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Peer Effects in Old-Age Employment Among Women

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Abstract

This paper exploits a unique norm-shifting setting—a German pension reform that equalized retirement ages across genders—to examine how old-age employment propagates through workplace networks. The reform raised women's earliest claiming age from 60 to 63 for cohorts born in 1952 onward. Using the universe of workgroups from social security records, I compare women whose peers were just above or below the reform cutoff. I find that women are more likely to remain employed at older ages when their peers do, with stronger effects in the regions of former West Germany, with its traditional gender norms. Gender-neutral pension reforms thus amplify their impact through peer influence, fostering regional convergence in late-career employment patterns.

Keywords: aging, gender, peer effects, old age employment, social norms
JEL Codes: D85, H55, J14, J16, J22, J26, Z13

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1 Introduction

Gender disparities in labor markets have long attracted policy attention. Firms and institutions have implemented measures such as equal pay legislation, family support policies, and corporate diversity initiatives to narrow these gaps (Blau and Kahn, 2017). Recently, demographic shifts, aging in particular, have led many OECD countries to dramatically raise retirement ages for women by adopting gender-neutral pensionable ages.[1] The workplace is likely to play an important role in shaping the change in gender-specific retirement norms that these reforms aim to achieve (Bramoullé et al., 2020).[2] However, we still know relatively little about how the interaction of workers within the workplace shapes old-age employment decisions. In this paper, I provide the first causal evidence of the presence and magnitude of coworker-peer effects in old age employment among women due to a shift to gender-neutral retirement ages in Germany—a norm-shifting setting where the perceptions for women's employment at older ages were at a relatively formative stage. I identify how workplace peers affect women's employment decisions at an older age by leveraging the quasi-random age composition of worker peer groups before the reform enactment.

Studying women in the context of peer effects in old-age employment is important for several reasons. First, rising old-age poverty has become a key policy concern (Börsch-Supan and Coile, 2018). Research consistently finds that older women face a persistently higher risk of poverty than men, due to lower lifetime earnings and reduced pension entitlements from shorter careers and more frequent employment interruptions (Ginn and Arber, 1999; Jefferson, 2009). In particular, Germany is the fifth country in the EU with the highest pension gap among retirees above 65 as of 2018, amounting to 37%.[3] While my analysis does not study pension levels directly, understanding why some women remain employed longer—and how peers and social norms influence this decision—is important for informing policies aimed at reducing old-age economic vulnerability. Second, addressing gender disparities in old-age employment in the labor market remains crucial not only for equity, given women's greater financial vulnerability and higher poverty rates in old age, but also for efficiency, because gender gaps can reflect a misallocation of talent and under-utilization of human capital (Hsieh et al., 2019).

Employment rates at older ages differ markedly even when workers have the same institutions and retirement rules (Krueger and Pischke, 1992), in particular for women

[1]For example, Austria plans to raise the retirement age for women from 60 to 65 between 2024 and 2033, while Switzerland aims to achieve full equalization by 2028.

[2]Some survey evidence exists. Using household survey data, Lancee and Radl (2012) show that social connectedness influences the timing of retirement among German workers. In particular, informal social participation, such as gatherings with friends and relatives, is associated with earlier retirement, whereas formal participation in voluntary associations tends to delay retirement in Germany. Vermeer et al. (2019) show a positive correlation between preferred retirement within social networks, such as those forced by coworkers.

[3]https://ec.europa.eu/eurostat/web/products-eurostat-news/-/ddn-20200207-1

(Blau and Goodstein, 2010; Börsch-Supan and Coile, 2018; Gruber and Wise, 2008). Although institutional incentives, such as gender-neutral statutory retirement ages, shape retirement behavior, women's labor market participation at older ages remains limited, partly due to persistent gender norms surrounding work and retirement. These social norms are reinforced by several factors. Retirement is frequently coordinated within couples, and women—who tend to have older spouses—often retire early to synchronize retirement timing for joint leisure (Lalive et al., 2023). Second, women are more likely to shoulder caregiving responsibilities for spouses, grandchildren, or elderly relatives, which can limit their continued attachment to the labor market. Therefore, some women could exit employment before reaching pensionable ages.

A key but often overlooked factor in old-age employment decisions is the role of coworkers in peer effects, which can shape individual retirement choices. Work occupies far more time than any other daily activity; therefore, work attitudes have a central influence on a person's life (Smith, 1965). Coworker peer groups are important to study in the context of employment-related decisions. For example, Meekes and van Lent (2025) find that peer effects in working hours are larger in coworker networks than in neighbor and family networks. A possible explanation for understudied co-worker peer effects in the context of old-age employment is the difficulty in detecting peer effects in naturally occurring coworker groups. Difficulties arise because peer groups are not formed exogenously due to three common problems: simultaneity, correlated effect bias, and endogenous group formation (Blume et al., 2011; Manski, 1993). For example, selecting specific occupations and establishments or experiencing common shocks can be wrongly attributed to peer effects. It is also difficult to argue which peer influenced the other. Moreover, the limited data on workgroups, i.e., sufficiently granular occupational structures within establishments that could proxy close worker interactions, further limit the scope of research on coworker peer effects.

To circumvent these problems related to causal inference and data availability, I employ a quasi-experiment in Germany, in which a reform in 1999 raised the early retirement age (hereafter, ERA, i.e., the age at which people can start claiming pensions) of women by at least three years (from 60 to 63) starting from the 1952 birth cohort, and thereby abolishing women's pathway to early retirement. There are several advantages to focusing on this specific quasi-experiment for estimating peer effects. First, the three-year increase in statutory retirement age is the largest increase for two consecutive cohorts that has occured in recent German public policy and thus is the most suitable reform for detecting peer effects in retirement, as opposed to a step-wise increase in retirement ages. Second, the reform is unique because it abolished the gender-dependent retirement age system, allowing for a shifting norm related to promoting old-age employment among women. This is particularly relevant in Germany—a country with heterogeneous gender norms that is still more inclined towards the breadwinner model than the dual-earner model, in particular among the older generations from the regions of the former West Germany.[4] Finally, Vermeer et al. (2019) show that willingness to postpone labor market exit given peer group preferences is higher if the planned retirement age is below the statutory retirement age, motivating the study of peer effects in the context of reforms targeting ERA.

The identifying variation stems from the exposure of my sample to peer women who were born in a narrow window either before or after the reform cutoff of 1952. By exploiting exogenous variations in retirement eligibility rules and assigning the peer groups before reaching their retirement ages, I prevail over the three problems of peer effect estimation. The universe of detailed German social security data enables the assignment of workers to their workplace peers within job cells based on (1) establishments—single locations of multisite firms—and (2) detailed 4-digit occupational codes. I define *peers* as workers employed in a given establishment who are directly affected by the reform, and *coworkers* as their colleagues who are younger and thus will reach old age after observing their peers' employment decisions, and being influenced by them. I thereby overcome the reflection problem, where the observed coworker outcome may be both a cause and a consequence of peer retirement behavior.

The identification strategy employed in this paper rests on the assumption that establishment-level characteristics are conditionally exogenous to the reform exposure, ensuring that the reform can be used as an exogenous shifter of old age employment. To further strengthen the identification, I control for a rich set of observable worker, establishment, sector, and regional characteristics. I start by estimating the direct effect of the reform on the average employment rate at age 62 among the peers. I find that raising the ERA from 60 to at least 63 leads to an 11.6 percentage point (p.p.) higher likelihood of being employed at age 62. Having established the significant effect of the reform on employment at older ages, I turn to estimating the peer effects through two-stage least squares. I find significant peer effects in old-age employment among women. An individual's probability of retiring increases by 1.4 p.p. when their immediate coworkers are ineligible for early retirement. Because the first-stage estimate on employment at age 62 is 11.6 p.p., such results translate into 12.6 p.p. of peer effects, i.e., a higher likelihood of staying employed at 62 if the peer decides to do so. The results remain robust across a variety of specifications concerning the definitions of coworker groups, peer groups, workgroups, and treatment. They also hold after including a comprehensive set of covariates. Placebo tests do not display peer effects, further supporting the credibility of my results.

There can be many reasons why peer effects exist. I analyze whether peer interactions shape individual choices through (1) conformity (Bernheim, 1994) and social norms (Stutzer and Lalive, 2004); (2) information diffusion about career concerns (Dahl et al.,

[4]Throughout the paper, I refer to East Germany to define New Länder (and additionally Berlin in the data), and to West Germany as the current regions of the former Federal Republic territory.

2014; Johnsen et al., 2024; Krstic and Hideg, 2019; Welteke, 2015) or the reform (Nicoletti et al., 2018), and (3) work complementarities within teams. Importantly, it is difficult to fully disentangle these mechanisms from one another in social security data; hence, I provide suggestive evidence about them by performing subsample analyses.

First, because the reform effectively raised the labor force participation of older women, it could shift social norms regarding women's employment at older ages by changing expectations about the appropriate retirement age. Conformity then acts as the behavioral channel through which such norms spread: individuals imitate their peers in order not to deviate from the group. In my setting, the two are therefore difficult to disentangle empirically. The East–West comparison is consistent with this interpretation: in the regions of the former West Germany, where female employment rates are lower and the breadwinner model more prevalent,[5] peer effects are stronger because conformity accelerates the diffusion of new, more egalitarian norms. In the regions of the former East Germany, where higher female labor force attachment is more common, the same conformity mechanism yields weaker incremental effects.

Second, retirement occurs within an institutional context where coworkers exchange knowledge about the reaction to and consequences of retiring at a certain age, such as pension schemes, employer reactions, health implications, wages, and financial preparedness. The behavior of coworkers thus provides information that can reduce uncertainty about the transition to retirement. If this mechanism prevails, the effects should be the largest under higher uncertainty, such as for less tenured coworkers, high turnover, and younger establishments, and if the peer is more informative, for example, a manager.

Finally, novel to the coworker peer effects literature, I test whether work complementarities, such as firm incentives related to team productivity, are crucial in coworker peer effects. Peer effects influence labor supply decisions, potentially creating spillover effects through strategic complementarities within the team across older workers and their younger counterparts, such as collaboration benefits, productivity, and shared workgroup-specific human capital and workload (Bartel et al., 2014; Jaravel et al., 2018; Jäger and Heining, 2022). I test this mechanism by proxying interactions by the main tasks performed in occupations, and by measuring the potential harm to the workgroup in employment interruptions due to turnover costs. I also test whether there are significant effects on old-age employment in the same workgroup, because staying in the same workgroup retains the workgroup-specific human capital, further confirming the importance of this channel.

I find that the peer effect is particularly pronounced in settings with traditional gender norms (regions of former West Germany), suggesting that social norms regarding

[5]For example, at least for older cohorts, West Germany is known as a country with a breadwinner model, opposed to Scandinavian countries with dual-income households, and has been used for studying peer effects in other contexts before (Dustmann et al., 2016; Pink et al., 2014; Welteke and Wrohlich, 2019).

women's old-age employment serve as the main channels for the peer effects. I find only limited evidence for information transmission and work complementarities channels. Finally, I estimate the cross-gender peer effects and find that the effects are more precisely estimated for female coworkers, as opposed to male coworkers, suggesting that establishment characteristics alone cannot explain the presence of peer effects (Casarico et al., 2025).

I proceed to compute the social multiplier: for every woman employed at an older age, an additional 0.13 coworker women remained employed at an older age due to peers. I also show how such a shift in social norms regarding old-age employment of women could lead to regional convergence in old-age employment among women in the West and the East of Germany. Nevertheless, disentangling the mechanisms or attributing certain subsample analyses to one specific mechanism is difficult; hence, these channels are only suggestive.

I contribute to three strands of the literature. First, I extend research on retirement reforms and older workers' labor supply (Carta and De Philippis, 2024; Deshpande et al., 2024; Geyer and Welteke, 2021; Lalive et al., 2023; Manoli and Weber, 2016; Mastrobuoni, 2009; Rabaté et al., 2024; Staubli and Zweimüller, 2013; Ye, 2020). Complementing evidence on the direct effects of the reform under this study (Badalyan, 2025; Geyer and Welteke, 2021), I show that peers amplify delayed retirement: women close in age are more likely to remain employed when their workplace peers do. The reform thus operated through a dual mechanism—firms retained workers due to substitutability incentives (Badalyan, 2025), while peers reinforced employment through conformity and social norms regarding old-age employment among women. More broadly, this paper demonstrates that retirement is not solely an individual decision but is shaped by group-level norms and behavioral responses to statutory retirement ages (Behaghel and Blau, 2012; Blundell et al., 2016; Seibold, 2021). Recognizing these peer dynamics is essential for policy design, as they can amplify the impact of social insurance changes (Dahl et al., 2014), alter long-run reform effects as norms evolve, and complicate inference since aggregate outcomes combine direct and peer responses (Glaeser et al., 2003; Grodner and Kniesner, 2008; Welteke, 2015).

Second, I contribute to the literature using quasi-experiments to estimate coworker peer effects in labor market decisions and social insurance programs.[6] While existing work on retirement-related peer effects has largely examined couples and family dynamics—often showing that women time their exits around their husbands' retirement (Atalay et al., 2019; Bloemen et al., 2019; García-Miralles and Leganza, 2024; Johnsen et al., 2022;

[6]Examples include disability pension participation (Rege et al., 2012), job search (Dustmann et al., 2016; Glitz, 2017; Saygin et al., 2021), productivity (Bandiera et al., 2009; Cornelissen et al., 2017; Herbst and Mas, 2015; Mas and Moretti, 2009; Messina et al., 2023), parental leave and labor supply (Casarico et al., 2025; Cavapozzi et al., 2021; Dahl et al., 2014; Welteke and Wrohlich, 2019; Nicoletti et al., 2018; Carlsson and Reshid, 2022), and welfare take-up (Bertrand et al., 2000).

Lalive and Parrotta, 2017; Oral et al., 2024; Selin, 2017; Zweimüller et al., 1996)[7]—I show that these norms are shifting: women increasingly align their retirement with workplace peers rather than spouses. This result represents a broader transition from domestic coordination to workplace-based coordination, underscoring how workgroup norms shape late-career decisions. Unlike most coworker peer effect studies, which focus on narrow firm-level cases or U.S. field experiments (Duflo and Saez, 2002, 2003; Brown and Laschever, 2012)[8], I exploit a universal German pension reform that reshaped retirement behavior across the labor market, using detailed coworker networks defined by establishment and occupation codes. I analyze compliance with the new age threshold—particularly relevant for women—and develop new measures of work complementarity that capture peer influence, extending the literature beyond retirement settings.

Finally, I contribute to the literature on reforms that aim to reduce gender gaps in labor markets. While most studies focus on women of childbearing age (Goldin et al., 2021; Kleven et al., 2019), much less is known about gender dynamics at older ages. Existing research shows that raising women's retirement age increases their employment, but I demonstrate that these effects propagate through workplace peer networks, especially in regions with traditionally stronger gender norms, such as those in the former West Germany. Closest to my mechanism, Boelmann et al. (2025) document that women from the former East Germany migrating west after reunification raised local women's employment by carrying their higher labor force attachment with them. My findings complement theirs by providing micro-level evidence that pension reforms also diffuse through coworkers within establishments. Together, these results highlight migration and peer spillovers due to reforms extending the careers of women as distinct yet reinforcing channels through which policy can reshape social norms around older women's employment, thereby narrowing the gender gaps in late-career labor supply.

The remainder of this paper is structured as follows. Section 2 outlines the institutional setting in Germany. Section 3 details the sample construction, peer group assignment, and identification strategy for causal inference. Section 4 presents the main results, followed by an analysis of underlying mechanisms in section 5, discussion in section 6, and the conclusion in section 7.

[7] Kaufmann et al. (2022) further show that a one-hour increase in grandmothers' hours worked causes adult daughters with young children to work half an hour less.

[8] Notable exception is the study by Oral et al. (2024), which analyzes peer effects across several networks in the Netherlands. I contribute by (1) analyzing coworker peer effects in more details, and (2) in a different type of a norm-shifting setting—gender-neutral retirement reform. Unlike the Dutch data, detailed occupational and establishment variables in my data allow me to construct workplace peers and analyze mechanisms relevant to the workplace peer effects.

2 Institutional settings

In this section, I provide details on the labor market and pension system in Germany, which helps contextualize the peer effects. I also describe the 1999 reform, which motivates the identification strategy in the next section.

Labor markets in Germany. Compared to most Western European countries, Germany has a low employment rate for women and a high gender wage gap. The gender gap in employment rates was particularly low in 1999, and shrank over two decades until 2019. It is noteworthy that although gender gaps around childbirth have received high attention, the gender employment gaps at older ages are just as striking (see Panel A of Figure 1). West German women traditionally had lower employment rates compared to East German women, due to the historic divide between the Federal Republic of Germany and the German Democratic Republic (GDR), religiosity in the West, and communism in the East (Rosenfeld et al., 2004). As shown in the right Panel of Figure 1, women in regions of the former East Germany have significantly higher employment-to-population ratios than women in the regions of the former West Germany (e.g., in 2009, the difference was 2.1 p.p.). In 2009, women in the regions of the former West Germany were 11.6 p.p. less likely to be employed than men, whereas in the regions of the former East Germany this gap was only 4.5 p.p., confirming the breadwinner household model in West Germany and the more egalitarian dual earner model in East Germany.

