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Who Benefits from Phased Retirement: Workers, Firms, or Public Pension Systems?

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Who Benefits from Phased Retirement: Workers, Firms, or Public Pension Systems?

Kai Robert Miele*

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Abstract

Extending working lives is a central response to population aging, yet older workers prefer to work less and firms are often reluctant to employ them. Phased retirement promises to reconcile these interests. This paper studies how German partial retirement (PR) shapes workers' careers and how firms use it to adjust their workforces by exploiting that the abolishment of a subsidy raised the cost of PR for firms. Using a sharp eligibility cutoff by birth date and linked data on the universe of workers and establishments, I show that the cost shock reduced lifetime PR take-up by about one-third. Instrumenting PR entry with subsidy eligibility, I find that PR does not delay job exit but reduces lifetime labor supply. The resulting loss in pension contributions falls on workers through lower entitlements rather than on the public pension system. At the firm level, establishments more exposed to the abolishment retained more workers, mostly aged 60 and older, suggesting that subsidized PR served firms as a cheap way to shed older staff. Consistent with a downsizing motive, establishments hit harder by the 2008–09 financial crisis kept a larger share of their PR use once the subsidy ended. Moreover, PR concentrated among high earners, who are more costly to employ. Thus, subsidized PR helped firms to reorganize their workforce rather than to extend workers' careers.

JEL-Classification: H55, J22, J23, J26

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1 Introduction

Population aging puts pension systems under pressure. In Germany, there are already 33 people aged 67 or older for every 100 people of working age, and this ratio is projected to rise to between 43 and 61 by 2070 (Statistisches Bundesamt, 2025). Sustaining pensions therefore requires longer careers, and many governments have raised retirement ages in response.[1] Such reforms, however, run against the interests of both sides of the labor market. Workers reveal a preference to reduce their workload at older ages (Gielen, 2009) and many firms rather aim to replace their older workforce with younger entrants (Huber et al., 2016; Frimmel et al., 2018). Thus, retirement age increases often meet strong political resistance[2] and they may trigger unintended responses, such as increased benefit take-up (Staubli and Zweimüller, 2013; Atalay and Barrett, 2015; Rabaté and Rochut, 2020; Geyer and Welteke, 2021).

Phased retirement schemes, that is, institutionalized hour-reduction programs for older workers, promise to reconcile these interests.[3] Workers report a willingness to postpone labor market exit if they can reduce working hours (Ameriks et al., 2020) and firms can fill the reduced working volume with younger workers. In practice, however, two concerns emerge. First, if workers do not extend their working lives, phased retirement simply replaces full-time with part-time work. Second, phased retirement often requires the employer's consent, and firms' motives may not align with those of workers or a social planner. Where older workers are costly and protected against dismissal, phased retirement gives firms a cheaper alternative to layoffs and a way to downsize or rejuvenate their workforce (Huber et al., 2016). Firms may then grant phased retirement to the workers they want to let go rather than to those who want to work longer.

This paper investigates both these concerns in the context of German partial retirement (Altersteilzeit). Partial retirement (PR) is an employment scheme in which workers reduce their prior working hours by half over a fixed period and

[1] Recent studies on the effects of higher retirement ages include Hanel and Riphahn (2012); Atalay and Barrett (2015); Cribb et al. (2016); Lalive and Parrotta (2017); Geyer et al. (2022); Lorenz et al. (2022); Lalive et al. (2023); Rabaté et al. (2024) for the normal retirement age, or Staubli and Zweimüller (2013); Geyer et al. (2020); Rabaté and Rochut (2020); Geyer and Welteke (2021) for the early retirement age.

[2] Examples include nationwide strikes following a French reform in 2023 that raised the minimum retirement age from 62 to 64 (The Guardian, 2023), as well as protests in Belgium (The Guardian, 2014), the United Kingdom (NPR, 2011), and Brazil (BBC News, 2019).

[3] Phased retirement schemes are used throughout many countries (see Eurofound (2016) for an overview), and their design varies substantially. Some schemes compensate part of the income loss from reduced hours through a partial pension (e.g., Sweden; Khoshghadam, forthcoming). Others tie the hour reduction to continued employment until the normal retirement age (e.g., Dutch civil servants; Rutten et al., 2025).

terminate their employment contract afterwards. PR makes each hour worked more expensive for firms, because for half the hours, firms must pay at least 60 percent of the previous gross wage, and firm-side pension contributions rise by 30 percent. The Federal Employment Agency spent about EUR 1.4 billion per year on a subsidy that compensated firms for these higher costs of labor (Bundesagentur für Arbeit, 2008, 2009). For workers, PR thus halves working hours at a much smaller loss in pay. Firms, on the other hand, can reduce the work volume of older workers and rejuvenate their staff, while keeping them employed and retaining their human capital. At the same time, firms may exploit PR as an instrument for mutual separations without facing the costs of employment protection.

In 2010, this subsidy was abolished, but the statutory wage and pension top-ups remained in place. PR thus became more expensive for firms, which had little means to pass these costs on to workers. I leverage this reform to answer two questions. First, I exploit variation in subsidy eligibility to identify how PR affects the labor supply, job-exit behavior, and pension contributions of older workers. The abolishment gave some workers an incentive to enter PR substantially before the early-retirement age. These workers could use PR either to extend their careers at fewer hours or as a pathway into early retirement. Second, I investigate how elastically firms respond to the costs of PR and how they use PR to adjust their workforce in a context where older workers enjoy strong employment protection. If firms truly use PR as a downsizing instrument, firms should respond less elastically to the cost shock whenever the need to downsize is large and older workers are costly to employ.

My identification rests on the policy design that workers become eligible for PR at age 55 and that the subsidy was abolished for new PR agreements starting in 2010. Spells starting before that date remained subsidized. Together, these rules generate a sharp eligibility cutoff by birth-date. Workers born in 1954 are eligible for the subsidy if they start PR at age 55, whereas workers born after January 1, 1955 are fully ineligible. For the worker-level analysis, I use this cutoff to employ a DiD approach comparing individuals around the 1955 birth cutoff to those born around January 1954. The comparison with an earlier cohort addresses season-of-birth effects that could otherwise confound the estimates.[4] For robustness, I extend the approach to a difference-in-discontinuities design, which flexibly accounts for trends in outcomes specific to one side of each cutoff. To investigate

[4]The normal retirement age rises by one month per birth cohort, from 65 years and 7 months (1953) to 65 years and 9 months (1955). Identification requires that a one-month increase has the same effect at both cutoffs. I show that exit age at the 1954 and 1955 cohort cutoffs jumps by the same margin among individuals with a near-zero chance of using PR. Moreover, Seibold (2021) shows that the same normal retirement age increase maps linearly into exit age.

how early PR affects workers' careers, I use these approaches as the first stage in a DiD-IV approach. The firm-level analysis exploits quasi-experimental variation in exposure to the cost shock from the relative shares of the 1954 and 1955 cohorts before subsidy abolishment. Keeping the general age structure constant, firms with a higher 1955-cohort share at baseline face higher costs of granting PR to workers from the focal cohorts. To measure a firm's need to reduce staff, I use its exposure to the 2008–09 financial crisis, which hit Germany mostly as an export shock and reduced labor demand in production-intensive industries (Möller, 2010).

This paper builds on administrative data on the universe of workers subject to German social security, which record employment, wages, and unemployment-benefit receipt on a daily basis. I link these data to the Establishment History Panel, an annual panel covering all German establishments with at least one employee.[5] I complement the administrative records with survey data from the Microcensus containing information on working hours and health outcomes.

Subsidy eligibility has a large effect on participation. At the cutoff, eligibility increases lifetime PR take-up by approximately 4 percentage points ($\approx 50\%$) throughout the overall workforce. Coherent with the design of the policy, workers at the margin of being eligible for the subsidy start PR at age 55, which I henceforth refer to as early PR. Regarding the working trajectories of participants, I find that for workers whose early entry into PR is induced by reimbursement eligibility, regular employment is replaced one-to-one by PR with no effects on labor market exit. Thus, subsidy-induced PR crowded out regular employment without extending careers. As a consequence, cumulative labor supply between ages 55 and 67 decreases by approximately 3.6 years and cumulative pension contributions by about EUR 7,300. Because lower contributions translate into lower pension claims, these losses are borne by the workers rather than by the pension insurance. I find no meaningful change in unemployment-benefit receipt and little evidence that PR improves job stability. Early PR does, however, reduce long-term sick leave with little support for the view that this effect reflects health improvements from reduced working hours.

At the establishment level, the abolishment changed how firms managed the exit of their older staff. Establishments whose workforce in 2008 contained relatively more workers from the 1955 than from the 1954 cohort, and which therefore lost more of their access to subsidized PR, employ fewer partial retirees from 2010 onward. At the same time, their regular employment rises by more than PR employment falls, hence total employment increases. The additional employment is concentrated among workers aged 60 and older, whereas employment of workers

[5]The worker-level data is the IEB v17.00.01 and the firm-level data is the BHP 7521 v1.

under 50 does not respond. Thus, when PR becomes more expensive, firms abandon strategies of systematically shedding older staff. To proxy a firm's demand to reduce staff size, I classify firms by their exposure to the 2008–09 financial crisis using a leave-one-out design. Consistent with a downsizing motive, firms in region-by-industry cells hit hard by the crisis kept a larger share of their PR use after the subsidy ended, whereas less affected firms retained more of their older workers in regular employment. A one-standard-deviation increase in crisis exposure offsets about half of the average decline in PR use.

The cost shock also changed who obtains PR, consistent with firms becoming more selective once they bear its cost. Using the reform to separate workers whose participation depended on the subsidy from those who would have entered PR in either case, I find that the former are 13 percentage points more likely to be women, have about EUR 20 (12%) lower daily earnings at age 54, and they work at firms with lower AKM effects and smaller staff sizes. Shifting the costs to firms therefore makes PR less common and leaves a participant pool tilted toward stronger wage and firm positions. However, even under the subsidy, partial retirees have a higher standing on the labor market compared to the median worker. Especially workers in physically strenuous jobs remain excluded.

This paper contributes to two strands of literature. First, it complements a growing literature on the effects of phased retirement on workers' careers. My findings contradict the results of several stated-choice experiments (Elsayed et al., 2018; Ameriks et al., 2020; Kantarcı et al., 2025b), where participants report that they would extend their working lives if they can reduce their hours. Thus, when facing the choice to retire, workers' revealed preferences may deviate from their stated ones. Moreover, my findings align with a growing causal literature finding ambiguous effects of phased retirement on workers' careers (Khoshghadam, forthcoming; Hernæs et al., 2025; Rutten et al., 2025).[6] Unlike in other countries, German PR sets little incentive to prolong careers. For example, workers can enter PR at a much earlier age and PR is often used in a design where workers initially maintain their original hours and then reduce hours to zero during the second half of the PR spell. Similarly, generous compensation for reduced hours in Sweden led full-time workers to switch to part-time work (Khoshghadam, forthcoming). Among Dutch civil servants, in contrast, hour reductions were tied

[6] A larger literature relies on correlational designs, comparing participants with matched non-participants, or firms with higher and lower availability (Allen et al., 2004; Gielen, 2009; Graf et al., 2011; Huber et al., 2016; Albinowski, 2024). Berg et al. (2020) compare cohorts before and after the introduction of German PR. Structural life-cycle models instead simulate counterfactual schemes (Haan and Tolan, 2019; Bernasconi and Kantarcı, 2024; Kantarcı et al., 2025b; Bernasconi and Kantarcı, 2026), but their predictions rest on functional-form assumptions and, in most cases, on worker behavior alone.

to continuation until normal retirement age, which increased total labor supply (Rutten et al., 2025). Taken together, the evidence suggests that design determines the sign. Without the right incentives, phased retirement schemes may unintentionally crowd out regular employment with little public benefit.

Second, this paper contributes to the literature on employment protection for older workers and on how firms manage the exit of their older staff. Employment protection keeps older workers in firms that would otherwise dismiss them (Schnalzenberger and Winter-Ebmer, 2009; Rabaté, 2019). Once protection ends, firms immediately separate from workers, especially those with long tenure and high pay (Saez et al., 2026; Morris et al., 2026). Where protection is intact and laying off older workers is expensive, separations shift to other routes, such as golden handshakes (Frimmel et al., 2018) and generous public exit options (Jäger et al., 2023). This paper shows that phased retirement offers another tool that firms utilize to circumvent employment protection of older workers. Previous evidence on this behavior is limited to studies finding that retirement age increases can steer workers into PR or unemployment instead of retaining them in regular employment. However, these studies infer firm behavior from worker responses to pension incentives (Geyer et al., 2022; Lorenz et al., 2022; Riphahn and Schrader, 2023), whereas I observe how firms respond when the price of PR changes. Rather than varying protection itself, the reform raised the price of a consensual exit instrument that leaves protection intact. Firms that lost access to subsidized PR retained more of their older workers, suggesting that protection binds once PR becomes unprofitable. Thus, the demand for shedding older staff is price-elastic and if a firm's need to downsize increases, its willingness to pay responds accordingly. Public subsidies for phased retirement can therefore finance the circumvention of employment protection.

The firm perspective also speaks to the puzzle of why phased retirement is rare and selective across countries (Eurofound, 2016), despite the preferences of workers and firms described above. My results suggest a key entry barrier to phased retirement lies on the firm side. While Hutchens (2010) and Kantarcı et al. (2025a) raise a similar point, this paper can directly link the employer-side costs and benefits of PR to its take-up. If costs are high and firms cannot pass them on to workers, PR is granted mainly to high earners, who are most costly to employ. For those workers, firms are willing to bear the cost of PR to circumvent employment protection and remove them from their workforce. If costs decrease due to a subsidy, or if the benefit of reducing staff increases, for example, due to a recession, this trade-off becomes viable for a broader range of workers and the number of partial retirees increases. On the worker side, in

contrast, I find no wage differential between partial retirees with and without subsidy eligibility, suggesting that changes in PR uptake are not driven by PR agreements becoming less generous. Thus, shifting the costs of PR from firms to the public equalizes access. However, vulnerable workers, particularly those in low-paying and physically strenuous jobs, remain excluded regardless of a subsidy.

The remainder of the paper proceeds as follows. Section 2 describes the German pension system, the PR rules, and the data. Section 3 presents the worker-level identification, the effects of subsidy eligibility on PR uptake, and the career effects of early PR. Section 4 covers the firm responses, including which workers firms select into PR. Section 5 concludes.

2 Institutional Background and Data

2.1 The German Statutory Pension System

Germany's statutory pension insurance (*Gesetzliche Rentenversicherung*, GRV) covers most employees and is financed on a pay-as-you-go basis through employee and employer contributions and federal transfers. Contributions are levied up to a statutory ceiling and are generally split equally between workers and employers (Federal Republic of Germany, 1989, Sections 1–5, 153, 157, 159, and 168). Benefit levels are based on pension points, which depend on earnings and the years of contribution. At claiming, accumulated points are multiplied by an access factor, a pension-type factor, and the current pension value (Federal Republic of Germany, 1989, Sections 3, 51, 55–58, and 63–64). Pension claiming and labor-market exit are legally distinct, which matters because PR is an employment arrangement that generally precedes claiming (Federal Republic of Germany, 1989, Section 41).

The regular pension requires five qualifying years and cannot be claimed before the normal retirement age. The 2007 "Retirement at 67" reform gradually raises this age from 65 to 67, reaching 67 for cohorts born in 1964 or later (Federal Republic of Germany, 1989, 2007). Workers with 35 qualifying years may claim from age 63 with a permanent reduction of 0.3 percent per month before their cohort-specific unreduced age. Those with 45 years can claim an unreduced pension earlier (Federal Republic of Germany, 1989, 2014).[7] For the 1953–1955 cohorts, which are focal to this paper, normal retirement ages are 65 years and 7, 8, and 9 months. The 35-year pathway remains available from 63 with deductions.

