, 2009; Cornelissen et al., 2017). When it comes to retirement behavior, one might thus expect coworkers to be one of the most influential peer groups. Similar to neighborhoods, coworker networks tend to be large in our data. The median individual has 53 potential workplace peers. Again, we implement both RDD and IV strategies in order to address this challenge in two complementary ways.

We begin with the RDD, where we utilize a number of socioeconomic characteristics in order to match individuals with their most similar workplace peer. We use the same set of characteristics as in the neighborhood analysis, namely an indicator for being foreign-born, earnings and gender, plus city of residence at age 57. As before, we construct similar-coworker samples focusing on unique matches between the individual and a peer who share the maximum number of characteristics. One issue faced by the workplace analysis is that the similar-coworker samples remain substantially smaller than the similar-neighbor samples, even when using the full set of characteristics in the matching procedure. This occurs mainly due to the skewness of the firm size distribution: many workers are employed in large firms where it is hard to identify unique peers. For this reason, we limit the number of birth cohorts in the sample to a slightly narrower range around the reform cutoff, 1947 to 1952. This improves the chance of finding unique peers, increasing the number of observations available for the RDD.

Figure 5 shows the main RDD results. Panel (a) displays a benchmark specification, which uses only firms to select peers. This variant of the RDD effectively limits the analysis to individuals who have a single coworker among the cohorts we study, which tends to occur in relatively small firms. The corresponding point estimate of 0.02 is small and insignificant. Panel (b) shows our preferred specification, using city of residence, foreign-born status, earnings and gender to match individuals with their most similar peer. In this sample, we find a large and significant spillover effect of 0.19 years.

Columns (7) and (8) of Table 2 present more details of these RDD specifications. In both coworker samples underlying Figure 5, a similar first-stage effect of the 2006 reform on peers' retirement behavior occurs. When we scale the reduced-form estimate among similar coworkers by the first stage, we find a spillover effect on individual retirement ages of 0.49 years (5.9 months) for each year of the coworker's later retirement. Furthermore, Appendix Figure A8 shows an ex-

[11] We do not use the alternative 1952 placebo cutoff here because increases in the NRA are implemented at an accelerated pace among these younger cohorts, which could lead to an upward bias in the placebo IV specification. Around the January 1948 cutoff, the NRA increases at a slower pace, minimizing the potential bias.

tended set of RDD results, varying the characteristics used in the matching procedure, indicating that both the magnitude and precision of estimated spillover effects increase with the degree of similarity between individuals. Finally, Appendix Figures A4 and A5 show the usual RDD validation checks and Appendix Figure A7 shows placebo RDD results for coworkers, lending support to our identification assumptions.

As the second empirical strategy, we implement the IV estimation leveraging the full variation in the reform exposure of workplace peers. Table 4 presents results.[12] In Column (1), we find a highly significant first stage of 0.83, implying that coworkers' leave-out average retirement age would increase by 0.83 years if they were all exposed to the reform. Column (2) shows a modest but precisely estimated spillover effect of 0.09 years (0.11 years or 1.3 months relative to the first stage). In Column (7), we run a placebo specification around an artificial reform cutoff in January 1948. The placebo test yields a small and insignificant estimate, indicating that selection into firms based on coworkers' age structure does not bias our main results.

5 Mechanisms

Our main analysis uncovers significant retirement spillovers within families, neighborhoods, and workplaces. As we further discuss in Section 6, these estimates of total spillover effects across different peer groups are relevant in terms of policy implications, as they govern the overall behavioral and fiscal externalities of pension reforms. Nonetheless, it is interesting to examine what mechanisms can explain the retirement spillovers we find. In this section, we investigate the role of three key potential mechanisms considered in the literature.

5.1 Information Spillovers

A first potential mechanism could be information spillovers. In our setting, changes in peers' retirement behavior are induced by the 2006 pension reform. Thus, spillover effects onto other individuals could be driven by peers passing on information about the post-reform retirement rules, or individuals may learn about optimal retirement behavior under the new legislation from observing the choices of peers exposed to the reform. Crucially, this type of information should only be valuable for individuals who are themselves subject to the post-reform rules, i.e. individuals who are themselves born in January 1950 or later. Hence, in order to test for an information mechanism, we investigate to what extent spillovers are concentrated among relatively young individuals who are themselves exposed to the reform.

Panel (a) of Figure 6 summarizes our findings in a coefficient plot. For couples and siblings, we estimate RDD specifications analogous to Figures 2 and 3, but split the sample into individuals

[12] Note that the sample size underlying the IV estimation in Table 4 is smaller than in the neighborhood analysis from Table 3 for two reasons. First, for the sake of consistency with our RDD results on coworkers, we use a slightly narrower cohort window. Second, a non-negligible fraction of individuals work in relatively small firms and do not have any workplace peers within our sample.

born before vs. after January 1950.[13] For instance, the first coefficient in Figure 6 shows the estimated spillover effect of husbands' reform exposure among women born before January 1950, and the second coefficient shows the spillover effect among women born in January 1950 or later. We find that retirement spillovers in couples are larger among women who are themselves exposed to the reform. For siblings, however, we find the opposite pattern. Spillover effects of a sibling's reform exposure are only significant among older individuals born before January 1950, but not among younger individuals who are themselves subject to the post-reform rules.

For neighbors and coworkers, we perform an analogous exercise within the IV framework. In order to capture differential spillovers by individuals' own reform exposure, we estimate equation (5) including an interaction between the fraction of peers exposed to the reform and an indicator for own treatment status. Panel (a) of Figure 6 shows the implied spillover effects for exposed and non-exposed individuals, and more detailed regression results are included in Tables 3 and 4. For neighbors, we find that retirement spillovers are large and significant among older individuals born before 1950 who are themselves not exposed to the reform, but smaller and insignificant among younger individuals. For coworkers, spillovers are again large among individuals not exposed to the reform, and the point estimate even turns negative among younger individuals.

Overall, we conclude that an information mechanism is unlikely to explain our results. Among siblings, neighbors and coworkers, spillover effects are not driven by individuals for whom information about the post-reform retirement rules would be relevant. On the contrary, we tend to find larger spillovers among older individuals who can still retire under the pre-reform rules. The only exception is given by spouses, where strong spillover occur among younger women who are exposed to the reform. Yet, it is not clear whether this pattern can be interpreted as an effect of reform exposure per se, as women's age is highly correlated with the age difference in the couple. This could in turn be related to gender attitudes, which may explain the larger reaction of younger women to their husband's retirement choices.

5.2 Leisure Complementarity

A second potential mechanism behind retirement spillovers could be leisure complementarity. That is, individuals may place higher value on leisure when it is spent in the company of others. If an important peer retires later, the individual may thus decide to postpone retirement, as the value of leisure is relatively lower in the meantime. There is evidence in favor of leisure complementarity in particular within couples (e.g. Georges-Kot et al., 2024). In the context of retirement, the empirical signature of leisure complementarity typically considered in the literature is individuals retiring at the same time (Garcia-Miralles and Leganza, 2024).

Hence, we examine to what extent joint retirement of individuals with their spouse or with other peers can explain our main results. We run RDD specifications analogous to Figures 2 to 5, splitting each sample into pairs retiring jointly vs. pairs retiring separately. In order to generously capture potential leisure complementarities, we define joint retirements as cases where the indi-

[13] RDD plots underlying the coefficients included in Figure 6 are displayed in Appendix Figure A9.

vidual retires within +/- 6 months of the peer's retirement date. Conversely, separate retirements are defined as occurring outside of this window. Panel (b) of Figure 6 summarizes our findings in a coefficient plot. Across all types of peers we consider, we find that spillover effects are particularly large among pairs retiring jointly. For example, couples who retire around the same time exhibit a retirement spillover of 0.57 years. The difference in RDD estimates between joint vs. separate retirements is significant among spouses, siblings, and neighbors, but insignificant among coworkers. Nevertheless, in terms of absolute magnitude, spillovers also remain sizable among pairs who retire separately in all peer groups. For instance, we find an estimate of 0.27 for couples retiring at different times.

Furthermore, it is important to note that across all types of peers, the fraction of joint retirements is limited. Even in couples, where prior literature suggests an important role for joint retirement, only 16% of pairs retire within +/-6 months of each other in our data. Among similar siblings, neighbors and coworkers, the fraction of joint retirements is 9%, 9%, and 13%, respectively. Overall, these results suggest that leisure complementarity can play a role in inducing strong retirement spillovers for a subset of individuals. However, since only few pairs retire jointly and spillovers also occur among individuals retiring far from their peers, this mechanism seems unlikely to explain the majority of spillover effects.

5.3 Social Norms

A third potential mechanism behind our results could be that social norms about retirement are transmitted via peer networks. Recent literature documents large-scale "anomalies" in retirement behavior that are hard to reconcile with standard models (Seibold, 2021; Gruber et al., 2022; Lalive et al., 2023). In particular, many workers retire exactly when they reach the Normal Retirement Age (NRA) set by the government, even when there are no financial incentives linked to this age. Seibold (2021) argues that these patterns are driven by the general perception of the NRA as a reference point, a "normal" time to retire. Social interactions could be an important factor in determining whether workers adopt such notions of normal behavior.

The 2006 reform provides a unique opportunity to test whether norm-related behavior spreads via social spillovers. As we discuss in Section 4.1, the reform substantially increases the probability of retiring exactly at the NRA among directly exposed individuals. Panel (b) of Figure 1 illustrates how the retirement spike at the NRA grows among the post-reform cohorts. Panel (a) of Figure 7 more formally quantifies the impact of the reform on the probability of bunching at the NRA in our full sample. We estimate a first-stage RDD specification akin to equation (2), replacing the individual's retirement age by their probability to retire at the NRA (defined as retirements within +/- 1 month of the NRA) as the outcome. Among workers born just below the January 1950 cutoff, the baseline fraction retiring at the NRA is around 10%. The RDD estimate implies an increase in the probability of bunching at the NRA of 5.2 percentage points, corresponding to a more than 50% increase in the prevalence of this behavior.

We then exploit this strong first stage in order to study whether individuals' propensity to retire at the NRA is influenced by their peers' behavior. Panel (b) of Figure 7 summarizes estimated spillover effects across the different peer groups we consider. For couples and siblings, we estimate a variant of the RDD from equation (3) with the probability of bunching at the NRA as the outcome. We find significant spillover effects in couples: women become 2.7 percentage points more likely to retire at the NRA when their husband is exposed to the reform. This effect is large, corresponding to more than 50% of the first-stage increase in the probability of bunching at the NRA. In addition, the figure shows that spillovers are largely driven by couples where both spouses retire at the same age.[14] This suggests that our specification does not just capture a generic tendency towards later retirement ages, but that spillovers arise in pairs where both individuals retire exactly at the NRA. Among similar siblings, we also find a sizable spillover effect of 1.3 percentage points. Again, the estimate is particularly large and significant among siblings who retire at the same age, suggesting that spillovers are driven by pairs where both retire at the NRA.

For neighbors, we turn to our IV framework. We estimate equations (4) and (5), replacing the individual's retirement age by the individual's probability to retire at the NRA, and neighbors' leave-out mean retirement age by the leave-out share of peers retiring at the NRA. Column (4) of Table 3 shows a strong and significant first-stage estimate of 0.095, implying that the fraction of neighbors retiring at the NRA would increase by 9.5 percentage points if the neighborhood's reform exposure moves from zero to one. Our main spillover effect estimate in Column (5) is a modest but precisely estimated 1.2 percentage points. This estimate is also displayed in Figure 7 for comparison to the other peer groups. Interestingly, Column (6) shows that bunching spillovers are mainly concentrated among individuals who are not themselves exposed to the reform. That the reaction to peers' bunching behavior is driven by individuals who can retire under the pre-reform rules and thus have no incentive to move towards the NRA strengthens the argument that social norms are at play. Finally, Column (8) displays results from a placebo specification around an artificial reform cutoff, indicating no other sources of correlation between the probability of retiring at the NRA and neighbors' cohort composition.

We proceed analogously for coworkers. In Column (4) of Table 4, we find a highly significant first-stage effect of 8.2 percentage points on the fraction of workplace peers bunching at the NRA. Column (5) shows a small and marginally insignificant spillover effect on the probability of retiring at the NRA of 0.7 percentage points. Similar to neighbors, spillover effects are particularly large and significant among individuals are who not themselves exposed to the reform. A placebo specification again yields no significant correlation between the likelihood of retiring at the NRA and the cohort composition of coworkers.

[14] Note that retiring at the same age generally does not imply retiring at the same time, except for couples where both spouses are born exactly in the same month.

6 Policy Implications

The key policy implication of our findings is that social spillovers exacerbate the overall impact of pension reforms on retirement behavior. Building on our comprehensive analysis across multiple peer groups, we can aggregate these effects and quantify the *social multiplier* of pension reform.

We consider the following thought experiment. Suppose a pension reform targets some representative individuals in a population, and leads to a one-year increase in the average retirement age through the direct impact on the behavior of these exposed individuals. We define the social multiplier as the magnitude by which social spillovers amplify the impact of the reform on the average retirement age in this population, relative to the direct effect. We can calculate the social multiplier M as

$$M = 1 + \sum_k \overline{N}_{-i,k} \sum_{\theta \in \Theta_k} p_\theta \mu_\theta \tag{6}$$

where k indexes peer groups (spouses, siblings, neighbors, and coworkers) and θ indexes types of peers within a group, for example male vs. female spouses or siblings with varying degrees of similarity. $\overline{N}_{-i,k}$ is the average number of peers within group k, Θ_k is the set of types in k, and p_θ is the fraction of type θ in the group. μ_θ denotes the estimated spillover effect for peers of type θ, scaled by the direct effect of the reform. For spillover effects estimated through our RDD framework, μ_θ corresponds to the coefficient β_1 from equation (3) divided by the first-stage coefficient α_1. In the IV framework, μ_θ corresponds to δ_1 from equation (5) divided by the first-stage effect ϕ_1.

For couples, we can operationalize equation (6) by combining the RDD estimates from Figure 2 with the fraction of married individuals and the gender composition of our sample. For siblings, we use the full set of estimates from Panel (c) of Figure 3 together with the average number of siblings and the relative sizes of the subsamples with different degrees of similarity. For neighbors and coworkers, we can directly use our IV estimates from Tables 3 and 4, as these correspond to the average impact of the retirement age of an individual onto their neighborhood or workplace peers.

Figure 8 illustrates the results from this calculation. Overall, we find a large social multiplier of 1.40. In other words, for a pension reform inducing a one-year increase in the average retirement age through direct effects, the overall impact of the reform on the average retirement age including social spillovers is 1.40 years. Out of the total spillover effect of 40%, spouses account for 17 percentage points and siblings account for 7 percentage points. Note that the contribution of siblings is modest compared to some of the large RDD estimates from Figure 3, as very similar pairs only make up a limited fraction of all siblings. Neighbors and coworkers contribute 6 and 11 percentage points to total spillovers, respectively. Although our IV point estimates are relatively small, these imply substantial contributions to the multiplier as they reflect average spillover effects.

Our social multiplier calculation likely provides a lower bound on how social interactions affect overall retirement responses to pension reforms for two reasons. First, although our analysis encompasses the types of peers considered in much of the literature on peer effects, there could in principle be further unobserved peer groups causing retirement spillovers, such as friends without any formal ties to the individual. As long as average spillover effects are positive within additional groups, these would add to the social multiplier of a pension reform. Second, we only focus on "first-round" spillover effects and do not take into account potential cascading effects. If individuals subject to peer effects in turn exert further retirement spillovers onto others, this would again exacerbate the multiplier (see e.g. Dahl et al., 2014).

7 Conclusion

In this paper, we provide novel empirical evidence on the role of social interactions for retirement behavior. We find large retirement spillovers in couples, which mainly occur due to women reacting to their husband's choices. While the influence of the average sibling, neighbor and coworker on individual retirement behavior is small, we estimate sizable spillover effects within all these groups when individuals are similar in terms of sociodemographic characteristics. These results are consistent with the notion of homophily in social networks, which posits that similar individuals are more likely to interact and influence each others' behavior. Furthermore, we provide evidence that retirement spillovers are driven both by leisure complementarities and by the transmission of social norms.