Figure 1: Employment over population by gender and territory over time

Panel A: Employment over population by gender and age

Panel B: Employment over population over time by regions of the former East and West Germany

Notes: **Panel A** displays employment shares by 5-year age groups and gender. Pink bars represent women and black bars represent men; narrow bars correspond to 1999, while wide bars correspond to 2019. **Panel B** shows the evolution of employment shares among the working-age population (ages 15–65) from 1991 to 2023, again disaggregated by gender. Pink lines represent women and black lines represent men; dashed lines refer to the regions of the former East Germany, while solid lines refer to the regions of the former West Germany. The underlying data are sourced from the Federal Statistical Office. Employment share is defined as the number of employed individuals divided by the total population in each respective group.

Since regions of the former East and West Germany share largely similar retirement rules, differences in norms surrounding old-age employment create a useful setting for studying peer effects. I expect these effects to be stronger in West Germany, where baseline employment rates before the reform were lower. Larger peer effects could, over time, contribute to a regional convergence in employment patterns at older ages.

Public pension system in Germany. The public pension system in Germany covers over 90% of the workforce, and operates on a "pay-as-you-go" basis, where younger workers finance the pensions of older workers. There are two statutory retirement ages: the early retirement age (ERA) and the normal retirement age (NRA). The ERA is the age at which a worker can begin claiming pensions (Panel A in Figure 2), while the NRA is the age at which full pensions can be claimed without any deductions (Panel B in Figure 2). Retiring between the ERA and NRA results in a 3.6% deduction in pension benefits for each year taken early.[9] Because early retirement deductions in Germany were smaller than actuarially fair levels, many workers retired as soon as they became eligible. In a peer effects setting, this means peers' retirement timing is strongly clustered at the ERA, making their behavior more salient and influential on coworkers' own decisions.

Old-age pension for women. There are several pathways to retirement, which individuals use depending on eligibility. Up until the 1952 birth cohort, conditional on having at least 15 years of contribution to social security, ten of which were contributed after the age of 40, women could claim pensions as early as 60 years old, thanks to the pathway to early retirement at 60 available to women but not men.[10] Geyer and Welteke (2021) show that in the total sample of women born in 1951, around 21% retired before 63.[11]

Employment exits before ERA. While most German workers transition directly from employment into retirement with pensions, several alternative exit routes remain available. Some studies highlight unemployment insurance (UI) as a bridge to retirement (Gudgeon et al., 2023). This option is attractive because UI benefits replace roughly 60% of prior wages, the period on UI counts toward pension contributions, and job search requirements are less strict for older workers (Geyer and Welteke, 2021). For those aged 57 and above, UI duration was generous for the cohorts unders tudy—up to two years—allowing, for example, a woman born in 1951 to exit employment at 58 and receive UI until retiring at 60. However, Gudgeon et al. (2023) find that because of the already-generous retirement pathway for women, using unemployment as a bridge to retirement was not as common for this cohort of women as for men. Other routes include

[9] For example, retiring three years before the NRA results in an 18% pension deduction.

[10] The women's pathway to early retirement was a popular pathway for women who wanted to exit the labor force early, because the other pathways either required more contribution years to the social security system or implied a later ERA.

[11] Almost 60% of all the women born in 1951 were eligible for the old-age pension for women, 35% of which retired before 63 by utilizing the old-age pension program for women.

disability insurance (DI) and inactivity due to caregiving, illness, self-employment, or personal preference. Women with sufficient contribution years (5 for the regular pathway and 35 for the long-insurance pathway) may choose inactivity before their ERA to care for family members, grandchildren, or to coordinate retirement with spouses (Lalive and Parrotta, 2017) before claiming pensions. These alternatives are relevant for the peer effects setting, as norm-driven increases in old-age employment and labor market activity could decrease early exits, thereby improving compliance with and enforcement of the higher old-age employment targeted by the reform.

The 1999 reform that abolished women's pathway to early retirement. Starting from the 1952 cohort, the women's old-age pension pathway was abolished, and the earliest age at which women could claim pension benefits rose by at least three years (see Table B.1).[12] Moreover, starting from the 1952 cohort, there were fewer opportunities to bridge the gap between employment and pension claiming with UI. Overall, this is the largest increase in retirement ages for two consecutive cohorts in Germany, as other reforms increase the retirement ages gradually, in incremental steps over a larger span of birth cohorts. This large and discontinuous increase in retirement rules helps me circumvent the problem of separating reform effects from time or cohort effects, facilitating the identification strategy in section 3. Importantly, unlike reforms in some other countries, such as the Netherlands, this reform was uniform across industries, occupations, and regions, consistently and homogeneously affecting the retirement ages, which supports the comparability of treated and control workers discussed in the identification strategy below.

The change in ERA across cohorts is shown in Panel A of Figure A.1. Geyer and Welteke (2021) find that women born in 1952 extended employment at ages 60–62 by 13.5 p.p.—about a 30% increase relative to the 1951 cohort—without affecting employment before age 60 despite the reform's pre-announcement. They also show rises in unemployment and inactivity, driven mainly by extensions of existing statuses rather than active substitution. In related work (Badalyan, 2025), I re-estimate the effects for women by their employment characteristics at the ages 58-59 and find a 17.3 p.p. increase at 60–62 (a 22% increase relative to the control mean).[13] Overall, these findings point to a strong direct effect of the reform on employment at ages 60–62, which may also generate peer effects.

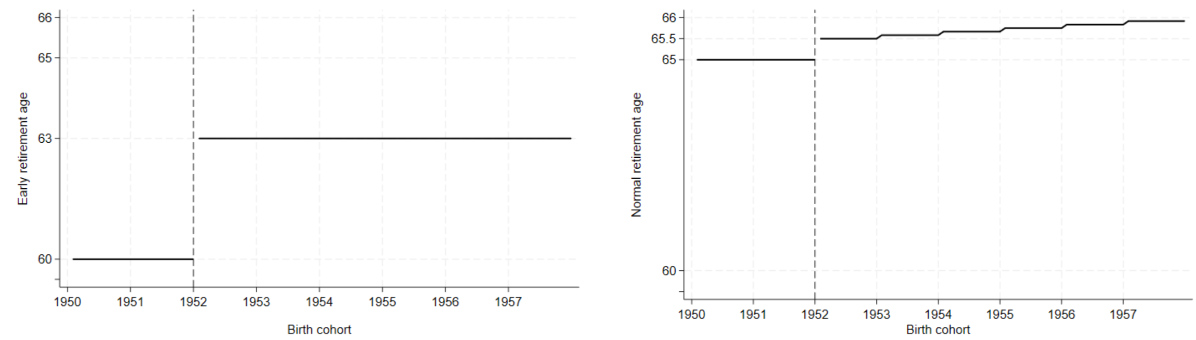
Figure A.1 displays the distribution of retirement age (proxied by the age at the last labor market activity spell) for the women who were employed at age 58, and reveals that

[12] For women with at least 35 years of social security contributions, the retirement age rose by three years; for those with just over five years of contributions, the retirement age rose by five years. Nevertheless, Geyer and Welteke (2021) show that 90% of women eligible for women's old age pensions were also eligible for pensions for long-insured.

[13] The higher estimate reflects stronger labor force attachment in my sample, where over 70% of the control group remain employed at 60–62.

Figure 2: Early and normal retirement age rules by cohorts

Panel A: Early retirement age **Panel B:** Normal retirement age



Notes: **Panel A** shows the policy rule for ERA, the earliest age a person could claim pensions, by birth cohorts. The dashed line presents the birth cohort cutoff, January 1952, starting from which the ERA rose by at least three years. **Panel B** depicts the assignment rule of NRA, the age at which workers can claim full pensions, by birth cohorts. Before the 1952 cohort, there was a women's pathway to retirement, which had a fixed NRA at the age of 65. The vertical dashed line at the January 1952 cohort indicates the birth cutoff from which the women's pathway to early retirement was abolished. Starting from the 1952 cohort, the NRA for people eligible for the regular pathway to retirement is equal to the NRA for long-term insured, which used to be 65, but was raised by monthly increments per birth year starting from the 1947 cohort. Since women born before 1952 had their own NRA rules, their NRA was set at 65, and started increasing only from the 1952 cohort, with a discontinuous 6-month rise from the 1952 cohort. The detailed tables with ERA and NRA by cohorts can be found in Table B.1.

there are employment exit peaks before age 60 for only the 1950-1951 cohorts, i.e., those women born before the reform cutoff who were eligible for women's pathway allowing earlier retirement. These peaks shift to later ages for the 1952-1957 cohorts, and the earlier exits are more evenly distributed before the age of 62. The gradual spikes shown at different months at the age of 65 can be attributed to the increase in the normal retirement age (see the assignment rule in panel B of Figure 2).

3 Empirical framework and data

This section describes the data and the identification strategy that I use to estimate causal peer effects in employment and retirement decisions at older ages. I begin by outlining the peer effects model, identifying three key issues that can bias the results, and discussing how the literature suggests that one can address these problems using quasi-experiments. Then I illustrate this approach within the reform setting of this study (see section 2). Next, I provide details on the data and sample construction necessary for applying this method. Finally, I outline the regression equations that I estimate following this approach and assess their validity.

3.1 Empirical methodology for identifying peer effects

Consider a workgroup that includes peers and their coworkers, where, throughout this paper, I define *peers* as individuals directly affected by the reform, and *coworkers* as those who may experience indirect effects through their peers. There is no overlap between the two groups. The focus of this study is on peer effects on coworkers, which can be modeled as follows:

$$Y_{ig} = \alpha + \tau \overline{Y_{-ig}} + X_{ig}'\beta + \overline{X_{-ig}}'\gamma + W_g'\eta + \epsilon_{ig} \tag{1}$$

where Y_{ig} represents coworker i's outcome (e.g., an employment indicator at age 62). The main coefficient of interest, τ, captures the effect of $\overline{Y_{-ig}}$, the mean employment outcome of peers (excluding the coworker i) in group g, on coworker i's retirement outcome, Y_{ig}. Additionally, the literature typically accounts for coworkers' individual characteristics (X_{ig}), peer characteristics ($\overline{X_{-ig}}$), and workgroup-level factors (W_g).

Identification challenges. Estimating peer effects may lead to biased results. Manski (1993) lists three challenges in estimating τ: correlated unobservables, endogenous group formation, and simultaneity. The problem of correlated unobservables arises when, even after controlling for the coworker (X_{ig}), peer ($\overline{X_{-ig}}$), and workgroup (W_g) characteristics, some unobserved factors remain correlated with the outcomes of the peers, leading to a bias in the estimated τ. For example, contextual factors or common shocks, such as workplace conditions or industry- and occupation-specific demand fluctuations, may lead both coworkers and their respective peers to delay retirement regardless of each other's decisions. Endogenous group membership occurs when peers and their coworkers self-select into specific occupations or establishments based on their work (or leisure) preferences and their attraction to particular peer groups. This selection process complicates causal inference because the composition of peer groups is not random. Simultaneity in interactions leads to the reflection problem, where the observed coworker outcome, Y_{ig}, may be both a cause and a consequence of peer retirement behavior, $\overline{Y_{-ig}}$. This bidirectional influence makes it difficult to disentangle the true effect of peer outcomes on coworker decisions.

The peer effects literature addresses identification challenges by exploiting instruments that shift peers' outcomes, $\overline{Y_{-ig}}$, without directly affecting individual outcomes Y_{ig}. One approach relies on partially overlapping networks (De Giorgi et al., 2010), while another leverages quasi-experimental variation from reforms that alter peers' employment incentives (Dahl et al., 2014). I adopt the latter, using the increase in ERA for the 1952 cohort as a source of exogenous variation (see section 2). This choice is motivated by the stronger predictive power of reform-based instruments. Consistent with this, Geyer and Welteke (2021) and Badalyan (2025) document sharp employment responses to the reform.[14] In addition, the reform-based strategy also permits the calculation of a social multiplier, relevant given ongoing global increases in retirement ages.

Overcoming identification challenges through a quasi-experiment. To identify peer effects, I exploit a reform that has heterogeneous implications for peers and homogeneous implications for coworkers. The 1999 pension reform raised the ERA starting with the 1952 birth cohort. For intuition, consider two women in two different workgroups: one born in 1951, still eligible to retire at 60, and another born in 1952, who now faces a higher cost of retiring early. Their retirement choices create exogenous variation in peer behavior.

Coworkers are then defined as women born 1953–1957, i.e., too young to make retirement decisions but close enough in age to be approaching retirement themselves. This design ensures that coworkers differ only in the peer exposure they receive, while their own retirement rules are the same constant. To avoid reflection, the peer and coworker samples are mutually exclusive: I use a "leave-out group" approach, excluding all peers from the coworker sample.

Figure 3 illustrates the idea. Peers are the reform-affected (1952 cohort) and unaffected (1951 cohort) women, whose retirement behavior may indirectly shape that of their coworkers. By aligning groups this way, the strategy addresses the three core challenges in the peer effects literature. First, correlated unobservables are mitigated by exploiting exogenous policy variation across birth cohorts. Second, endogenous group membership is limited since peer groups are defined before members reach retirement age. Third, simultaneity is avoided by focusing on younger coworkers who have the same rules among each other; therefore, any differences displayed should be attributed to their peers' influence.

If I had a single peer (e.g., spouses or siblings) within a group, a regression discontinuity design may suffice (Dahl et al., 2014). In establishments, however, peer groups often contain multiple treated and untreated members. Restricting to just one peer would reduce external validity and make it impossible to study mechanisms across subgroups. I therefore extend the strategy to allow for multiple peers within workgroups, as long as they are born to one side of the cutoff, as detailed in the next subsection.

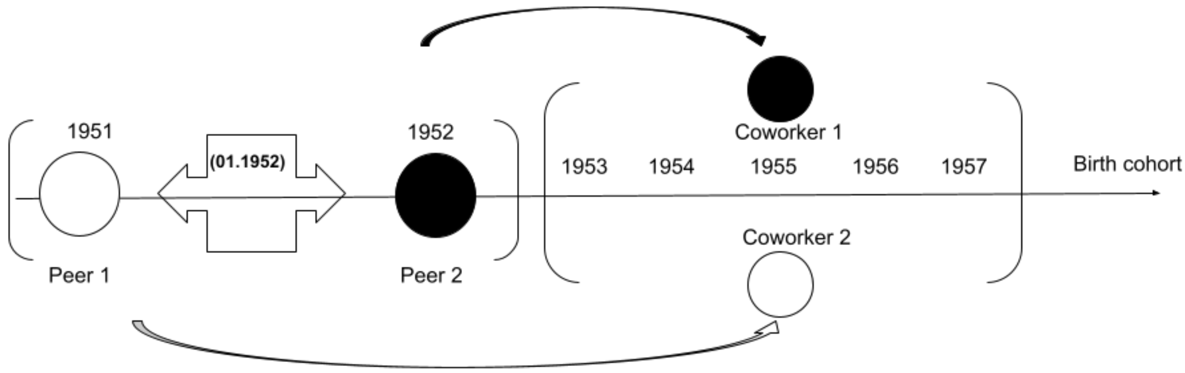
3.2 Social security data and sample construction

Integrated Employment Biographies. I use German social security data from the Integrated Employment Biographies (IEB), provided by the Data- and IT-Management (DIM) at the Institute for Employment Research (IAB).[15] These data cover all workers

[14] Another reason is data limitations—German social security records lack household identifiers needed for overlapping network designs.

[15] I use data from the full universe of German employment records *(IEB, version 17˙00˙00˙202212)* of the IAB. Due to its administrative origin, these data are confidential and can only be accessed on-site at IAB. Access for guest researchers requires clearance from the German Federal Ministry of Labour and Social Affairs.

Figure 3: Simplified identification strategy



Notes: The figure illustrates the assignment of workers into groups. *Peers* are women born around the 1952 cutoff: those born in 1951 could still retire at 60, while those born in 1952 faced a higher ERA. Their retirement behavior may, in turn, influence their *coworkers*, who are younger cohorts in the same workgroup. Treated peers (1952) and their coworkers are shaded black; untreated peers (1951) are shaded white.

subject to social security until 2022, with records starting in 1975 for West Germany and 1991 for East Germany. Employers are required to report information on the full workforce as of June 30$^{\text{th}}$ each year, as well as any changes in employment spells, including job entry, exit, or contract modifications. The reported information includes details such as workplace variables (start and end dates of spells, occupation, contract type, detailed daily wages, industry, and location of establishments), and some basic demographic information (birth year and birth month, gender, education, and place of residence). This dataset is well-suited for the study because its detailed employment records of all workers within the workplace and occupational codes allow for the precise assignment of peer groups across the entire German workforce, while gender and birth dates allow for assigning the treatment groups according to the reform. Throughout this paper, I use the first four digits of *Klassifikation der Berufe (KldB) 2010* (see Paulus et al. [2013] for an overview). The rest of the variables, such as tenure, location, occupations, industries, etc, are useful for testing peer effects mechanisms.

Due to data security restrictions, it is impossible to observe the universe of social security data at once. To comply with the data access rules, I received data on the universe of private sector establishments (and all the workers within these establishments) that employed at least one woman born in the 1950-1953 cohorts in 2008, and who had no history of working as a sailor or miner, as their retirement rules differ. The data were extracted in 2008 because it is the last year when all four cohorts of workers were below 60 years of age. In the baseline specification, I focus on peers born in 1951–1952, but the 2-year bandwidth allows for sensitivity and robustness checks in additional specifications. I further restrict the sample to the establishments with at least five and at most 500 workers, as mega-large establishments above 500 workers have large worker turnover, and removing them helps me save data size and meet data parsimony rules. Table B.2 describes the original sample extract from the social security records. These data include 190,228 establishments, with 26.5 million workers ever employed in them, around 9.1 million of whom were employed in 2008.