[7]Workers with a recognized severe disability and 35 qualifying years have a separate pathway. For the 1953–1955 cohorts, it can be claimed with deductions at ages 60 years and 7, 8, and 9 months and without deductions three years later (Federal Republic of Germany, 1989, Section 236a).

2.2 Partial Retirement

Old-age part-time employment (*Altersteilzeit*), hereafter partial retirement (PR), is an employment scheme that was introduced in 1996 to provide older workers with flexible jobs and to open positions for labor-market entrants.[8] Statutory eligibility begins at age 55 and requires 1,080 insured days in the previous five years. Agreements must extend to the earliest date of old-age-pension eligibility. There is no legal right to PR. Instead, participation and duration are contractually agreed on with the employer (Federal Republic of Germany, 1996, Sections 1–2 and 8), often governed by collective or works agreements (Wanger, 2025).

Average weekly hours must equal half the worker's previous schedule, defined as contracted hours immediately before PR and capped at their 24-month average (Federal Republic of Germany, 1996, Sections 2 and 6). Under the uniform model, hours are halved throughout. Under the block model, an initial phase at the previous schedule is followed by an equally long release phase with zero hours. Pay, employment, and social-insurance coverage continue in both phases (Federal Republic of Germany, 1996; Deutsche Rentenversicherung Bund and Bundesagentur für Arbeit, 2010). The block model therefore preserves recorded employment without active work and accounts for about 80 percent of PR arrangements (Wanger, 2025).

When hours are halved, regular pay during PR falls to half of the worker's previous remuneration. The employer must add a top-up of at least 20 percent of this reduced pay. Thus, workers receive at least 60 percent of their previous pay, but agreements may be more generous (Federal Republic of Germany, 1996; Deutsche Rentenversicherung Bund and Bundesagentur für Arbeit, 2010). The top-up is exempt from taxes and social security contributions, hence it is not recorded in administrative data. Regular pension contributions of workers and firms are based on the reduced wage and therefore drop by about 50%. However, firms must additionally pay a supplementary contribution, so that total contributions fall by no more than 10%. This top-up amounts to the full contribution rate, including the worker's share, on 80% of the reduced wage (i.e., on 40% of the previous wage). Thus, including the regular and the supplementary component, firms pay pension contributions as if the worker earned 30% more than before entering PR. The statutory top-up and supplementary contribution are required for at most six years, although agreements may be longer or more generous (Federal Republic of

[8] A partial old-age pension (*Teilrente*) is another institutionalized work arrangement, which allows pension-eligible workers to claim part of their pension (Federal Republic of Germany, 1989, Section 42). However, it is rarely used. In 2009–2012, it accounted for only 0.02 percent of old-age pensions in payment (Deutsche Rentenversicherung Bund, 2025).

Germany, 1996, Sections 2–3).

2.3 The Abolishment of the Subsidy

Before 2010, the Federal Employment Agency reimbursed qualifying employers for the wage top-up and the additional pension contribution for up to six years. Reimbursement required an eligible replacement hire. For employers with at most 50 workers, such a hire conclusively established that the released capacity had been replaced (Federal Republic of Germany, 1996, Section 3(1)). PR could nevertheless be offered without reimbursement, and statutory minimum payments did not depend on subsidy receipt (Federal Republic of Germany, 1996, Sections 4 and 8(2)).

A reform enacted in 2000 limited reimbursement to arrangements whose PR began by December 31, 2009, so the deadline was known almost a decade in advance. Pre-deadline arrangements could continue to receive reimbursement. For new arrangements, the statutory wage and pension top-up remained unchanged, but employers now had to finance them (Federal Republic of Germany, 2000; Deutsche Rentenversicherung Bund and Bundesagentur für Arbeit, 2010). Hence "reimbursement eligibility" denotes the opportunity to begin a potentially reimbursable arrangement, not observed subsidy receipt, which additionally required an application and replacement hire.[9] Workers born before January 1, 1955 had a window after turning 55 to begin such an arrangement; those born later did not, generating the eligibility cutoff exploited throughout the paper. The aggregate series make the bite of the deadline visible. The number of workers in PR rises through the 2000s, peaks in 2009, and declines steadily thereafter, and the cohort-specific counts show successive cohorts entering in smaller numbers once reimbursement is no longer available (Figure A1). Public expenditures follow the same timing. The Federal Employment Agency spent between EUR 1.32 and 1.38 billion per year on PR reimbursements in 2007–2009 (Bundesagentur für Arbeit, 2008, 2009). Because arrangements begun before the deadline remained reimbursable for up to six years, spending stayed at this level through 2013 and fell to EUR 0.18 billion in 2016 as the last reimbursed spells expired (Figure A2).

[9]The replacement had to be hired in connection with the transition into PR, either on the vacated position or on a position freed up by an internal transfer (Federal Republic of Germany, 1996, Section 3(1)). In the block model, the replacement could be hired after the work phase, that is, at the start of the release phase. Reimbursement then began, and the amounts for the work phase were paid retrospectively (Federal Republic of Germany, 1996, Sections 3(3) and 12(3)).

2.4 Data

The primary data are a custom full-population extract from the IAB's Integrated Employment Biographies (IEB), containing all records for the 1953–1955 birth cohorts. The IEB combine employer notifications of social-security and marginal employment with records of benefits under Social Code Books II and III, job-search registration, and active labor-market programs, permitting daily employment and unemployment histories. Employment spells report exact dates, status (including PR), earnings, occupation, establishment, and notification reason. Individual records include sex, birth date, nationality, education, and residence. The IEB do not observe civil-service employment, most self-employment, pension receipt, or unregistered inactivity, so gaps cannot generally be assigned to a labor-market state.

I link the individual-level data to the Establishment History Panel (*Betriebs-Historik-Panel*, BHP), an annual panel covering all German establishments with at least one employee. The BHP is built from the same employer notifications as the IEB and aggregates them to the establishment level on June 30 of each year. It reports the number of employees by type of employment, including regular employment and PR, as well as staff composition by sex, qualification, and five-year age group. It also contains the wage structure of full-time employees and the industry and location of the establishment. Linking both data sets provides additional measures for the worker-level analysis, such as local labor-market concentration, and allows me to measure how strongly establishments were exposed to the abolishment through their staff's cohort composition. For this purpose, I count the members of the 1954 and 1955 cohorts in regular employment at each establishment on June 30, 2008, and relate these counts to the establishment's regular employment in the BHP (Section 4.1).

For supplementary evidence, I use data from the German Microcensus, a large repeated cross-sectional survey operated by the Federal Statistical Office. It covers 1% of German households per year. Participation is mandatory. Importantly, the Microcensus provides information on self-reported PR-related hours reductions, actual weekly hours, net earnings including tax-exempt top-ups, and health outcomes, which the IEB do not observe (see Appendix D for details).

I collapse the spells to a person-by-age panel. Within age, I count distinct days in regular employment, marginal employment, PR, and unemployment benefits receipt, and compute aggregate earnings. I can distinguish between PR in the block and the uniform format. I assign the wage, occupation, and establishment of the spell with the highest total earnings in that age year, excluding one-time payments. "Regular employment" means employment subject to full social-security

contributions, and it therefore includes part-time work. Marginal employment mostly contains mini-jobs with monthly earnings below EUR 400 (EUR 450 since 2013). Wages are collected on a daily basis and deflated to 2020 prices. In regular employment, the wage measures contribution-liable gross pay, capped at the contribution ceiling. Following common practice, I impute censored wages using the approach of Card et al. (2013). During PR, employers report the reduced wage plus the supplementary pension base, but not the top-up. At the statutory minimum and below the contribution ceiling, reported earnings therefore amount to about 90% of the previous wage, although regular pay is 50% and the cash payment 60% of it. Thus, reported wages during PR do not measure cash earnings, although they still allow me to compute pension contributions.

Job exit age is one of the key outcomes of this paper. It is defined as the end of the last employment or unemployment spell, not counting marginal employment. While job exit does not imply retirement and the receipt of pension benefits, its distribution is concentrated at the statutory pension ages. A first mass exists just after 63, and a second at the cohort-specific normal retirement ages (Figure A3). Labor supply is measured as days in regular employment plus 0.5 times days in PR, reflecting the statutory halving of hours. Moreover, I compute total pension contributions and the resulting pension points. Last, I infer extended sickness-related absence from a gap following a spell that ends with the notification reason "earnings-replacement benefits" (*Entgeltersatzleistungen*). Such notifications usually follow the end of employer-financed wage continuation while employment persists. The proxy therefore excludes short absences (below six weeks) and reveals neither a diagnosis nor the precise benefit (Schmucker and vom Berge, 2025, Sections 4.1.1 and 5.5.15).[10]

Table 1 provides an overview of covariates measured at age 54, hence shortly before becoming eligible for PR. They include demographics, ages-50–54 employment, establishment size, and physical and mental job strain, assigned on ten-point scales from five-digit occupations (Kroll, 2015). Local concentration is an employment-share HHI within year, local labor-market region (50 regions, see Institut für Arbeitsmarkt- und Berufsforschung (2022)), and two-digit industry cells. Moreover, the data contain the AKM effects (Abowd et al., 1999) of individuals and the establishments they work in (Card et al., 2013; Lochner and Wolter, 2025).[11]

[10] Imputing long-term sick leave based on earnings-replacement benefits leaves room for measurement error, as the same notification appears at the start of parental leave or when parents abstain from work to care for a sick child. Because this paper studies individuals at age 55 onward, these reasons for notification should be rare.

[11] I use the AKM effects estimated for 2001–2008.

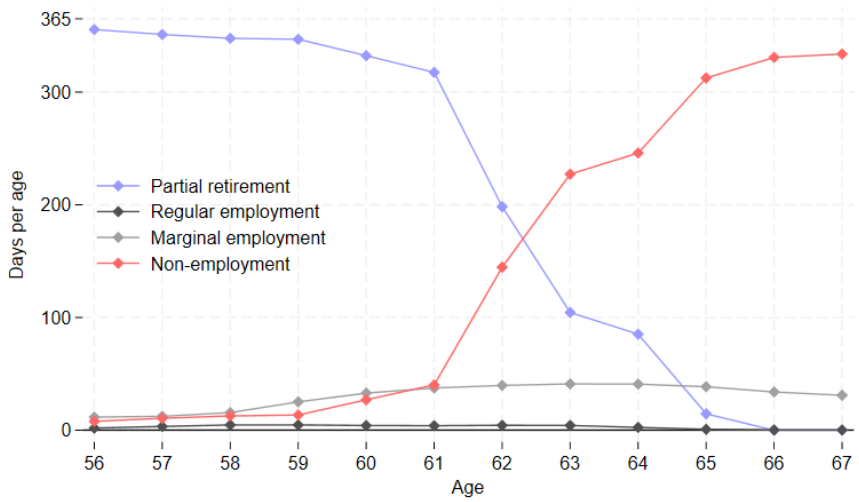
2.5 Descriptive Evidence

The worker-level data contains all members of the 1953–1955 cohorts who were regularly employed at age 54. In addition, I exclude several foreign nationality groups that exhibit severe bunching of their birth-date in January (Figure A4). This bunching may be due to public administrations setting the birth-date to January 1 if documentation is missing, or strong incentives for birth-date manipulation in home countries. The resulting population comprises 1,833,468 workers.

Table 1 shows age-54 characteristics of this population and of workers who will enter PR eventually. Compared to the general workforce, PR participants are highly selective. They have higher age-54 wages (EUR 165 versus 113), work at larger establishments (2,276 versus 686 employees), and hold less strenuous jobs. Among individuals entering partial retirement as soon as possible, some of these differences are smaller, but still immense.

Figure 1 follows the selected group of age-55 entrants and illustrates how recorded employment states evolve after PR begins. PR accounts for nearly all of their observed employment through age 61; as days in PR fall thereafter, nonemployment rises almost one-for-one, while transitions back into regular employment remain negligible. Thus, for most participants, the end of PR coincides with labor-market exit rather than a return to regular employment. Only a small share of workers engages in marginal employment as their working hours decrease to zero.

Figure 1. Employment After Starting PR at Age 55



Notes: The figure reports average annual days by employment status for members of the 1953–1955 birth cohorts who enter PR at age 55. The sample is restricted to workers with at least one regular-employment spell during age 54.

Table 1. Worker Characteristics at Age 54

	All workers	Entering partial retirement	
		Ever	At age 55
	(1)	(2)	(3)
Individual characteristics			
Female	0.47 (0.50)	0.40 (0.49)	0.46 (0.50)
German citizen	0.96 (0.20)	0.98 (0.15)	0.98 (0.12)
High school	0.20 (0.40)	0.22 (0.41)	0.20 (0.40)
Years employed, age 50–54	4.51 (1.04)	4.92 (0.25)	4.91 (0.22)
Former GDR	0.21 (0.41)	0.17 (0.38)	0.22 (0.41)
Independent city	0.26 (0.44)	0.27 (0.44)	0.28 (0.45)
Job and firm characteristics			
Daily wage	112.63 (89.62)	164.88 (105.09)	150.58 (88.43)
Mental job strain	5.30 (2.93)	4.42 (2.79)	4.15 (2.79)
Physical job strain	5.35 (2.95)	4.50 (2.92)	4.11 (2.82)
Worker AKM	0.09 (0.42)	0.28 (0.34)	0.27 (0.30)
Firm AKM	-0.00 (0.26)	0.17 (0.15)	0.14 (0.15)
Firm size	685.63 (2022.22)	2276.38 (4058.36)	2333.07 (4293.33)
ln HH-index	-4.38 (1.49)	-3.46 (1.48)	-3.51 (1.48)
Partial retirement			
Block format		0.83 (0.37)	0.85 (0.35)
Entry age		57.72 (2.43)	55.25 (0.27)
Duration in years		5.43 (2.24)	7.44 (1.82)
N	1833468	215129	67019

Notes: The table reports means for workers in the 1953–1955 birth cohorts with at least one regular-employment spell during age 54. Standard deviations are reported in parentheses. Mental and physical job strain are measured on ten-point scales based on Kroll (2015) and assigned using five-digit occupations (KldB-2010). Wages are expressed in 2020 euros. The AKM effects are based on Lochner and Wolter (2025). The Herfindahl–Hirschman (HH) index is calculated from establishment employment shares within year, local labor-market region (50 regions; Institut für Arbeitsmarkt- und Berufsforschung 2022), and two-digit industry (WZ-2008).

2.6 Sample

From this population of 1,833,468 workers, I construct two estimation samples. The main sample comprises a four-month window on each side of the January 1, 1954 and 1955 cutoffs. These windows include 812,212 individuals. The first column of Table 2 reports characteristics at age 54. Because PR is uncommon in smaller firms and lower earnings groups, many of these individuals have a near-zero baseline propensity to use PR. Thus, I set up a sample for the IV analysis that mostly excludes these never-taking individuals. In this IV sample, I retain age-54 wage-by-establishment-size vigintile cells with lifetime PR uptake of at least 5 percent among the 1953–1954 cohorts, yielding 526,339 workers (i.e., the dark blue cells of Figure A5 are excluded). The second column emphasizes that this sample is more male and more strongly attached to the labor market, with higher wages and jobs in larger, better-paying firms.

For the establishment-level analysis, I use the BHP for 2004–2016 and retain establishments that have 5 to 2,000 employees in 2008 and employ at least one member of the 1954 or 1955 cohort in regular employment on June 30, 2008. The last column of Table 2 describes the 334,363 establishments in this sample in 2008. On average, members of the 1954 and 1955 cohorts account for 8.9 percent of their workforce, and about a quarter of their employees are aged 50 to 59. Establishments are followed in an unbalanced panel, and Section C.1 shows that the results hold in the balanced panel.