Our findings have an important implications for pension policy. Since a change in an individual's retirement age influences others in their social network, there is a *social multiplier* effect that exacerbates the overall impact of pension reforms on retirement behavior. Not taking into account this multiplier would lead researchers and policymakers to underestimate the total labor market and fiscal effects of reforms. Furthermore, social spillovers can significantly alter the welfare impact of policies. Fiscal externalities arising from behavioral responses are a key input into welfare evaluations of pension reforms, as they reflect the moral hazard cost of additional government spending on pensions (Kolsrud et al., 2024). Our results imply that for a given value of benefits to recipients, optimal pension system generosity may be lower when taking into account social spillovers that amplify fiscal externalities. Similarly, the marginal value of public funds (MVPF) of pension reforms depends on total behavioral responses (Hendren and Sprung-Keyser, 2020), and incorporating our social multiplier would lower the MVPF of extra spending on pension benefits.

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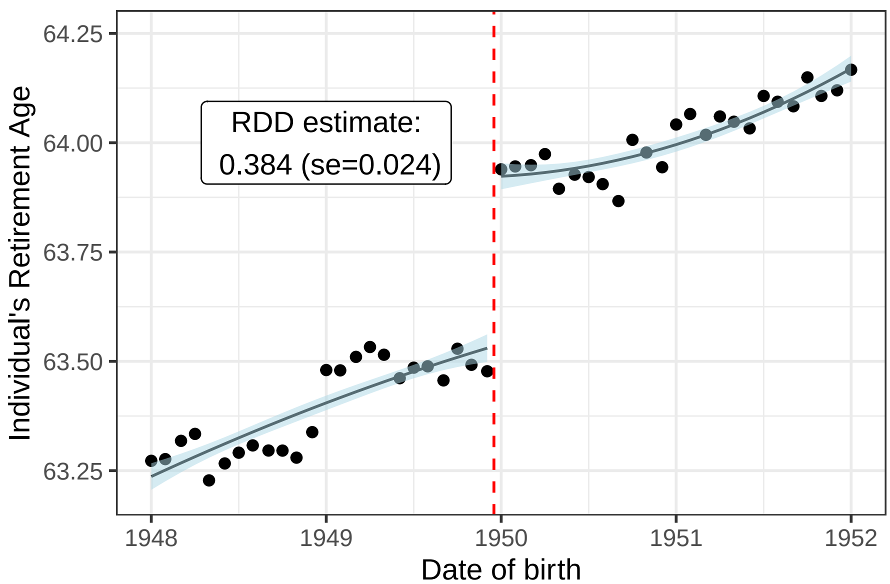
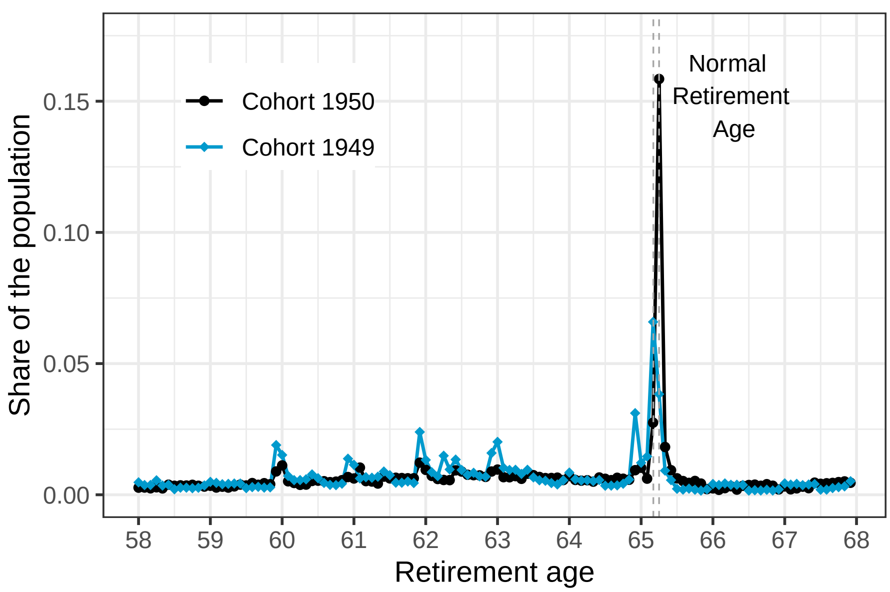
FIGURE 1: DIRECT EFFECT OF THE 2006 REFORM

(a) Average Retirement Age



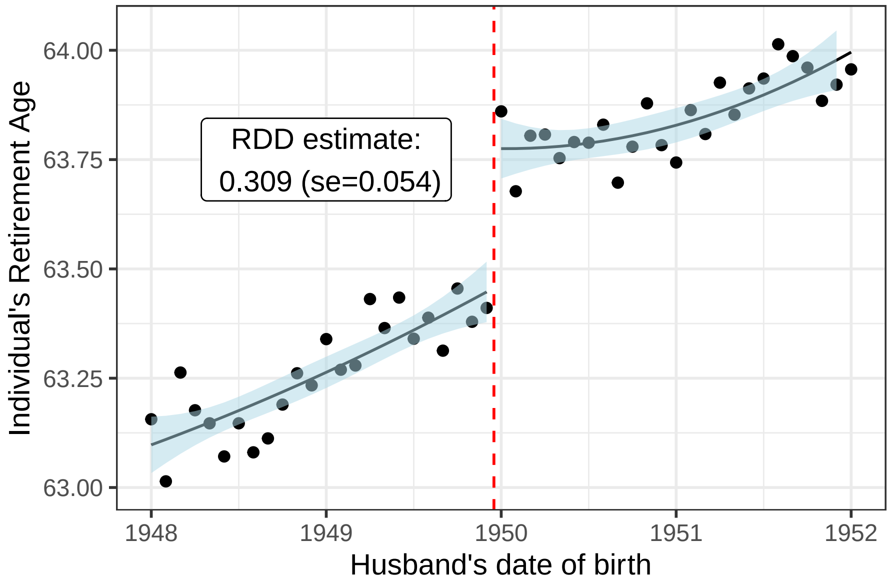
(b) Retirement Age Distribution



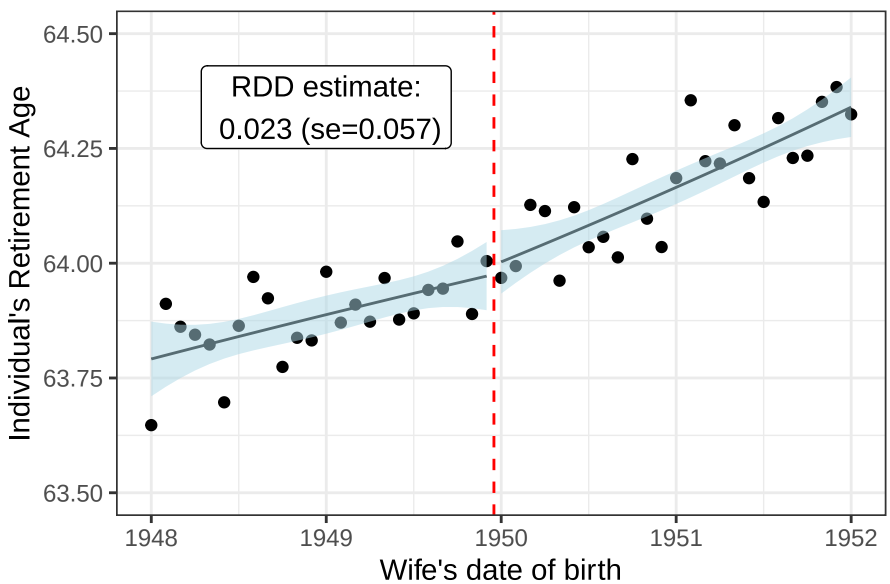
Notes: Panel (a) of the figure shows individuals' average retirement ages by their own birth date in monthly bins. The red dashed line demarcates the January 1950 cutoff of the 2006 pension reform. The black solid lines depict a quadratic fit estimated separately on each side of the cutoff, with 95% confidence bands shaded in light blue. An estimate of the jump in retirement ages at the cutoff from a regression discontinuity design is included in the text box, with its standard error in parentheses. Panel (b) shows the distribution of retirement among individuals born in 1949, the last year below the reform cutoff, and among individuals born in 1950, the first year above the reform cutoff. The dashed vertical lines demarcate the Normal Retirement Age, which is 65 years and 2 months for most individuals born in 1949 and 65 years and 3 months for most individuals born in 1950.

FIGURE 2: SPILLOVERS IN COUPLES

(a) Women

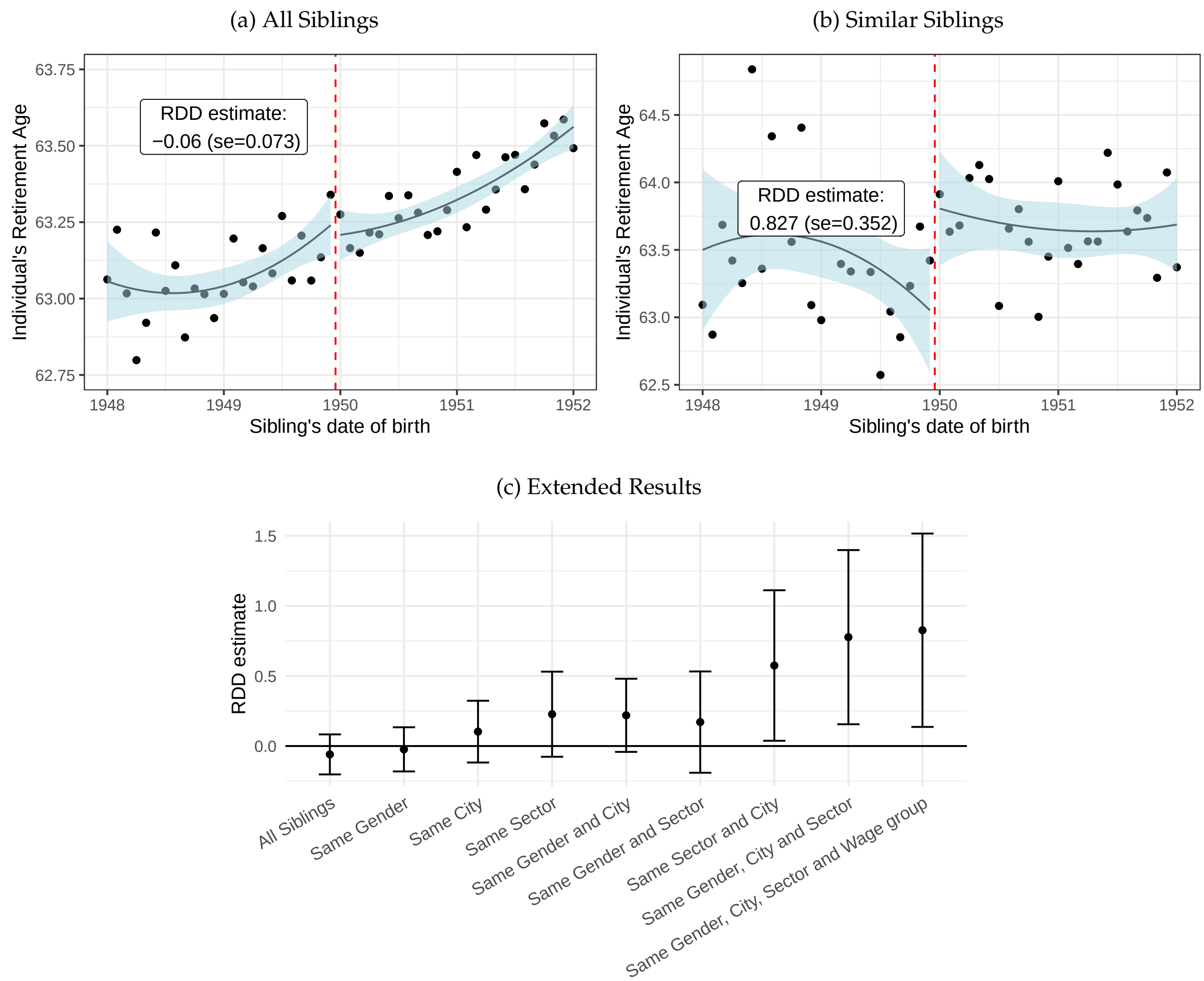


(b) Men



Notes: The figure shows individuals' average retirement ages by their spouse's birth date in monthly bins. Panel (a) focuses on women and Panel (b) focuses on men. In each panel, the red dashed line demarcates the January 1950 cutoff of the 2006 pension reform. The black solid lines depict a quadratic fit estimated separately on each side of the cutoff, with 95% confidence bands shaded in light blue. An estimate of the jump in retirement ages at the cutoff from a regression discontinuity design is included in the text box, with its standard error in parentheses.

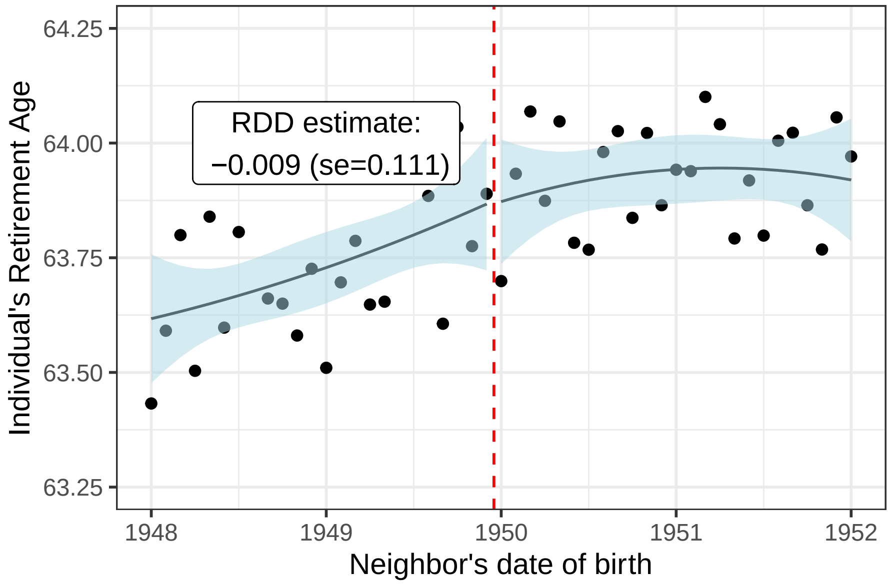
FIGURE 3: SPILLOVERS BETWEEN SIBLINGS



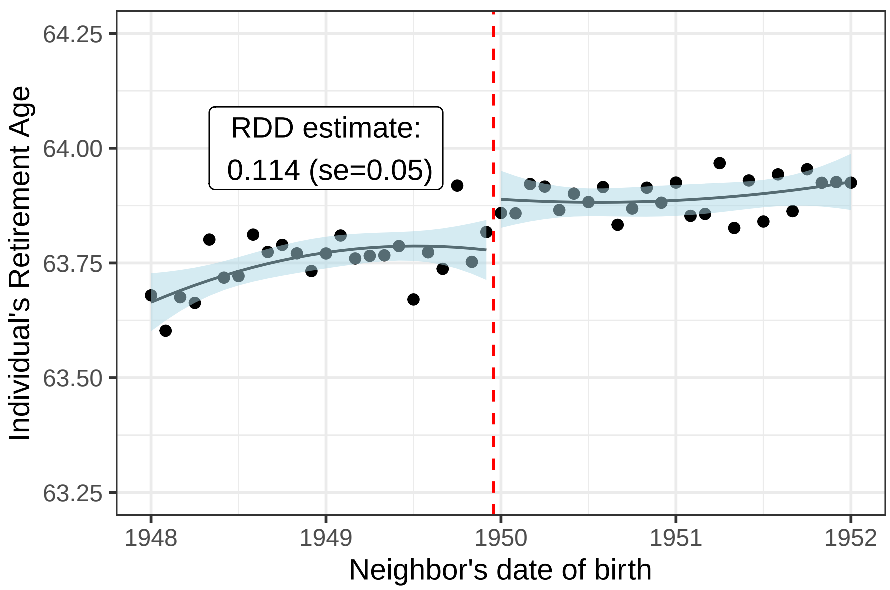
Notes: Panels (a) and (b) of the figure show individuals' average retirement ages by their sibling's birth date in monthly bins. Panel (a) includes all siblings and Panel (b) includes similar siblings who have the same gender, live in the city, work in the same 2-digit sector, and are in the same earnings group (above/below median). The red dashed line demarcates the January 1950 cutoff of the 2006 pension reform. The black solid lines depict a quadratic fit estimated separately on each side of the cutoff, with 95% confidence bands shaded in light blue. An estimate of the jump in retirement ages at the cutoff from a regression discontinuity design (RDD) is included in the text box, with its standard error in parentheses. Panel (c) shows an extended set of RDD estimates with 95% confidence intervals for sub-samples of siblings indicated by the labels on the horizontal axis. All results displayed in the figure correspond to spillover effects on older siblings. Analogous results for younger siblings are shown in Appendix Figure A3.