Sample construction. Following the empirical methodology outlined in the previous subsection, I proceed with the definition of peer groups.

Workgroup definitions. To avoid misattributing spillover effects across broader organizational units, I restrict the peer exposure to within-establishment and same-occupation groups to ensure that the estimated peer effect is not diluted by non-overlapping work environments. Workgroups are defined at the 4-digit occupation level to capture coworkers who are likely to interact regularly and perform similar tasks. This classification improves the relevance of peer effects by reflecting shared work environments, comparable responsibilities, and similar exposure to firm-level shocks (Jäger and Heining, 2022). Previous literature has used 4-digit occupations to define peer groups, taking advantage of the job cells available in social security records (Fietz and Schmeißer, 2024; Messina et al., 2023).

Peers: women who transfer the peer effects. I begin by marking peer women employed at the ages of 56-57 and belonging to either the 1951 birth cohort (treatment group) or the 1952 birth cohort (control group). I focus on women employed at ages 56-57 to address the reflection problem: all peers and coworkers are below the pre-reform retirement age of 60 for women.[16] Conditioning on employment at a certain age would be problematic if there was discontinuity in employment rates among the treated and control peers. However, Geyer and Welteke (2021) show that the employment statuses do not change due to the reform until the workers reach age 60.[17] This restriction results in 214,435 peer women, 110,796 of whom are treated, and 103,639 are in the control group.

Before the reform, women could retire at age 60 only if they had accumulated at least 15 years of contributions (see section 2). While one might consider restricting the sample to those who meet this threshold, I deliberately avoid doing so for three reasons. First, the peer-effects question in this paper concerns *how workplace exposure to treated peers shifts the employment behavior of all coworkers at the policy threshold*, not only those who would have qualified for the discontinued "women's pathway." Conditioning on 15 contribution years would change the estimand from a network-level spillover among the full coworker population to a narrower effect among a selected, highly attached subgroup.

[16]This restriction also helps overcome another problem. Since the originally provided data include establishments with at least one peer employed in 2008, it potentially introduces selection on tenure and job attachment, because the older cohorts (treated peers, born in 1952) satisfy more stringent tenure criteria than the younger ones (control peers, born in 1951). By aligning the peers as those employed in the sampled establishments at the ages of 56-57, I thereby avoid asymmetry in tenure-based selection.

[17]I am unable to confirm this result, as that would require requesting entirely new social security data from the data provider.

This is at odds with how peer interactions and social norms operate in workgroups that include coworkers with heterogeneous attachment histories, and norms diffuse along that full network. Second, the 15-year rule is no longer a relevant decision margin for the younger cohorts who serve as coworkers in my design. Post-1952 cohorts did not face incentives to accumulate 15 years for early claiming; restricting to that legacy threshold would therefore remove precisely those coworkers for whom peer influence is most policy-relevant (e.g., women who might otherwise be inactive, self-employed outside the system, or on DI at 62), and would understate the potential reach of norm transmission inside firms. For example, Geyer and Welteke (2021) report that 32% of all the women in the 1952 birth cohort are non-employed at the age of 62-63. Hence, employment at 62 provides a good measure of a new norm and compliance with it. Third, the outcome I study—labor-market activity at age 62—directly targets the policy's compliance margin (delaying exits to at least age 63). Whether a coworker meets a defunct eligibility rule is orthogonal to this compliance margin: what matters for peer spillovers is whether exposure to treated peers keeps coworkers *active* at age 62. Conditioning on a legacy eligibility criterion would reduce external validity for the policy audience, who care about aggregate compliance and spillovers across *all* workers in affected workgroups.

Next, I group all women who belong to the same workgroup, defined as an occupation group within an establishment. I count the number of peer women born in the 1951-1952 interval and drop any groups where peer women from both cohorts are present or where no women from either cohort are in the workgroup. This restriction ensures that the peer groups include at least one woman who could claim pensions at age 60 or 63, resulting in 153,647 peer women, 80,114 of whom are treated and 73,533 are in the control group. Thus, the simplified model from Figure 3 is extended to allow multiple peers, all of whom share the same retirement rules. Because I dropped the groups with peers from both sides of the cutoff, all the peers in each workgroup are subject to the same rules.

Coworkers: women who receive the peer effects. I proceed to mark the coworkers as workers in the 1953-1957 birth cohorts, who were employed in the year when the peers were at the age of 57; therefore, the coworkers were 51-56 years old at the date of exposure. In the baseline specification, I focus on female coworkers, but I also extend the analyses to the male coworkers for mechanisms and robustness checks. I do not analyze the younger cohorts because they are right-censored beyond age 65 in my data.[18] I do not include older cohorts (e.g., born before 1951) in my coworkers group because peers could observe the retirement decisions of older cohorts of coworkers, thereby leading to the reflection problem. I further drop the coworkers who were exposed to more than one workgroup with peers employed at 57, for example, due to a switch of employment or simultaneous employment.[19] In total, there are 503 such workers (368 women and 135

[18] At the time when this paper was written, social security data were available up until year 2022.
[19] Note that I do not perform a similar restriction for peers; therefore, they can affect several workplaces.

men) who are removed. I assign the resulting coworkers as "treated" if their peer(s) were born in 1952 (and had to wait an additional three years to claim pensions) and "control" if their peer(s) were born in 1951. I keep the workgroups that had at least one coworker in the year when the peers were 57. The roadmap for sample construction is depicted in Figure 3. To perform peer effect analyses, I compute the average observable and outcome characteristics over all the peers in the workgroup and transfer such information to all the coworkers. Therefore, my final data consists of coworkers. The final sample includes 64,324 workgroups with exposure to 86,593 peers (45,603 treated and 40,990 control) and their 246,057 female coworkers. In the sample with female coworkers, the coworker has 59 workgroup colleagues on average, and the median number of colleagues is 34 workers, which motivates allowing several peers to influence the coworkers, instead of restricting to small workgroups.

The sample construction details for alternative samples, together with respective sample sizes (including the number of coworkers, peers, and workgroups), can be found in Appendix C and Table C.1.

Outcome variables. The main outcome variable is employment at 62 years of age. Because the reform raised the earliest retirement age from 60 to at least 63, focusing on an indicator for employment at 62 constitutes a clean test of whether the reform induced delayed labor market exit through continued activity. The binary nature of this variable allows for clear predictions of changes in employment shares and enhances interpretability. It also provides a direct measure of compliance with the new earliest retirement age, which was the reform's explicit target.

Employment at 62 is constructed from the inferred "last labor market activity date" in the social security data, where activity includes three sources of spells—regular employment, UI, and welfare receipt. Given that, post-1952, bridging the gaps between employment and retirement was limited (see section 2), the vast majority of those who are active in the labor market at the age of 62 are employed;[20] therefore, for simplicity, I use the terms interchangeably. Women who are not counted as active in the labor market at 62 therefore fall into the complement category, which primarily consists of: early retirees (rare post-reform, but possible because of DI), inactive women (e.g., caregiving, household work), those receiving disability benefits, self-employed outside the social security system, or those who emigrated or otherwise left the labor force. Large peer effects on this measure thus reflect peers shifting women from these alternative non-covered or inactive states into active labor market participation at the policy-relevant threshold.

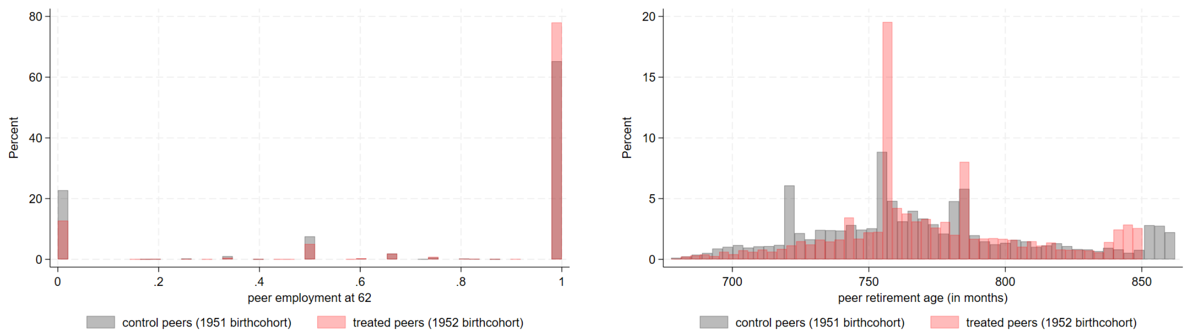
Studying a binary threshold such as "employment at 62" is particularly relevant for evaluating whether the reform achieved its intended goal. Even modest changes in contin-

[20]The last labor market is created as a biographical variable; therefore, it includes all the labor market activity states, which are not decomposed into the employment and unemployment states due to required data limitations.

uous retirement age may have limited policy significance if they do not move individuals across this threshold, whereas changes in the share active at 62 directly capture compliance with the higher earliest retirement age.

Figure 4: Distribution of peer outcome variables by treatment status

Panel A: Employment at 62 **Panel B:** Retirement age (in months)



Notes: These graphs show the distributions of peer outcome variables (dummy for employment at 62 in **Panel A** and continuous retirement age in months in **Panel B**) by treatment groups. The peer outcomes are computed as averages over peers. The groups are treated if all the peers are born in 1952 (whose retirement age has been raised), and untreated if all the peers are born in 1951 (eligible to retire at 60 through women's pathway to early pension claiming).

In a complement analysis, I use indicators for employment between 58 and 64 years of age, mean employment between 60 and 62 years of age, mean employment in the same workgroup, and continuous retirement age (in months). The continuous retirement age captures the extensive margin and longer-term adjustments, offering a fuller picture of peer influence. However, it is noisier due to right-censoring of data in 2022 and because it is proxied from the last spell of labor market activity rather than actual pension claiming. Panel B of Figure A.1 shows the distributions of employment at 62 and retirement age in months in my sample, relative to the distribution of the 2% random sample of workers born in 1951-1957 (in Panel A). The distributions of these outcome variables display similar patterns.

Figure 4 displays the distributions of the average peer (1) employment at 61 and (2) retirement ages (in months) by workgroup treatment status in my final sample. It is evident that the likelihood of being employed at 60 is shifting towards 62 for treated peers (see Panel A in Figure 4), while the continuous measure of retirement ages in months shifts towards 756 months, which corresponds to 63 years old (see Panel B in Figure 4).

3.3 Identification strategy: instrumental variable approach

To address challenges in identifying coworker peer effects, I employ an instrumental variable (IV) approach that leverages quasi-random variation in pensionable retirement ages induced by the January 1952 reform cutoff. Let Z_g denote the workgroup treatment status (eligibility of peers to claim old-age pensions at 60). All peers within a group share the same binary treatment status. The sample includes multiple peers per group, allowing estimation of peer effects on retirement decisions using a two-stage least squares (2SLS) framework.

First stage: I instrument the average peer outcome, $\overline{y_{1g}}$ (employment at age 62 or retirement age), with Z_g:

$$\overline{y_{1g}} = \alpha_0 + \lambda Z_g + W_g'\alpha_1 + X_{ig}'\alpha_2 + \overline{X_{-ig}}'\alpha_3 + u_{1g}, \tag{2}$$

In baseline specifications, I show the results with no controls, and with controls, where the individual controls include wages and education categories, because previous literature confirms that education is an important determinant of employment at an older age (Geyer et al., 2022). The workgroup controls include establishment size, workgroup size, number of peers, number of coworkers, and a dummy for the locations of the establishments in a region of the former East Germany. I additionally include fixed effects for the coworkers' birth cohorts to control for their rises in the normal retirement age (see Panel B of Figure 2 for the rule).[21] Controlling nonparametrically for birth cohorts mitigates concerns that age composition drives peer group formation.

Interpretation: λ measures how much peers' retirement outcomes respond, on average, to the reform-induced treatment.

Reduced form: The effect of the workgroup treatment on coworkers' outcomes is estimated as

$$y_{2ig} = \beta_0 + \delta Z_g + W_g'\beta_1 + X_{ig}'\beta_2 + \overline{X_{-ig}}'\beta_3 + v_{2ig}. \tag{3}$$

Interpretation: δ represents the overall change in coworkers' outcomes that occurs when the peers are exposed to the reform, i.e., intention-to-treat (ITT) effect.

Peer effects: The peer effect is obtained by dividing the reduced-form coefficient δ by the first-stage coefficient λ. It is obtained through an instrumental variable approach, where the average peer outcomes $\overline{y_{1g}}$ are instrumented by the workgroup treatment status Z_g:

$$y_{2ig} = \gamma_0 + \tau\widehat{\overline{y_{1g}}} + W_g'\gamma_1 + X_{ig}'\gamma_2 + \overline{X_{-ig}}'\gamma_3 + w_{2ig}. \tag{4}$$

Interpretation: τ captures the effect on a coworker's retirement outcome of a full-scale increase in the average peer retirement outcome, from its minimum to maximum, induced by the reform.[22]

[21] I also include the same set of controls for ITT and 2SLS regressions.

[22] Since peer exposure is measured as the average share of treated peers, a one-unit change corresponds to a switch from none of the peers being treated (0) to all peers being treated (1). Hence, the peer effect captures how the employment of co-workers responds when peer retirement behavior changes throughout this entire range.

Standard errors: All regressions cluster standard errors at the workgroup level to account for the correlation within the group.

Identification assumptions. The IV strategy rests on four assumptions. First, *relevance* requires that the reform significantly shifts peers' retirement behavior. Second, *independence* requires that treatment assignment is as good as random around the cutoff, conditional on covariates. Birth dates are exogenous, and strategic manipulation is implausible, as parents could not anticipate the reform.[23] Because eligibility for women's pension claiming at 60 required long contribution histories and the reform was unanticipated, selective sorting into workgroups is unlikely. Third, the *exclusion restriction* requires that the reform affects coworkers' outcomes only through their peers. Since coworkers are younger and face the same retirement rules across groups, the only systematic channel of influence is peer behavior. Finally, *structural invariance* requires that the relationship between predicted peer outcomes and coworkers' outcomes is stable across individuals. This condition ensures that the 2SLS estimate recovers the causal peer effect.

The F-test on the first-stage regression assesses relevance. Summary statistics for peers and coworkers (Table B.6 and Table B.7) show that treated and control groups are broadly comparable. Standardized mean differences are generally small (below 0.1),[24] with slightly larger imbalances for group size, number of peers, and share of older employees. Overall, the results support the assumption that differences in outcomes can be attributed to peers' treatment rather than pre-existing group differences. Robustness checks in the next section—adding rich controls, using placebo cutoffs and samples, redefining workgroups, coworkers, and peers, and testing heterogeneity by number of peers —support the validity of the remaining assumptions in this setting.

4 Results

In this section, I proceed in three steps. First, I show the baseline results for peer effects in employment at each age and at retirement age (in months). Next, I perform comprehensive sensitivity and robustness checks to confirm these results.

4.1 Baseline results

Direct and peer effects by age. I estimate reduced-form and IV effects of peer exposure on employment at all the ages surrounding 60 (58–64). Interestingly, the ITT effects remain sizable, even comparable to those at ages 60-62. However, the first

[23]Birth dates are widely used for identification; in this context, see Geyer and Welteke (2021).

[24]While the table reports raw means and t-tests for differences, I summarize covariate balance using standardized differences in the text. Standardized differences express differences in units of standard deviations, allowing meaningful comparison across variables with different scales and avoiding overinterpretation of statistically significant but practically negligible differences in large samples.

stage—the extent to which peers change their employment at these earlier ages—is considerably smaller, particularly at ages 56–57. As a result, the IV estimates (peer effects) are mechanically larger at younger ages. This pattern should not be interpreted as implausibly large behavioral responses, but rather as an indication of anticipatory peer effects. That is, women may adjust their own employment trajectory in response to their peers' delayed exit—even when those peers are only gradually transitioning or expected to stay longer. Such forward-looking behavior aligns with peer-effect channels such as norm adaptation, workplace dynamics, and expectation formation, where observing colleagues' decisions about retirement timing may influence one's own planning horizon and incentives, even slightly in advance.

Baseline results: employment at 62. Having shown that the first stage is the largest, around 60-63, I focus on the indicator for employment at 62 as the main outcome. The baseline results are presented in Figure A.2, and reveal positive and significant effects in the first stage, reduced form, and peer effects.

First stage and instrument validity. In the first stage (first column in Table 1), the instrument—whether a worker's peers were subject to the higher ERA—significantly predicts peer employment at 62, i.e., an 11.6 p.p. increase in probability to be employed at 62 (16.2% increase relative to the control mean in the workgroup where peers could retire with old rules). This effect is slightly smaller than in Badalyan (2025) and Geyer and Welteke (2021), who report that a rise in the ERA led to a 13.5 p.p. (30.6%) and 17.1 p.p. (22.1%) increase in employment at the ages of 60-62, respectively. Such a difference stems from the specifics of my sample—the peers in this paper are employed at the ages of 56-57, and in such workgroups where all the peers are born on one side of the cutoff. The corresponding first-stage F-statistic is approximately 768.52, indicating a strong instrument. The confidence intervals of the specification that controls for workgroup, coworker, and peer characteristics (second column of Table 1) include the original estimate of 11.6 p.p.

Reduced form. Having established a strong first-stage effect, I now estimate the effect of being exposed to peer women who experienced a rise in the ERA on coworkers' probability of being employed at the age of 62. In the reduced form (third and fourth columns in Table 1), workers in treated peer groups are 1.5 p.p. (1.9% increase relative to the control mean) more likely to be employed at 62, and this result is robust to the inclusion of covariates.