3 Effects on Workers

3.1 Identification

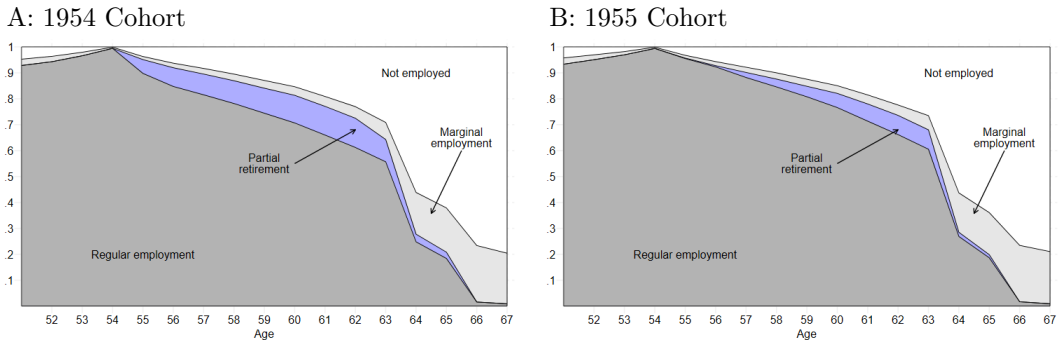
To identify how firm-side costs of PR affect the uptake, I leverage that the 1955 cohort is fully excluded from the subsidy, while workers from the 1954 cohort remain eligible if they enter PR at age 55. Figure 2 contrasts the uptake of PR for both cohorts over the life-cycle. By construction, all workers are in regular employment at age 54. From age 55 onward, PR uptake of the 1954 cohort increases substantially. Despite being ineligible for subsidy, many workers of the 1955 cohort still enter PR, but the share is substantially lower and the entry age is higher. In both cohorts, employment sharply declines at age 63, as early retirement becomes available. By age 67, individuals have passed the normal retirement age and employment approaches zero.

Table 2. Summary Statistics of the Estimation Samples

	Full sample	IV sample	Firm sample
Individual characteristics (age 54)			
Female	0.47 (0.50)	0.38	
German citizen	0.96 (0.20)	0.96	
High school	0.20 (0.40)	0.23	
Years employed (50–54)	4.51 (1.04)	4.78	
Former GDR	0.21 (0.41)	0.16	
Independent city	0.26 (0.44)	0.28	
Job and firm characteristics (age 54)			
Daily wage	111.39 (86.44)	143.50	
Mental job strain	5.30 (2.92)	5.00	
Physical job strain	5.35 (2.96)	5.00	
Unit AKM	0.08 (0.42)	0.23	
Firm AKM	−0.00 (0.26)	0.09	
Firm size	99.00 (2022.05)	228.00	
ln HH-index	−4.38 (1.49)	−4.10	
Establishment characteristics (2008)			
Employees			58.58 (122.92)
Share in PR			0.012 (0.038)
Share in 1954/55 cohorts			0.089 (0.064)
Share aged 50–59			0.266 (0.137)
Share aged under 50			0.666 (0.165)
Share female			0.492 (0.310)
Mean full-time daily wage			85.55 (40.23)
N	812212	526339	334363

Notes: The table reports means with standard deviations in parentheses. For the firm size of workers, it reports the median instead of the mean. Worker characteristics are measured at age 54. The full sample covers individuals used for the local DiD estimation with a four-month bandwidth. The IV sample excludes workers in age-54 wage-by-firm-size cells whose baseline PR uptake among the 1953–1954 cohorts is below five percent (see Figure A5). All individuals have at least one regular-employment spell during age 54. Worker wages are expressed in 2020 euros. The firm sample covers establishments with 5 to 2,000 employees that employ at least one member of the 1954 or 1955 cohort. Establishment characteristics are measured in 2008. Some baseline characteristics are missing for a subset of observations.

Figure 2. Employment over the Lifecycle



Notes: The panels report cohort-specific employment profiles by age for the 1954 and 1955 birth cohorts. In each year of age, an individual's main employment status is the employment type that generates the highest labor income, excluding one-time payments. The sample comprises workers with at least one regular-employment spell during age 54.

Figure 3 shows lifetime participation in PR by month of birth. Participation rates weakly decline by month of birth and abruptly drop from 12.5 to 8 percent at January 1955 when subsidy-eligibility ends. To identify the overall effect of being ineligible for the subsidy, I employ a difference in difference design comparing windows around the January 1955 and 1954 cutoffs. Unlike local randomization or regression discontinuity designs, the local DiD approach can account for season-of-birth effects as long as they are stable across cohorts. For the main specification, I use the four birth months on either side of each cutoff, as visualized by the red intervals.

Let $W55_i$ indicate that individual i belongs to the window around January 1955 rather than the window around January 1954, and let $Right_i$ indicate birth on or after the January cutoff in the relevant window. I estimate the local difference-in-differences specification

$$y_i = \alpha + \beta^{DiD}(Right_i \times W55_i) + \gamma\, Right_i + \delta\, W55_i + \varepsilon_i. \tag{1}$$

Thus, β^{DiD} is the change in the January discontinuity between 1954 and 1955 and hence the reduced-form effect of losing reimbursement eligibility on outcome y_i.

As a complementary approach, I estimate separate linear trends in month of birth on each side of each cutoff. Let x_i^c denote month of birth relative to cutoff $c \in \{54, 55\}$ and define $f_c(x, Right) = (1 - Right)\lambda_c^- x + Right\lambda_c^+ x$. The four parameters λ_{54}^-, λ_{54}^+, λ_{55}^-, and λ_{55}^+ therefore allow the slope to differ across both cutoffs and both sides of a cutoff, with each trend normalized to zero at its

threshold. The difference-in-discontinuities specification is

$$\begin{aligned} y_i = \alpha + \beta^{DiDisc}(Right_i \times W55_i) + \gamma\, Right_i + \delta\, W55_i \\ + W55_i\, f_{55}(x_i^{55}, Right_i) + (1 - W55_i)\, f_{54}(x_i^{54}, Right_i) + \varepsilon_i. \end{aligned} \tag{2}$$

Thus, β^{DiDisc} compares the trend-adjusted breaks at the two January cutoffs.[12]

Both designs require that, absent reimbursement expiration, the discontinuity in potential outcomes at January 1955 would have equaled that at January 1954. The DiDisc additionally conditions on cutoff- and side-specific linear birth-month trends. The local DiD requires within-window birth-month gradients to be negligible or common across cutoffs. These assumptions allow recurring January effects but exclude any other event producing a differential break in January 1955. Figure B2 shows the resulting density of the running variable in both the full and the IV sample. Neither sample has a discontinuity in size at January 1955 that is absent at January 1954.

The reduced-form estimates capture the effect of reimbursement eligibility, not the effect of entering PR itself. To estimate the latter, I instrument whether a worker participates in PR, D_i, with the reform-induced change in subsidy eligibility. Because the subsidy deadline created an incentive to enter PR at age 55 for the 1954 cohort and at age 56 for the 1953 cohort, I define the endogenous variable as participating in PR by age 56 and refer to such entry as early PR. Other definitions, such as entry by age 55, do not change the estimates but can generate mechanical jumps in the data that may be prone to misinterpretation (see, e.g., Figure B3). In the IV sample, I thus estimate

$$\begin{aligned} D_i &= \alpha_1 + \pi(Right_i \times W55_i) + \gamma_1\, Right_i + \delta_1\, W55_i + u_i, \\ y_i &= \alpha_2 + \beta^{IV} D_i + \gamma_2\, Right_i + \delta_2\, W55_i + v_i. \end{aligned} \tag{3}$$

Thus, the local DiD approach used in the reduced form estimation serves as the first stage. In addition to the first stage being strong, the IV interpretation requires exclusion and monotonicity. Exclusion imposes that subsidy eligibility affects y_i, for example job exit age, only through the uptake of PR. Monotonicity requires that no worker decides against PR because their firm can claim the subsidy. Under these conditions, β^{IV} identifies the local average effect of PR on workers born near the cutoff, whose entry by age 56 is induced by reimbursement.

To assess the credibility of the exclusion restriction, Figure B1 presents es-

[12] The estimation of the trends benefits from a longer bandwidth. When using a bandwidth exceeding six months, some individuals fall both in the group right to the 1954- and left of the 1955-cutoff. In this case, I follow the idea of a stacked DiD design and duplicate these observations, assigning one clone to each group.

timates of eq. (3) with age-54 covariates as placebo outcomes. Both in the full sample and the IV sample, only residence in the former GDR strikes out. After excluding workers from the former GDR from the data, the placebo tests no longer hint at any cohort-specific differences. The main results of this paper, in contrast, are insensitive to their inclusion (Figure B11), hence the main results are estimated for the whole of Germany. As an additional placebo check, I estimate the effects using individuals with a near-zero baseline propensity of PR. Among these individuals, PR uptake responds only marginally to the subsidy and the effects on long-run career outcomes are close to zero (Table B1).

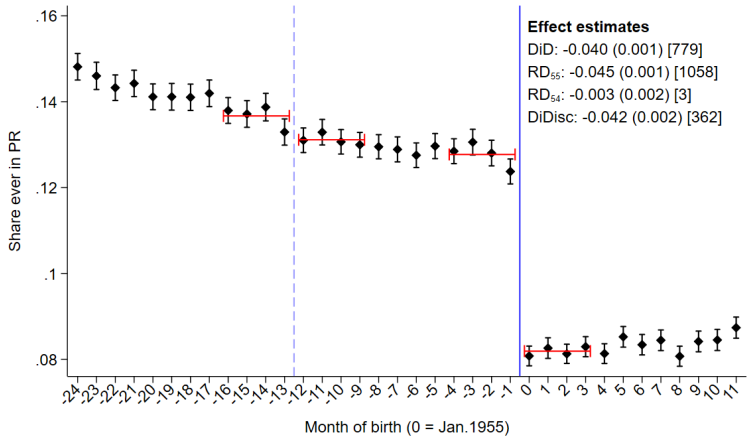
3.2 Effects of Subsidy Eligibility on PR Uptake

Figure 3 shows that reimbursement expiration reduced lifetime PR uptake. The discontinuity is −0.045 at January 1955, compared with −0.003 at January 1954. Differencing these breaks yields a 4.0 percentage point decrease in the likelihood of entering PR, about one-third of uptake immediately before the reform cutoff. Equivalently, eligibility raises uptake by approximately 50 percent. The DiDisc estimate is similar, indicating that smooth cohort trends do not explain the result. The response is particularly distinct in white-collar occupations such as secretarial and assistant management jobs, insurance, and banking (Figure B4), and homogeneous among the rest.

Separating the two PR formats, reimbursement expiration reduces block-model uptake by 3.4 percentage points and uniform-model uptake by 0.6 points (Figure B5). The block model thus accounts for about 85 percent of the decline, close to its share of 83 percent among all PR participants (Table 1). The effects are therefore roughly proportional to the prevalence of each format, and the abolishment did not shift PR toward the uniform model. The distinction nevertheless matters, because the release phase of the block model later accounts for the recorded-absence result in Section 3.3.4.

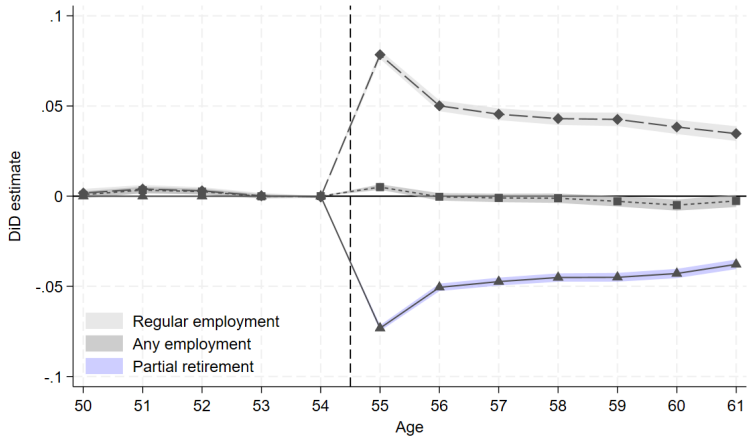
Figure 4 traces the employment response over the life cycle. Estimates at ages 50–54 are tightly centered around zero. At age 55, reimbursement expiration lowers PR employment by about 7.5 percentage points and raises regular employment by the same magnitude. Effects on overall employment are absent. Thus, instead of switching to PR, workers remain in their previous regular employment contract without dropping from the workforce. Over time, both effects shrink as ineligible workers start to enter PR, but even at age 60, hence five years after the cutoff-specific incentive to enter PR, its uptake is substantially lower among ineligible workers.

Figure 3. Uptake of Partial Retirement



Notes: The figure reports the mean uptake of PR by recentered month of birth. The solid and dashed vertical lines mark January 1955 and January 1954, respectively. Whiskers denote 95% confidence intervals. Estimates in the upper-right corner are local DiD and DiDisc estimates from eqs. (1) and (2), respectively. The DiD bandwidth is four months and is indicated by the red intervals. The DiDisc bandwidth is seven months, with separate linear birth-month trends on each side of each cutoff. Standard errors, clustered at the individual level, are shown in parentheses; F-statistics are in square brackets.

Figure 4. Dynamic DiD Estimates



Notes: The figure reports DiD estimates obtained by repeatedly estimating eq. (1) with a four-month bandwidth separately for each age and employment-status outcome. The outcome in each specification is an indicator for an individual's main employment status at the relevant age. Shaded areas denote 95% confidence intervals. Standard errors are clustered at the individual level.

Table 3 summarizes how the incentive to enter PR by age 56 manifests in uptake differences in the IV sample. Reimbursement expiration lowers entry by age 56 by 7.1 percentage points, almost 70 percent of the 10.2-percent pre-reform mean. Estimates range from −0.071 to −0.074 after adding controls or using

DiDisc. These estimates serve as the first stage of the IV estimation based on eq. (3). This first stage is highly relevant even among worker and firm types who generally use PR to a lesser degree (Figure B6).

Table 3. First-Stage Estimates

	Local DiD		Diff-in-Disc	
	(1)	(2)	(3)	(4)
Dep. variable: Entering PR by age 56				
Estimate	-0.071***	-0.073***	-0.072***	-0.074***
	(0.002)	(0.002)	(0.002)	(0.002)
Controls	No	Yes	No	Yes
N	526,339	488,883	1,063,437	986,822
F-statistic	2048	2087	895	917
Pre-cutoff mean	0.102	0.105	0.105	0.107

Notes: The table reports first-stage estimates from the local DiD and DiDisc specifications in eqs. (1) and (2), using four- and seven-month bandwidths, respectively. The outcome is an indicator for entering PR by the end of age 56. The table is based on the IV sample. The specifications in Columns (2) and (4) additionally control for the age-54 characteristics listed in Table 2, except the worker AKM effect, which is omitted because of its high rate of missing values. Standard errors are clustered at the individual level. Asterisks denote statistical significance at the 10 percent (*), 5 percent (**), and 1 percent (***) levels.

3.3 Effects of Early PR on Workers' Careers

This subsection exploits the subsidy abolishment as quasi-experimental variation in the uptake of partial retirement to identify how participation affects workers' careers.[13] The IV estimates show how PR affects long-term career outcomes of workers who only participated in PR since their mid fifties due to subsidy eligibility.

3.3.1 Job Exit and Labor Supply

Figure 5 reports reduced-form and IV estimates for job exit age, as well as cumulative labor supply, pension contributions, and pension points between the age of 55 and 67. Figure B8 shows the effects for different sample splits, but the average effects are rather homogeneous across worker and firm types. Note that by age 67, all individuals passed normal retirement age and their labor supply

[13]The results of this subsection are based on the IV sample, that is, workers in age-54 wage-by-firm-size cells with at least a 5 percent likelihood of using PR. Figure B7 re-estimates each outcome while varying the minimum baseline PR uptake, showing that the IV estimates barely change. The choice of 5 percent therefore trades sample size against first-stage strength without driving the results.

approaches zero. Because the instrument indicates reimbursement expiration and has a negative first stage, reduced-form and IV estimates generally have opposite signs.