FIGURE 4: SPILLOVERS BETWEEN NEIGHBORS

(a) All Neighbors



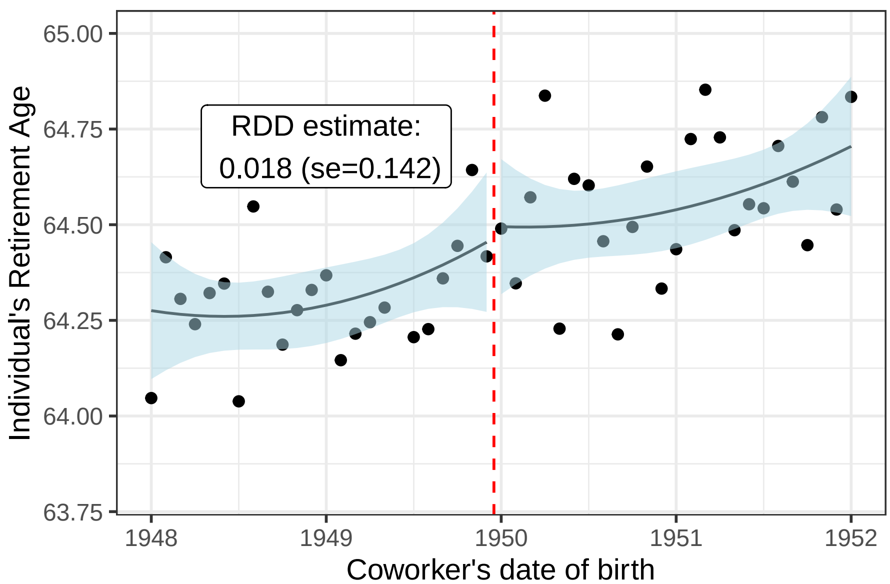
(b) Similar Neighbors



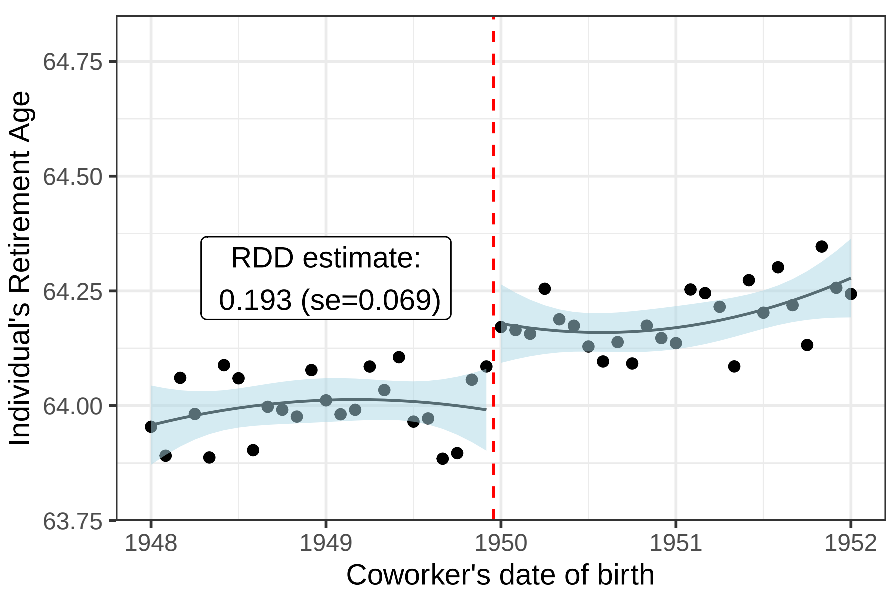
Notes: The figure shows individuals' average retirement ages by their neighbor's birth date in monthly bins. In Panel (a), neighborhood peers are selected out of all neighbors. Panel (b) uses individuals' most similar neighbors based on 2-digit sector of work and an indicator for being foreign-born as peers. In each panel, the red dashed line demarcates the January 1950 cutoff of the 2006 pension reform. The black solid lines depict a quadratic fit estimated separately on each side of the cutoff, with 95% confidence bands shaded in light blue. An estimate of the jump in retirement ages at the cutoff from a regression discontinuity design is included in the text box, with its standard error in parentheses. An extended set of RDD estimates is shown in Appendix Figure A8.

FIGURE 5: SPILLOVERS BETWEEN COWORKERS

(a) All Coworkers



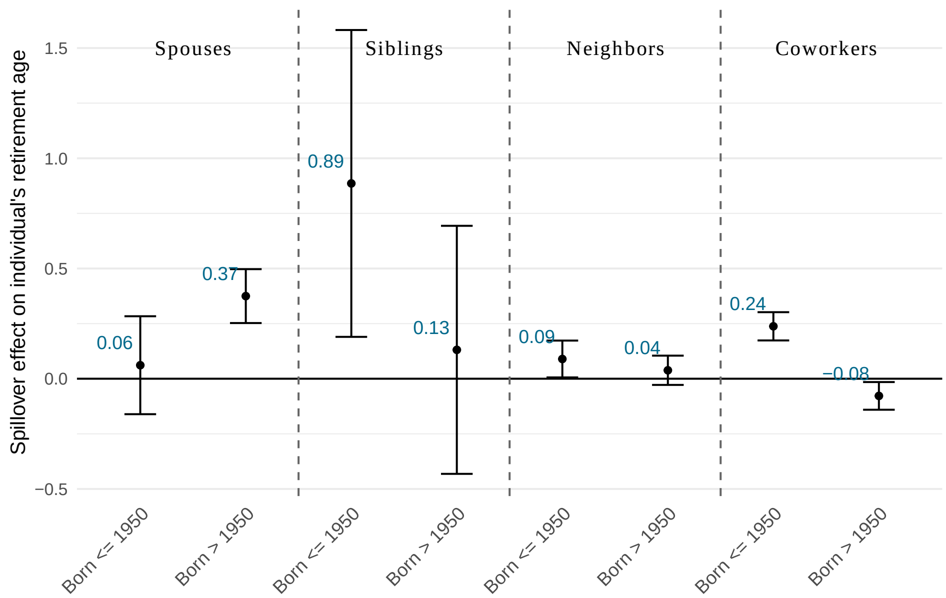
(b) Similar Coworkers



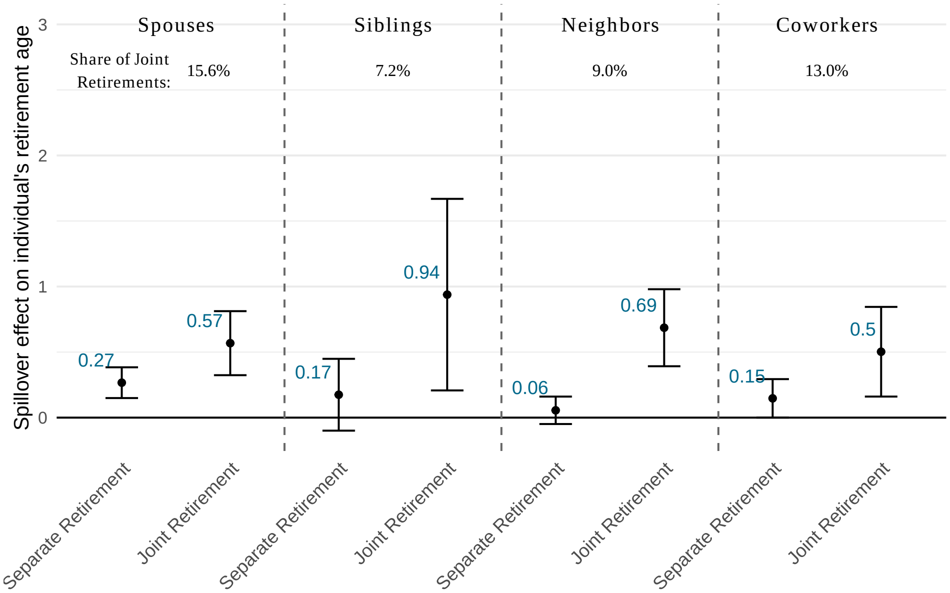
Notes: Panels (a) and (b) of the figure shows individuals' average retirement ages by their coworker's birth date in monthly bins. In Panel (a), workplace peers are selected out of all coworkers. Panel (b) uses individuals' most similar coworkers based on an indicator for being foreign-born, gender, city of residence and earnings group (above/below median) as peers. In each panel, the red dashed line demarcates the January 1950 cutoff of the 2006 pension reform. The black solid lines depict a quadratic fit estimated separately on each side of the cutoff, with 95% confidence bands shaded in light blue. An estimate of the jump in retirement ages at the cutoff from a regression discontinuity design is included in the text box, with its standard error in parentheses. An extended set of RDD estimates is shown in Appendix Figure A8.

FIGURE 6: EVIDENCE ON MECHANISMS

(a) Spillover Effects by Own Reform Exposure



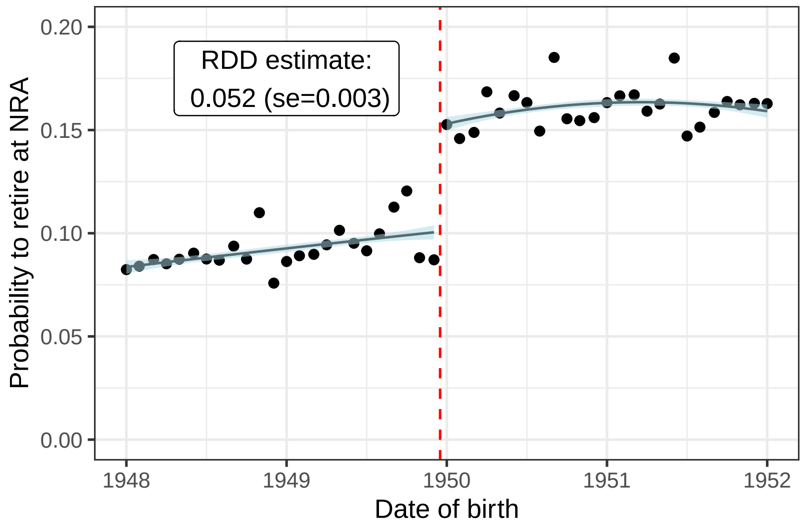
(b) The Role of Joint Retirement



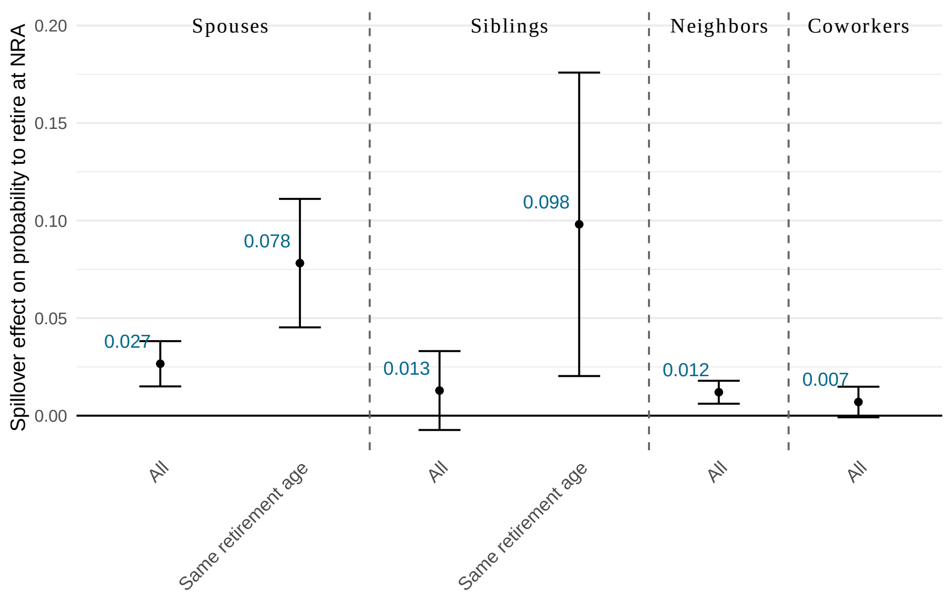
Notes: Panel (a) of the figure shows estimated retirement spillovers by individuals' own reform exposure. The results for spouses and siblings are obtained via a regression discontinuity design (RDD), where the respective sample is split into individuals born below vs. above the January 1950 cutoff of the reform. See Appendix Figures A9 for full RDD results. The results for neighbors and coworkers are obtained via the instrumental variable (IV) approach, introducing an interaction effect between own reform exposure and peers' leave-out average reform exposure. The effects shown here are based on the estimates from Column (3) of Table 3 and Column (3) of Table 4. Panel (b) of the figure displays retirement spillovers among pairs retiring jointly vs. retiring separately. The results for all peer groups are obtained via an RDD, where the sample is split into individuals retiring within +/- 6 months of their peer's retirement date vs. individuals retiring outside of this window. See Appendix Figure A10 for full RDD results.

FIGURE 7: PROPAGATION OF SOCIAL NORMS

(a) First Stage (Full Sample)

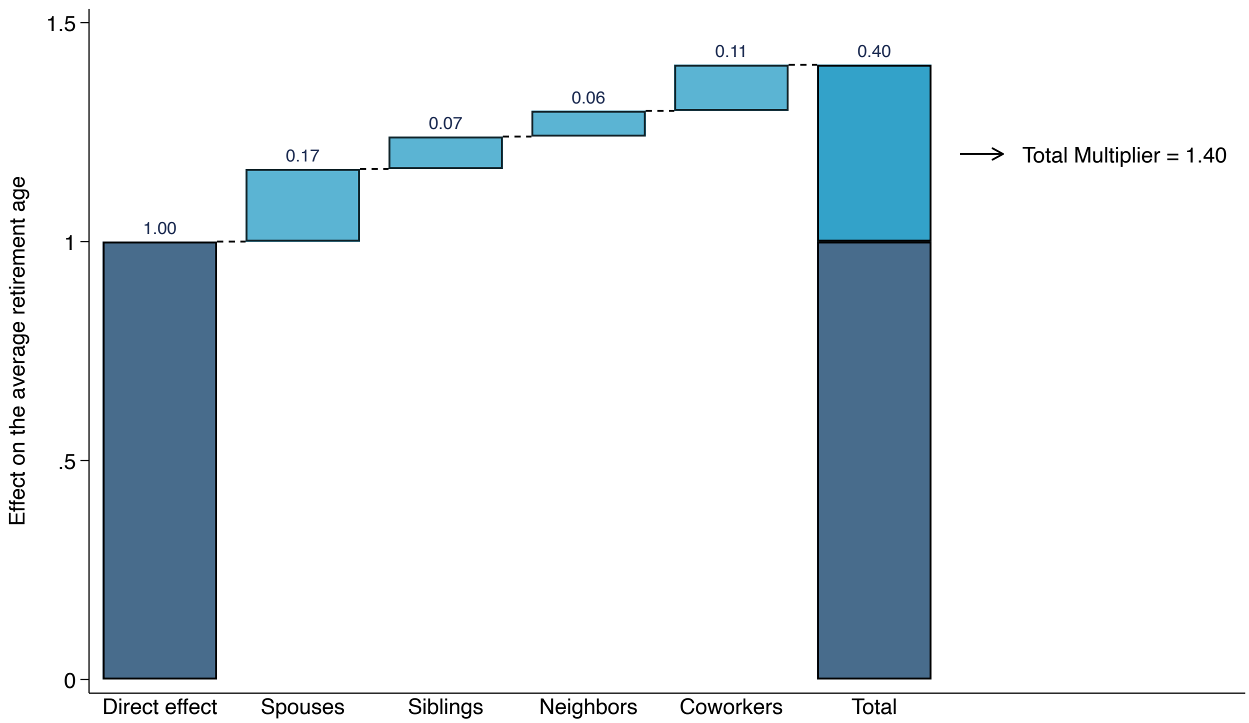


(b) Spillover Effects



Notes: Panel (a) of the figure shows individuals' probability of retiring at the Normal Retirement by their own birth date in monthly bins. The red dashed line demarcates the January 1950 cutoff of the 2006 pension reform. The black solid lines depict a quadratic fit estimated separately on each side of the cutoff, with 95% confidence bands shaded in light blue. An estimate of the jump in the share retiring at the NRA at the cutoff from a regression discontinuity design (RDD) is included in the text box, with its standard error in parentheses. Panel (b) of the figure shows estimated spillover effects on the probability of retiring exactly at the NRA when a peer is exposed to the reform. The results for spouses and siblings are obtained via a regression discontinuity design (RDD). Estimates are shown for all individuals, as well as for those retiring at the same age (within +/- 3 months) as their peer. See Appendix Figures A11 for full RDD results. The results for neighbors and coworkers are obtained via the instrumental variable (IV) approach, using the probability of retiring at the NRA as the regression outcome. The effects shown here are based on the estimates from Column (5) of Table 3 and Column (5) of Table 4.