This estimate captures the ITT effect of peer exposure to the reform: it reflects the change in a worker's probability of remaining employed at 62 due solely to being surrounded by affected peers, without accounting for the degree of peer response. Importantly, this estimate does not rely on strong assumptions about the exclusion restriction and therefore offers a transparent, policy-relevant summary of peer effects from the ERA reform within workgroups.

Table 1: First stage, reduced form, and peer effect regressions for employment at 62

	First stage		Reduced form		Peer effect	
Z	0.116***	0.113***	0.015***	0.014***	0.133***	0.126***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.019)	(0.019)
firm size		-0.000		0.000		0.000
		(0.000)		(0.000)		(0.000)
peer group size		0.000		-0.000***		-0.000***
		(0.000)		(0.000)		(0.000)
N peers		-0.011***		-0.001		0.001
		(0.003)		(0.002)		(0.002)
N female coworkers		0.001		-0.000		-0.000
		(0.001)		(0.000)		(0.000)
East Germany		-0.018***		0.024***		0.026***
		(0.005)		(0.003)		(0.003)
peer vocational education		0.019**		0.017***		0.015***
		(0.007)		(0.004)		(0.004)
peer university education		0.044***		0.028***		0.023***
		(0.011)		(0.006)		(0.006)
peer earnings		0.000***		-0.000***		-0.000***
		(0.000)		(0.000)		(0.000)
vocational education		0.004		0.033***		0.033***
		(0.004)		(0.004)		(0.004)
university education		0.017***		0.050***		0.048***
		(0.006)		(0.005)		(0.005)
earnings		-0.000***		0.000***		0.000***
		(0.000)		(0.000)		(0.000)
Control mean	0.716		0.785			
Observations	182584	182584	182584	182584	182584	182584
N workgroups	64324	64324	64324	64324	64324	64324
Controls	No	Yes	No	Yes	No	Yes
R squared	0.023	0.040	0.000	0.012	-0.002	0.011

Notes: The outcome variable is *employment at 62*. This table shows the effect of the rise in ERA on peers' own employment: 1st stage (Equation 2), ITT (Equation 3), and IV regressions (Equation 4). The control means are the average values of the outcomes when I limit the sample to the workgroups with peers born in 1951, i.e., who could claim pensions at 60, conditional on contribution years. Robust standard errors in parentheses are clustered at the workgroup level.

* ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Peer effects. The last two columns in Table 1 show that the peer effects in employment at 62 are around 13.3 p.p. This result implies that coworkers are around 13.3 p.p. more likely to be employed at the age of 62 if their peers decide to do so in response to the rise in their pensionable ages. Given the instrument's binary nature, the peer effects are obtained by scaling the reduced form effect (columns 3-4) by the first stage estimates (columns 1-2). Importantly, since I allow coworkers to be exposed to several peers, as long as they are subject to the same rules, this estimate captures the effect of all the peers in the workgroup remaining in employment at 62 on coworkers' own employment at 62. This estimation feature also explains the relatively large peer effect estimates.

Contextual or exogenous effects. Following the standard distinction in the peer effects literature, the model accounts for both endogenous effects (peer behavior) and contextual or exogenous effects (peer or workgroup characteristics). The latter are captured by the observable peer-level controls and help ensure that the estimated peer effect is not confounded by differences in peer group composition. Beyond peer behavior, the results also point to contextual effects: the composition of the workgroup—including peers, coworkers, and the broader workplace—shapes individual employment decisions at age 62. Higher education levels among peers, in particular university education, are positively associated with continued employment, suggesting that working alongside more educated colleagues may influence expectations or perceived value of remaining in the workforce. These findings suggest that the social and structural environment, not just direct peer behavior, plays a role in shaping old-age employment.

4.2 Robustness checks

In the following subsection, I perform several robustness and specification tests. The first set of these tests corrects for further potential imbalances between the treated and control groups by including a set of additional covariates. The further four specifications alter the definitions of treatment, peers, workgroups, and coworkers. All the results confirm the existence of peer effects and the robustness of the estimates presented above. Finally, I re-estimate the peer effects on a placebo sample with no change in the retirement regime and show no effects, confirming that the effects studied in the baseline specification can be attributed to peer effects. These robustness checks help ensure that my estimated peer effects reflect behavioral responses to the continued presence of older coworkers, rather than institutional frictions, selection, or unrelated workplace trends.

Sensitivity to the inclusion of covariates. I test the validity of my findings by including additional coworker variables (see Panel B in Table 2). These covariates include co-workers' full-time status, experience, a dummy for being foreign, management status, and estimated AKM worker fixed effects. To rule out that changes in peer characteristics—rather than their continued presence—drive my results, I also re-estimate peer effects controlling for additional cohort-specific average peer characteristics (same variables as for coworkers in Panel B) at the workgroup level, and confirm that the results are robust (see Panel C in Table 2).

Additionally, the coworkers employed with treated peers may differ from those employed with the untreated peers due to different workgroup characteristics. Since the reform was pre-announced, I test whether the peers could also select into different workgroups at the age of 56-57 (the ages when I define the workgroups) based on their treatment status. Adding additional workgroup-level characteristics at the year when peers were 57, such as share of women, share of older workers above 55 years old, share of full-time workers, median wages, number of coworkers, joint tenure of peers and coworkers, establishment AKM fixed effects, number of coworkers, number of female coworkers of each birth cohort (see Panel D in Table 2) does not alter the baseline results because the confidence intervals still include the coefficients from the baseline specification. Thus, such a selection is unlikely to drive my results because defining the workgroups at the ages when the peer was 56-57 is early enough; moreover, Geyer and Welteke (2021) and Badalyan (2025) do not find anticipatory responses before these ages.

The results are also robust to controlling for aggregated occupation and industry-fixed effects (see Panel E in Table 2); therefore, I exclude that the effects are confounded by the global financial crisis of 2008-2009 hitting different industries by treatment status. Finally, to show that the effects are not driven by local policies, regional unemployment, or kindergarten availability (as older women might help with care-taking for their grandchildren), I include administrative district ("*kreis*") fixed effects, as such indicators, and corresponding reforms for unemployed and kindergarten expansions vary at the administrative district level. The results are similar to those in the baseline, confirming that the baseline results are not driven by local policies (see Panel F in Table 2).

Extensive vs. intensive margin of treatment. In Panel B of Table 3, I explore whether peer effects vary with the number of treated colleagues. While the main specification (Panel A) compares workgroups where all peers are treated versus not treated, it does not account for differences in the number of peers. Since both a group with one peer and one with five peers are coded as "treated", this specification may miss variation in treatment intensity.

To capture this, I interact the treatment indicator (dummy whether all the peers are treated) with the number of peers in the group. The resulting coefficients can be interpreted as the effect of an additional treated peer. As expected, the estimates are smaller in magnitude compared to the binary treatment, since they represent marginal rather than total effects. The direction and significance of the coefficients, however, confirm that peer effects are stronger with an additional peer, consistent with the reinforcement of influence through multiple treated colleagues. This supports the robustness of the main findings while highlighting variation in peer exposure strength.

Table 2: Robustness and sensitivity checks. First stage, reduced form, and peer effect regressions by included covariates

	First stage		Reduced form		Peer effect	
Panel A: baseline controls						
E at 62	0.116***	0.113***	0.015***	0.014***	0.133***	0.126***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.019)	(0.019)
Panel B: baseline + additional coworker controls						
E at 62	0.116***	0.111***	0.015***	0.016***	0.133***	0.146***
	(0.004)	(0.005)	(0.002)	(0.003)	(0.019)	(0.023)
Panel C: baseline + additional peer controls						
E at 62	0.116***	0.104***	0.015***	0.015***	0.133***	0.143***
	(0.004)	(0.005)	(0.002)	(0.003)	(0.019)	(0.025)
Panel D: baseline + firm and workgroup controls						
E at 62	0.116***	0.114***	0.015***	0.015***	0.133***	0.133***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.019)	(0.019)
Panel E: baseline + occupation and industry FE						
E at 62	0.116***	0.111***	0.015***	0.014***	0.133***	0.127***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.019)	(0.019)
Panel F: baseline + municipality FE						
E at 62	0.116***	0.113***	0.015***	0.015***	0.133***	0.130***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.019)	(0.019)
Control mean	0.716		0.785			
Observations	182584	182584	182584	182584	182584	182584
N workgroups	64324	64324	64324	64324	64324	64324
Controls	No	Yes	No	Yes	No	Yes

Notes: This table shows the effect of the rise in ERA on *employment at 62*: 1st stage (Equation 2), ITT (Equation 3), and IV regressions (Equation 4). **Panel A** includes baseline control variables: coworker and average peer education, wages, dummy for the location of establishments in the regions of the former West Geramany, establishment and workgroup sizes, number of peers, and coworkers. **Panel B** adds coworker full-time status, experience, foregn dummy, managerial position, estimated AKM worker fixed effect, while **Panel C** adds the same variables for peers, averaged over peers. **Panel D** adds share of women, share of older workers above 55 years old, share of full-time workers, median wages, number of coworkers, joint tenure of peers and coworkers, establishment AKM fixed effects, number of coworkers, number of female coworkers of each birth cohort. **Panel E** adds fixed effects for 2-digit industries and occupations (classification by Blossfeld (1985)). **Panel F** adds fixed effects for the administrative districts ("*kreis*"). The control means are the average values of the outcomes when I limit the sample to the workgroups with peers whose ERA was fixed at 60. Robust standard errors in parentheses are clustered at the workgroup level.

* ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Group size and linear-in-means assumption. As a robustness check, I examine whether the estimated peer effect varies with the number of peers a coworker has. The

Table 3: Robustness and sensitivity checks. First stage, reduced form, and peer effect regressions by treatment definition

	First stage		Reduced form		Peer effect	
Panel A: discrete treatment (baseline)						
Treatment	0.116***	0.113***	0.015***	0.014***	0.133***	0.126***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.019)	(0.019)
Panel B: continuous treatment: Treatment $\times$ N peers						
N treated peers	0.039***	0.061***	0.002*	0.006***	0.060*	0.105***
	(0.002)	(0.003)	(0.001)	(0.001)	(0.034)	(0.023)
Control mean	0.716		0.785			
Observations	182584	182584	182584	182584	182584	182584
N workgroups	64324	64324	64324	64324	64324	64324
Controls	No	Yes	No	Yes	No	Yes

Notes: This table shows the effect of the rise in ERA on *employment at 62*: 1st stage (Equation 2), ITT (Equation 3), and IV regressions (Equation 4). The control means are the average values of the outcomes when I limit the sample to the workgroups with peers whose ERA was fixed at 60. Robust standard errors in parentheses are clustered at the workgroup level.
* ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

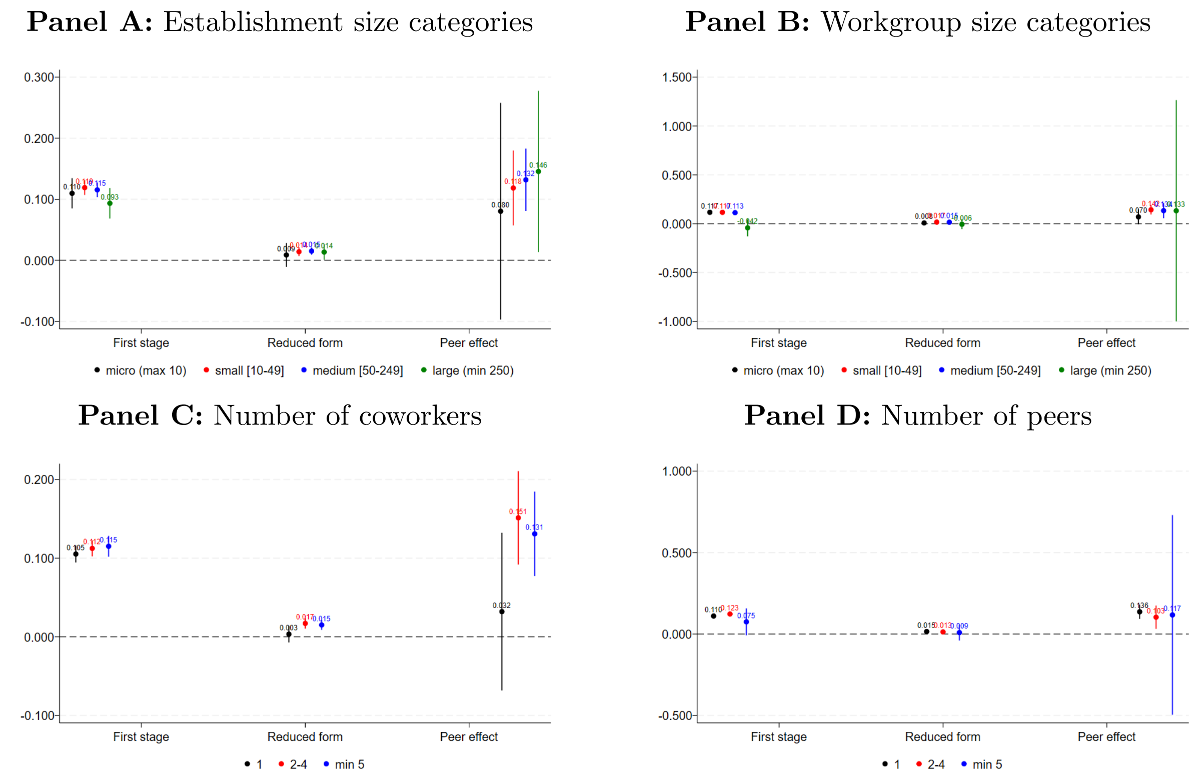
identification strategy relies on a linear-in-means structure, i.e., the marginal effect of a change in average peer employment should not depend on group size. If effects differed systematically between coworkers with few versus many peers, the structural invariance assumption would be violated. To test this, I re-estimate the peer effect across peer group size categories and interact the first-stage prediction with indicators for the number of peers. The estimates are stable across specifications, supporting the validity of the linear-in-means model.

To further account for workgroup composition, I conduct subsample analyses by establishment size, workgroup size, and number of coworkers (Panel A-C in Figure 5). Peer effects are somewhat larger in larger establishments and workgroups, but differences are not statistically significant. Similarly, varying the number of coworkers does not alter the results. Examining heterogeneity not only by the number of peers but also by workgroup and establishment size is important, as peer effects could be diluted in larger social contexts or confounded with institutional differences. The stability of estimates across these dimensions strengthens the interpretation of the results as genuine peer effects rather than artifacts of group composition.

Peer definition. In the baseline specification, I define peers as women employed at 56-57 years old, born either one year before or after the reform cutoff, i.e., 1951 (control) and 1952 (treatment) cohorts. I test whether losing this restriction leads to different peer effects. Table 4 shows the results from several alternative definitions of peers.

One standard assumption in settings that exploit discontinuity is manipulating the

Figure 5: Subsample analyses by establishment and workgroup size



Notes: Coefficient plots. The columns correspond to the first stage, reduced form (ITT) and 2SLS (peer effect) regressions for employment at 62. The points represent the estimated coefficients, and the bars represent the 95% confidence intervals. The subsample analyses are performed by categories of establishment size (**Panel A**), and workgroup size (**Panel B**), where micro establishments are those with less than 10 workers, small in the range 10-49, medium in the range 50-249, and large above 250. The bottom **Panels C and D** show subsample analyses by number of coworkers and peers: 1, 2-4, or more than 5. The estimates are based on the models that control for baseline characteristics (see Table 1 for the list.)

running variable. I exclude December 1951 and January 1952 cohorts from my peers from my regressions ("donut hole") and re-estimate the effects. The results in the baseline specification are robust to excluding the peers in the donut hole.

Next, in Panel C of Table 4, I expand the peer group definition to include peers born up to two years before and after the reform cutoff (1950–1953). This allows for broader peer exposure while maintaining a clear separation from coworkers (born 1953–1957). The estimated effects are larger across the first stage, reduced form, and IV specifications. This is consistent with stronger peer spillovers in larger or more intensively treated peer groups. Broader peer definitions likely capture more social interactions, learning, and norm diffusion, amplifying behavioral responses to the reform. These results reinforce the robustness of the peer effects and suggest that exposure intensity plays a key role in shaping responses to retirement age changes. This specification is less preferred than the baseline specification, because 1951 and 1952 cohort peers are more comparable to one another than peers born in a wider range around the reform cutoff.

Finally, in Panel D of Table 4, I relax the restriction that all peers must be born either before or after the reform cutoff. I instead allow for mixed peer groups and instrument employment at 62 using the number of treated peers, controlling for the number of peers and workgroup size, along with all the same covariates specified in baseline regressions. The estimated effects remain statistically significant but are smaller in magnitude compared to earlier panels. This attenuation is expected: variation in the intensive margin of peer exposure (i.e., partial treatment) results in weaker peer spillovers than the extensive margin contrast between fully treated and untreated peer groups. The smaller first stage reflects reduced contrast in peer group exposure and increased heterogeneity in group composition. Nevertheless, the results support the presence of meaningful peer effects even under more diffuse exposure conditions.

Workgroup definition. In the baseline specification, I defined workgroups as 4-digit occupations interacted with establishment, because the workers in more precisely defined occupations are likely to interact more with each other. In this set of robustness checks, I define workgroups as 3-digit occupations interacted with establishments (Panel B in Table 5), and 2-digit occupations interacted with establishments (Panel C in Table 5). Neither of these workgroup definitions leads to statistically different results from the baseline specification.