Panel A shows job exit age by month of birth. Around both the 1954 and the 1955 cutoff there is a substantial jump in exit age, reflecting the one-month difference in normal retirement age by birth cohort. Due to the DiD design, the reduced-form and IV estimates remain valid as long as the effect of a one-month later normal retirement age is similar across cutoffs.

Exit age jumps by a similar magnitude at both cutoffs, hence the IV estimate on job-exit age is near zero and statistically indistinguishable from zero. To confirm these estimates are not driven by the increase in retirement age, I re-run the analysis for workers with a low baseline propensity of using PR, who face the same increase in the retirement age but are barely affected by the subsidy abolishment. Their exit age shows no differential jump at the 1955 cutoff (Table B1, Column 2). Moreover, I replicate Panel A using the gap between the observed exit age and normal retirement age as the outcome. For this measure, a jump at the 1954 cutoff is mostly absent, yet the IV effect on early exit is still zero (Figure B9).[14]

By contrast, Panel B shows that early PR reduces cumulative labor supply between ages 55 and 67 by 3.6 years. Labor supply is measured as equivalents of regular employment, which, according to the Microcensus, corresponds to 30 to 34 weekly hours depending on age (Figure D2). These figures could understate hours in the IV sample, because it is selected on high age-54 earnings. The decline stems entirely from subsidy-induced entrants replacing regular employment with PR, not from leaving employment. Workers entering PR by age 55 spend, on average, 7.4 years in PR and these years only contribute half the labor supply as their regular employment alternatives.[15]

[14] Descriptive evidence among observed PR users points the same way. An approximately two-year later entry is associated with 1.65–1.80 fewer years in PR but only 0.24–0.28 years later job exit, so 83–86 percent of a delay in entry is absorbed by a shorter spell rather than by a later exit (see Table B2. Figure B10 plots the three underlying discontinuities). These estimates condition on realized PR use and do not identify the causal effect of delaying entry, but they are consistent with the IV null.

[15] Older workers may reduce their working hours outside partial retirement, which would not be documented in the IEB. The Microcensus data show that, while hours generally fall with age, there is a noticeable drop in hours from about age 59 onward that is specific to workers from subsidy-eligible cohorts (Figure D1). When excluding workers in PR from the data, this drop vanishes and working hours move parallel across cohorts (Figure D2). This suggests that subsidy eligibility and hence the higher uptake of PR led to reduced working hours.

3.3.2 Pension Accrual

Panel C considers cumulative employment-based pension contributions, including employee and employer contributions on reported remuneration and the supplementary PR base, but excluding credits during unemployment. Reimbursement expiration raises contributions by EUR 518. Scaling by the first stage implies that early PR lowers them by EUR 7,341, in 2020 euros. For the pension insurance, these losses add up. With a first stage of 7.1 percentage points, about 28,000 of the roughly 395,000 members of the 1954 cohort in the IV sample entered PR early because of the subsidy. Had this cohort not been eligible for the subsidy at age 55, it would thus have paid about EUR 206 million (in 2020 euros) more in contributions. Because the IV sample excludes cells with low PR uptake, this figure is conservative. For earlier cohorts, the loss may be even larger, as they could enter subsidized PR at higher ages and lifetime PR uptake rises the earlier workers were born (Figure 3).

Panel D considers cumulative pension points earned through employment between ages 55 and 67. For each year, I divide pension-relevant earnings, which include the supplementary PR base and are capped at the contribution ceiling, by the average earnings of all insured workers in that year, uprating earnings in the former GDR (Federal Republic of Germany, 1989, Sections 70 and 256a, Anlagen 1, 2, 2a, and 10). Points credited during unemployment are excluded.[16] A worker with average earnings thus accrues one point per year, which currently translates into EUR 42.52 of monthly gross pension. Reimbursement expiration raises cumulative points by 0.067. Scaling by the first stage suggests that subsidy-induced early PR lowers them by 0.918 points, or about EUR 39 of monthly pension. Relative to the 9.6 points that workers born just before the cutoff accrue after age 55, the percentage benefit effect is

$$100 \times \frac{-0.918}{EP_{i,<55} + 9.6},$$

where $EP_{i,<55}$ denotes points accumulated before age 55. For 25–40 points, the implied reduction is 1.9–2.7 percent, or 2.1 percent at 35 points.[17] This calculation holds claiming age and all other pension parameters fixed and therefore concerns

[16] Under certain circumstances, individuals can accumulate pension points during unemployment. Because the level of unemployment benefits is not reported in the IEB, these pension points cannot be measured accurately. Thus, the reported pension points are a lower bound. However, early PR does not affect unemployment-benefit receipt (Section 3.3.3), so the effect on pension points from employment should closely approximate the effect on overall pension points.

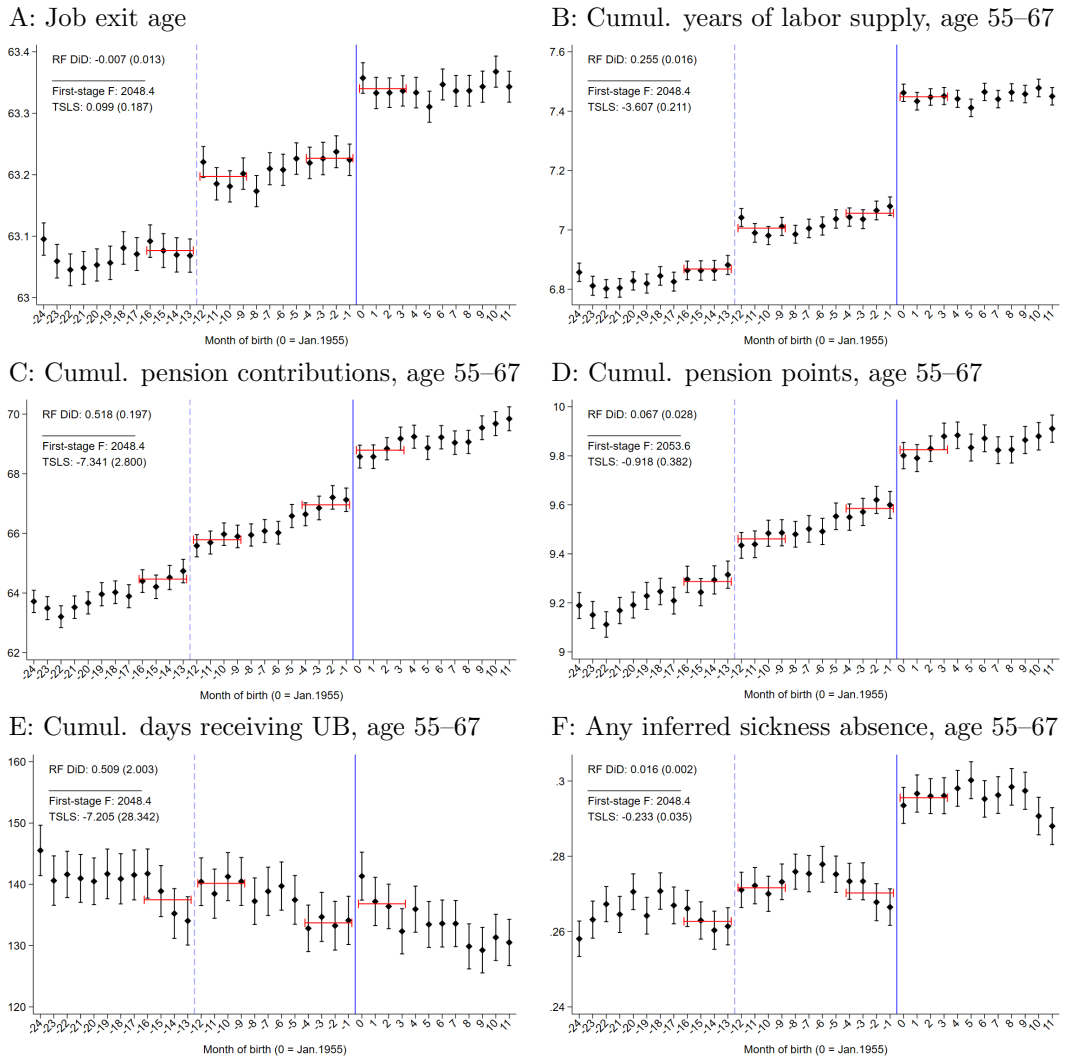
[17] A worker would have accumulated 35 points by age 55 if they entered the labor market at age 20, remained continuously employed, and earned the average wage of all insured workers.

benefit levels, not discounted lifetime pension wealth.

For the pension insurance, the contribution losses are largely offset by lower pension payments. At the 2020 pension value of EUR 34.19 per point and a remaining cohort life expectancy at age 63 of about 21 years for men and 24 years for women, the 0.918 fewer pension points reduce pension payments by about EUR 8,400 per worker, or EUR 6,700 when discounted at 2 percent.

Overall, early PR reduces labor input and pension accrual without extending recorded labor-market attachment. The results are robust across bandwidths, control sets, donut specifications, and the exclusion of the former GDR (Figure B11). Splitting the IV sample at the median of each age-54 characteristic leaves the picture unchanged as well. The zero effects on job exit age are homogeneous across worker types and the labor-supply and pension effects are negative throughout (Figure B8).

Figure 5. Labor-Market, Pension, and Benefit Effects of Early Partial Retirement



Notes: The panels plot outcome means by recentered month of birth for the IV sample, with 95% confidence intervals. The upper-left corner reports reduced-form local DiD and DiD-IV estimates from eqs. (1) and (3) with a four-month bandwidth. The endogenous variable is entry into PR by the end of age 56. Standard errors in parentheses are clustered at the individual level. Job-exit age is the age at which the final regular-employment or unemployment-benefit spell ends. Labor supply counts years in regular employment plus half the years in PR. Pension contributions (employee and employer shares, thousands of 2020 euros) and pension points (pension-relevant remuneration, capped at the contribution ceiling and uprated in the former GDR, relative to the average earnings of all insured workers) exclude credits during unemployment. UB includes ALG I and ALG II. Sickness absences are imputed through "earnings-replacement benefits" and contain sick leave lasting longer than six weeks or absences due to workplace injuries.

3.3.3 Exits into Unemployment or Sick Leave

Early PR may also change whether workers reduce their labor supply through other routes before fully leaving the labor market. For example, workers may

spend more time in unemployment, either due to a higher likelihood of dismissal or by exiting voluntarily, or in long-term sick leave.[18] Both types of labor market absences are common among older workers. In the IV sample, workers aged 55 and above on average receive unemployment benefits for 140 days, and one quarter claims replacement benefits for a longer sickness absence.

Panel E of Figure 5 finds no effect on unemployment-benefit receipt. Thus, workers do not opt into unemployment if partial retirement becomes unavailable, an unintended reaction that is commonly observed for other retirement policies (Staubli and Zweimüller, 2013; Geyer et al., 2020; Geyer and Welteke, 2021; Rabaté et al., 2024). Another path to exiting the labor market is through sick leave. The IEB do not record medically certified sickness days. Short employer-paid absences leave the employment spell intact, but instances where individuals receive earnings-replacement benefits are reported in the data. Such notifications cover sickness absences exceeding six weeks, as well as injury and rehabilitation benefits. The outcome is therefore an inferred extended sickness-related interruption, not a direct health measure.

Panel F of Figure 5 shows that reimbursement expiration raises the probability of an inferred absence between ages 55 and 67 by 1.6 percentage points. The corresponding IV estimate implies that early PR significantly lowers this probability by 23.3 points. It also reduces cumulative inferred-absence duration by 37.5 days (Figure B13). Thus, early PR does not protect workers from unemployment, but it reduces recorded extended sickness-related absences.

3.3.4 Are the Sick-Leave Effects Driven by Health?

Reduced hours could improve health by lowering job strain, but existing evidence is mixed. Finland's part-time pension slightly increased prescription-drug purchases without affecting severe diagnoses or mortality (Ravaska, 2023). U.S. estimates find smaller adverse health effects under partial than complete retirement (Dave et al., 2008), whereas European estimates associate part-time and full-time work with similar increases in depressive symptoms relative to retirement (Kantarcı et al., 2026).

The decline in absences need not indicate better health. Workers in the release phase of the block model experience little incentive to report a long-lasting sickness to their employer as workers perform no active work and replacement benefits during long-term sick leaves only cover 70 percent of the gross wage and at most

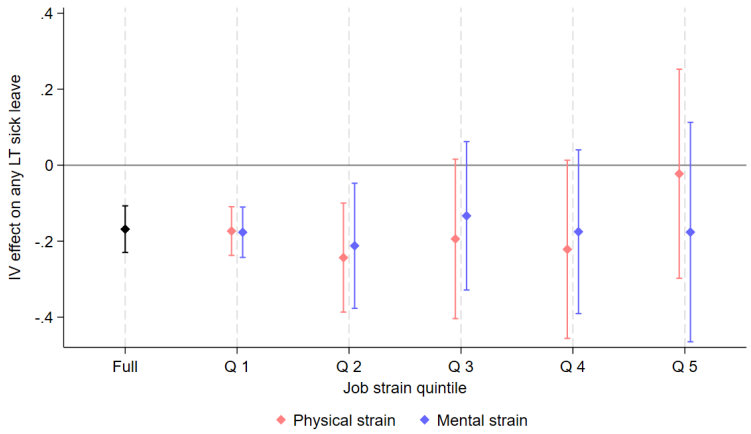
[18] PR locks both workers and firms into a fixed-term employment match. However, subsidy abolishment had no detectable effects on job stability or the number of firm switches after age 55 (Figure B12).

90 percent of the net wage (Federal Republic of Germany, 1988, Section 47). Moreover, workers in regular employment may use sickness absence to reduce their labor supply, a form of moral hazard.

If reduced working hours truly improved health, the decline should be largest in physically and mentally strenuous jobs. However, the IV effects in Figure 6 do not become systematically larger with physical or mental job strain, although inferred absence is much more common in strenuous occupations (Figure B14).[19]

Supplementary outcomes also provide no clear evidence of severe health effects. The IEB record mortality during unemployment and for employed workers with a sickness absence of less than six weeks. Deaths after a long sickness absence or after labor-market exit are not recorded.[20] I find no effect on mortality recorded in the employment records (Figure B16). The null estimate is, however, not precise, because notifications of mortality are rare in the data. In addition, data from the Microcensus show no persistent differential break in nursing-home residence or long-term-care receipt (Figure D3 and Figure D4). Likewise, these rare, severe outcomes provide descriptive evidence rather than precise null estimates. Overall, the evidence points towards factors other than health driving the decline in sickness absence among workers in PR.

Figure 6. IV Effects on Inferred Long-Term Sickness Absence by Job Strain



Notes: The figure reports DiD-IV estimates from eq. (3) by quintiles of physical and mental job strain in the age-54 main job, using a four-month bandwidth and the IV sample. The outcome is an indicator for any inferred long-term sickness absence between ages 55 and 67. The specifications additionally control for demographic characteristics and an indicator for inferred long-term sickness absence at ages 50–54. Whiskers denote 95% confidence intervals. Standard errors are clustered at the individual level.

[19]The same strain profile without the demographic and pre-period controls is reported in Figure B15 and is likewise flat, so the absence of a strain gradient is not produced by the conditioning set.

[20]Thus, the reported mortality in the IEB likely disproportionately reflects sudden and unexpected causes of death, such as accidents or strokes.