FIGURE 8: THE SOCIAL MULTIPLIER OF PENSION REFORM



Notes: The figure illustrates how we calculate the social multiplier of a pension reform. As the first bar on the left shows, we consider a pension reform with a direct effect on the average retirement of one year. The next four bars depict the relative magnitude of spillover effects onto spouses, siblings, neighbors, and coworkers based on our estimation results. The last bar on the right illustrates the overall social multiplier aggregated across all peer groups.

TABLE 1: 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		

Notes: The table shows summary statistics of our data. The statistics shown for siblings, neighbors and coworkers reflect the number and characteristics of these peers within our sample. "S.D." denotes standard deviation.

Table 2: Regression Discontinuity Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Couples		Siblings		Neighbors		Coworkers	
	Men	Women	All	Similar	All	Similar	All	Similar
Panel A: First Stage	*Outcome: Peer's Retirement Age*							
Peer's birth date above 1950 cutoff	0.314^{***}	0.406^{***}	0.555^{***}	0.980^{***}	0.331^{**}	0.474^{***}	0.319^{**}	0.397^{***}
	(0.055)	(0.053)	(0.068)	(0.331)	(0.108)	(0.049)	(0.142)	(0.068)
Panel B: Spillover Effect (Reduced Form)	*Outcome: Individual Retirement Age*							
Peer's birth date above 1950 cutoff	0.023	0.309^{***}	-0.060	0.827^{**}	-0.009	0.114^{**}	0.018	0.193^{***}
	(0.057)	(0.054)	(0.073)	(0.352)	(0.111)	(0.050)	(0.142)	(0.069)
Panel C: Spillover Effect (Scaled by First Stage)	*Outcome: Individual Retirement Age*							
Peer's birth date above 1950 cutoff	0.075	0.762^{***}	-0.108	0.844^{*}	-0.028	0.241^{**}	0.058	0.486^{**}
	(0.182)	(0.166)	(0.132)	(0.459)	(0.336)	(0.109)	(0.447)	(0.193)
Observations in bandwidth	106,867	109,245	74,893	3,311	29,610	144,291	14,085	54,174
F-statistic (first stage)	32.06	58.86	66.51	8.76	9.43	94.61	5.01	33.90

Notes: The table summarizes regression discontinuity estimates for all peer groups. Column titles indicate the respective type of peer. Panel A shows first-stage results for each sample. The coefficients shown are obtained from a regression discontinuity design (RDD), estimating the impact of the respective peer's reform exposure on their own retirement age. Panel B displays spillover effects obtained from an RDD estimating the impact of the peer's reform exposure on the individual's retirement age. The estimates in Panel B corresponds to the reduced-form spillover effects depicted in Figures 2 to 5. Panel C shows spillover effects scaled relative to the respective first-stage effect. Standard errors are in parantheses. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

TABLE 3: INSTRUMENTAL VARIABLE ESTIMATES FOR NEIGHBORS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Main Results			Social Norms			Placebo Results	
	First Stage: Neighbors' retirement age	*Spillover:* Individual retirement age		*First Stage:* Fraction of neighbors retiring at NRA	*Spillover:* Probability to retire at NRA		*Spillover:* Individual retirement age	*Spillover:* Probability to retire at NRA
Fraction of neighbors treated	1.022***	0.060**	0.090**	0.095***	0.012***	0.016***	0.027	0.003
	(0.034)	(0.028)	(0.043)	(0.004)	(0.003)	(0.005)	(0.026)	(0.003)
Individual treated × fraction of neighbors treated			-0.051			-0.009		
			(0.051)			(0.006)		
Observations	1,314,479	1,314,479	1,314,479	1,314,479	1,314,479	1,314,479	1,310,599	1,310,599
R-squared	0.267	0.060	0.060	0.234	0.038	0.038	0.060	0.038
F-statistic (first stage)	906.37			490.16				

Notes: The table shows results from the instrumental variable estimation for neighbors. The respective regression outcomes are indicated by the column titles. All columns include fixed effects for wider neighborhoods (4-digit postcodes) and gender interacted with month-by-year of birth fixed effects as controls. Standard errors clustered by 5-digit postcode areas in parantheses. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

TABLE 4: INSTRUMENTAL VARIABLE ESTIMATES FOR COWORKERS

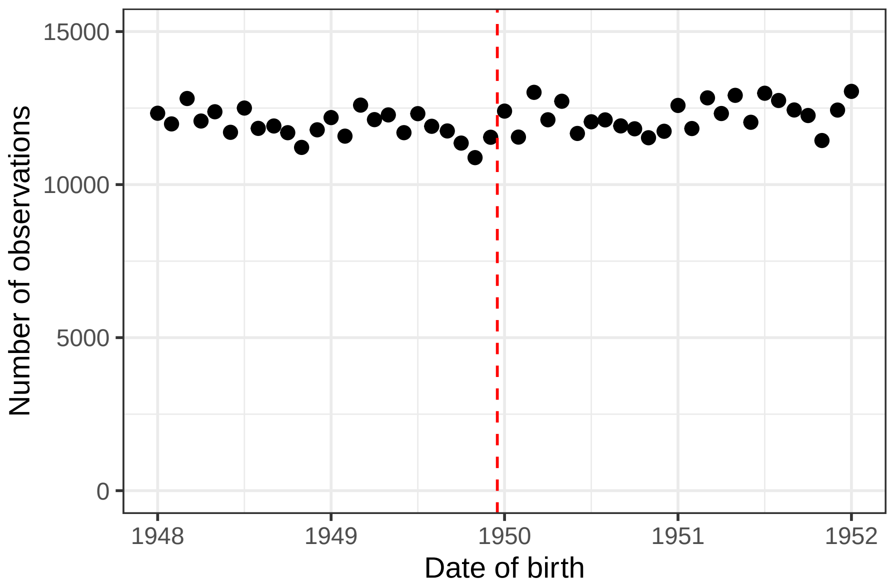
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Main Results			Social Norms			Placebo Results	
	First Stage: Coworkers' retirement age	*Spillover:* Individual retirement age		*First Stage:* Fraction of coworkers retiring at NRA	*Spillover:* Probability to retire at NRA		*Spillover:* Individual retirement age	*Spillover:* Probability to retire at NRA
Fraction of coworkers treated	0.827***	0.087***	0.238***	0.082***	0.007	0.031***	0.019	0.000
	(0.023)	(0.024)	(0.033)	(0.004)	(0.004)	(0.005)	(0.025)	(0.004)
Individual treated × fraction of coworkers treated			-0.316***			-0.051***		
			(0.044)			(0.007)		
Observations	594,202	594,202	594,202	594,202	594,202	594,202	537,244	537,244
R-squared	0.178	0.078	0.078	0.212	0.056	0056	0.079	0.059
F-statistic (first stage)	1262.10			453.49				

Notes: The table shows results from the instrumental variable estimation for coworkers. The respective regression outcomes are indicated by the column titles. All columns include sector fixed effects and gender interacted with month-by-year of birth fixed effects as controls. Standard errors clustered by firm in parantheses. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Online Appendix

A Appendix Figures and Tables

FIGURE A1: DISTRIBUTION OF BIRTH DATES (FULL SAMPLE)



Notes: The figure shows the distribution of birth dates, the running variable for our regression discontinuity design, in monthly bins within the full sample. The red dashed line demarcates the January 1950 cutoff of the 2006 pension reform.

FIGURE A2: SPILLOVERS IN COUPLES

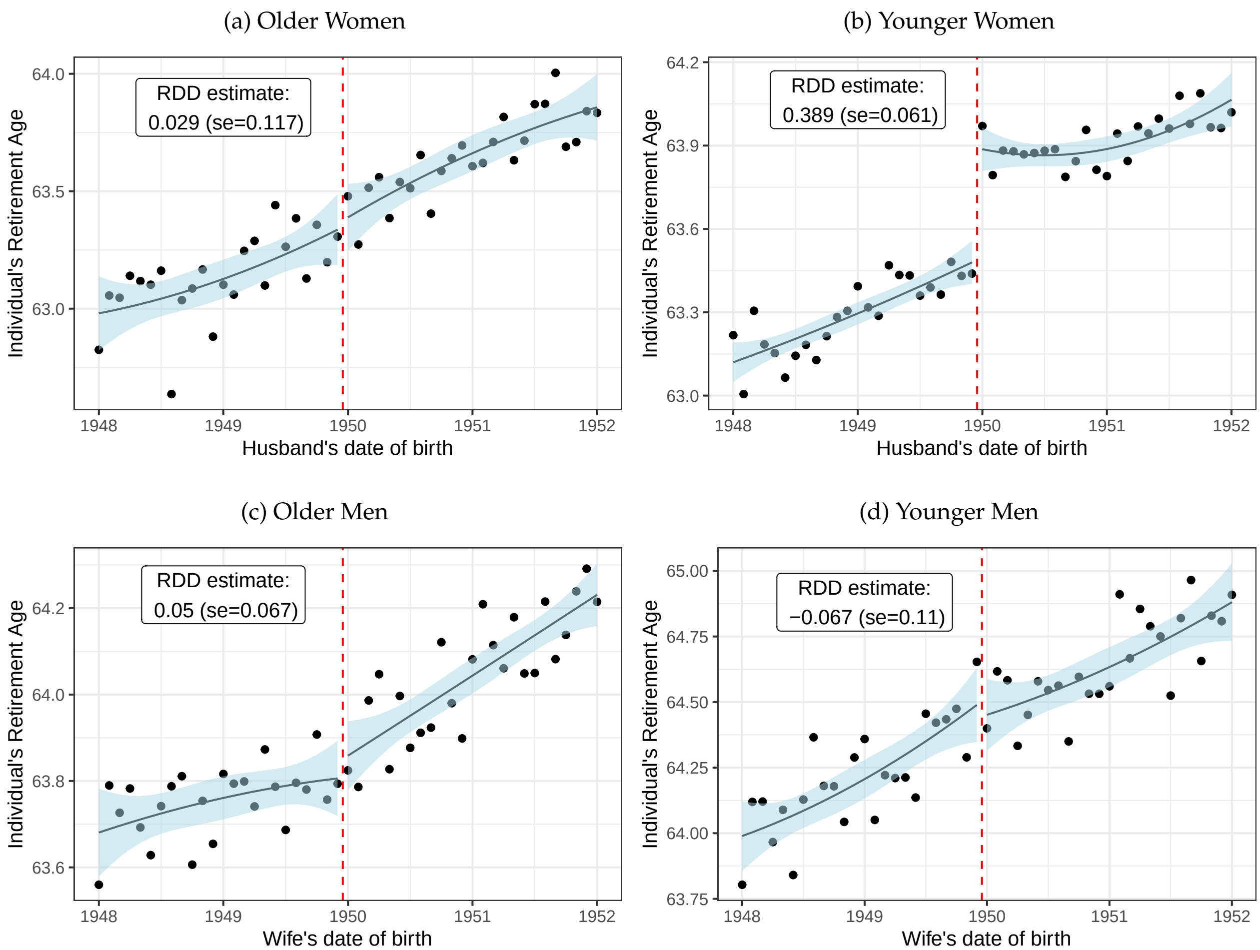


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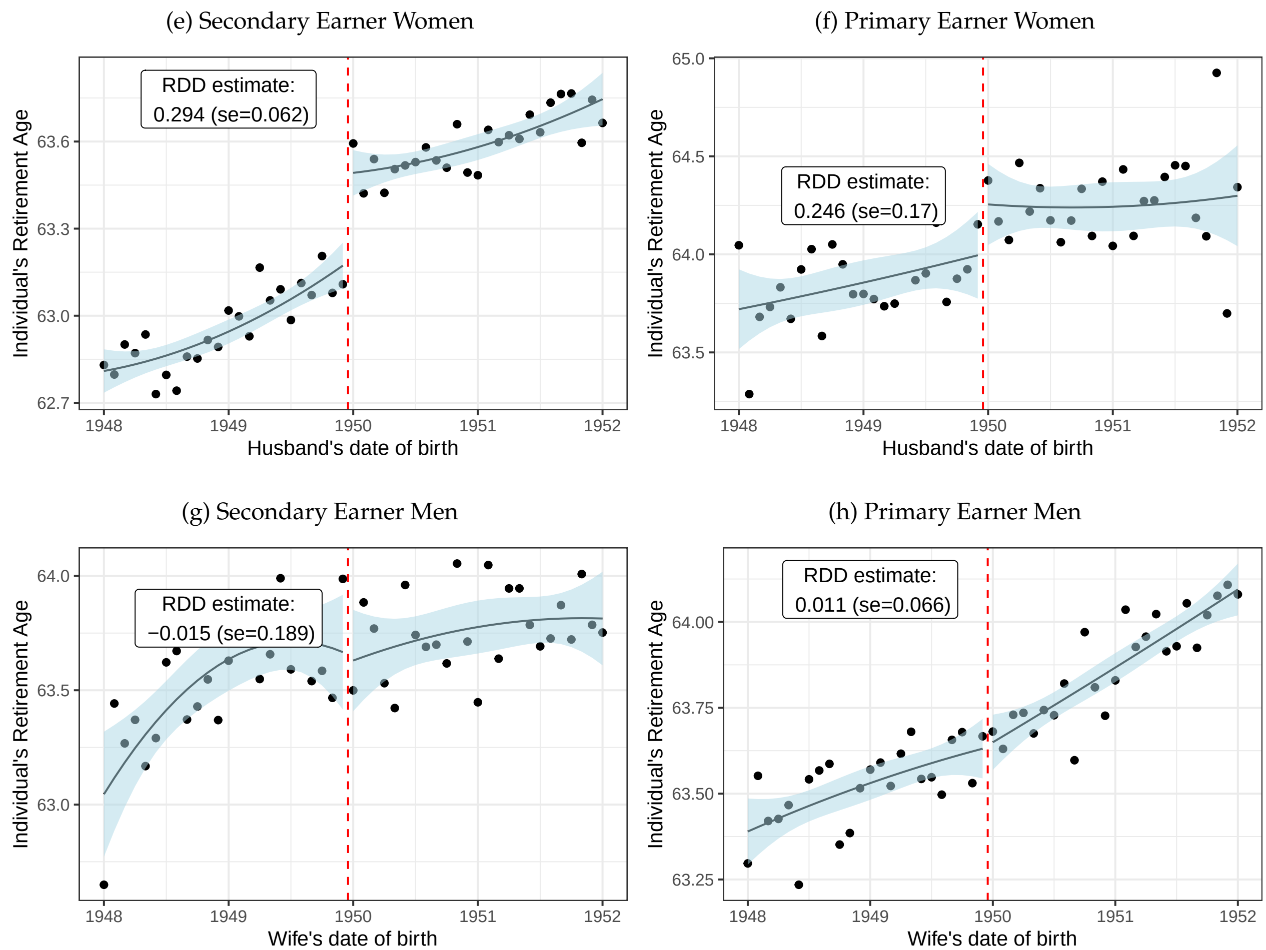
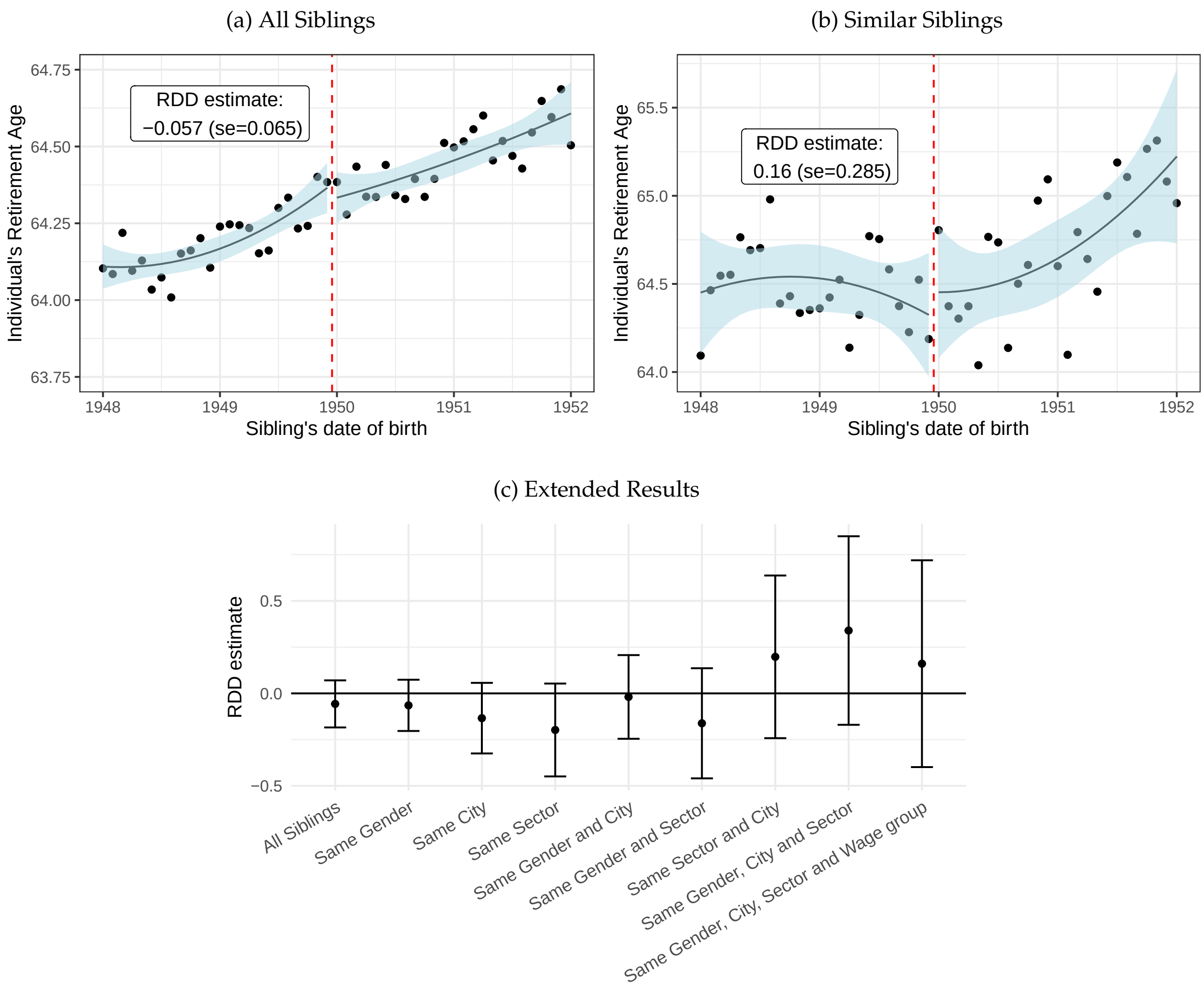
Figure A2: Spillovers in Couples (continued)