Coworkers' definition. The baseline specification defines coworkers as women in birth cohorts 1953-1957, employed in the year when their peer was 57. In this set of robustness tests, I restrict the sample of coworkers to those born starting from 1955, because these coworkers likely observe the retirement behavior between 60 and 62 fully, being at least two years younger than them (Panel B in Table 6). I further test whether the effects are stronger in the subsample of coworkers whose joint tenure with the peers is larger than two years in Panel C of Table 6. Finally, to exclude a concern that establishment closures or coworker deaths are wrongly attributed to peer effects, I estimate the analyses on coworkers whose establishments survived at least until 2019, and who did not exit (Panel D of Table 6) the labor market due to death (Panel E of Table 6). All three estimates—first stage, reduced form, and peer effects—are positive and significant, and the peer effects confidence intervals of models in both Panels D and E include the original peer effects estimate, highlighting that the effects are not explained by pooröy performing establishments or unhealthy coworkers.[25]

To test whether peer effects are driven by a specific cohort of coworkers, Figure 6 presents subsample analyses by coworker birth cohort. While the peer effects are not significantly different across cohorts, the point estimate for the 1956 cohort is notably

[25]In the baseline specification, I do not make these restrictions because that would lead to conditioning on post-treatment variables, potentially leading to bias. Here, such analyses are performed just to argue that the effects are not driven by bankruptcies of the establishment.

Table 4: Robustness and sensitivity checks. First stage, reduced form, and peer effect regressions by peer definitions

	First stage		Reduced form		Peer effect	
Sample A: 1 year peer bandwidth (baseline)						
Employment at 62	0.116***	0.113***	0.015***	0.014***	0.133***	0.126***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.019)	(0.019)
Control mean	0.716		0.785			
Observations	182584	182584	182584	182584	182584	182584
N workgroups	64324	64324	64324	64324	64324	64324
Sample B: 1 year peer bandwidth, excluding Dec. 1951 and Jan. 1952						
Employment at 62	0.114***	0.111***	0.016***	0.015***	0.141***	0.132***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.019)	(0.020)
Control mean	0.715		0.784			
Observations	180154	180154	180154	180154	180154	180154
N workgroups	61365	61365	61365	61365	61365	61365
Sample C: 2 years peer bandwidth						
Employment at 62	0.115***	0.112***	0.016***	0.014***	0.138***	0.130***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.019)	(0.019)
Control mean	0.716		0.784			
Observations	190784	190784	190784	190784	190784	190784
N workgroups	65690	65690	65690	65690	65690	65690
Sample D: 1 year peer bandwidth, peers on both sides						
Employment at 62	-0.000	0.041***	-0.002***	0.008***	4.418	0.188***
	(0.000)	(0.002)	(0.000)	(0.001)	(2.741)	(0.028)
Control mean	0.717		0.778			
Observations	327156	327156	327156	327156	327156	327156
N workgroups	108696	108696	108696	108696	108696	108696
Sample E (i): 1 year bandwidth around 1951 cohort (false sample)						
Employment at 62	0.010	-0.008	0.030*	0.025	3.081	-3.345
	(0.042)	(0.041)	(0.018)	(0.017)	(13.378)	(18.414)
Control mean	0.697		0.768			
Observations	2626	2626	2626	2626	2626	2626
N workgroups	895	895	895	895	895	895
Sample E (ii): 1 year bandwidth, males (false sample)						
Employment at 62	0.089	0.094**	0.015	0.045*	0.174	0.477
	(0.067)	(0.044)	(0.029)	(0.025)	(0.365)	(0.343)
Control mean	0.802		0.769			
Observations	1288	1288	1288	1288	1288	1288
N workgroups	419	419	419	419	419	419
Controls	No	Yes	No	Yes	No	Yes

Notes: This table shows the effect of the rise in ERA on *employment at 62*: 1^{st} stage (Equation 2), ITT (Equation 3), and IV regressions (Equation 4). Robust standard errors in parentheses are clustered at the workgroup level. Details about sample definitions can be found in subsection 3.2 and Appendix C. * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table 5: Robustness and sensitivity checks. First stage, reduced form, and peer effect regressions by workgroup definitions.

	First stage		Reduced form		Peer effect	
Sample A: establishments × 4-digit occupations (baseline)						
Employment at 62	0.116***	0.113***	0.015***	0.014***	0.133***	0.126***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.019)	(0.019)
Control mean	0.716		0.785			
Observations	182584	182584	182584	182584	182584	182584
N workgroups	64324	64324	64324	64324	64324	64324
Sample F: establishments × 3-digit occupations						
Employment at 62	0.117***	0.115***	0.016***	0.015***	0.138***	0.131***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.019)	(0.019)
Control mean	0.714		0.783			
Observations	181854	181854	181854	181854	181854	181854
N workgroups	65410	65410	65410	65410	65410	65410
Sample G: establishments × 2-digit occupations						
Employment at 62	0.115***	0.112***	0.016***	0.014***	0.138***	0.130***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.019)	(0.019)
Control mean	0.716		0.784			
Observations	190784	190784	190784	190784	190784	190784
N workgroups	65690	65690	65690	65690	65690	65690
Controls	No	Yes	No	Yes	No	Yes

Notes: This table shows the effect of the rise in ERA on *employment at 62*: 1st stage (Equation 2), ITT (Equation 3), and IV regressions (Equation 4). The control means are the average values of the outcomes when I limit the sample to the workgroups with peers whose ERA was fixed at 60. Robust standard errors in parentheses are clustered at the workgroup level. Details about sample definitions can be found in subsection 3.2 and Appendix C.
* ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

larger. One possible explanation is that younger coworkers in this cohort were able to fully observe the retirement behavior of their peers before making their own retirement decisions. In contrast, the 1957 cohort may be more distanced—both temporally and socially—from the peer group, leading to weaker identification with peers and thus smaller peer effects.

Falsification tests. I perform two types of placebo tests: using a placebo cutoff of 1951, i.e., adjacent non-treated cohorts, and using a placebo gender for peers (males).

First, I compare younger coworkers exposed to the 1950 vs. 1951 cohort of females, where both older cohorts were eligible to retire at age 60. For sample construction, see subsection 3.2. This test shows whether any observed differences are unique to the 1952 reform-treated cohort and not driven by broader age-structure trends in the firm. If the results provided above are attributed to peer effects, the first stage and reduced form effects should be insignificant when I center the reform window around a false cutoff, when

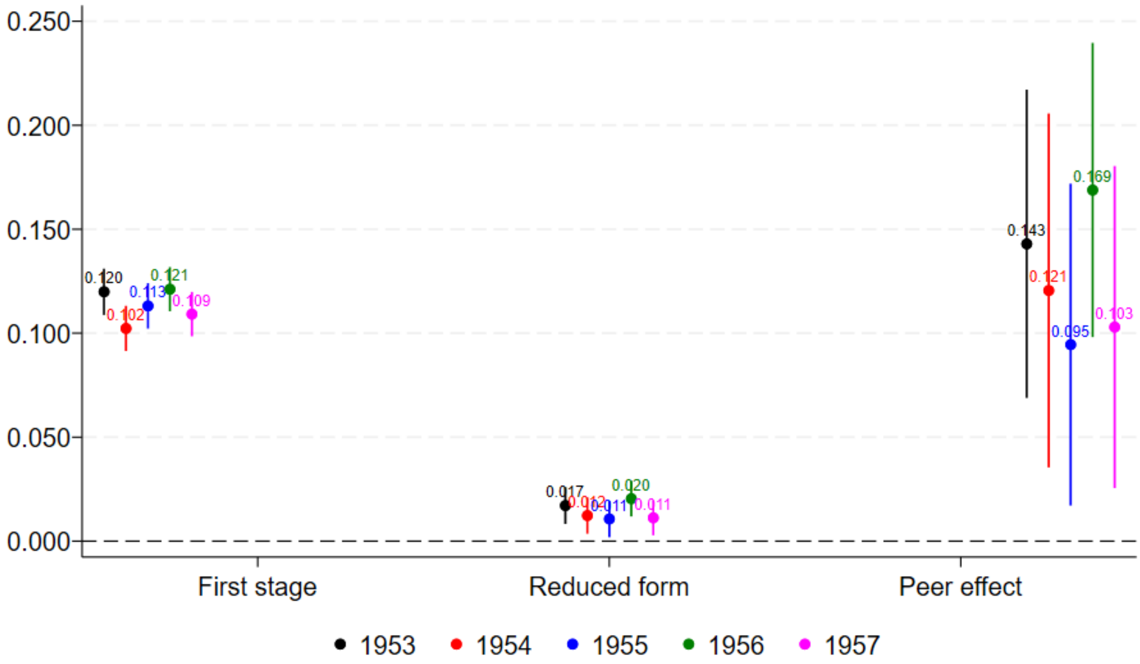
Table 6: Robustness and sensitivity checks. First stage, reduced form, and peer effect regressions by coworker definitions

	First stage		Reduced form		Peer effect	
Panel A: all the coworkers (baseline)						
Employment at 62	0.116***	0.113***	0.015***	0.014***	0.133***	0.126***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.019)	(0.019)
Control mean	0.716		0.785			
Observations	182584	182584	182584	182584	182584	182584
N workgroups	64324	64324	64324	64324	64324	64324
Panel B: coworkers born after 1955						
Employment at 62	0.117***	0.114***	0.016***	0.014***	0.133***	0.124***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.023)	(0.023)
Control mean	0.716		0.779			
Observations	114230	114230	114230	114230	114230	114230
N workgroups	51241	51241	51241	51241	51241	51241
Panel C: coworkers with joint experience over 2 years						
Employment at 62	0.116***	0.113***	0.016***	0.015***	0.140***	0.129***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.020)	(0.020)
Control mean	0.719		0.792			
Observations	155358	155358	155358	155358	155358	155358
N workgroups	57875	57875	57875	57875	57875	57875
Panel D: coworkers in firms that survived until 2019						
Employment at 62	0.105***	0.103***	0.009***	0.009***	0.084***	0.086***
	(0.005)	(0.004)	(0.002)	(0.002)	(0.022)	(0.022)
Control mean	0.739		0.804			
Observations	146949	146949	146949	146949	146949	146949
N workgroups	51293	51293	51293	51293	51293	51293
Panel E: coworkers who did not die						
Employment at 62	0.116***	0.113***	0.016***	0.015***	0.135***	0.129***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.019)	(0.019)
Control mean	0.716		0.787			
Observations	182149	182149	182149	182149	182149	182149
N workgroups	64241	64241	64241	64241	64241	64241
Controls	No	Yes	No	Yes	No	Yes

Notes: This table shows the effect of the rise in ERA on *employment at 62*: 1st stage (Equation 2), ITT (Equation 3), and IV regressions (Equation 4). The control means are the average values of the outcomes when I limit the sample to the workgroups with peers whose ERA was fixed at 60. Robust standard errors in parentheses are clustered at the workgroup level.
* ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

there was no change in the retirement age rules. If the first stage estimates turn out to be significant, then there is a difference between a year older vs. a year younger cohort (or workers born in 1951 vs. 1952), which is attributed to the birth cohort effects rather than the reform effects. A significant positive reduced form estimate would imply that

Figure 6: Subsample analyses by cohorts of coworkers



Notes: Coefficient plots. The columns correspond to the first stage, reduced form (ITT) and 2SLS (peer effect) regressions for employment at 62. The points represent the estimated coefficients, and the bars represent the 95% confidence intervals. The subsample analyses are performed by the cohorts of coworkers, 1953-1957.

the coworkers exposed to older (or 1952) birth cohorts are reacting to the birth cohort's attributes rather than the reform. Sample E (i) in Table 4 displays the insignificant first stage, reduced form, and peer effect results; therefore, the placebo test confirms that my main peer estimates are not driven by mechanical correlations or group-level shocks.

Second, I re-estimate the effects using placebo gender, i.e., male peers. The coworkers are still defined as women. Sample E (ii) in Table 4 displays the results. The first stage is somewhat significant when controlling for covariates, because the reform also removed the unemployment pathway to retirement starting from the 1952 cohort, which mostly affected men (Gudgeon et al., 2023). The insignificant reduced form and peer effect results confirm that my main peer estimates are not driven by some attributes specific to the 1951 vs. 1952 cohorts (which are fixed by gender), but rather by the reform that closed routes to early retirement for women.

5 Mechanisms

In the section above, I established the presence of sizable peer effects on old age employment and retirement delay. Although all coworkers had the same retirement rule, having a peer born before or after the reform cutoff altered their retirement decisions. In this section, I aim to understand the underlying mechanisms behind these peer effects. To explain the presence of peer effects, I test for three main potential mechanisms of peer effects in old-age employment: (1) conformity and social norms, (2) information transmission, and (3) work complementarities.[26] These mechanisms may help understand the relevant policy implications. For example, if information transmission about the reform is the main channel, then education intervention would be a relevant policy instrument. In contrast, an information fair may not help if the main channel is social norms in old-age employment.

5.1 Conformity and social norms

Gender role attitudes are particularly shaped through interactions with peers (Bramoullé et al., 2020). Theoretical frameworks from sociology and psychology emphasize that individuals are more likely to form connections with—and be influenced by—those who share similar characteristics. Theories of homophily (McPherson et al., 2001) and similarity-based attraction (Byrne, 1971) suggest that people gravitate toward peers who resemble them in age, gender, or background. In parallel, social identity and categorization theories propose that individuals adopt behavioral norms consistent with the social groups they identify with (Tajfel, 1981; Oakes et al., 1987). Applied to the workplace, these insights imply that women nearing retirement age may look to the behavior of similarly aged female coworkers when making their own employment decisions.

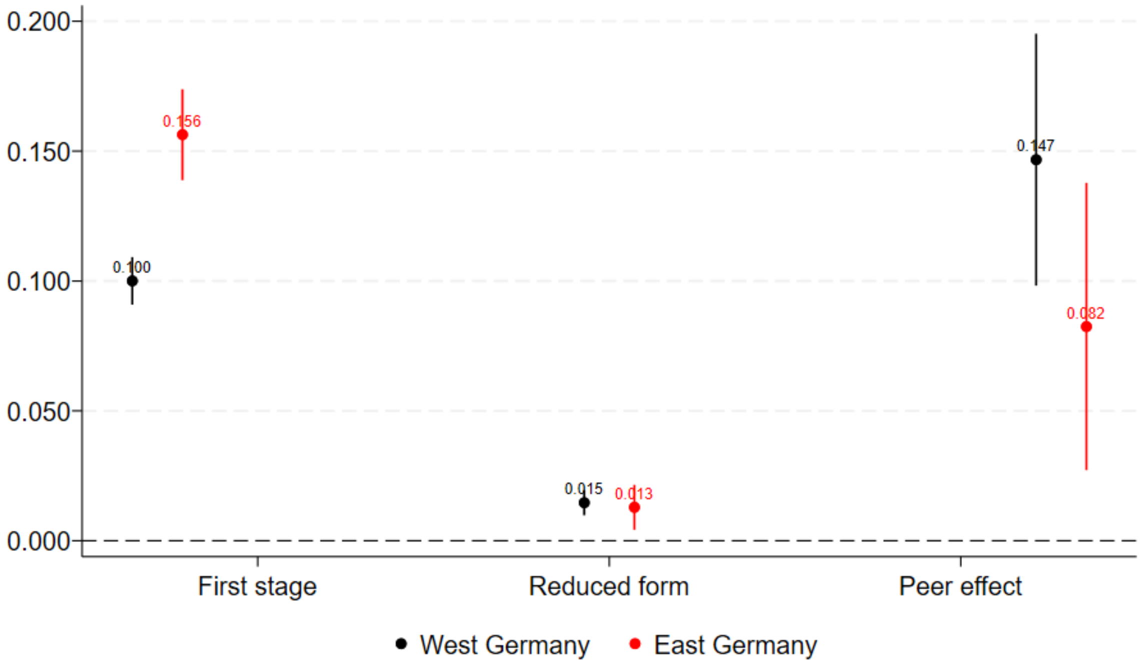
Geographical segregation in norms. East and West Germany provide a natural setting to test this conformity and social norms channel in old-age employment. Historical labor market institutions and cultural expectations diverged sharply before reunification: East German women had higher lifetime labor force participation, more continuous work histories, and stronger norms of full-time employment. If peer effects operate partly through shifting norms, they should be more pronounced where norms are *in flux*—namely, in West Germany. According to Welteke and Wrohlich (2019), conformity is expected in places with changing social norms. I therefore split my sample by the current workplace location in East versus West federal states (classifying Berlin as East).[27]

Figure 7 shows that the first stage estimates are larger for the East German women. These findings are in line with the findings in Geyer and Welteke (2021), who argue

[26]Peer effects literature in other settings, such as spousal retirement coordination, often discusses leisure complementarities as a channel (García-Miralles and Leganza, 2024). Leisure complementarities imply that coworkers prefer to retire in the same calendar month to spend more time together outside the firm. I do not analyze this channel, because leisure complementarities are less likely among coworkers than family members, and it is difficult to test for them in social security data without accompanying survey evidence on leisure activities.

[27]Given data limitations, I cannot observe workers' location prior to 1991; thus, classification is based on workplace location in the observation window rather than origins.