4 Effects on Firms

This section turns to the second question of this paper, that is, how firms use PR to adjust their workforce. In the government's own framing, the reimbursement was a job-rotation instrument. Section 3(1) of the AltTZG conditioned it on hiring a replacement, so that the released capacity would be filled by an unemployed person or a trainee. Moreover, older workers enjoy strong employment protection and are expensive to dismiss, and PR offers firms a consensual way to move them out. Whether the subsidy created positions, merely relabeled them, or financed the exit of older workers is a question the worker-level estimates cannot answer. I therefore first exploit that establishments differed in how many of their workers lost access to subsidized PR. Then, I test whether PR use proved more resilient where the 2008–09 financial crisis raised firms' need to reduce staff. Last, I examine whether the costs of PR determine which type of workers firms grant PR to.

4.1 Design

I use the establishment sample described in Section 2.6, which follows establishments with 5 to 2,000 employees in the BHP from 2004 to 2016. The treatment exploits the same birth-cohort discontinuity as the worker-level analysis, but at the level of the establishment's workforce. Workers born in 1954 could begin reimbursable PR in 2009, while workers born in 1955 could not. For establishment j, let N_j^{1954} and N_j^{1955} denote the number of employees born in each cohort and N_j^{reg} the number of regular employees, all measured in 2008. The treatment is the relative cohort difference

$$z_j = \frac{N_j^{1955} - N_j^{1954}}{N_j^{reg}}, \tag{4}$$

so that higher values indicate a more severe cost increase to PR after 2009. Using this measure, I estimate

$$\ln y_{jt} = \alpha_j + \delta_t + \sum_{\tau \neq 2008} \beta_\tau \left(z_j \times \mathbb{1}[t = \tau] \right) + \varepsilon_{jt}. \tag{5}$$

The model traces how an outcome of firm j, such as staff size, evolves around the subsidy abolishment.[21] The year 2008 serves as the reference period. The firm and calendar year fixed effects α_j and δ_t absorb time-invariant variation within firms as well as common shocks. The coefficients of interest β_τ then capture how this change in outcome differs by cohort composition z_j, hence by exposure to

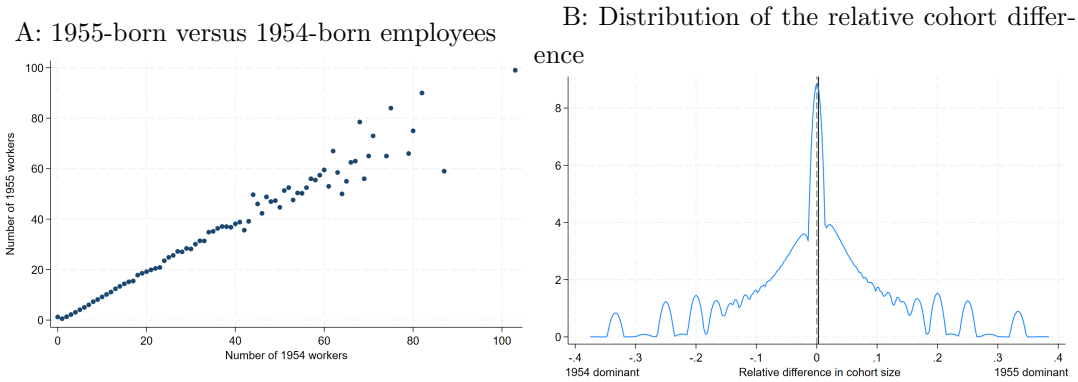
[21] Outcomes, such as firm size, are heavily skewed to the right, making a log-transformation crucial to identify effects in the firm-level data.

the subsidy abolishment. Under the assumption that, absent the abolishment, the cohort composition z_j is unrelated to changes in outcomes, β_τ identify how a firm's workforce adjusts to higher costs of PR.

To assess the credibility of this assumption, it is important to note that z_j measures a difference between adjacent cohorts and not the overall age of the workforce. Thus, eq. (5) does not build on comparing firms with older and younger workforces. Instead, z_j largely captures idiosyncratic variation in hiring. Panel A of Figure 7 shows this directly. The number of 1955-born employees tracks the number of 1954-born employees almost one for one, meaning that a firm with n workers from the 1955 cohort on average employs n workers born in 1954. Accordingly, the distribution of z_j is symmetric and centered around zero (Panel B). Thus, firms do not systematically favor or discriminate against either cohort.

Table C1 confirms that z_j is essentially unrelated to firm characteristics at baseline. Regressing each 2008 characteristic on z_j yields standardized effects of at most 0.013 standard deviations. Two associations are statistically detectable, but with several hundred thousand establishments this reflects precision rather than imbalance, and both are economically negligible. The firm fixed effects thereby account for any firm-side differences related to z_j in level, and the results are insensitive to controlling for baseline characteristics on a yearly basis (Table C2).

Figure 7. Cohort Composition Across Establishments



Notes: Panel A plots the number of employees born in 1955 against the number born in 1954 across establishments in 2008. Panel B plots the kernel density of the treatment z_j in eq. (4), the difference between the two cohort counts divided by regular employment; negative values indicate establishments with relatively more 1954-born (reimbursement-eligible) workers, positive values relatively more 1955-born (ineligible) workers.

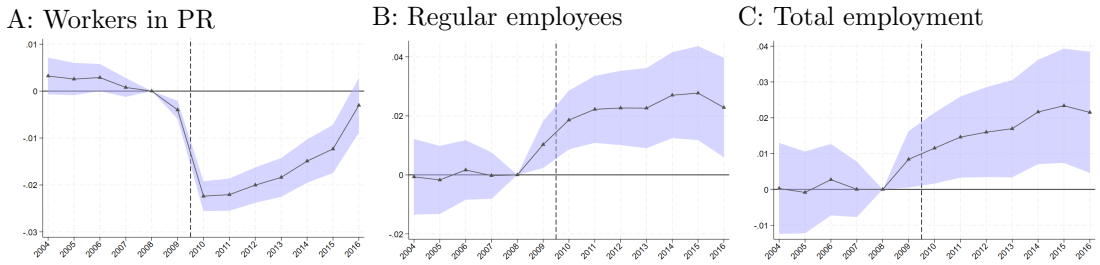
4.2 Effects on PR Use and Employment

Figure 8 reports the event-study coefficients for three outcomes: the number of workers in PR, the number of regular employees excluding PR, and total employment. All three are flat before 2008. Panel A shows that establishments with

relatively more 1955-born workers have fewer employees in PR once the subsidy is abolished. The response appears in 2009, when the 1954 cohort has its only chance to enter subsidized PR, and is largest in the following years, when new cohorts become age-eligible for PR but the subsidy has expired. The effect then fades toward zero by 2016, when the subsidy-induced PR spells of the 1954 cohort end.

Panel B shows the counterpart in regular employment. The same establishments have more regular employees from 2009 onward, by 1.9 log points per unit of z_j in 2010 and between 2.2 and 2.8 log points thereafter. If the reform had only relabeled workers, Panel C would be flat. A worker moving from PR to regular employment leaves total headcount unchanged. Instead, total employment rises, hence establishments facing a higher cost shock ended up with more employees, not the same number differently classified.[22] The additional employment is concentrated among employees aged 60 and older, whose count rises by 3.8 log points per unit of z_j by 2016, whereas the count of employees under 50 moves by at most one log point and is never statistically distinguishable from zero (Figure C3).

Figure 8. Establishment-Level Event Studies



Notes: The panels report the coefficients β_τ from eq. (5), the interaction of the relative cohort difference z_j with year indicators, with 2008 as the reference year. The outcome is the log of one plus the number of workers in PR in Panel A, which retains establishments without PR workers, the log number of regular employees in Panel B, and the log of total employment in Panel C. All specifications include establishment and year fixed effects. The dashed vertical line marks the expiration of reimbursement for PR arrangements starting after December 2009. Shaded areas denote 95% confidence intervals based on standard errors clustered at the establishment level.

The magnitudes are small per establishment but not per affected worker. A one-standard-deviation increase in z_j raises total employment by about 0.3 percent

[22]These estimates are robust to several checks. Allowing the outcome to trend with each 2008 establishment characteristic moves the 2016 coefficient on total employment by at most 0.002 from its baseline value of 0.027, and including all six characteristics yields 0.023 with an unchanged pre-period (Table C2). Restricting the sample to establishments observed in every year from 2008 to 2016, so that closures cannot drive the pattern, yields slightly larger estimates (Figure C1). A placebo that applies the same design to the 1953 and 1954 cohorts, which share the one-year age gap but faced no change in reimbursement, shows no rising post-2009 pattern, with coefficients between -0.012 and 0.011 (Figure C2).

by 2016. Replacing one 1954-born employee with one 1955-born employee moves z_j by $2/N_j^{reg}$, so the 2016 coefficient implies roughly 0.04 additional employees per worker whose access to reimbursement was removed.

Two implications follow. First, the sign of the total-employment effect rules out the replacement-hire channel as the dominant mechanism. Had reimbursed PR routinely triggered hiring, establishments with more eligible workers would have carried both the released worker and a replacement, and the loss of reimbursement would have reduced headcount. Instead, it raised it. Second, the concentration among employees aged 60 and older points to retention rather than to a labor-demand response. Establishments that could not offer subsidized PR kept more of their oldest workers, while employment of workers under 50 did not change.

This pattern is consistent with a reading under which reimbursed PR served firms as a low-cost instrument for reducing the headcount of older workers, who enjoy strong employment protection. Especially in the block model, PR converts an open-ended employment relationship into a fixed-term path to exit, and public reimbursement lowered its price. Once the subsidy was abolished, firms had to weigh the full costs of PR against the savings from removing older workers, which is consistent with PR becoming concentrated among high earners, who are the most costly to employ (see Section 4.4 below). Thus, the overall strategy to replace older workers and reshape the age-structure of the workforce becomes more expensive. As a consequence, firms may not only keep potential PR candidates in regular positions but also retain other older workers who would otherwise have left through other exit routes.

4.3 Partial Retirement as a Downsizing Instrument

Dismissing older workers is particularly expensive. Their wages reflect seniority, and German dismissal protection requires employers to weigh age and tenure in selecting whom to lay off, so that older, long-tenured employees are the last to be selected in a collective dismissal and the first to attract severance claims and works-council resistance if they are. A PR agreement, in contrast, is concluded by mutual consent and fixes the exit date in advance. If PR serves as a substitute for costly dismissals, its value to a firm should be highest when the need to downsize is greatest. The financial crisis provides a setting in which the need to shrink varied sharply across establishments over a short period. Between June 2008 and June 2009, employment in the tenth of region-by-industry cells hit hardest fell by more than 7 percent, and in some by more than 15 percent, while employment in the least affected tenth grew (Table C3).

4.3.1 Crisis Exposure

I measure crisis exposure at the level of the establishment's labor market. It is the employment-weighted mean of the 2008–09 employment change among all *other* establishments with more than five employees in the same commuting zone and two-digit industry that are still observed in 2009.[23] The measure is sign-reversed so that higher values indicate a larger decline.[24] The employment change is the symmetric growth rate of Davis et al. (1996). Establishments no longer observed in 2009 do not enter the cell mean, because a disappearing establishment identifier frequently reflects an identifier change, a takeover, or a restructuring rather than a closure (Hethey-Maier and Schmieder, 2013). Excluding the establishment's own employment change removes any mechanical link between exposure and its own outcomes. Exposure is fixed at its 2008–09 value throughout and standardized to unit variance. Hence, I define crisis exposure (before standardization) as

$$S_j := -\sum_{\substack{k \in c(j),\, k \neq j \\ E_{k,2009} > 0}} \omega_k \, g_k, \qquad \omega_k = \frac{E_{k,2008}}{\sum_{\substack{l \in c(j),\, l \neq j \\ E_{l,2009} > 0}} E_{l,2008}}, \tag{6}$$

where $c(j)$ denotes the commuting-zone-by-industry cell of establishment j, $E_{k,t}$ employment of establishment k on June 30 of year t, and $g_k = (E_{k,2009} - E_{k,2008}) / \frac{1}{2}(E_{k,2008} + E_{k,2009})$ its symmetric growth rate. Section C.2 documents its distribution, shows that it predicts the establishment's own employment decline in 2008–09 with a slope of 0.65, and confirms that it is unrelated to the cohort-difference treatment. A one-standard-deviation increase in z_j is associated with a change in exposure of 0.002 standard deviations (Table C4).

Using the measure for crisis exposure, I extend eq. (5) to

$$\ln y_{jt} = \alpha_j + \delta_t + \sum_{\tau \neq 2008} \Big[\beta_\tau \, z_j + \theta_\tau \, S_j + \gamma_\tau \, (z_j \times S_j) \Big] \times \mathbb{1}[t = \tau] + \varepsilon_{jt}, \tag{7}$$

where interacting crisis exposure with time fixed effects absorbs common changes

[23] The analysis excludes establishments in employment activities (WZ 78), which mostly comprise temporary work agencies. Agency workers are counted at the agency rather than at the client establishment, so that the decline in this industry, by far the largest of all industries in 2008–09, mainly reflects the adjustment of client firms in other industries.

[24] Figure C4 and Figure C5 show average exposure by industry and by labor market region. Export-oriented manufacturing industries such as textiles, fabricated metal products, electronics, and other transport equipment show the largest declines. Health, social work, education, and public administration show the smallest declines or even growth. This matches accounts of the recession in Germany, which mainly hit export-oriented manufacturing and left most services unaffected (Möller, 2010). Across regions, exposure is highest in the industrial areas of North Rhine-Westphalia, southeastern Lower Saxony, Saxony, and Thuringia, and lowest in large parts of Bavaria, but regional differences are small relative to those across industries.

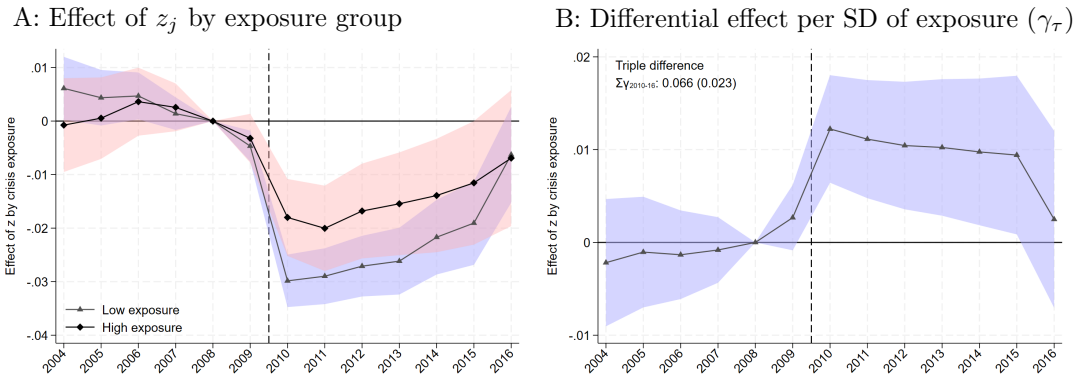
in outcomes among firms affected by the crisis to a similar extent. The new coefficients of interest, γ_τ, are identified through the triple interaction of time, the cost shock, and crisis exposure. They measure how the response to losing reimbursement varied with exposure. If a firm's willingness to pay increases with its need to downsize, one would expect $\gamma_\tau > 0$ for $\tau \geq 2010$, hence the negative effect of z_j on PR use should be attenuated in more crisis-exposed firms.

Because S_j enters in interaction with z_j, it need not be randomly assigned. What is required is that z_j be unrelated to S_j, which Table C4 confirms, and that no other establishment characteristic correlated with exposure independently changes the response to z_j after 2009.