Notes: The figure shows individuals' average retirement ages by their spouse's birth date in monthly bins. The respective sample is indicated by the panel titles. The red dashed line demarcates the January 1950 cutoff of the 2006 pension reform. The black solid lines depict a quadratic fit estimated separately on each side of the cutoff, with 95% confidence bands shaded in light blue. An estimate of the jump in retirement ages at the cutoff from a regression discontinuity design is included in the text box, with its standard error in parentheses.

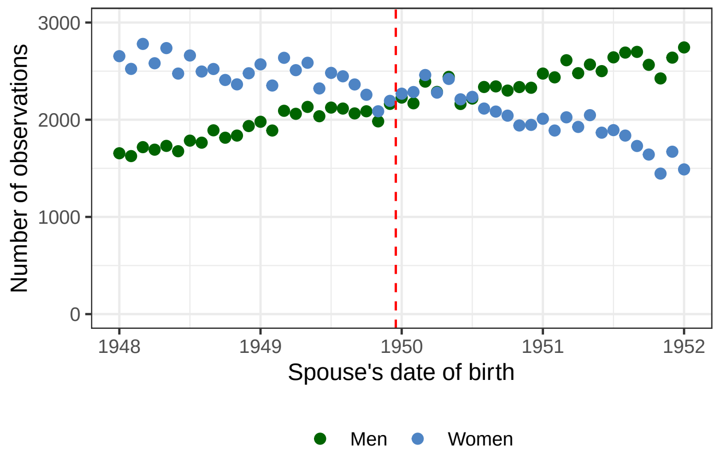
FIGURE A3: RESULTS FOR YOUNGER SIBLINGS



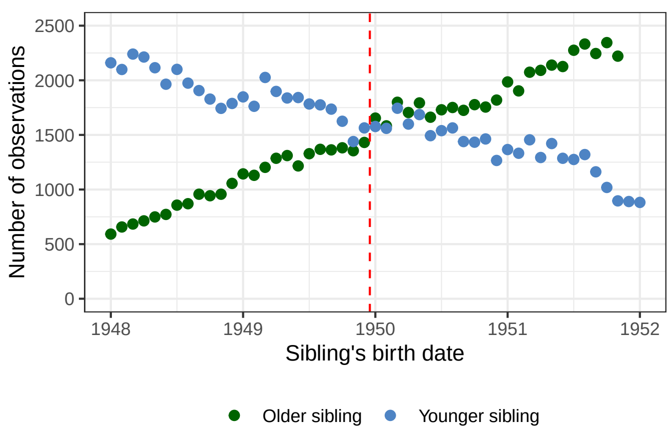
Notes: Panels (a) and (b) of the figure show individuals' average retirement ages by their sibling's birth date in monthly bins. Panel (a) includes all siblings and Panel (b) includes similar siblings who have the same gender, live in the city, work in the same 2-digit sector, and are in the same earnings group (above/below median). The red dashed line demarcates the January 1950 cutoff of the 2006 pension reform. The black solid lines depict a quadratic fit estimated separately on each side of the cutoff, with 95% confidence bands shaded in light blue. An estimate of the jump in retirement ages at the cutoff from a regression discontinuity design (RDD) is included in the text box, with its standard error in parentheses. Panel (c) shows an extended set of RDD estimates with 95% confidence intervals for sub-samples of siblings indicated by the labels on the horizontal axis. All results displayed in the figure correspond to spillover effects on younger siblings, complementing the results for older siblings shown in Figure 3.

FIGURE A4: DISTRIBUTION OF BIRTH DATES IN THE RDD SAMPLES

(a) Couples



(b) All Siblings



(c) Similar Siblings

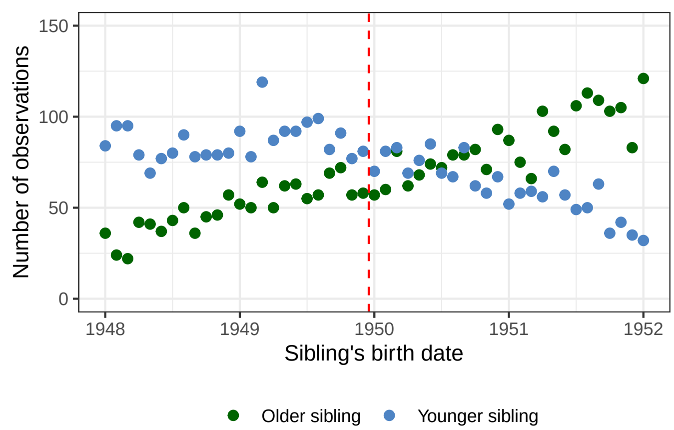


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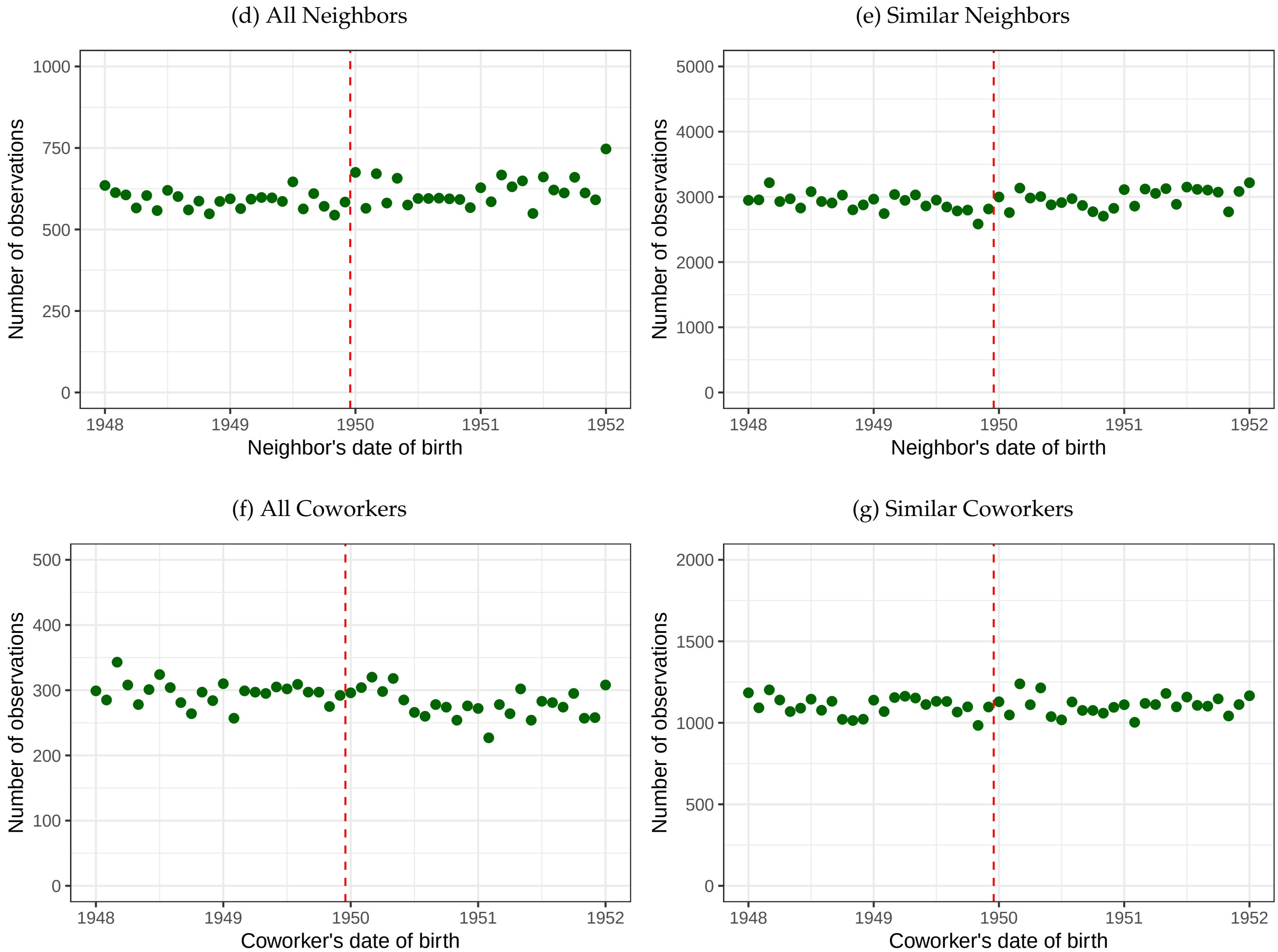
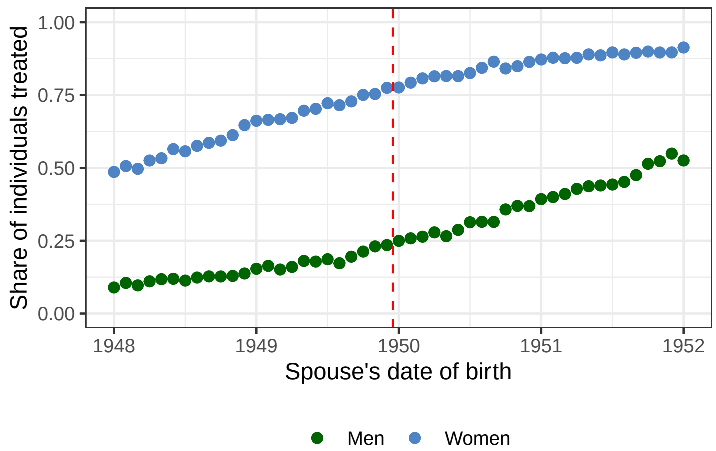
FIGURE A4: DISTRIBUTION OF BIRTH DATES IN THE RDD SAMPLES (CONTINUED)



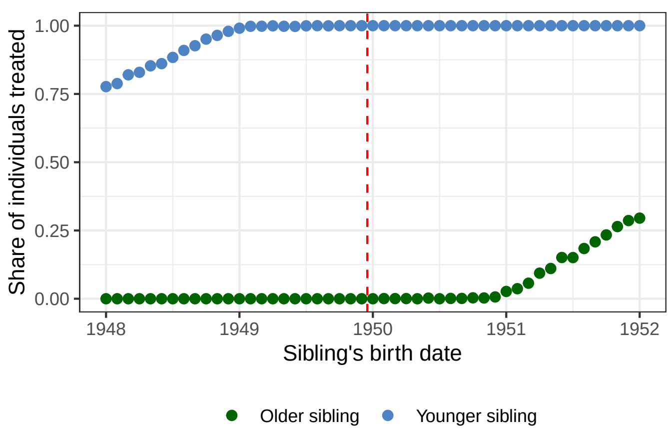
Notes: The figure shows the distribution of birth dates, the running variable for our regression discontinuity design, in monthly bins for each of the samples underlying our main regression discontinuity estimates. Panel titles and figure legends indicate the respective sample. The red dashed line demarcates the January 1950 cutoff of the 2006 pension reform.