Figure 7: Subsample analyses by conformity measures (East vs. West Germany)



Notes: *Notes*: Coefficient plots. The columns correspond to the first stage, reduced form (ITT) and 2SLS (peer effect) regressions for employment at 62. The points represent the estimated coefficients, and the bars represent the 95% confidence intervals. The subsample analyses are performed by the workgroup locations being in the regions of former East and West Germany.

that East German women are more likely to fulfill the requirement for old-age pension (15 years of contribution years, ten of which are accumulated after the age of 40, see section 2 for details). My results are significantly higher for East German women by a 5.6 p.p. difference. The figure also confirms that where the reform substantially changed the social norms regarding women's old-age employment, the peer effects were larger. I find that in West Germany, there are 14.7 p.p. peer effects, compared to only 8.2 p.p. in the East. The East–West comparison likely reflects differences in social norm evolution: West German women, historically more aligned with the breadwinner model, appear to shift more toward peer-based coordination in late-career employment, whereas East German women had long experienced higher female labor force participation. This interpretation, however, warrants caution. Historical differences in social networks—such as the lower prevalence of "weak ties" in the former GDR due to surveillance concerns (Völker and Flap, 2001)—may have also limited the diffusion of peer effects in East Germany.[28] Nevertheless, because peer groups are constructed in 2008–2009 (when the women were 57), nearly two decades after reunification, much of the observed difference is plausibly driven by social norms in old-age employment. Moreover, the findings remain

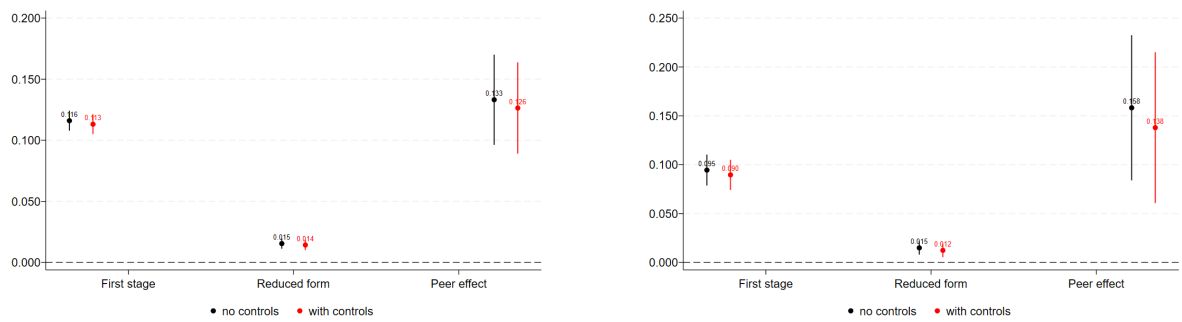
[28] I thank Kerstin Ostermann for suggesting this perspective.

robust when controlling for detailed occupation and industry fixed effects, suggesting they are not merely a reflection of industrial segregation.

Cross-gender peer effects. If norms are gendered, peers of the same gender should exert stronger influence on retirement decisions. Figure 8 confirms this hypothesis: first-stage and ITT effects are somewhat larger when coworkers are female, resulting in noisier peer effects for male coworkers. This asymmetry reinforces the interpretation that peer influence here operates partly through gender-specific norms about work at older ages, rather than workplace attributes or HR practices

Figure 8: Cross-gender first stage, reduced form, and peer effect regressions

Panel A: Female coworkers **Panel B:** Male coworkers



Notes: Coefficient plots. The columns correspond to the first stage, reduced form (ITT) and 2SLS (peer effect) regressions for employment at 62. The points represent the estimated coefficients, and the bars represent the 95% confidence intervals. The analyses are performed for female coworkers (baseline sample, **Panel A**) and male coworkers (**Panel B**). More details can be found in Table B.8.

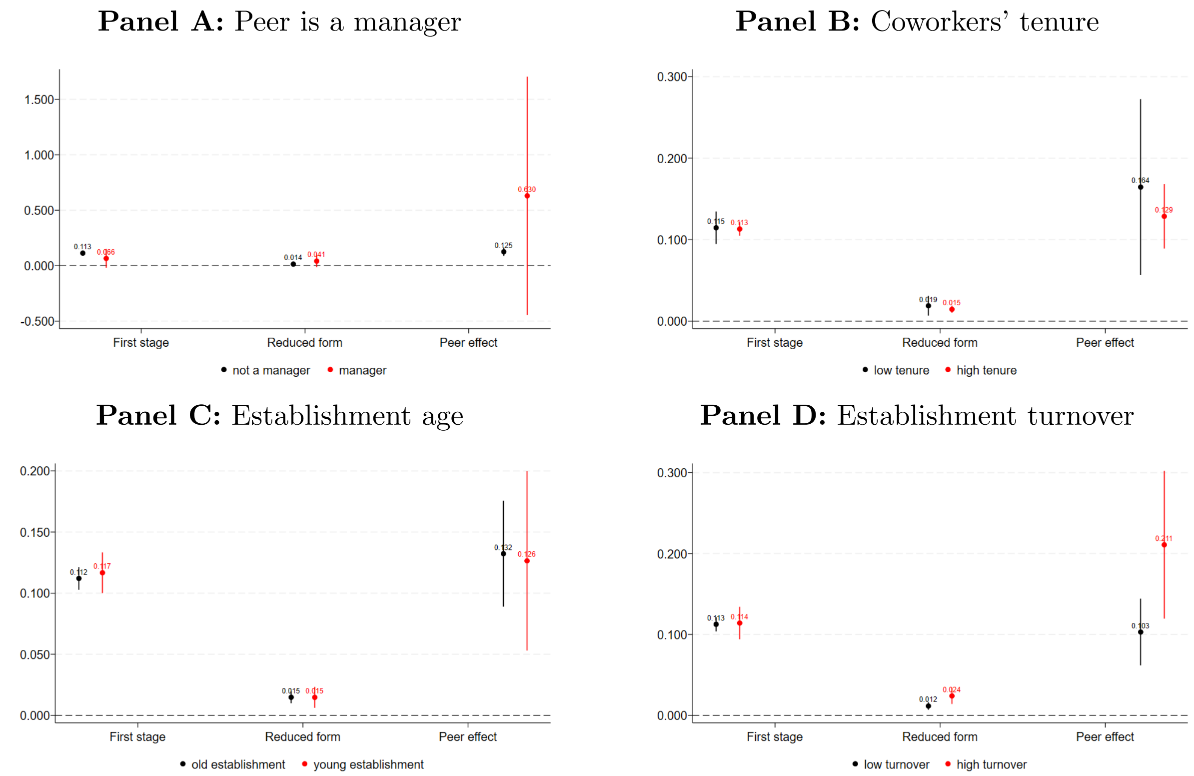
5.2 Information channel

I distinguish between two types of information that are important in the coworker-peer effects context: (1) information about the (new) retirement rules, and (2) the information provision channels about the costs and benefits of delaying retirement.

Information transmission about the program. Providing information about the costs and benefits of retiring at a certain age differs from providing information about the program itself. It is unlikely that coworkers learn from peers about the retirement pathways and changes to them because the retirement rules in Germany are universal, and there is an online platform to compute the retirement rules for individual cases.

Information transmission about the costs and benefits of delaying retirement. Even if the retirement rules are known to the coworkers, decisions on when to retire could be influenced by information about the consequences of retiring at a certain age. For example, some career-related uncertainties, such as uncertainties related to employer reactions, could be decreased by observing the peers' retirement decisions. According to social learning in networks (Goyal, 2011), and empirical findings on peer effects in other contexts (Dahl et al., 2014; Welteke and Wrohlich, 2019), the peer effects are expected to be larger for the coworkers who experience greater career-related uncertainty.

Figure 9: Subsample analyses by information channels



Notes: Coefficient plots. The columns correspond to the first stage, reduced form (ITT) and 2SLS (peer effect) regressions for employment at 62. The points represent the estimated coefficients, and the bars represent the 95% confidence intervals. The subsample analyses are performed by hierarchical position in the job (**Panel A**), coworkers' tenure (**Panel B**), establishment age, where young establishments are defined as those with at most ten years of existence (**Panel C**), and establishment turnover defined as turnover rate (share of the difference in hiring and separations in the establishment) being above or below 30% (**Panel D**).

I test for this channel by performing heterogeneity analyses on several measures, showing career uncertainty: the tenure of coworkers, the hierarchical position of the peer, age of the establishment, and establishment turnover. High career uncertainty is expected to be highly correlated with the information transmission channel; therefore, if this channel dominates, short-term tenured coworkers, and managerial positions of peers should be associated with higher peer effects, because managers often possess deep institutional knowledge, leadership experience, and relationship-specific investments that are difficult to transfer across firms, and are valuable particularly to less tenured workers. In addition, young or high turnover establishments, where there is larger uncertainty and job stability, should be associated with larger peer effects.

I define a coworker's tenure as low if she has been employed for less than two years

(730 days). Managerial occupations are defined based on the last two digits of 5-digit occupational codes.[29] I define young establishments as those that have operated for less than ten years. The establishments with high turnover are those that experienced over 30% turnover rate, defined as the difference in the number of hires and separations in the total workforce. All the variables are defined in the year when the peers were 57 years old.

Results, displayed in Figure 9, reveal that there are larger peer effects in establishments with larger turnover, while the rest of the measures do not display significant differences. Therefore, there is limited support for this channel. In combination with the strong suggestive evidence for the social norms mechanism, this finding could reveal that there is a social learning model, where information provided by peers reduces social, and to some extent, career-related uncertainty, similar to the findings on parental leave by Welteke and Wrohlich (2019).

5.3 Work complementarities

Beyond the standard peer effects channels of conformity and information transmission, I consider a novel mechanism relevant in the context of old-age employment: work complementarities, which capture productive complementarities between older and slightly younger coworkers. Opposed to leisure complementarities, work complementarities occur if the retirement delay of one worker prevents her coworkers from leaving the workforce. In settings where job tasks are interdependent, older workers may possess firm- or task-specific knowledge that enhances the productivity of their younger peers. When pension reforms delay the retirement of older peers, these complementarities can generate positive spillovers on the employment of their younger coworkers, either by increasing job satisfaction, team output, or by inducing firms to retain matched coworker pairs longer. This mechanism implies that peer exposure can increase coworker employment not through imitation but through improved workplace viability.

The literature on peer effects on performance highlights the influence of connections and peers in exerting effort. Kato and Shu (2009) provide evidence for peer effects in exerting effort within the hierarchies, while Bandiera et al. (2009) show how social connections in the workplace can operate across hierarchies. Such peer effects could also operate within the context of delayed retirements. In particular, this is likely to hold in connected teams, where the peers share workgroup-specific human capital (Bartel et al., 2014; Jaravel et al., 2018; Jäger and Heining, 2022), and therefore the delay of retirement of one worker would lead to the delayed retirement of his coworkers.

To explore this mechanism empirically, I exploit variation across occupations in the

[29] I create a variable showing managerial or supervisory status based on the last two digits of the 5-digit occupations. I pool the supervisors and managers into the dummy variable *manager*. I thank Philipp vom Berge for information on how to define managers in social security records.

degree of within-establishment task connectedness, i.e., interaction dependence between coworkers. In addition, I test whether the workgroups that are performing occupations that are not easily substitutable externally from the commuting zones are more likely to lead to higher peer effects due to the loss of productivity because of the interruption of peer connections. Finally, I classify the occupations as "bottleneck", and industries as tradable vs. untradable, because employees in such environments that are difficult to substitute for could lead to larger peer effects due to the disruption of work.

Measure group 1: main tasks of the workgroup. In my main specification, I define peer groups at the four-digit occupation-by-establishment level, capturing settings in which workers likely perform closely related tasks. Under the assumption that task complementarity is higher within narrowly defined occupations, stronger peer effects in these cells would be consistent with a complementarity channel. Although direct measures of teamwork intensity are unavailable in administrative data, the heterogeneity in peer effects is tested by the main tasks implemented in a given workgroup occupation.

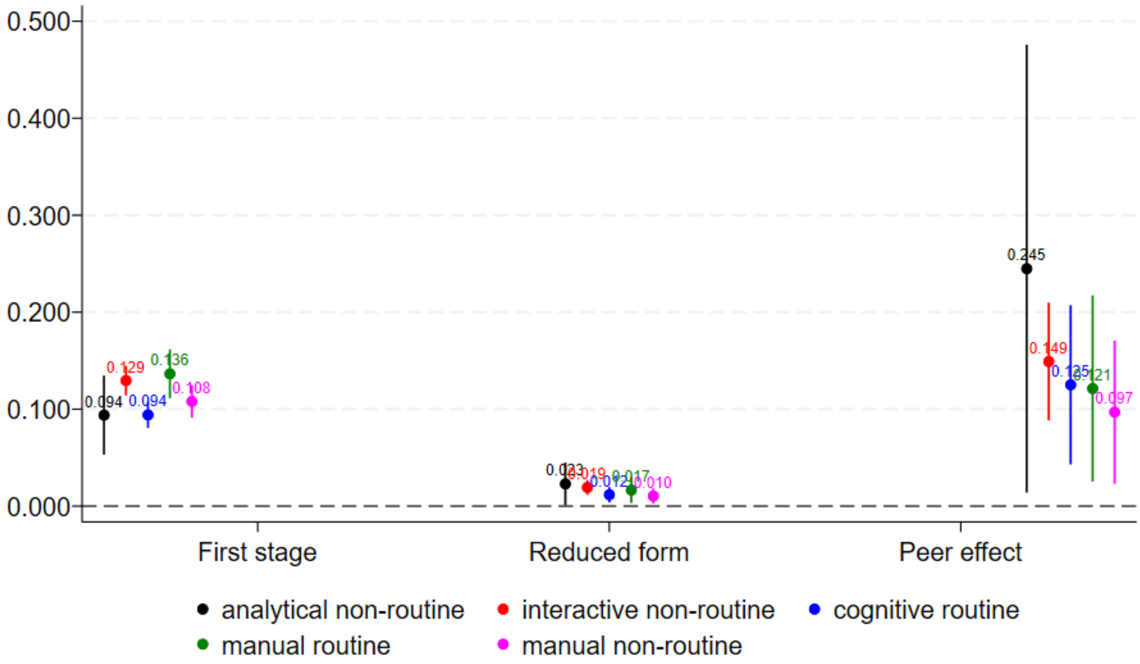
Following the classification in Dengler et al. (2014), I assign occupations to categories along two key dimensions: (i) the predominant skill content—analytical, interactive, cognitive, or manual tasks; and (ii) the degree of routineness—routine versus nonroutine.[30] Occupations differ in the extent to which coworkers' actions affect individual employment decisions. Following the task classification in Dengler et al. (2014), I expect complementarities to be strongest in *interactive non-routine* and *analytical non-routine* jobs, where output relies on frequent information exchange, coordination, and mutual learning. In such settings, the continued employment of peers may directly influence one's own decision to remain, by sustaining team productivity and preserving workgroup-specific knowledge. Even certain *cognitive routine* tasks, when embedded in coordinated workflows, can exhibit similar spillovers. In contrast, *manual routine* and *manual non-routine* occupations—particularly those performed independently—are less likely to generate strong complementarities.

Figure 10 displays the results. I find no statistically significant difference in peer effects across the task types in workgroups. Although point estimates suggest slightly larger effects among analytical non-routine, interactive non-routine, and cognitive routine tasks, the standard errors are large, and differences are not statistically significant. These findings suggest that the influence of peer retirement behavior can operate broadly in demographic and workplace environments.

Measure group 2: external substitutability. In addition, I examine heterogeneity in peer effects using three proxies for job substitutability: external labor market thickness (ELMT), industry tradability, and bottleneck occupations. These measures

[30]The task classification is matched to the main dataset using three-digit occupation codes. Since baseline workgroups are defined at the four-digit occupation level, the main task classification is, therefore, more aggregated than the workgroup definition.

Figure 10: Subsample analyses by main tasks in the workgroups



Notes: Coefficient plots. The columns correspond to the first stage, reduced form (ITT) and 2SLS (peer effect) regressions for employment at 62. The points represent the estimated coefficients, and the bars represent the 95% confidence intervals. The subsample analyses are performed by five main task categories for a given occupation.

capture the degree of work complementarity versus substitutability. In thin external labor markets, bottleneck occupations, and non-tradable sectors, internal workers are less easily replaced by external hires. Consequently, disruptions within such groups are more likely to entail substantial productivity and profit losses through higher inter-dependence of such workers, amplifying the potential for peer effects. These measures and hypotheses are motivated by the worker substitutability literature (Badalyan, 2025; Ginja et al., 2023; Jäger and Heining, 2022; Huebener et al., 2024; Schmutte and Skira, 2023), and are described below.

External labor market thickness captures how easily workers in a given industry or occupation can be substituted from the local commuting zone. I create 141 local labor markets based on high within-region and low between-region commuting for work (Kropp and Schwengler, 2011). Next, I create an index that captures the local share of occupation (or industry) employment over the national share of occupation (or industry) employment, following Ginja et al. (2023). In the main specification, I compute the index by counting female employment only, to take into account the industry and occupation-based segregation by gender in Germany.[31] For example, automobile industry workgroups

[31]For industry or occupation k and commuting zone c, the index is computed as follows: $ELMT_{kc} =$

in Hamburg would have more difficulties replacing their workers than those in Munich.

Bottleneck occupation indicators. I merge my data with the bottleneck occupation indicators from the Bundesagentur für Arbeit, mapped to 4-digit KldB 2010 codes.[32] I utilize the earliest available classifications at the national level; that is, in 2011-2012, and classify an occupation as a bottleneck if it was a bottleneck occupation in 2011 or 2012.