4.3.2 Results

Figure 9 presents the evidence in two ways. Panel A splits the sample at the top tercile of exposure and estimates eq. (5) separately for the two groups within a single regression, allowing year effects to differ by group. Both groups are close to zero before 2008 and both show the 2010 drop in PR use, but its size differs. In establishments with low crisis exposure, the PR count falls by about 3 log points per unit of z_j in 2010 and 2011 and is still about 2 log points below the reference level in 2015. In establishments with high exposure, the drop is 1.8 to 2.0 log points in 2010 and 2011 and narrows to about 1.2 log points by 2015. By 2016, both groups are about 0.7 log points below the reference level. Panel B reports the γ_τ from eq. (7) with continuous exposure. The coefficients are indistinguishable from zero in 2004–2007, and turn positive in 2010, at 1.2 log points per unit of z_j and standard deviation of exposure.

Figure 9. The Response of PR Use to Losing Reimbursement, by Crisis Exposure



Notes: The outcome in both panels is the log of one plus the number of workers in PR, with 2008 as the reference year. Panel A reports the event-study coefficients on the relative cohort difference z_j from eq. (5), estimated separately for establishments in the top tercile of crisis exposure (high exposure) and the remaining two terciles (low exposure) within a single regression that includes establishment fixed effects and year-by-exposure-group fixed effects. Panel B reports the coefficients γ_τ on the triple interaction of z_j, standardized continuous crisis exposure, and year indicators from eq. (7). The legend reports the sum of γ_τ over 2010–2016 with its standard error. Crisis exposure is measured as the average employment decline between June 2008 and June 2009 of all other establishments in the same region and industry that are still observed in 2009, standardized to unit variance (see Section C.2). Establishments in employment activities (WZ 78) are excluded. The dashed vertical line marks the expiration of reimbursement. Shaded areas denote 95% confidence intervals based on standard errors clustered at the establishment level.

The differential is sizable relative to the average response, which amounted to a reduction of the PR count by about 2.2 log points per unit of z_j in 2010 and 2011 (Figure 8, Panel A). A one-standard-deviation increase in crisis exposure offsets about half of this response, so that establishments one standard deviation above the mean reduced their PR use by about half the average when reimbursement ended, while establishments one standard deviation below reduced it by about one and a half times the average. The differential persists through 2015 and fades in 2016. Thus, establishments that had a stronger need for downsizing more often continued to conclude PR agreements at their own expense, whereas establishments without such a need let PR lapse more quickly once the public funding disappeared.

This pattern supports the view that firms use PR as a substitute for costly dismissals, since its value to the firm rises with the need to shed labor. The establishments most willing to pay for PR after 2009 were not those with the strongest interest in retaining experienced staff at reduced hours, but those with the strongest interest in letting them go. One limitation of the analysis is that industries hit hard by the crisis, manufacturing above all, may differ in the preva-

lence of sectoral PR agreements, which could sustain PR use after 2009 regardless of any downsizing motive. While collective agreements are unobservable in the data, past PR use can serve as a second-best proxy. The correlation between pre-2008 PR rates and crisis exposure is small and negative ($\rho = -0.056$), suggesting that, if anything, collective PR agreements are less common in crisis-exposed industries. In line with this, the estimates of γ_τ are robust to controlling for past PR rates (Figure C9).

4.4 Which Workers Do Firms Select into PR?

If firms use PR to let go of older workers whose costs of labor exceed their value, the subsidy should also change who enters PR. Because PR requires the employer's consent, firms can compare the costs of PR with the long-run benefit of a consensual separation with an older worker irrespective of employment protection, and offer PR only to those for whom this trade-off is profitable. Increasing the costs of PR would thereby tighten this profitability condition, making PR more exclusive to the more costly workers.

To identify how the subsidy changed the composition of partial retirees, I return to the worker-level data. As a first step, I identify heterogeneity in the effect on PR uptake. The subsidy-induced reduction in uptake is present across all worker types, but the effects are largest among women, higher earning individuals (based on earnings and their worker AKM effects), and workers in larger firms (Figure C10).[25] Sample splits, however, provide limited information about complier characteristics, because PR uptake is concentrated among workers with a strong standing on the labor market irrespective of subsidy eligibility. Thus, even if the response is more negative for above-median earners, average earnings among PR participants may still rise after the subsidy expires.

To address this limitation, I follow the standard complier-characteristics argument of Abadie (2003); Angrist and Pischke (2009), and estimate eq. (3) where the endogenous variable D_i' is an indicator for ever using PR and the outcome y_i' is an age-54 covariate interacted with an indicator for PR participation ($y_i' \equiv y_i \times D_i'$). Due to this interaction, never-takers contribute zero to y_i' on both sides of the cutoff, and always-takers contribute their covariate value on both sides, so neither group shifts the mean of y_i' at the cutoff. Under monotonicity, the entire change

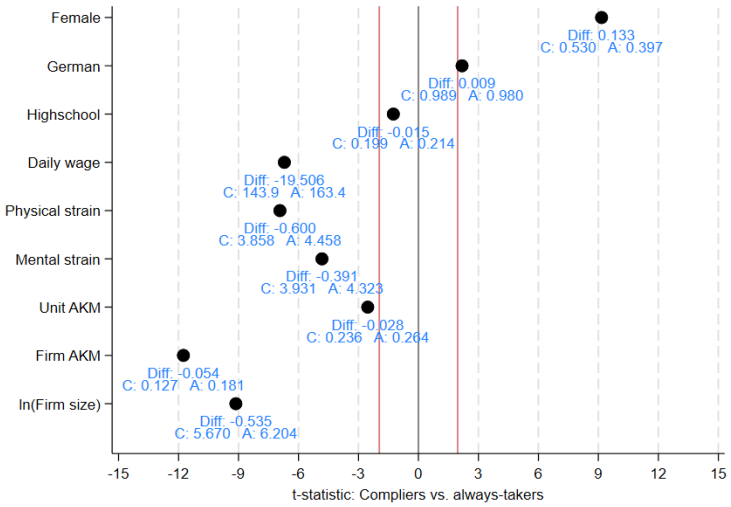
[25] As a multivariate extension, a causal forest (Wager and Athey, 2018) estimates heterogeneity across age-54 covariates using a data-driven algorithm. The heat maps in Figure C11 show negative point estimates in virtually every wage–strain cell, with larger contractions at higher wage deciles and in the least-strenuous decile. Elsewhere, the wage gradient is more pronounced than any monotonic strain gradient. Because the plots do not test cross-cell differences, these patterns are descriptive.

therefore comes from compliers, who participate when eligible but not otherwise. The reduced-form effect on y_i' thus equals the complier share times the complier mean of y_i, whereas the first stage equals the complier share alone. Their ratio, β^{IV}, is the mean of y_i among compliers.

Figure 10 visualizes how compliers and always-takers differ in their age-54 characteristics. For example, 53% of the workers who exclusively participate in PR due to subsidy eligibility are women, whereas the female share is only 40% among workers participating irrespective of subsidy eligibility. Thus, the subsidy contributed to a more equal uptake of PR across genders. Similarly, compliers are characterized by lower earnings and they work at smaller establishments, with 0.535 fewer log points of establishment size, or roughly 40 percent fewer employees. Moreover, both AKM wage components are lower among compliers, in particular the establishment effect, so subsidy-induced participants held lower-paying jobs and worked at lower-premium employers. Thus, the subsidy extended access to economically more vulnerable workers. Without it, PR concentrates among men, higher earners, and larger, higher-wage employers. These are the workers who are most costly to employ, which fits the downsizing motive documented in Section 4.2 and Section 4.3.

In contrast, compliers also come from less demanding jobs. Physical and mental strain, which varies at the 5-digit occupation level on a ten-point scale (Kroll, 2015), is lower among the complier population. The raw data show the same pattern. PR uptake among workers born in the first quarter of 1955 is approximately uniformly distributed across the physical and mental strain distribution, whereas workers born in the last quarter of 1954 exhibit excess mass at the lower end of the strain distribution (Figure C12). Thus, workers in demanding jobs, who are frequently in the center of debates on retirement policy, respond inelastically to the firm-side cost of PR.

Figure 10. Age-54 Characteristics of Compliers and Always-Takers



Notes: The figure compares age-54 characteristics between compliers and always-takers, using the full sample. Compliers enter PR when reimbursement is available but not once it expires; always-takers enter under either regime. Each estimate comes from eq. (3) with a four-month bandwidth, using an indicator for ever entering PR as the endogenous variable and the product of that indicator and the age-54 characteristic listed on the vertical axis as the outcome; scaling by the first stage yields the subpopulation means. Blue annotations report the complier mean (C), the always-taker mean (A), and their difference. Black markers show the t-statistic for the difference, and the red vertical lines mark two-sided 5% critical values. Standard errors are clustered at the individual level.

4.5 Did the Terms of the Agreements Change?

The differences are consistent with firms becoming more selective in whom they grant access to PR once they bear the cost. In addition, the subsidy abolishment may reduce the generosity of PR agreements, in particular, the wage and pension top-up. While the statutory floors limit how far this could go, some workers may still receive less generous offers, excluding those with lower willingness to pay for work-time reductions.

Two pieces of evidence speak against a large downward adjustment. First, the pension-relevant earnings base changes almost identically at PR entry for eligible and ineligible entrants. Figure C13 compares the percentage change in IEB-reported earnings at PR entry for ineligible 1955-cohort entrants with that of matched reimbursement-eligible entrants.[26] As Section 2.4 explains, reported

[26]The sample comprises entrants at age 55 or 56 who are in regular employment in the year before entry and in PR in the year after. Ineligible entrants are the 1955 cohort and 1954-born workers entering at 56, and eligible entrants are 1953-born workers and 1954-born workers entering at 55. I match the two groups one to one and without replacement, exactly on entry age, sex, residence in the former GDR, continuous regular employment at ages 50–54, and one-digit occupation, and on the nearest propensity score based on age-54 daily earnings, log establishment size, the firm AKM effect, and log local labor-market concentration.

earnings during PR consist of the regular pay, which mechanically falls to 50 percent of previous earnings, plus the supplementary base on which employers pay additional pension contributions. The tax-free wage top-up is not reported. At the statutory minimum, reported earnings therefore fall by about 10 percent, close to the medians of −8.5 percent for ineligible and −8.7 percent for matched eligible entrants. The absence of a difference suggests that firms did not reduce the supplementary pension contribution once reimbursement ended.

Second, because the IEB do not record the cash top-up, I turn to the Microcensus, which reports current net monthly earnings. Among respondents aged 55–62 who report working reduced hours because of PR, Table D1 regresses net monthly earnings on an indicator for the ineligible 1955–1956 cohorts. Participants from the ineligible cohorts thereby do not report lower net wages. Instead, their earnings during PR are slightly higher, a pattern that is insensitive to controlling for demographic characteristics, two-digit occupation fixed effects, and working hours.

Had firms responded to the cost shock by cutting negotiated supplements substantially, participant earnings should have fallen. Since neither estimate shows a negative association between firms' PR costs and the generosity of agreements, the reform appears to have operated mainly on the extensive margin, with firms' refusal to grant PR as the main barrier to entry. Note, however, that the presented evidence is not a causal effect, but a comparison of observed participants across regimes. The composition results above imply that the later cohorts are drawn from a different pool of workers. Matching and controlling for observables addresses this problem, but unobservables may distort the relationship between wages and subsidy eligibility among participants.

5 Conclusion

Phased retirement promises to reconcile three interests that diverge as populations age. Older workers prefer to reduce their labor supply, firms are often reluctant to employ them, and pension systems depend on longer careers. Using the abolishment of a German subsidy that raised firms' costs of partial retirement, this paper shows how PR balanced these interests in practice. The subsidy raised uptake by about one half, but subsidy-induced participants did not delay job exit. Instead, PR replaced regular employment and reduced lifetime labor supply and pension contributions. The resulting pension losses fell on the participants themselves, hence the loss for the pension insurance was negligible. Establishments that lost access to the subsidy retained more of their oldest workers, but establishments with a strong need to downsize were more likely to keep using PR at their own

expense. Moreover, once firms bore the full cost, PR concentrated among high earners. Together, these results suggest that subsidized PR benefited firms and participating workers, but not the goal of longer careers.

The interests of all three sides of the labor market are difficult to reconcile within a single scheme. Firms currently value PR mainly because it ends employment matches, in which wages may have diverged from productivity but dismissal protection makes them costly to dissolve. Workers value PR because it reduces hours at a modest loss of income, and the pension system benefits only if it postpones exit. The German design met the first two interests by trading shorter hours for an earlier effective exit. Alternative designs shift the balance between these interests. Tying hour reductions to continued employment until the normal retirement age, as in Dutch gradual-retirement plans, potentially raises labor supply, but such conditions shift the cost of shorter hours back to firms or to the public budget. All three interests align only where firms value extended worker careers in their own right, for instance to retain experienced staff. Thus, there is a potential for synergies between phased retirement and policies that make part-time work of older employees attractive to firms. At the same time, these policies may enhance barriers younger workers face in the labor market, especially during times when entry-level jobs are scarce.

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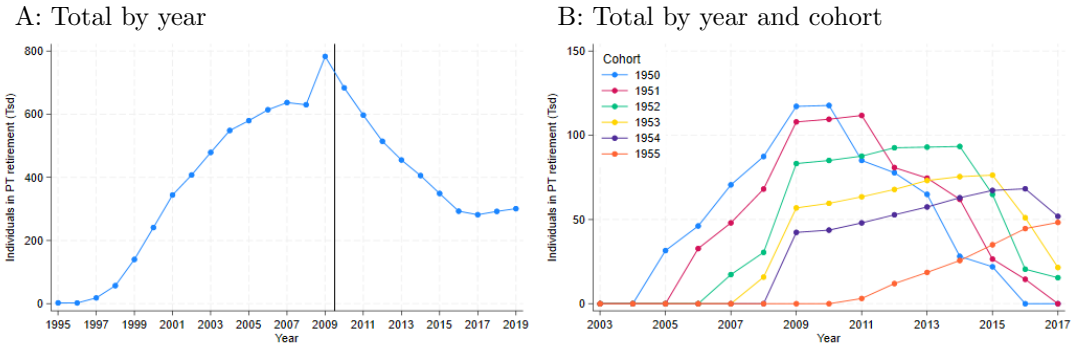
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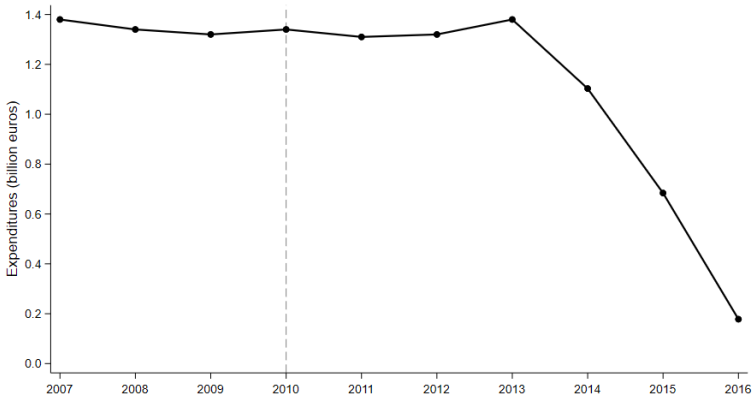
A Additional Background Information

Figure A1. Number of Workers in Partial Retirement



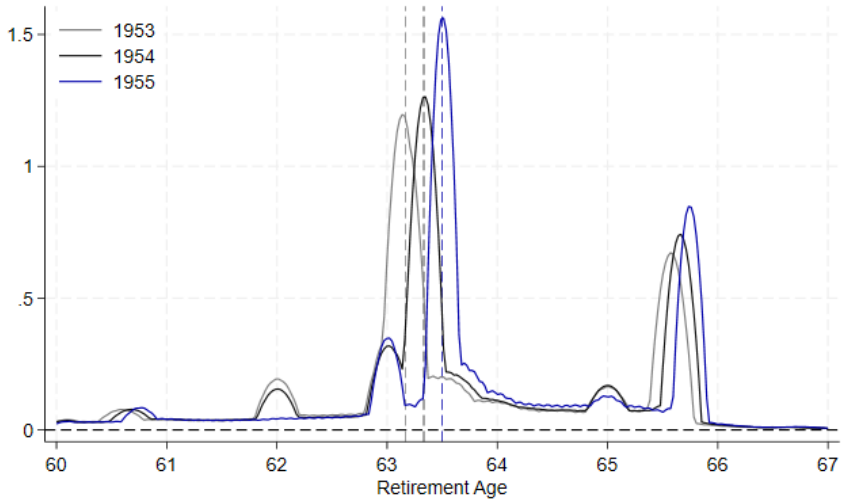
Notes: Panel A reports the annual number of workers in PR. Panel B reports the corresponding counts separately by birth cohort. The counts are extrapolated from a 2 percent sample of German labor-market histories for workers subject to social security contributions.