FIGURE A5: TREATMENT EXPOSURE CORRELATION IN THE RDD SAMPLES

(a) Couples



(b) All Siblings



(c) Similar Siblings

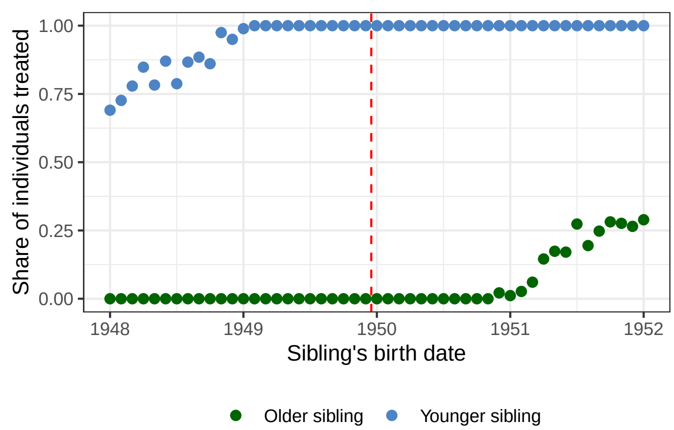


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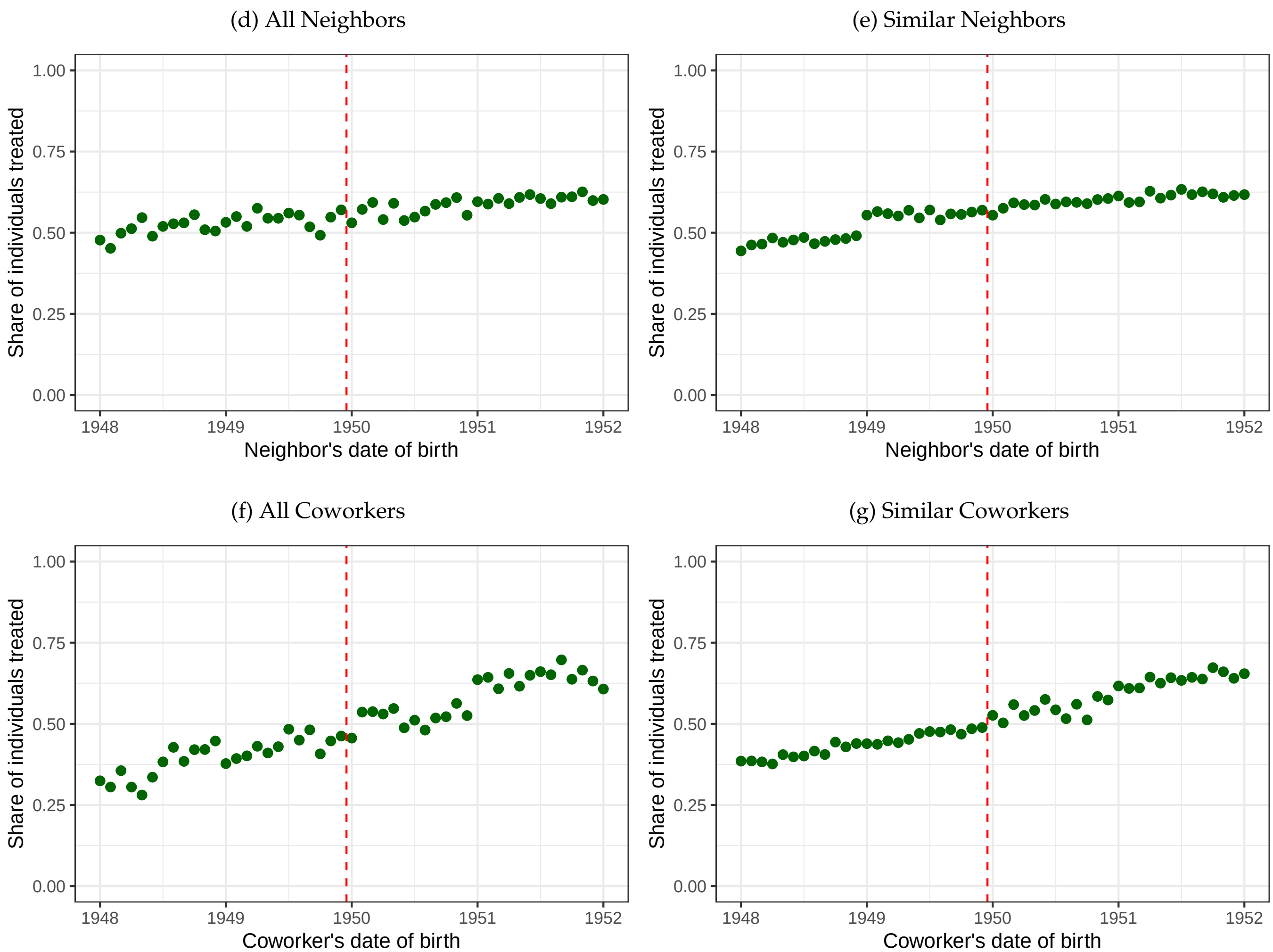
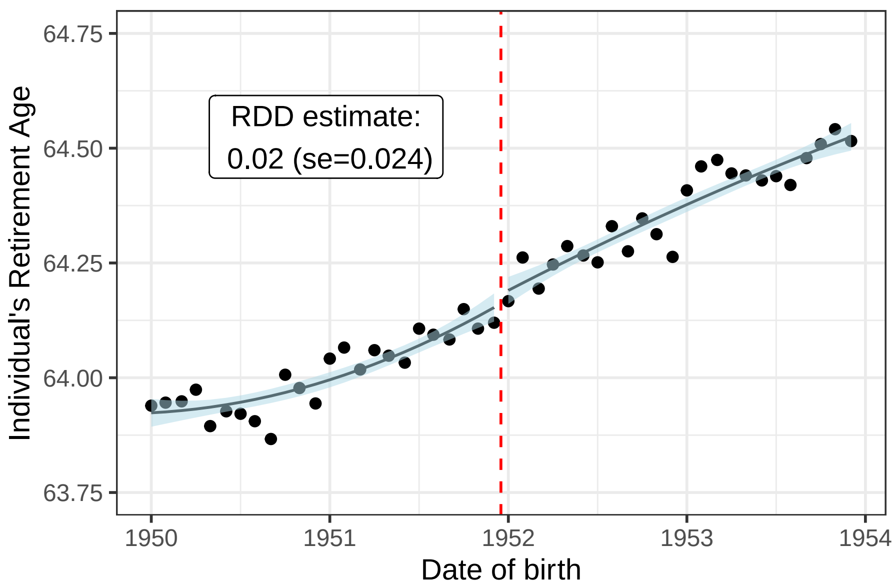
FIGURE A5: TREATMENT EXPOSURE CORRELATION IN THE RDD SAMPLES (CONTINUED)



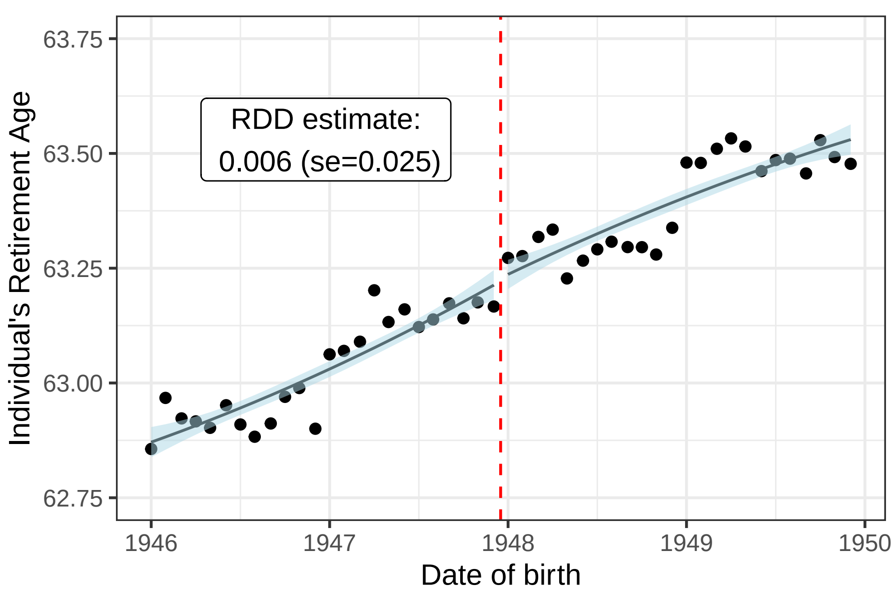
Notes: The figure shows the correlation of individuals' treatment exposure and peers' birth date for each of the samples underlying our main regression discontinuity estimates. Each panel displays the share of individuals exposed to the reform, i.e. born above the January 1950 cutoff, by the peer's birth date in monthly bins. Panel titles and figure legends indicate the respective sample. The red dashed line demarcates the January 1950 cutoff of the 2006 pension reform.

Figure A6: Placebo First-Stage Results

(a) 1952 Placebo Cutoff



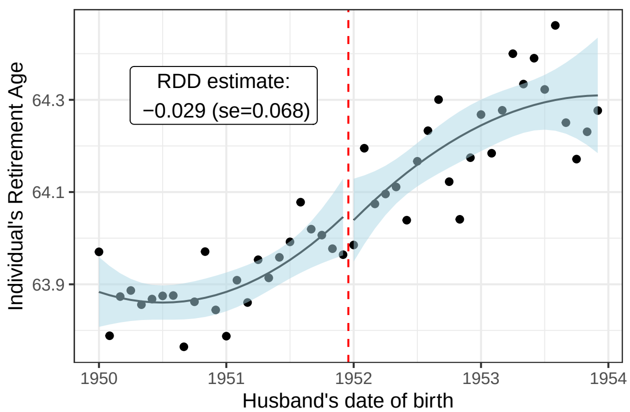
(b) 1948 Placebo Cutoff



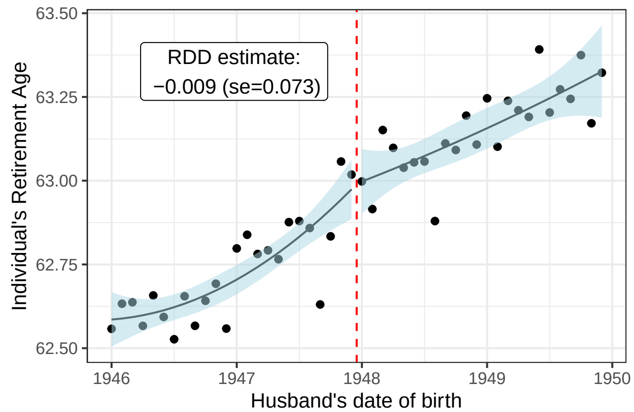
Notes: The figure shows placebo first-stage specifications around artificial reform cutoffs in January 1948 and January 1952, respectively. Each panel depicts individuals' average retirement ages by their own birth date in monthly bins. The red dashed line demarcates the respective artificial reform cutoff. The black solid lines depict a quadratic fit estimated separately on each side of the cutoff, with 95% confidence bands shaded in light blue. An estimate of the jump in retirement ages at the cutoff from a regression discontinuity design is included in the text box, with its standard error in parentheses.

Figure A7: Regression Discontinuity Placebo Checks

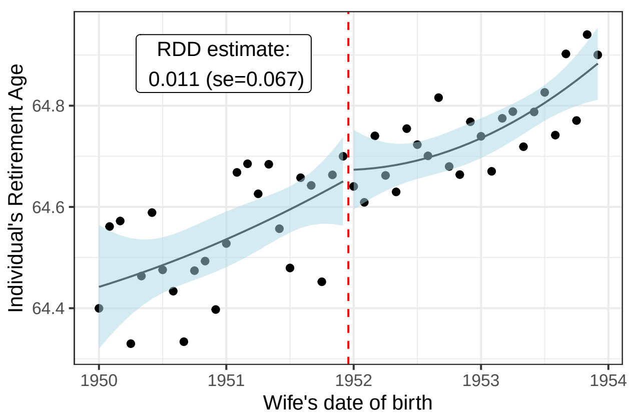
(a) Couples: Women, 1952 Placebo



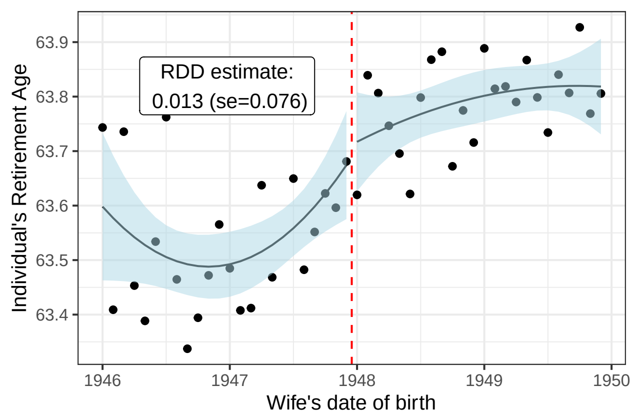
(b) Couples: Women, 1948 Placebo



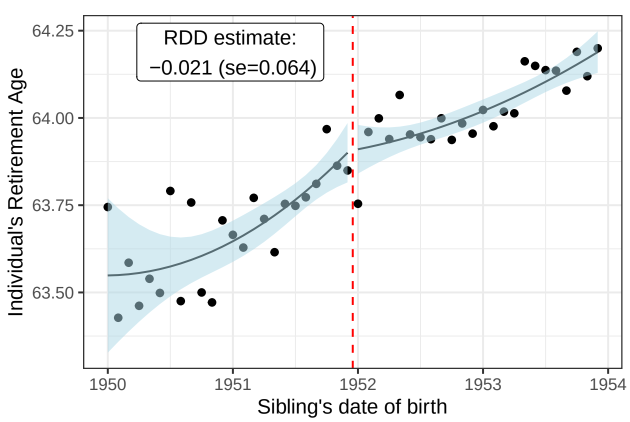
(c) Couples: Men, 1952 Placebo



(d) Couples: Men, 1948 Placebo



(e) All Siblings, 1952 Placebo



(f) All Siblings, 1948 Placebo

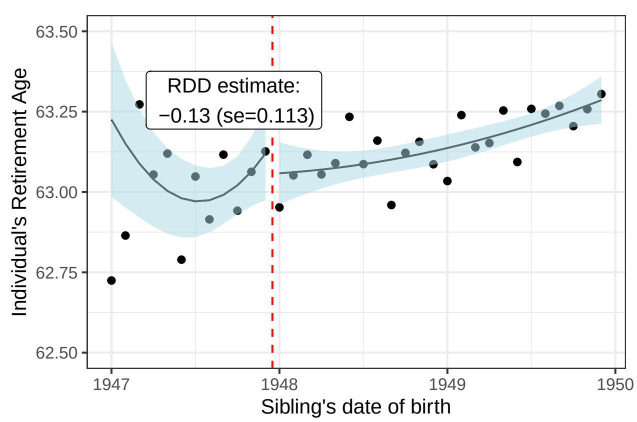


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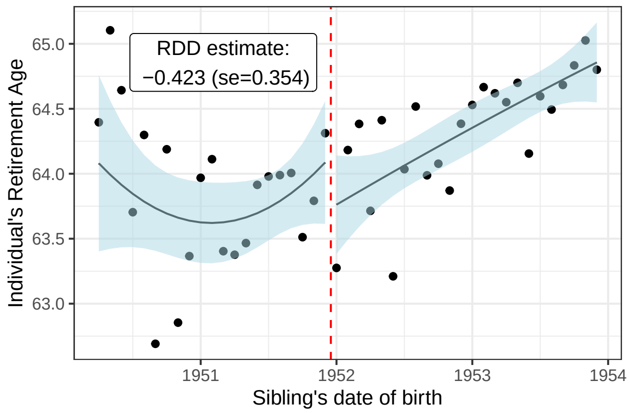
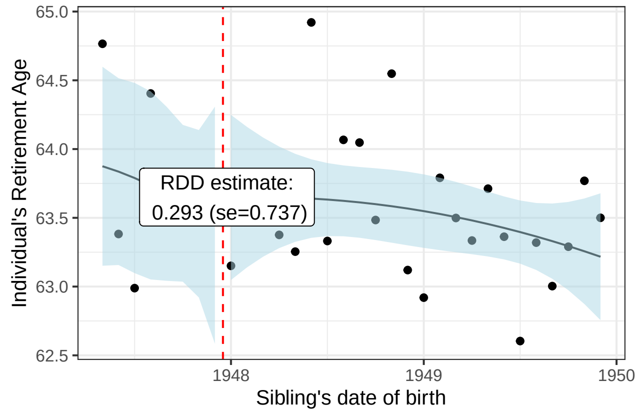
FIGURE A7: REGRESSION DISCONTINUITY PLACEBO CHECKS (CONTINUED)