Industry tradability. In tradable industries, it is easier to substitute workers through outsourcing than in non-tradable industries (Drenik et al., 2023). I classify the industries by tradability following Gregory et al. (2022).[33]

Figure 11) displays the results. While industry tradability and occupational ELMT categories do not produce significantly different peer effects, the effects are somewhat larger in thin ELMT industries, where external substitutability is lower. The sample size is small for bottleneck occupations because there were very few in 2011-2012, and the standard errors are too large to draw firm conclusions. Overall, while I cannot fully rule out work complementarity mechanisms, I find no consistent pattern suggesting they systematically drive the results.

Measure group 3: old-age employment in the same workgroup. To further explore the work complementarity channel, I re-estimate the effects focusing on employment within the same workgroup as in the exposure year (when the peer was 57). This setting likely increases daily interaction and task interdependence, amplifying complementarities while mitigating pure crowd-out effects. The results (last row of Table B.4) reveal a smaller first stage compared to the full sample—consistent with a more restricted subset of the total effect—but the reduced form estimate nearly doubles. This suggests that sustained exposure to peers who delay retirement significantly strengthens influence on individual retirement decisions. Correspondingly, the peer effect estimates in this subsample are more than twice as large as in the full sample, highlighting the importance of prolonged within-group interaction.

6 Discussion

This section addresses three limitations, reconciles the findings with the literature, and quantifies the reform's social multiplier.

Crowd-out effects. The first concern is that delayed retirement blocks career pro-

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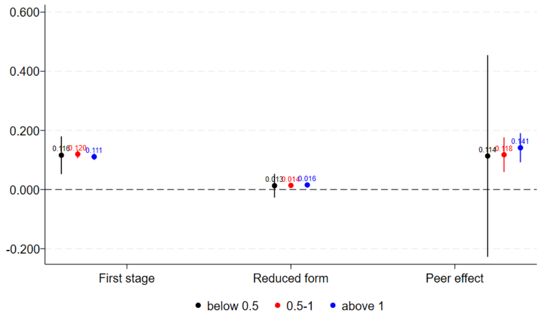
$\frac{(NW_{kc}/NW_c)}{(NW_k/NW)}$, where NW is the number of women.

[32] The classification is based on six indicators, including vacancy duration, job seeker ratios, unemployment specialization, and wage dynamics. I thank Hannah Illing and Anton Klaus for their help with the data.

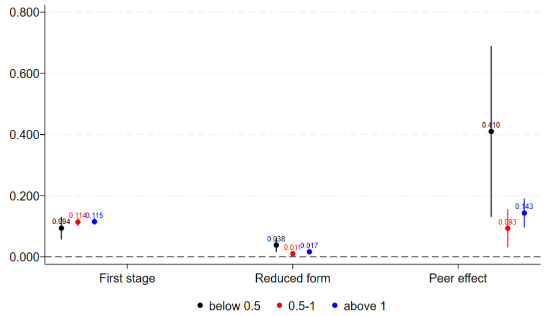
[33] *Tradable industries* are: Mining (WZ08: B); Manufacturing (WZ08: C); Electricity, water supply (WZ08: D, E); Transport, storage (WZ08: H); Financial services (WZ08: K); Real estate (WZ08: L); Agriculture (WZ08: A); Information and communication (WZ08: J); Scientific and technical services (WZ08: M). I thank Duncan Roth for the help with the data.

Figure 11: Subsample analyses by complementarities and substitutabilities

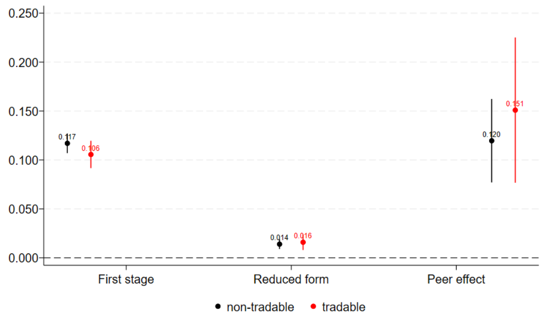
Panel A: ELMT (occupation, women only)



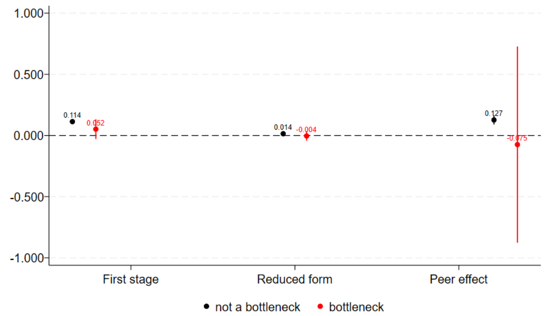
Panel B: ELMT (industry, women only)



Panel C: Industry tradability



Panel D: Bottleneck occupations



Notes: Coefficient plots. The columns correspond to the first stage, reduced form (ITT) and 2SLS (peer effect) regressions for employment at 62. The points represent the estimated coefficients, and the bars represent the 95% confidence intervals. The subsample analyses are performed by *external labor market thickness (ELMT)*, based on the commuting zone at most half as concentrated in a given occupation (**Panel A**) or industry (**Panel B**) relative to the country-level ($ELMT < 0.5$), or at least half as concentrated but less concentrated than the country-level ($0.5 < ELMT < 1$), and at least as concentrated as the country-level concentration ($ELMT > 1$). **Panel C** displays results by tradable sectors, and **Panel D** by bottleneck occupations.

gression or job slots of coworkers (Bianchi et al., 2023), biasing estimates downward. Several factors mitigate this. Peer effects remain significant after controlling for firm shocks, industry and regional fixed effects, and local labor market conditions. Moreover, the stronger effects within workgroups at older ages are more consistent with complementarities—where experienced coworkers raise productivity and reinforce norms—than with substitutability. Restricting the analysis to coworkers who remain in the same workgroup, I still find significant effects; therefore, compositional bias is less likely. Thus, while crowd-out cannot be ruled out, the opposing directions of crowd-out and complementarities suggest that the estimated peer effects represent a conservative lower bound.

Outcome variable and decomposition. The main outcome—employment at age 62—captures whether the last observed labor market activity occurred at or after 62, and may include unemployment. Since the data track only establishment employment histories, a full decomposition is not possible. However, Table B.4 shows that about 75% of this variable reflects continued employment at the same firm, with the remainder spread across other workgroups or unemployment. Importantly, unemployment is unlikely to drive results, as Geyer and Welteke (2021) find little substitution into unemployment. Peer effects are even stronger when restricting outcomes to same-firm employment, reinforcing that the measure mainly captures genuine employment spillovers.

Descriptive evidence further supports this. In the pre-reform 1951 cohort, women aged 60–62 were distributed across unemployment (6.2%), disability (8.3%), and inactivity (4.8%). After the 1952 cutoff, unemployment and inactivity rose modestly due to the passive continuation of prior states, while disability remained unchanged. My ITT results imply that unemployment at 60–62 fell by 1.6 p.p. when peers delayed retirement, corresponding to an 18.8 p.p. rise in labor force activity—a sizeable reduction in inactivity. Compared with other social insurance contexts, such as peer spillovers in parental leave (Welteke and Wrohlich, 2019), the effects are smaller but still substantial given the smaller ITT.

Mechanisms and social multiplier. Peer effects may reflect information channels, work complementarities, and social norms. While subsample analyses lack the power to fully disentangle these channels, no evidence contradicts the importance of social norms.

To assess aggregate importance, I compute a behavioral social multiplier: the ratio of total (direct plus peer) observed employment effects to direct effects alone, which could lower the marginal value of public funds (MVPF) of retirement age increase (Hendren and Sprung-Keyser, 2020). With a direct reform effect of 11.6 p.p. and a peer effect of 1.5 p.p., the multiplier equals 1.13. Thus, for every woman employed at an older age, an additional 0.13 coworker women remained employed due to peers.

Regional heterogeneity. Effects differ across regions. Direct responses are larger in the East (15.6 p.p.) than in the West (10.0 p.p.), consistent with East Germany's longer history of female employment. Yet peer effects are stronger in the West (14.7 vs. 8.2 p.p.), suggesting that West German workplaces are more susceptible to norm-based change. This pattern points to peer influences as a mechanism for convergence: even if direct responses are weaker, norms can shift collectively, allowing slower-responding regions to catch up.

Overall, the discussion highlights that retirement reforms do not operate in isolation. Peer interactions amplify their reach, help shift workplace norms, and may facilitate regional convergence in old-age employment behaviors.

7 Conclusion

Institutional rules and financial incentives alone cannot fully explain retirement behavior. Even when reforms raise retirement ages, some women remain inactive at the targeted age of the reform, due to prior pension eligibility, health, caregiving, self-employment, or joint leisure with spouses. A small fraction may also be on UI or DI benefits. This paper highlights a behavioral factor: peer effects in retirement decisions. Using a German reform that changed early retirement eligibility, I show that coworkers' retirement choices—especially within close workgroups—substantially influence individual employment at older ages. Evidence points mainly to conformity and social norms, rather than information transmission or work complementarities.

Retirement decisions are shaped by the workplace social context. As women's labor force participation gradually extends beyond age 60 in many countries, peer influences regarding old-age employment can shift norms from non-employment to employment at the reform-targeted age. Regional differences between East and West Germany suggest that preexisting social norms condition how new behaviors spread, with peers potentially contributing to convergence—or persistence—of regional patterns.

The findings have two key implications. First, policy success depends not only on financial incentives but also on shaping or leveraging workplace norms. Second, group-level interventions—such as team-based retention programs or role-model strategies—may achieve behavioral change more efficiently than individual incentives. Future research could explore similar reforms in other countries and examine how the shift to gender-neutral retirement rules spills over into women's education decisions, intra-family bargaining, and household gender norms, such as division of caregiving responsibilities.

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Peer Effects in Old-Age Employment Among Women

Sona Badalyan

August 20, 2025

Supplementary Online Appendix

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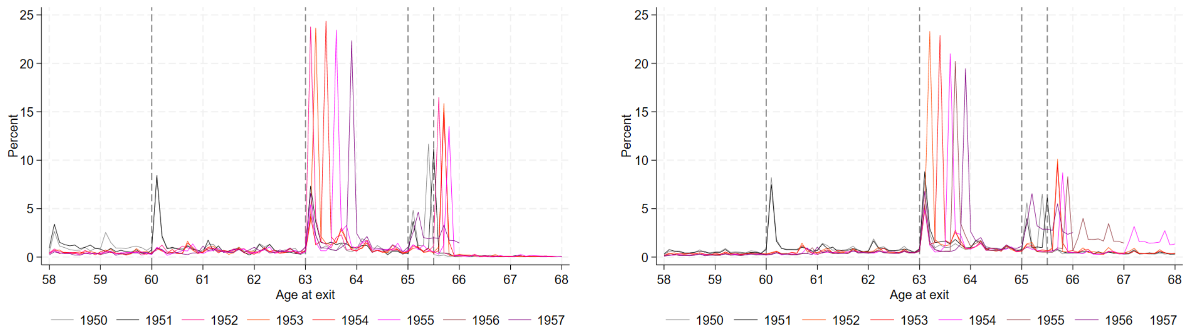
A Appendix figures

Figure A.1: Retirement age distribution by birth cohorts

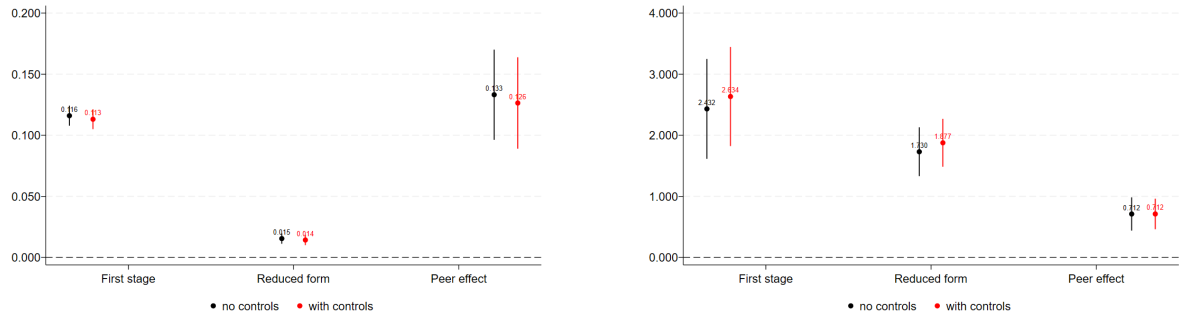
Panel A: 2% random sample **Panel B:** Sample in this study



Notes: This graph shows the distribution of retirement ages of the cohort 1950-1957, employed at 57 years old. The graph is generated from the 2% random sample of the population of IEB records (**Panel A**), and the sample used in this paper (**Panel B**). The gray and black lines correspond to the 1950 and 1951 cohorts of women– the cohorts that allowed claiming pensions as early as 60. The rest of the lines demonstrated the retirement age distribution for the cohorts 1952-1957, for whom the retirement age was raised.

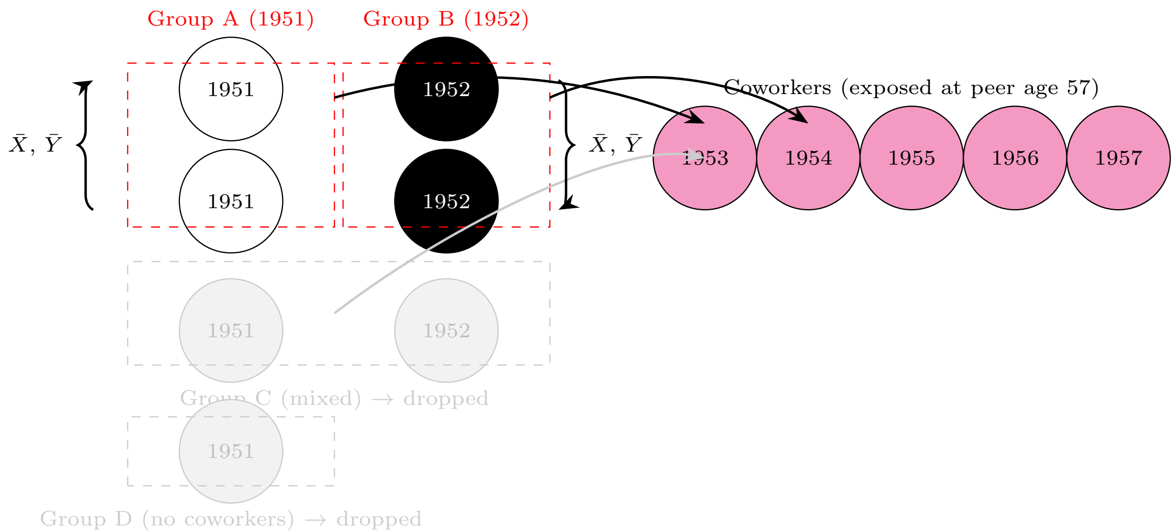
Figure A.2: Baseline reduced form and peer effect regressions

Panel A: Employment at 62 **Panel B:** Retirement age (in months)



Notes: Coefficient plots. The columns correspond to the first stage, reduced form (ITT), and 2SLS (peer effect) regressions for employment at 62 (**Panel A**) and retirement age in months (**Panel B**). The points represent the estimated coefficients, and the bars represent the 95% confidence intervals. The black estimates represent the regressions with no controls, while the red estimates show regressions with controls. Corresponding tables with more details can be found in Table 1 and the first row in Table B.4.

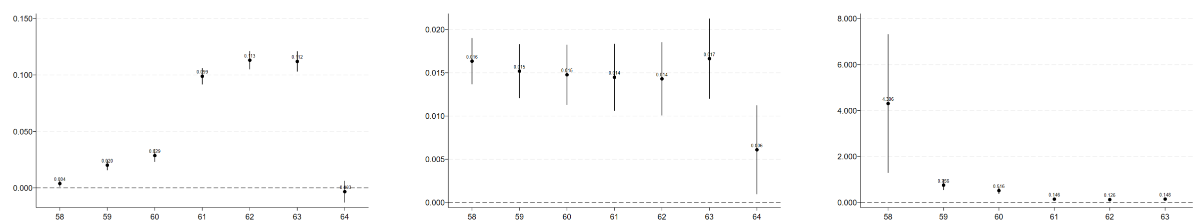
Figure A.3: Sample construction: visual walkthrough



Notes: This diagram illustrates the sample construction for baseline regressions. Peers are defined as those employed at the age of 56-57, belonging to the 1951 (control group, colored in black) or 1952 (treatment group, colored in white). I allow peers to be in either the control group (**Group A**) or the treatment group (**Group B**). I exclude the mixed workgroups, where peers appear on both sides of the cutoff (**Group C**, shaded out in gray). From the remaining workgroups, I remove those workgroups (**Group D**, shaded out in gray) where the peers have no coworkers (defined as women of birthcohorts 1953-1957 who were employed in the same workgroup when the peer was 57 years old, colored in pink). The sample size after each restrictions can be found in The visual representation of sample construction can be found in Table B.3.

Figure A.4: First stage, reduced form and peer effect regressions by age

Panel A: First stage **Panel B:** Intention-to-treat **Panel C:** Peer effect



Notes: Coefficient plots. The columns correspond to the first stage (**Panel A**), reduced form (ITT, **Panel B**), and 2SLS (peer effect, **Panel C**) regressions for employment by age. The points represent the estimated coefficients, and the bars represent the 95% confidence intervals. I control for the coworker and average peer education, wages, West residence, establishment, and workgroup sizes, number of peers, and coworkers. Corresponding results with more details can be found in the fourth and sixth columns of Table B.4.