Figure A2. Public Expenditures on PR Reimbursements



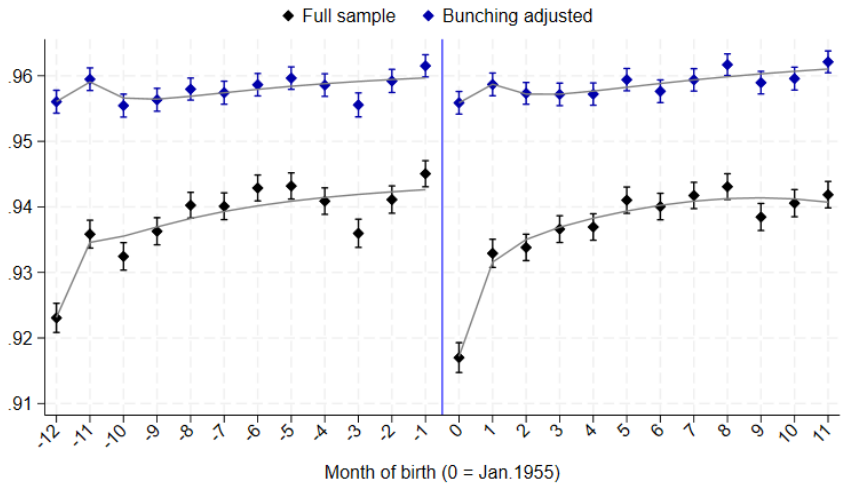
Notes: The figure reports the Federal Employment Agency's annual expenditures on reimbursements to employers under the Old-Age Part-Time Act, in billions of nominal euros. The dashed line marks 2010, the first year in which newly starting arrangements could no longer be reimbursed. Arrangements begun by December 31, 2009 remained reimbursable for up to six years. Values for 2007–2013 are taken from the annual report of the respective year (the 2007 value from the 2008 report); values for 2014–2016 from the reports on the financial development of unemployment insurance. Later reports revise some earlier values slightly. Sources: Bundesagentur für Arbeit, Geschäftsbericht 2008–2016.

Figure A3. Distribution of Job Exit Age



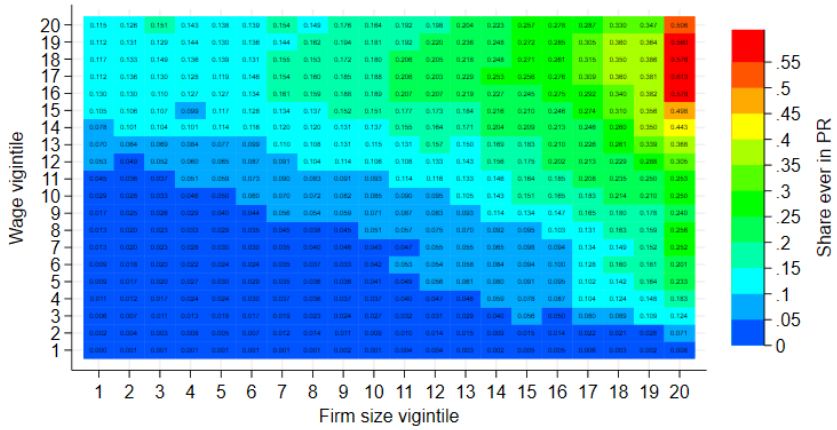
Notes: The figure plots cohort-specific kernel densities of the age at which an individual's final regular-employment or unemployment-benefit spell ends. Solid and dashed vertical lines denote the cohort-specific means and medians, respectively.

Figure A4. Share of German Nationality by Month of Birth



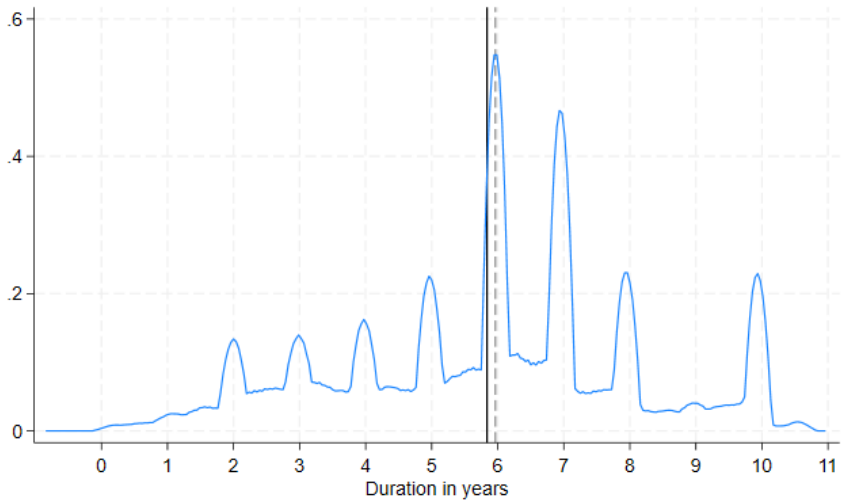
Notes: The figure plots the share of individuals with German citizenship by recentered month of birth in the IEB data. Black markers use the raw sample; blue markers use the bunching-adjusted analysis sample. The adjustment is based on a regression of an indicator for January birth on nationality fixed effects, with German citizenship as the reference category. The adjusted sample excludes nationality groups whose estimated fixed effect deviates strongly from the German baseline. Whiskers denote 95% confidence intervals. Standard errors are clustered at the individual level.

Figure A5. Lifetime Uptake of PR by Wage and Firm Size at Age 54



Notes: The figure reports mean lifetime uptake of PR for the 1953 and 1954 birth cohorts by vigintiles of the daily wage and firm size at age 54.

Figure A6. Distribution of Partial Retirement Duration

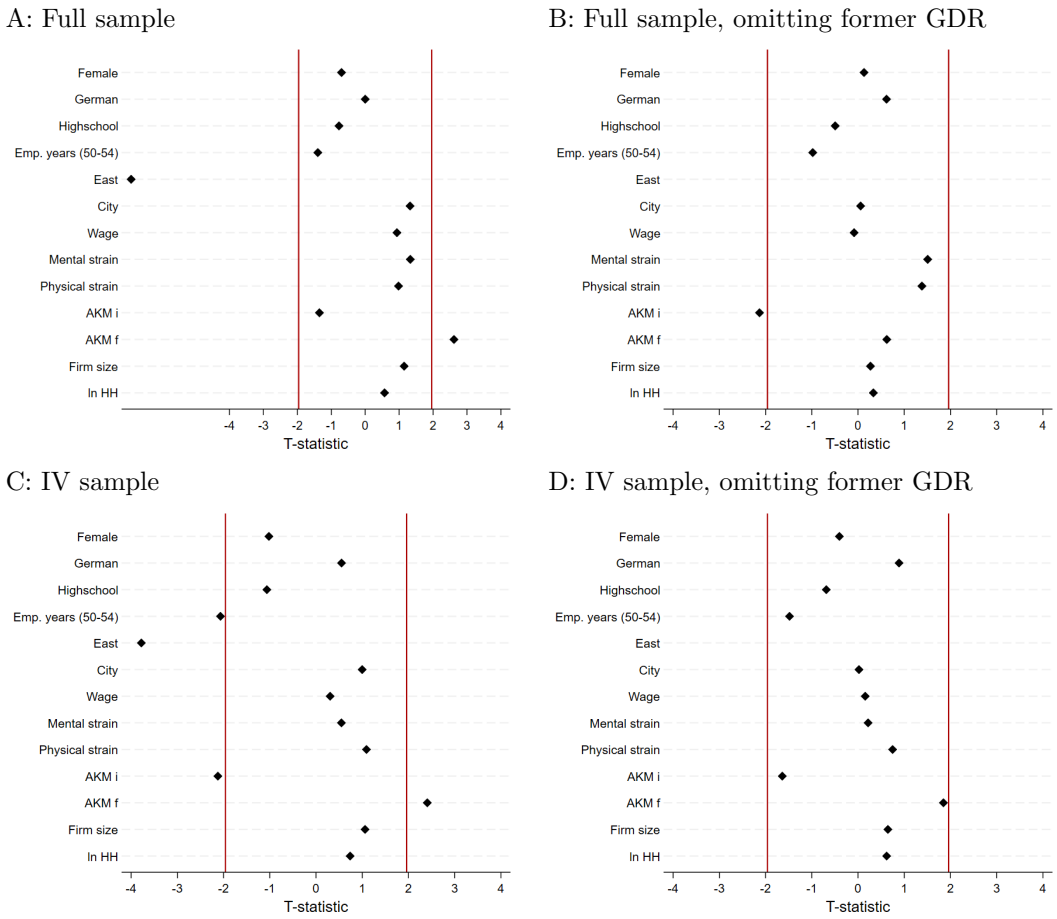


Notes: The figure plots the kernel density of the duration of all partial-retirement spells for the 1953–1955 birth cohorts. The solid and dashed vertical lines denote the mean and median spell duration, respectively.

B Worker-Level Analysis: Additional Results

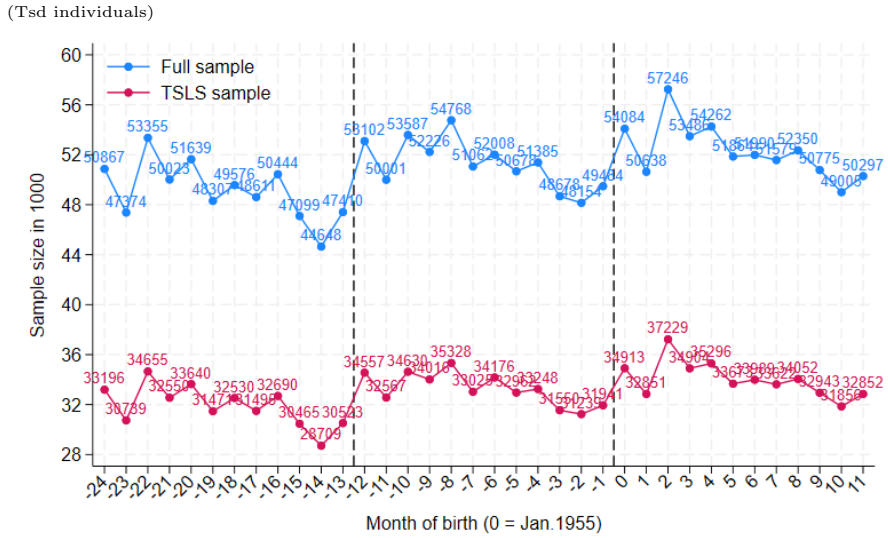
B.1 Identification

Figure B1. IV Placebo Estimates for Age-54 Covariates



Notes: The figure reports t-statistics from DiD-IV estimates of eq. (3), using the covariates measured at age 54 and listed in Table 2 as outcomes. Panels A and B use the full sample, and Panels C and D the IV sample. Panels B and D additionally exclude individuals from the former GDR. The red vertical lines mark two-sided 5% critical values. Standard errors are clustered at the individual level.

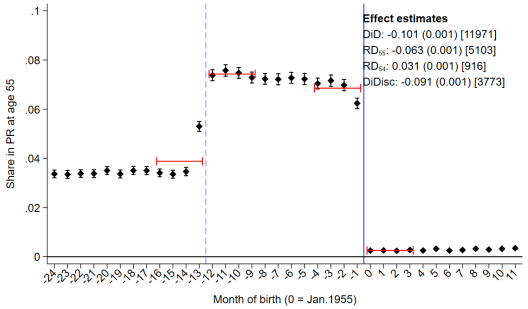
Figure B2. Sample Size by Month of Birth



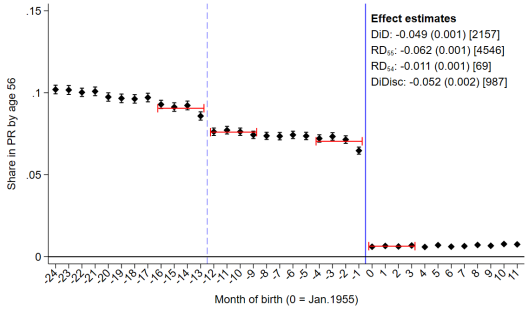
Notes: The figure reports the number of unique individuals by recentered month of birth in the IEB data after adjusting the sample for bunching among foreign-nationality groups. The solid and dashed vertical lines mark January 1955 and January 1954, respectively.

Figure B3. Coding of Endogenous Variable

A: Start PR at 55



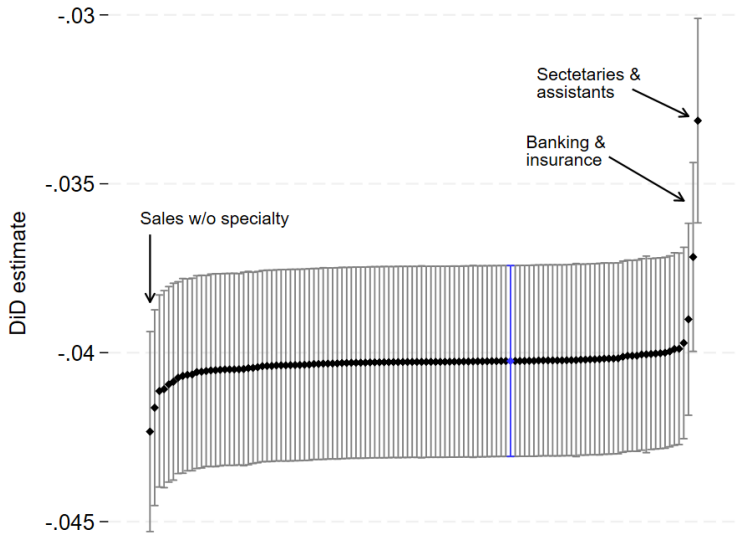
B: Start PR before 57



Notes: The figure replicates the analysis in Figure 3 using alternative definitions of the endogenous treatment: entry into PR at age 55 in Panel A and entry into PR by age 56 in Panel B.

B.2 Effects on PR Uptake

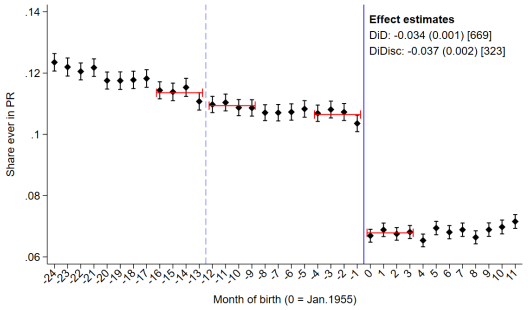
Figure B4. Occupation Leave One Out for PR Uptake



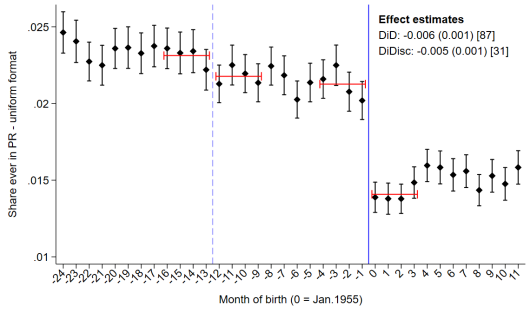
Notes: The figure shows local DiD reduced-form estimates from eq. (1) using the uptake of PR as outcome and a four-month bandwidth. The main estimate is shown in blue. The gray markers replicate this estimate while omitting one 3-digit occupation, measured at age 54. Whiskers denote 95% confidence intervals. Standard errors are clustered at the individual level.