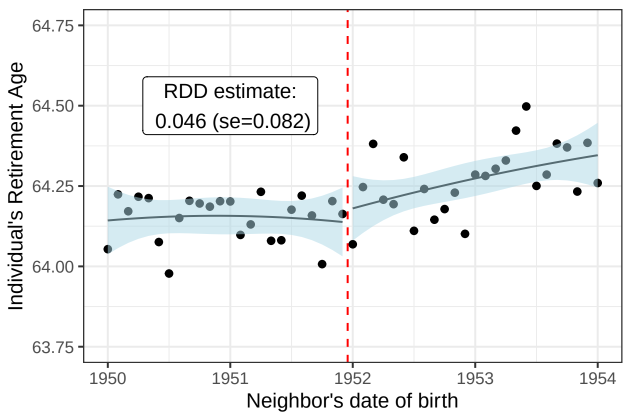
(g) Similar Siblings, 1952 Placebo



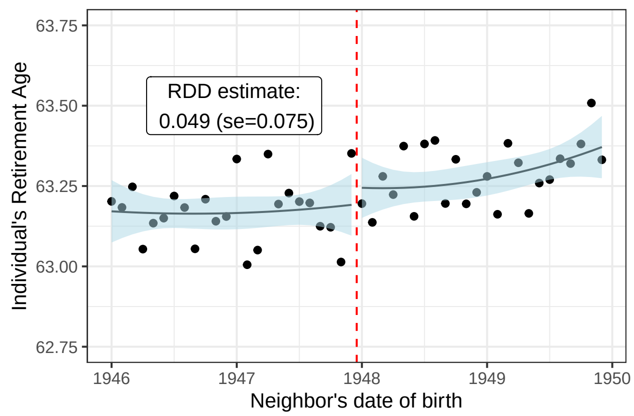
(h) Similar Siblings, 1948 Placebo



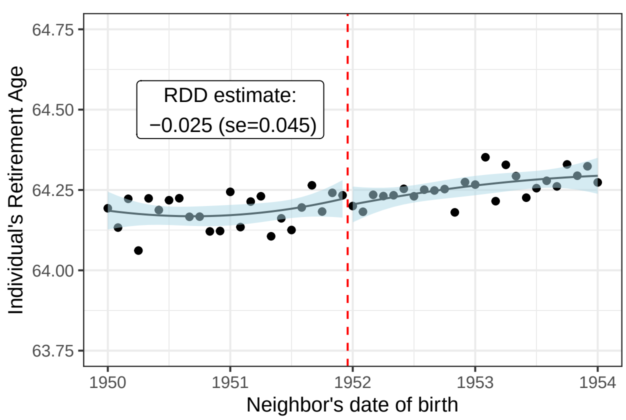
(i) All Neighbors, 1952 Placebo



(j) All Neighbors, 1948 Placebo



(k) Similar Neighbors, 1952 Placebo



(l) Similar Neighbors, 1948 Placebo

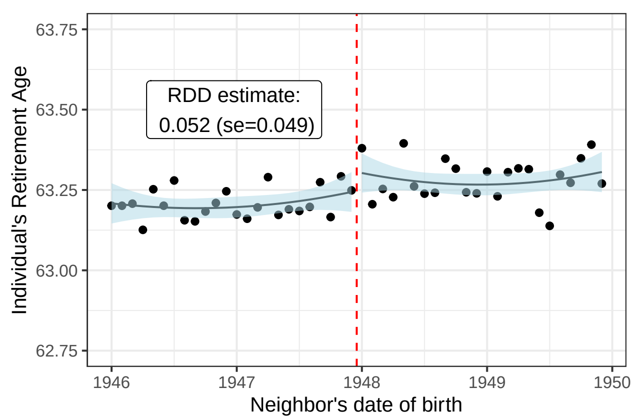


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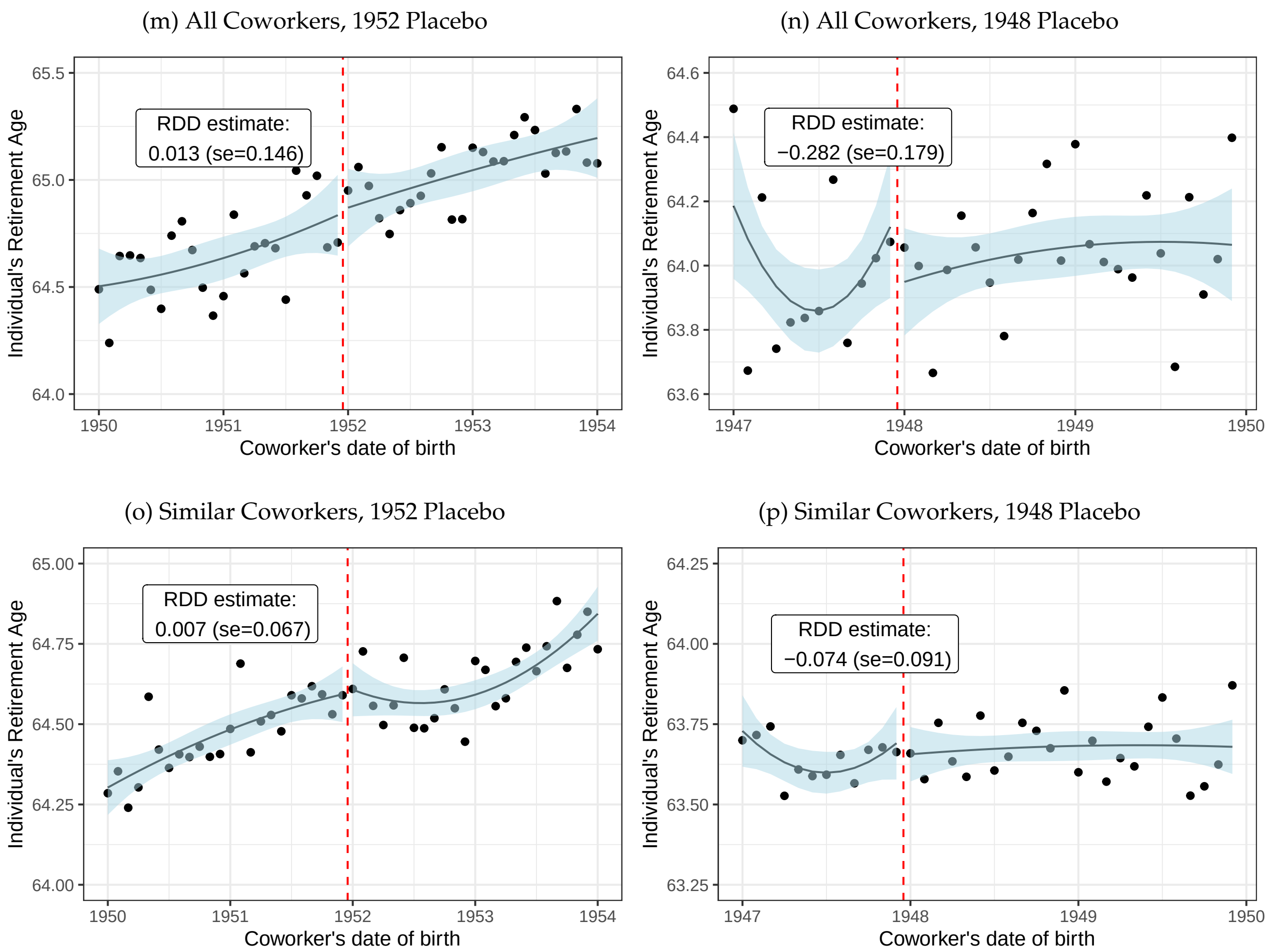
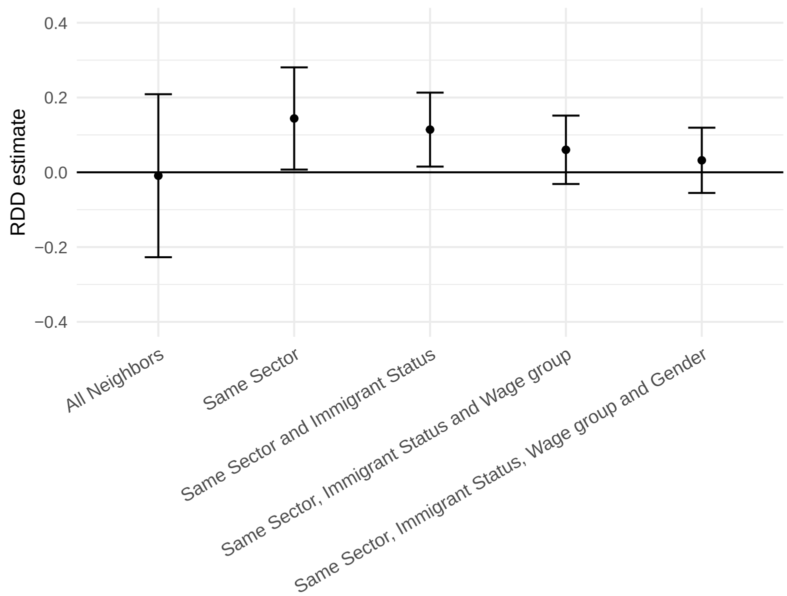
FIGURE A7: REGRESSION DISCONTINUITY PLACEBO CHECKS (CONTINUED)



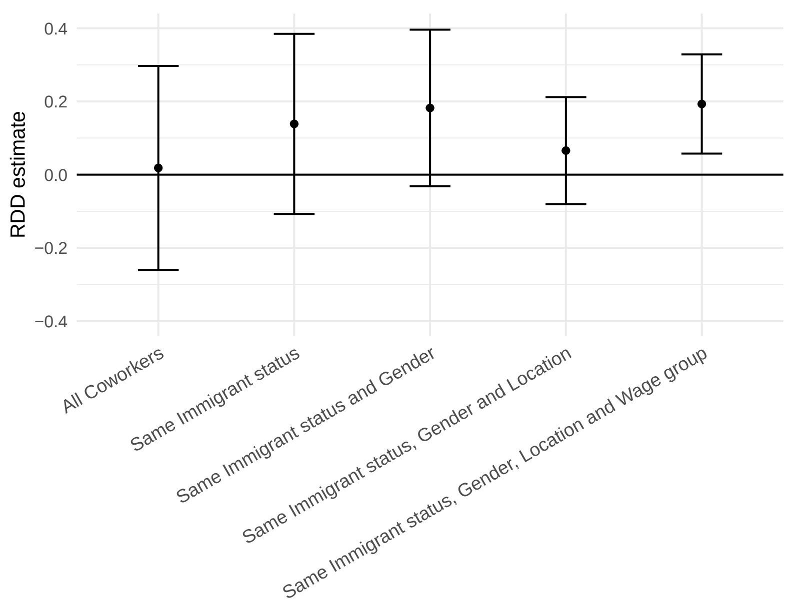
Notes: The figure shows placebo regression discontinuity design (RDD) specifications around artificial birth date cutoffs. Each panel displays individuals' average retirement ages by their peer's birth date in monthly bins. The respective placebo cutoff and sample are indicated by the panel titles. The red dashed line demarcates the placebo birth date cutoff. The black solid lines depict a quadratic fit estimated separately on each side of the cutoff, with 95% confidence bands shaded in light blue. An estimate of the jump in retirement ages at the cutoff from a regression discontinuity design is included in the text box, with its standard error in parentheses.

FIGURE A8: EXTENDED RESULTS FOR NEIGHBORS AND COWORKERS

(a) Neighbors

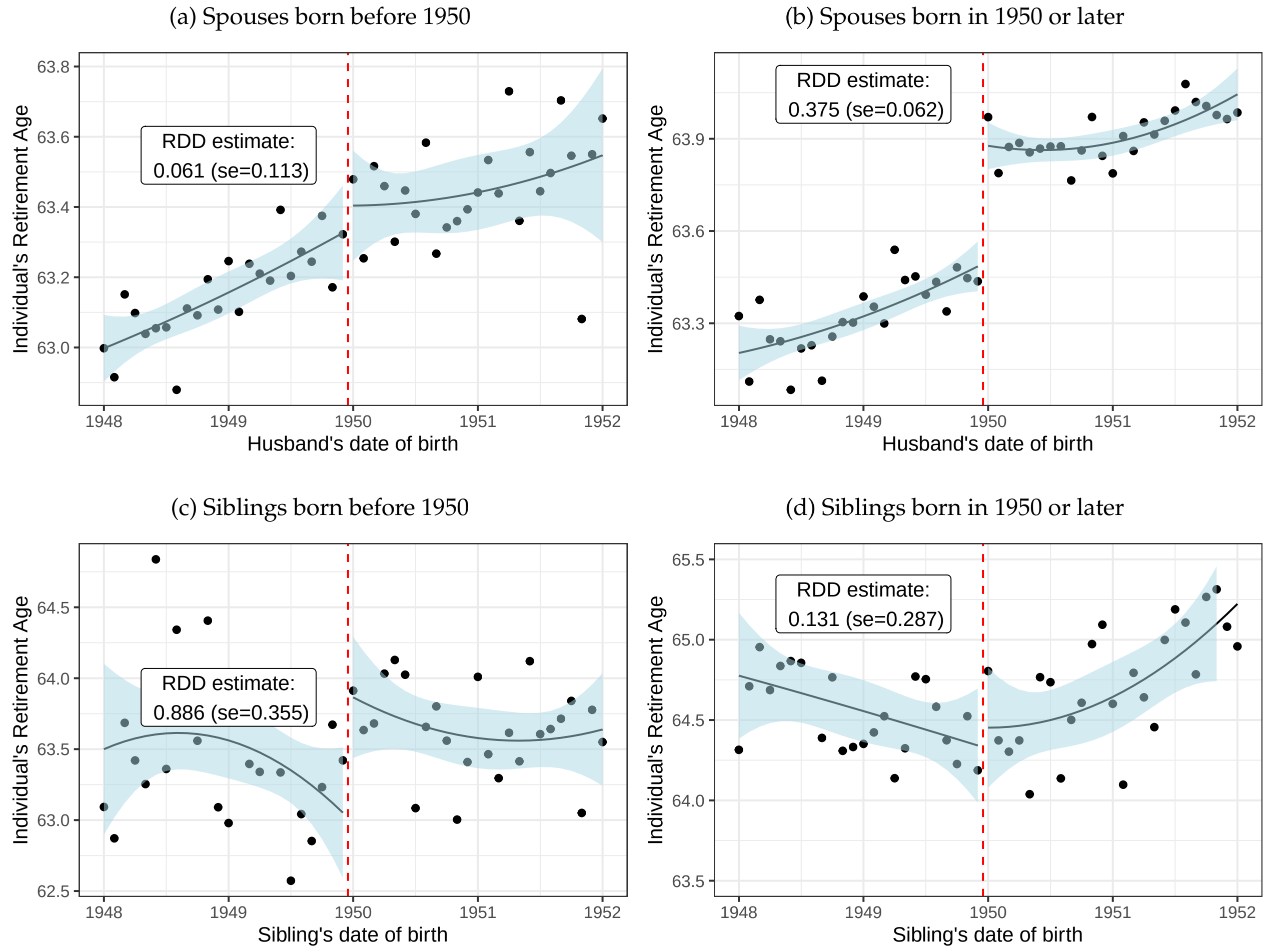


(b) Coworkers



Notes: The figure shows an extended set of regression discontinuity estimates for neighbors and coworkers. Both panels display spillover effects of a peer's reform exposure on an individual's retirement age estimated via a regression discontinuity design, with 95% confidence intervals. The respective sub-samples of neighbors or coworkers are indicated by the labels on the horizontal axis.

Figure A9: Spillover Effects by Own Reform Exposure



Notes: The figure shows individuals' average retirement ages by their spouse's or sibling's birth date in monthly bins. The respective sample is split individuals' own reform exposure, as indicated by the panel titles. In each panel, the red dashed line demarcates the January 1950 cutoff of the 2006 pension reform. The black solid lines depict a quadratic fit estimated separately on each side of the cutoff, with 95% confidence bands shaded in light blue. An estimate of the jump in retirement ages at the cutoff from a regression discontinuity design is included in the text box, with its standard error in parentheses.