B Appendix tables

Table B.1: Retirement rules by birthcohorts and pathways

Birth cohorts	Pathway					
	women's		long-insured		regular	
	ERA	NRA	ERA	NRA	ERA	NRA
1950	60	65	63	65 y 4 m	65 y 4 m	
1951	60	65	63	65 y 5 m	65 y 5 m	
1952	-	-	63	65 y 6 m	65 y 6 m	
1953	-	-	63	65 y 7 m	65 y 7 m	
1954	-	-	63	65 y 8 m	65 y 8 m	
1955	-	-	63	65 y 9 m	65 y 9 m	
1956	-	-	63	65 y 10 m	65 y 10 m	
1957	-	-	63	65 y 11 m	65 y 11 m	

Notes: This table demonstrates the statutory retirement ages by cohorts and pathways. Starting from the 1952 cohort, the women's pathway to retirement was abolished. NRA stands for Normal Retirement Age, while ERA stands for the Early Retirement Age. "y" is an abbreviation for Year, while "m" is an abbreviation for months. For example, a person born in 1952 is not eligible for women's pathway to retirement (marked by "-"), but can claim pensions at 63 (by long-insurance pathway), or, for the full benefit amount, can retire at 65 years and 6 months (the NRA of long insured and regular pathways). The visual representation of these rules can be found in Figure 2

Table B.2: Sample restrictions to obtain the original data extract

Restriction	N establishments	N workers
Universe of establishments and workers in 1995-2019	8,241,529	69,296,143
+ observed in 2008	1,958,754	23,798,218
+ employed at least 1 focal worker in 2008	352,836	15,364,408
+ private sector	317,912	13,388,108
+ at least 5 employees in 2008	193,612	13,059,745
+ at most 500 employees in 2008	190,228	9,117,917
Universe of affected establishments, and their employed workers in 1995-2019	190,228	26,593,003

Notes: This table shows the number of establishments and workers after each restriction in the data extract requested.

Table B.3: Baseline sample size after each restriction in German social security data

	N workgroups	N treated peers (1952)	N control peers (1951)	N coworkers (1953-1957)
Unrestricted sample	**286,046**	**110,796**	**103,639**	
Restriction 1	139,833	80,114	73,533	184,986
Restriction 2	64,324	40,627	36,828	182,584

Notes: This table records the sample size after each of the restrictions in German social security data. The first column names the restrictions. Workgroups are defined as establishments interacted with 4-digit occupations. **"Unrestricted sample"** stands for the sample of employees in workgroups with at least 1 "peer", defined as a woman born in 1951-1953, who never worked as a miner or sailor, and was employed in given workgroups at the age 56-57. The **"Restriction 1"** restricts the workgroups to those where the peers are born either before (in 1951) or after (in 1952) reform cutoff. **"Restriction 2"** represents the baseline sample. It further restricts the sample to the workgroups which had at least 1 coworker employed at the year when the workgroup peer was 57. The visual representation of sample construction can be found in Figure A.3.

Table B.4: First stage, reduced form, and peer effect regressions for indicators for employment at 58-64

	First stage		Reduced form		Peer effect	
Retirement age (in months)	2.432***	2.634***	1.730***	1.877***	0.712***	0.712***
	(0.417)	(0.414)	(0.204)	(0.200)	(0.139)	(0.127)
Control mean	769.753		760.848			
E at 58	0.005***	0.004***	0.017***	0.016***	3.747***	4.306***
	(0.001)	(0.001)	(0.001)	(0.001)	(1.156)	(1.539)
Control mean	0.978		0.918			
E at 59	0.021***	0.020***	0.016***	0.015***	0.754***	0.756***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.106)	(0.110)
Control mean	0.935		0.894			
E at 60	0.030***	0.029***	0.016***	0.015***	0.517***	0.516***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.071)	(0.075)
Control mean	0.897		0.869			
E at 61	0.102***	0.099***	0.016***	0.014***	0.152***	0.146***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.020)	(0.020)
Control mean	0.778		0.830			
E at 62 (baseline)	0.116***	0.113***	0.015***	0.014***	0.133***	0.126***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.019)	(0.019)
Control mean	0.716		0.785			
E at 63	0.113***	0.112***	0.017***	0.017***	0.154***	0.148***
	(0.005)	(0.005)	(0.002)	(0.002)	(0.021)	(0.021)
Control mean	0.648		0.731			
E at 64	-0.007	-0.003	0.002	0.006**	-0.264	-1.791
	(0.005)	(0.005)	(0.003)	(0.003)	(0.467)	(2.783)
Control mean	0.456		0.422			
E at 60-62	0.083***	0.080***	0.016***	0.015***	0.188***	0.181***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.023)	(0.023)
Control mean	0.797		0.828			
E at 60-62 (same firm)	0.062***	0.065***	0.027***	0.029***	0.444***	0.447***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.046)	(0.044)
Control mean	0.235		0.242			
Observations	182584	182584	182584	182584	182584	182584
N workgroups	64324	64324	64324	64324	64324	64324
Controls	No	Yes	No	Yes	No	Yes

Notes: This table shows the effect of the rise in ERA on labor market outcomes (first column): 1st stage (Equation 2), ITT (Equation 3), and IV regressions (Equation 4). The control means are the average values of the outcomes when I limit the sample to the workgroups with peers whose ERA was fixed at 60. Robust standard errors in parentheses are clustered at the workgroup level.

* ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table B.5: Post-reform labor market shares and treatment effects at ages 60–62

Panel A: results from Geyer and Welteke (2021)			
	share (cohort 1951)	RDD around 1952	
Employment	44.1	13.5	
Unemployment (UI)	6.2	5.2	
Employment+UI	50.3	17.5	
Disability (DI)	8.3	–	
Inactivity	4.8	6.2	
DI + Inactivity	19.3	6.2	
Panel B: results from this paper			
	share (cohort 1952)	1^{st} stage	
LM activity of peers	79.7	11.6	
	share (exposed to 1951 peers)	ITT effect	peer effect
LM activity of coworkers	82.8	1.6	18.8

Notes: All the numbers are presented in percentage points. **Panel A** displays the shares based on Geyer and Welteke (2021), which include more detailed labor market activity states for peers (birth cohorts 1951-1952), but no workgroup information for defining their coworkers (birth cohorts 1953-1957). **Panel B** displays the results in this paper, which cannot disentangle between the "active states" but includes detailed workgroup data. "LM activity" includes any social security contributions in IEB records, such as regular employment and UI receipt.

Table B.6: Descriptive statistics and balance test for peers born before and after the reform cutoff

	Before the reform			After the reform				
Variable	Mean	SD	N	Mean	SD	N	Difference	t
vocational education	0.806	0.395	36357	0.807	0.395	40191	.001	-0.317
university education	0.073	0.261	36357	0.082	0.275	40191	-.009	-4.555
earnings	25978.191	13464.784	36828	26607.134	13921.251	40627	-628.943	-6.378
full-time	0.587	0.492	36828	0.571	0.495	40627	0.016	4.536
tenure	4401.402	2973.67	36828	4397.013	3045.511	40627	4.389	0.2026
foreign	0.043	0.203	36828	0.043	0.204	40627	-0.000	-0.186
manager	0.005	0.072	36828	0.006	0.08	40627	-.001	-2.354
worker AKM effect (2000-2006)	4.328	0.389	25196	4.341	0.39	27662	-.013	-3.757
establishment size	41.064	50.121	36828	44.965	56.282	40627	-3.901	-10.146
workgroup size	28.312	38.002	36828	31.511	44	40627	-3.198	-10.774
N peers	1.423	0.794	36828	1.481	0.93	40627	-.058	-9.296
East Germany	0.219	0.413	36828	0.233	0.423	40627	-.014	-4.635
share 55+ in workgroup	0.248	0.166	36828	0.265	0.184	40627	-.017	-13.341
share full-time in workgroup	0.629	0.337	36828	0.615	0.338	40627	.014	5.649
share of women in workgroup	0.805	0.219	36828	0.799	0.218	40627	.006	3.623
median workgroup wage in workgroup	25973.615	11680.799	36828	26684.72	12124.672	40627	-711.105	-8.294
N coworkers in workgroup	4.279	5.189	36828	4.68	6.113	40627	-.400	-9.773
establishment AKM effect (2000-2006)	0.087	0.224	35525	0.095	0.225	37768	-0.007	-4.396
N coworkers (1953 birth cohort)	0.595	0.937	36828	0.629	1.043	40627	-0.034	-4.809
N coworkers (1954 birth cohort)	0.625	0.964	36828	0.665	1.08	40627	-0.040	-5.376
N coworkers (1955 birth cohort)	0.633	0.992	36828	0.698	1.136	40627	-0.065	-8.479
N coworkers (1956 birth cohort)	0.679	1.049	36828	0.738	1.199	40627	-0.059	-7.253
N coworkers (1957 birth cohort)	0.687	1.056	36828	0.759	1.203	40627	-0.071	-8.718

Notes: This table shows the summary statistics and balance test for observable characteristics by treatment status (coworkers in workgroups with peers whose ERA raised, vs. those whose ERA was 60.)

Table B.7: Descriptive statistics and balance test for coworkers exposed to peers born before and after the reform cutoff

	Before the reform			After the reform				
Variable	Mean	SD	N	Mean	SD	N	Difference	t
vocational education	0.792	0.406	113413	0.789	0.408	129509	.002	1.4094
university education	0.104	0.305	113413	0.114	0.318	129509	-.010	-7.906
earnings	29543.22	16055.18	114464	30614.501	16687.902	130756	-1071.281	-16.1422
full-time	0.652	0.476	114464	0.637	0.481	130756	0.016	8.187
tenure	4042.044	3096.933	114464	4072.381	3170.994	130756	-30.337	-2.389
foreign	0.048	0.213	114464	0.046	0.21	130756	0.001	1.406
manager	0.004	0.062	114464	0.005	0.07	130756	-0.001	-3.646
worker AKM effect (2000-2006)	4.401	0.412	84905	4.423	0.413	96410	-.023	-11.622
establishment size	71.108	74.068	114464	79.079	83.328	130756	-7.970	-24.879
workgroup size	57.08	66.680	114464	63.828	75.044	130756	-6.748	-23.393
N peers	1.377	0.776	114464	1.454	0.935	130756	-0.077	-22.050
East Germany	0.213	0.409	114464	0.234	0.423	130756	-0.021	-12.531
share 55+ in workgroup	0.195	0.133	114464	0.211	0.145	130756	-.0157768	-27.9140
share full-time in workgroup	0.664	0.325	114464	0.649	0.325	130756	.0147866	11.2550
share of women in workgroup	0.713	0.26	114464	0.705	0.257	130756	.0073516	7.0252
median workgroup wage in workgroup	28189.586	12866.3	114464	29311.531	13528.143	130756	-1121.945	-20.9613
N coworkers in workgroup	8.984	9.543	114464	10.018	10.969	130756	-1.033763	-24.7290
establishment AKM effect (2000-2006)	0.113	0.216	111144	0.123	0.215	121978	-.010	-10.825
1953 birth cohort	0.185	0.388	114464	0.179	0.383	130756	.005401	3.4609
1954 birth cohort	0.194	0.395	114464	0.19	0.392	130756	.0037983	2.3835
1955 birth cohort	0.197	0.398	114464	0.202	0.401	130756	-.0051422	-3.1794
1956 birth cohort	0.208	0.406	114464	0.211	0.408	130756	-.0025493	-1.5480
1957 birth cohort	0.217	0.412	114464	0.218	0.413	130756	-.0015079	-0.9026

Notes: This table shows the summary statistics and balance test for observable characteristics by treatment status (coworkers in workgroups with peers whose ERA raised, vs. those whose ERA was 60.)

Table B.8: First stage, reduced form, and peer effect regressions on employment at 62 by gender of coworkers

	First stage		Reduced form		Peer effect	
Panel A: female coworkers (baseline sample)						
Employment at 62	0.116***	0.113***	0.015***	0.014***	0.133***	0.126***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.019)	(0.019)
Control mean	0.716		0.785			
Observations	182584	182584	182584	182584	182584	182584
N workgroups	64324	64324	64324	64324	64324	64324
R squared	0.023	0.040	0.000	0.012	-0.002	0.011
Panel B: male coworkers						
Employment at 62	0.095***	0.090***	0.015***	0.012***	0.158***	0.138***
	(0.008)	(0.008)	(0.004)	(0.004)	(0.038)	(0.039)
Control mean	0.723		0.793			
Observations	80655	80655	80655	80655	80655	80655
N workgroups	28772	28772	28772	28772	28772	28772
R squared	0.014	0.031	0.000	0.018	-0.007	0.014
Controls	No	Yes	No	Yes	No	Yes

Notes: This table shows the effect of the rise in ERA on *employment at 62*: 1st stage (Equation 2), ITT (Equation 3), and IV regressions (Equation 4). The control means are the average values of the outcomes when I limit the sample to the workgroups with peers whose ERA was fixed at 60. Robust standard errors in parentheses are clustered at the workgroup level.
* ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

C Alternative sample definitions.

I create five main alternative samples to perform robustness and sensitivity checks in section 4. They rely on altering two dimensions of dataset construction: (1) the bandwidth, (2) the occupation group. The first group consists of four samples:

1. **Sample B.** redefining peers by excluding those in a “donut hole” (December 1951, and January 1952).

2. **Sample C.** allowing for the peers to be born two years (opposed to one year) around the reform cutoff. I redefine the peers as those born in 1950-1953 and coworkers as those born after 1953. For such samples where the peers can belong to two (or more) cohorts (Samples C and D), I define coworkers employed in the earliest year when the peer turns 57. For example, in a 2-year bandwidth specification with peers born in 1950 and 1951, the coworkers would be defined in 2007, i.e., when the oldest peer turns 57. The main observables and controls are also defined this year. Additionally, coworkers in these samples belong to 1954-1957 (1953 is excluded) cohorts, such that they do not overlap with peers, which now can include the 1953 cohorts.

3. **Sample D.** allowing for mixed peers. This sample relaxes the restriction that drops the workgroups where peers were born on both sides of the cutoff.

4. **Sample E (i).** Falsification test sample (false cutoff). In this sample, women of the 1951 cohort are falsely attributed to the treated group, while the women of the 1950 cohort are attributed to the control group. I keep workgroups which have peers either in the 1950 cohort (control) or the 1951 cohort (false treatment group). The coworkers are defined as those born in the 1952-1956 cohorts, so that they are younger than the peers, thus circumventing the reflection problem. For details about the reflection problem, see section 3. I use the Sample of Integrated Employer-Employee Data (SIEED7518), a random 1.5% sample of all establishments in Germany, because of data sensitivity.[34] Since the data is right-censored, unlike the baseline sample, I exclude the 1957 cohort from the coworkers: their employment status at 62 is not observed.

5. **Sample E (ii).** Falsification test sample (false gender). For the second falsification test, I repeat the data creation of the baseline sample on the SIEED7518 data, except that the peers are now defined as men, as opposed to women. The 1957 cohort is removed from the coworkers due to the right-censored data (see the point above).

[34]Due to data sensitivity, I observe the universe of affected workgroups only for the baseline sample, and the false samples are created based on a random sample of establishments. Overall, the data resemble the data used in the baseline specifications, but include only employment spells, and are smaller.

The second group relates to the workgroup definitions (and the corresponding reform window) and consists of two samples:

1. **Sample F.** workgroups defined as 3-digit occupations in establishments
2. **Sample G.** workgroups defined as 2-digit occupations in establishments

All of these alternative samples focus on female coworkers only. The rest is identical to the baseline sample definition, described in subsection 3.2. The sample sizes, including the number of coworkers, peers (by treatment), and workgroups, are recorded in Table C.1.

Table C.1: Sample sizes in baseline and alternative samples

	N workgroups	N treated peers (1952)	N control peers (1951)	N coworkers (1953-1957)
Panel A: baseline samples				
Sample A (female)	64,324	40,627	36,828	182,584
Sample A (male)	28,772	18,779	16,368	80,655
Panel B: alternative samples				
Sample B	61,365	38,518	34,754	180,154
Sample C	76,228	54,664	45,072	159,103
Sample D	108,696	128,977	114,592	327,156
Sample E (i)	895	440	643	2626
Sample E (ii)	419	295	207	1288
Sample F	63,444	41,011	37,282	181,854
Sample G	87,259	41,598	37,872	190,784

Notes: This table describes the number of workgroups, peers (by treatment), and coworkers in the alternative samples. For the details on baseline and alternative sample definitions, see subsection 3.2 and Appendix C.

Abstrakt

Tento článek využívá jedinečnou situaci změny sociálních norem – německou důchodovou reformu, která sjednotila věk odchodu do důchodu mezi ženami a muži – k analýze, jak se zaměstnanost ve vyšším věku šíří prostřednictvím pracovních kolektivů. Reforma zvýšila nejnižší možný věk odchodu žen do důchodu z 60 na 63 let pro ročníky narozené od roku 1952. S využitím úplného souboru údajů o pracovních skupinách ze záznamů sociálního zabezpečení porovnávám ženy, jejichž kolegyně se narodily těsně před nebo těsně po hranici reformy. Zjišťuji, že ženy mají větší pravděpodobnost zůstat zaměstnané i ve vyšším věku, pokud tak činí i jejich kolegyně, přičemž tento efekt je silnější v regionech bývalého Západního Německa, kde přetrvávají tradiční genderové normy. Genderově neutrální důchodové reformy tak zesilují svůj dopad prostřednictvím vlivu vrstevníků a podporují regionální sbližování v zaměstnanosti na konci kariéry.

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