FIGURE A10: THE ROLE OF JOINT RETIREMENT

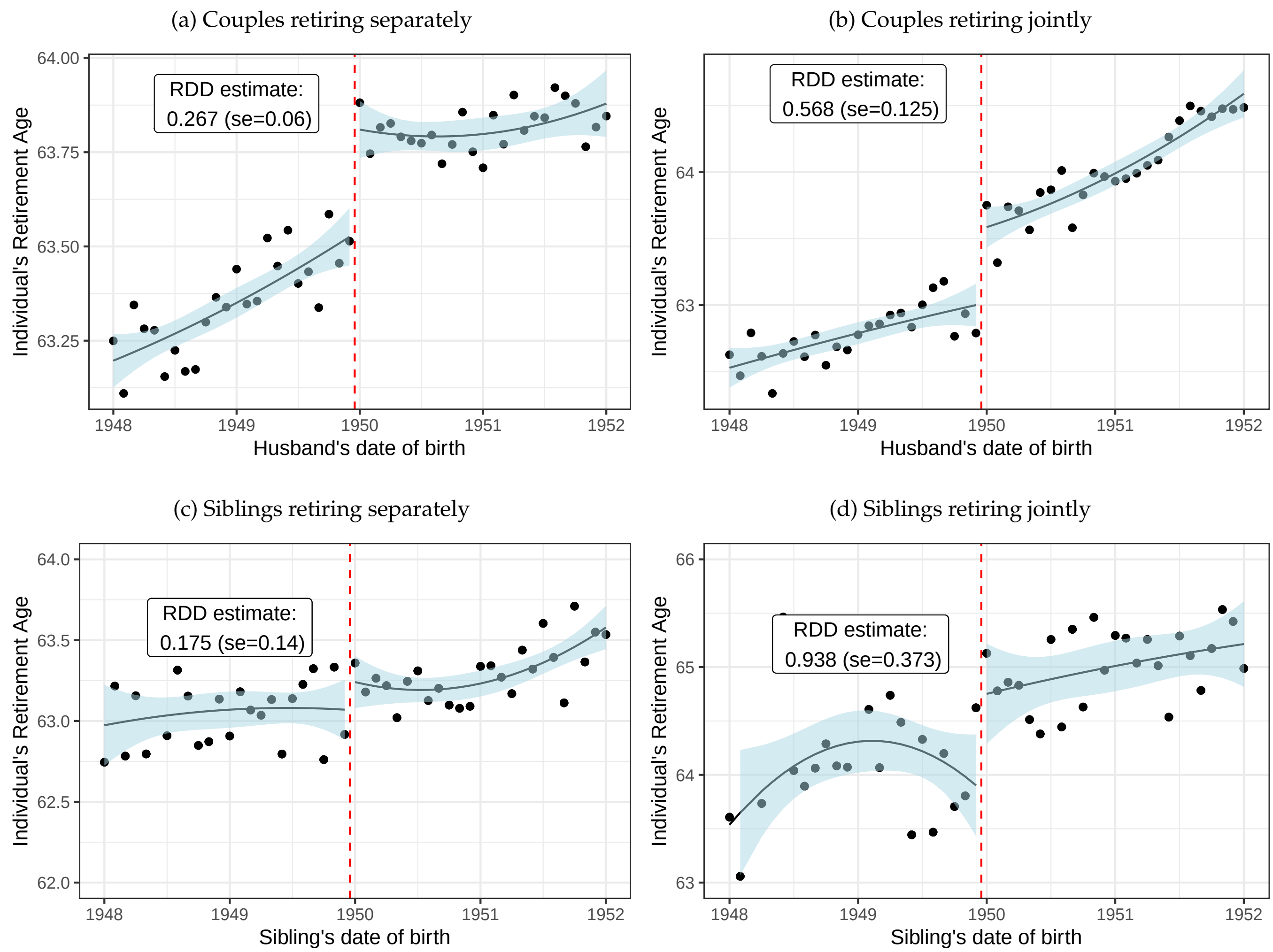


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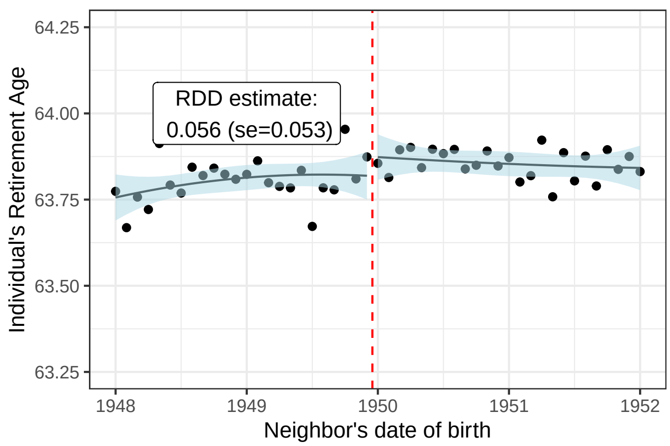
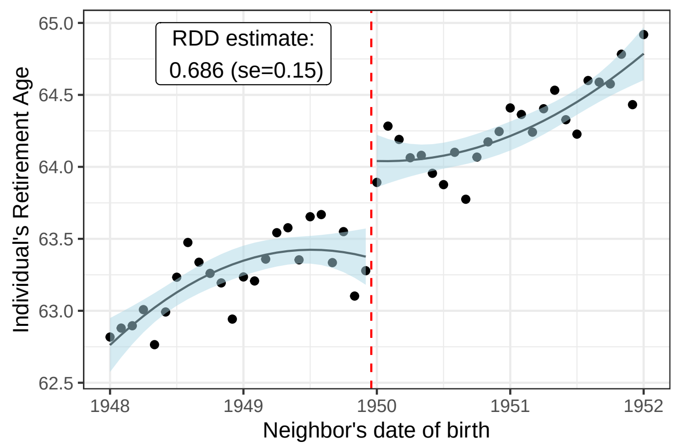
FIGURE A10: THE ROLE OF JOINT RETIREMENT (CONTINUED)

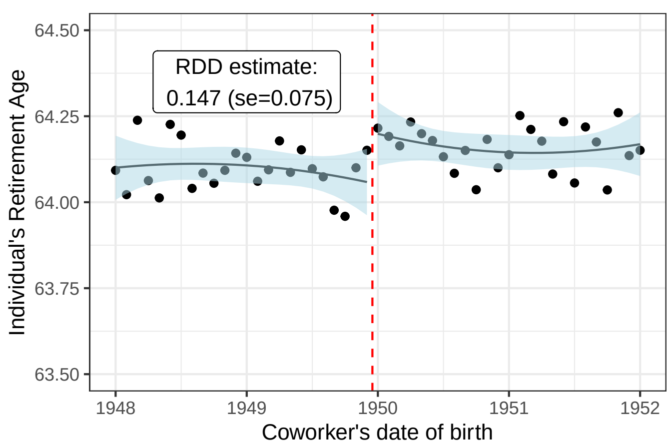
(e) Neighbors retiring separately



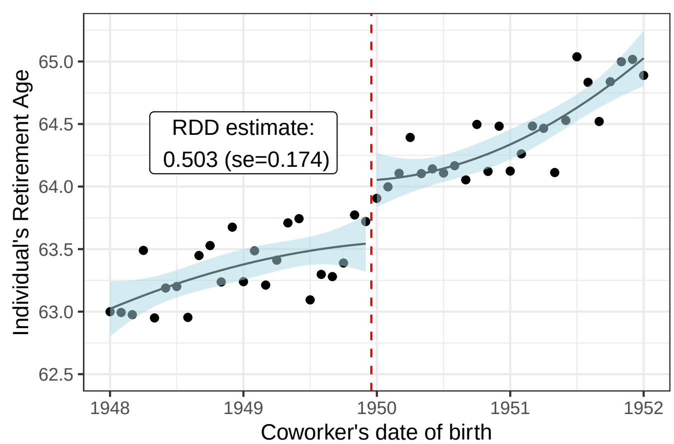
(f) Neighbors retiring jointly



(g) Coworkers retiring separately

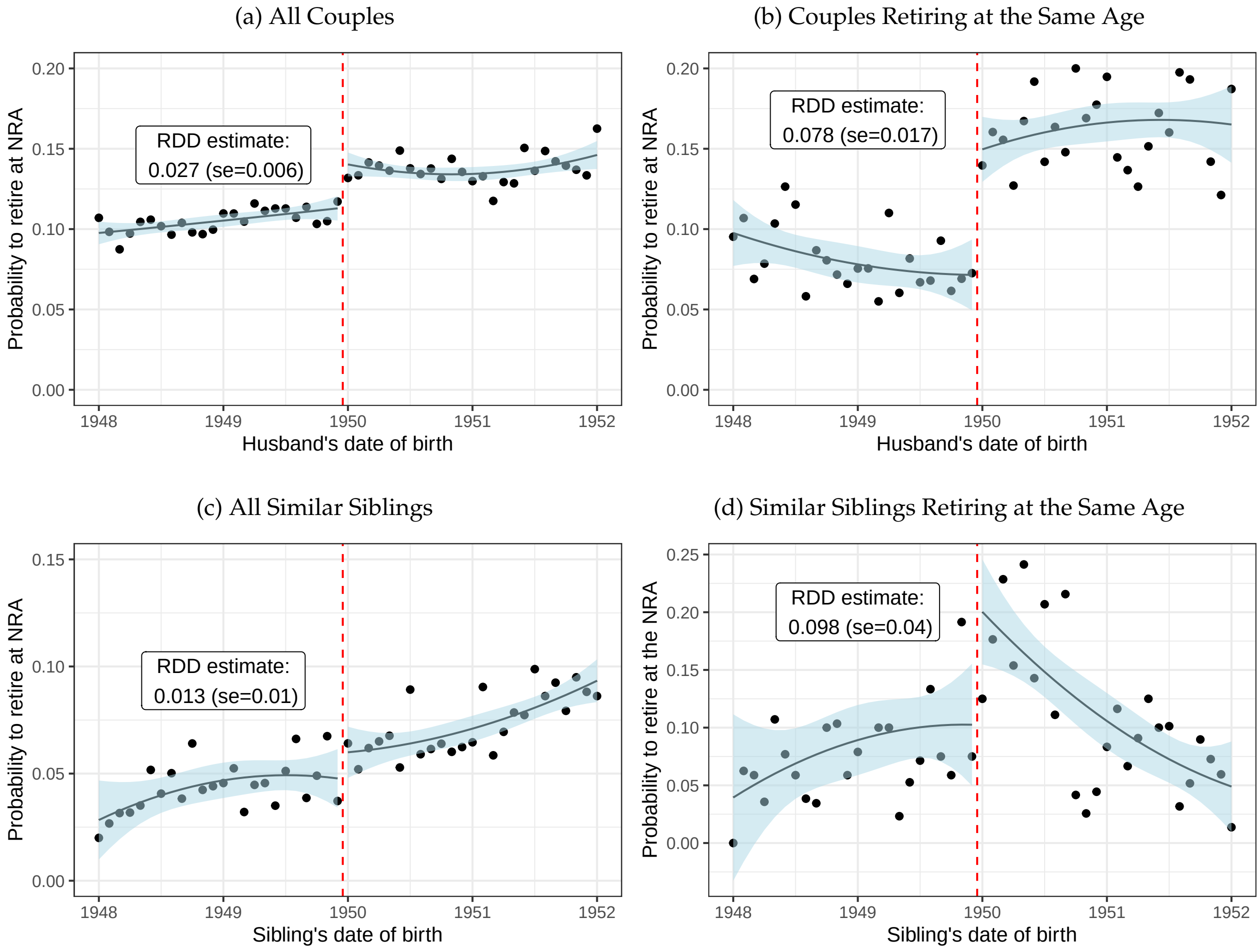


(h) Coworkers retiring jointly



Notes: The figure shows individuals' average retirement ages by their peer's birth date in monthly bins. The respective sample is split into pairs retiring jointly vs. pairs retiring separately, as indicated by the panel titles. Joint retirement is defined as individuals retiring within +/- 6 months of the peer's retirement date, and separate retirement is defined as individuals retiring outside of this window. In each panel, the red dashed line demarcates the January 1950 cutoff of the 2006 pension reform. The black solid lines depict a quadratic fit estimated separately on each side of the cutoff, with 95% confidence bands shaded in light blue. An estimate of the jump in retirement ages at the cutoff from a regression discontinuity design is included in the text box, with its standard error in parentheses.

Figure A11: Propagation of Social Norms



Notes: The figure shows individuals' probability of retiring at the NRA by their spouse's or sibling's birth date in monthly bins. The respective sample is indicated by the panel titles. Retiring at the same age is defined as retiring within +/- 3 months of the peer's retirement age. In each panel, the red dashed line demarcates the January 1950 cutoff of the 2006 pension reform. The black solid lines depict a quadratic fit estimated separately on each side of the cutoff, with 95% confidence bands shaded in light blue. An estimate of the jump in retirement ages at the cutoff from a regression discontinuity design is included in the text box, with its standard error in parentheses.

B Administrative Data

In this paper, we use administrative data covering the universe of Dutch residents provided by Statistics Netherlands (CBS). The data consists of a number of individual- and household-level registers, which are accessible via a remote access environment. Registers can be linked at the individual level via pseudonymized individual identifier numbers. We use civil status histories, which contain panel information on civil partnerships, marriages, separations, divorce, and widow(er)hood. We link individuals to their spouses and children based on a different register dataset. A dataset based on municipal registries contains information on individual addresses, which we can utilize to link individuals to their neighbors. We also use data on individual income sources, which are available from 1999 onward. This data provides monthly information on the type and amount of income. Finally, we use employer-employee matched labor market data, which is available from 2006 onward, in order to link individuals to their coworkers. This data also contains information on wages and hours worked.

In the following, we provide more information about the main datasets we use. A documentation of the respective dataset can be accessed by clicking on the links embedded in the names. Table B.1 displays a full list of all data sources.[15]

GBAPERSOONTAB

This dataset contains demographic information, including gender, date of birth, and migration history, for the full Dutch population who have been registered in the Basic Register of Persons (BRP) since 1 October 1994.

GBAOVERLIJDENTAB

This dataset includes the date of death of all persons who were registered in the Basic Register of Persons (BRP) and died since 1 October 1994. It also includes the date of death of non-residents who were residents of the Netherlands at least once after 1 October 1994, and whose death information is available in the Register of Non-Residents (RNI). The dataset is primarily sourced from municipal registries (Gemeentelijke Basisadministratie Persoonsgegevens, GBA).

GBAADRESOBJECTBUS

For the universe of residents defined in the GBAPERSOONTAB data, this dataset contains information on individual addresses, with unique object identifiers at the level of buildings. The data is provided in a spell format, such that address changes can be observed. The main source of this data are municipal registries (Gemeentelijke Basisadministratie Persoonsgegevens, GBA).

[15] See the CBS website for a detailed catalogue of all available administrative datasets (in Dutch): `https://www.cbs.nl/nl-nl/onze-diensten/maatwerk-en-microdata/microdata-zelf-onderzoek-doen/catalogus-microdata`

POLISBUS and SPOLISBUS

These databases cover all employment spells in the Netherlands from 2006 onward. For each spell, they include information about the individual such as wage, hours worked, and social insurance contributions paid, employer identifiers, and additional information about the employer, such as sector of activity and collective bargaining agreements.

SECM datasets

These are a collection of datasets containing detailed information on individuals' monthly income by type, including wage earnings (SECMWERKNDGAMNBEDRABUS), profits from self-employment (SECMZLFMNDBEDRAGBUS), income from other activities (SECMOVACTMNDBEDRAGBUS), pension income (SECMPENSIOENMNDBEDRAGBUS), unemployment benefits (SECMWERKLMNDBEDRAGBUS), disability and sickness benefits (SECMZIEKTAOMNDBEDRAGBUS), welfare benefits (SECMBIJSTMNDBEDRAGBUS), and other benefits (SECMSOCVOORZOVMNDBEDRAGBUS). The information is available since 1999.

The SECM datasets are compiled by Statistic Netherlands drawing on various administrative data sources, including tax returns, social security data, and information from pension funds. The initial data is at the spell level, providing information about the start and end date of each income spell and the associated monthly amount of income. The SECMBUS data consolidates the different income sources into a single dataset containing the main source.

TABLE B.1: ADMINISTRATIVE DATASETS

Key Variables Included	Name of Dataset	Years Used
Date of birth, gender	GBAPERSOON2021TAB (V1)	2021
Date of death	GBAOVERLIJDENTAB2021TAB (V1)	2021
Marital status	GBABURGERLIJKESTAAT2021BUS (V1)	2021
Households characteristics	GBAHUISHOUDENS2021BUS (V1)	2021
Residential location	GBAADRESOBJECT2021BUSV1	2021
Parent-child linkage	KINDOUDER2021TAB (V1)	2021
Monthly income		
Wage earnings	SECMWERKNDGAMNBEDRABUSV20211	2021
Profits from self-employment	SECMZLFMNDBEDRAGBUSV20211	2021
Income from other activities	SECMOVACTMNDBEDRAGBUSV20211	2021
Pension income	SECMPENSIOENMNDBEDRAGBUSV20211	2021
Unemployment benefits	SECMWERKLMNDBEDRAGBUSV20211	2021
Disability and sickness benefits	SECMZIEKTAOMNDBEDRAGBUSV20211	2021
Welfare benefits	SECMBIJSTMNDBEDRAGBUSV20211	2021
Other benefits	SECMSOCVOORZOVMNDBEDRAGBUSV20211	2021
Firm, sector, wage, hours worked	POLISBUS and SPOLISBUS	2006–2021