Figure B5. Ever in Block or Uniform Partial Retirement by Month of Birth

A: Block format

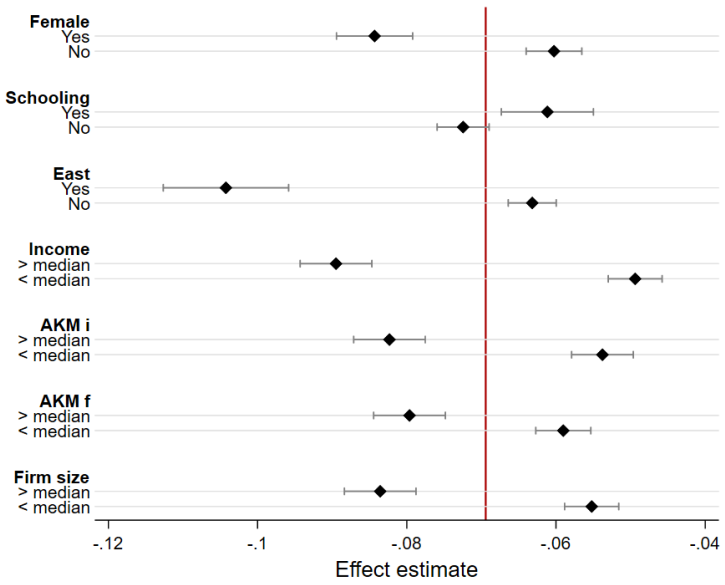


B: Uniform format



Notes: The figure reports mean uptake of PR in the block format (Panel A) and the uniform format (Panel B) by recentered month of birth. The solid and dashed vertical lines mark January 1955 and January 1954, respectively. Whiskers denote 95% confidence intervals. Estimates in the upper-left corner are local DiD and DiDisc estimates from eqs. (1) and (2), respectively. The DiD bandwidth is four months and is indicated by the red intervals; the DiDisc bandwidth is seven months. Standard errors are shown in parentheses, F-statistics in square brackets, and standard errors are clustered at the individual level.

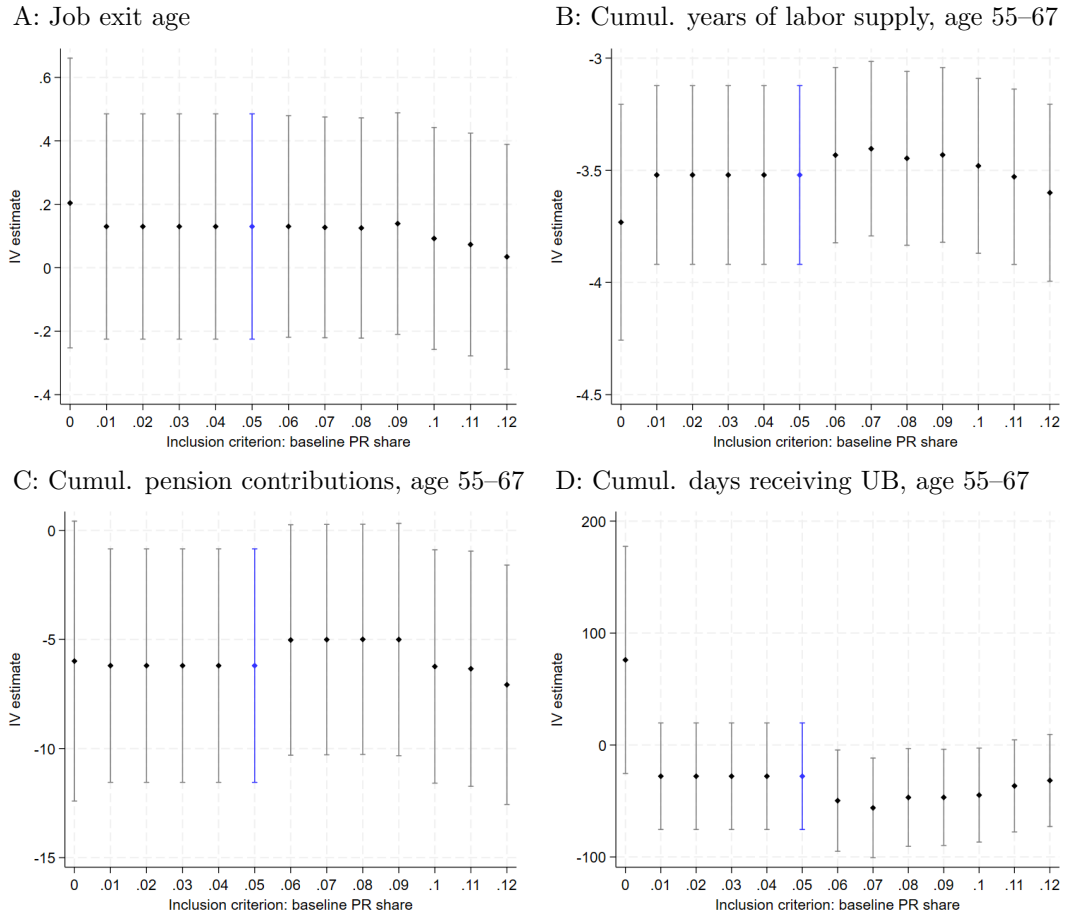
Figure B6. Heterogeneous First-Stage Effects



Notes: The figure reports local DiD estimates from eq. (1), using a four-month bandwidth, the IV sample, and an indicator for entering PR by age 56 as the outcome. Estimates are shown separately for the two groups defined by each age-54 characteristic listed on the vertical axis. The red vertical line is the full-sample estimate. Whiskers denote 95% confidence intervals based on standard errors clustered at the individual level.

B.3 Effects of Early PR

Figure B7. IV Estimates Across IV-Sample Inclusion Thresholds



Notes: The figure replicates the DiD-IV estimates of Figure 5 while varying the rule that defines the IV sample. The horizontal axis is the minimum lifetime PR uptake among the 1953–1954 cohorts that an age-54 wage-by-establishment-size vigintile cell must reach to be retained, from 0 (the full baseline sample) to 0.12 in steps of 0.01. The blue marker at 0.05 is the threshold used throughout the paper. Estimates come from eq. (3) with a four-month bandwidth; the endogenous variable is entry into PR by the end of age 56. Outcomes are identified in the panel headings and defined as in Figure 5. Whiskers denote 95% confidence intervals based on standard errors clustered at the individual level.

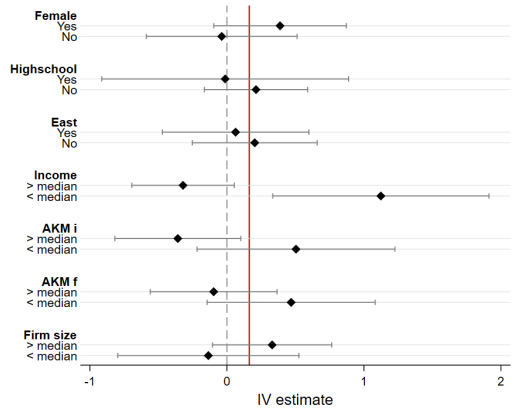
Table B1. Placebo: Workers Outside the IV Sample

	First stage	Reduced form					
	PR by age 56	Job-exit age	Labor supply	Pension contrib.	Pension points	Days in UB	Any sick leave
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Workers outside the IV sample (placebo)							
DiD estimate	−0.009***	−0.013	0.059**	0.149	0.025	−13.657**	0.003
	(0.001)	(0.022)	(0.025)	(0.136)	(0.019)	(5.990)	(0.003)
Pre-cutoff mean	0.012	62.869	6.428	26.577	3.706	416.085	0.269
N	285,873	285,455	285,873	285,873	285,873	285,873	285,873
F-statistic	140						
Panel B: IV sample							
DiD estimate	−0.071***	−0.007	0.254***	0.519***	0.076***	0.509	0.016***
	(0.002)	(0.013)	(0.016)	(0.197)	(0.028)	(2.003)	(0.002)
Pre-cutoff mean	0.102	63.227	7.057	66.957	9.461	133.708	0.273
N	526,339	526,322	526,339	526,339	526,339	526,339	526,339
F-statistic	2,048						

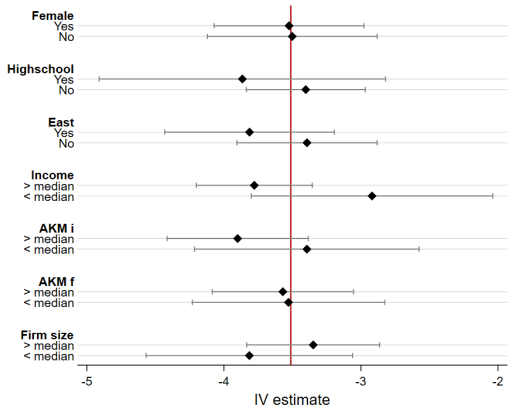
Notes: The table reports local DiD estimates from eq. (1) using a four-month bandwidth. Column (1) reports the first stage for entering PR by the end of age 56, and Columns (2) to (7) report reduced-form effects on the outcomes of Figure 5, defined as in that figure. Panel A comprises workers in age-54 wage-by-firm-size cells whose baseline PR uptake among the 1953–1954 cohorts is below five percent and who are therefore excluded from the IV sample. Panel B comprises the IV sample. Pre-cutoff means refer to workers born between September and December 1954. Standard errors are clustered at the individual level. Asterisks denote statistical significance at the 10 percent (*), 5 percent (**), and 1 percent (***) levels.

Figure B8. Heterogeneous IV Effects of Early PR

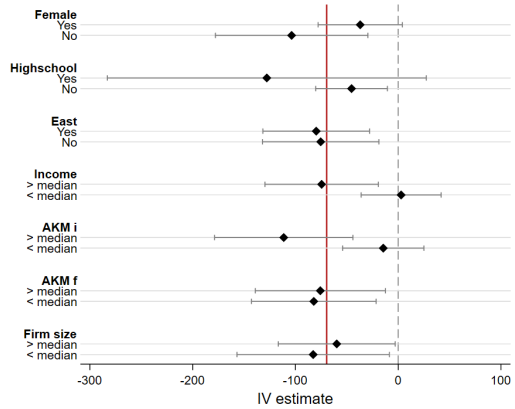
A: Job exit age



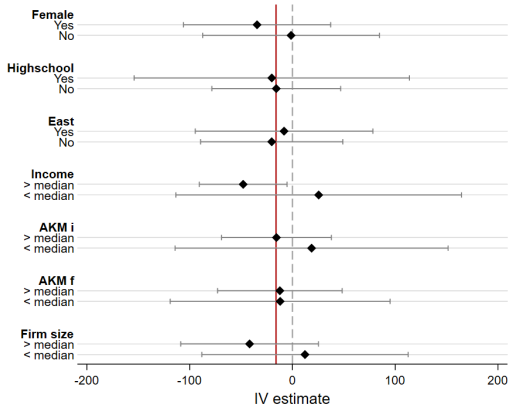
B: Cumul. years of labor supply, age 55-67



C: Cumul. pension contribution, age 55-67

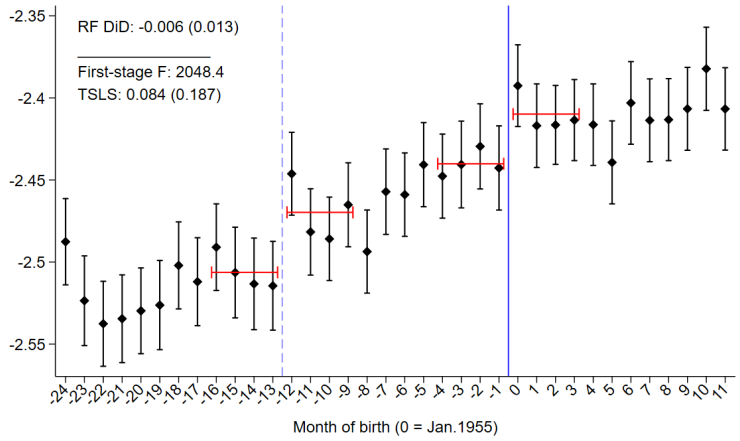


D: Cumul. pension points, age 55-67



Notes: The figure reports DiD-IV estimates from eq. (3), using a four-month bandwidth and the IV sample. Outcomes are identified in the panel headings. Estimates are shown separately for the two groups defined by each age-54 characteristic listed on the vertical axis. The red vertical line is the full-sample estimate. Whiskers denote 95% confidence intervals based on standard errors clustered at the individual level.

Figure B9. Job Exit Age Relative to Statutory Retirement Age by Month of Birth



Notes: The figure plots mean job-exit age minus the normal retirement age by recentered month of birth for the IV sample. Job-exit age is the age at which the final regular-employment or unemployment-benefit spell ends. Whiskers denote 95% confidence intervals. Estimates in the upper-left corner are local DiD reduced-form estimates from eq. (1) and DiD-IV estimates from eq. (3), using a four-month bandwidth. The endogenous variable is entry into PR by age 56. Standard errors are shown in parentheses and clustered at the individual level.

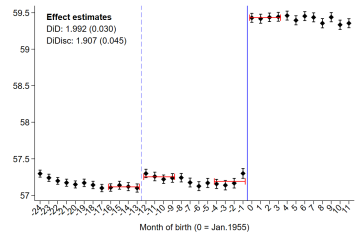
Table B2. Intensive-Margin Local DiD Effects

	Without controls			With controls		
	PR entry age (1)	PR duration (2)	Job exit age (3)	PR entry age (4)	PR duration (5)	Job exit age (6)
DiD	2.105***	-1.800***	0.237***	1.992***	-1.647***	0.283***
	(0.0305)	(0.0285)	(0.0183)	(0.0296)	(0.0277)	(0.0175)
Effect share		85.7%	11.3%		82.7%	14.2%
Adj. R^2	0.122	0.083	0.018	0.189	0.149	0.096
Oster δ				12.052	9.722	-16.017
DMP δ				0.959	0.949	0.915
N	91,884	91,884	91,884	91,884	91,884	91,884

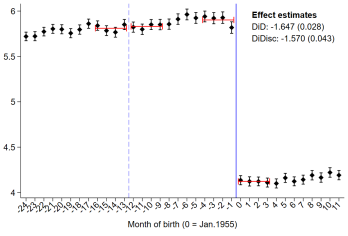
Notes: The table reports reduced-form local DiD estimates from eq. (1), using a four-month bandwidth and restricting the sample to individuals who ever use PR. Columns 4–6 additionally control for the age-54 characteristics listed in Table 2, except the worker AKM effect, which is omitted because of missing values. The effect share measures the extent to which delayed PR entry is reflected in shorter PR duration and later job exit. The Oster delta and the delta of Diegert et al. (2026) (DMP) report sensitivity to unobserved confounding. Standard errors are clustered at the individual level. Asterisks denote statistical significance at the 10 percent (*), 5 percent (**), and 1 percent (***) levels.

Figure B10. Intensive-Margin Outcomes by Month of Birth

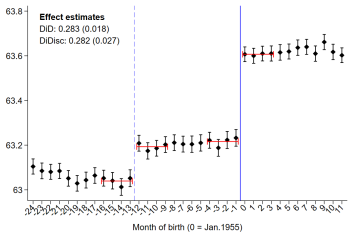
A: PR entry age



B: PR duration

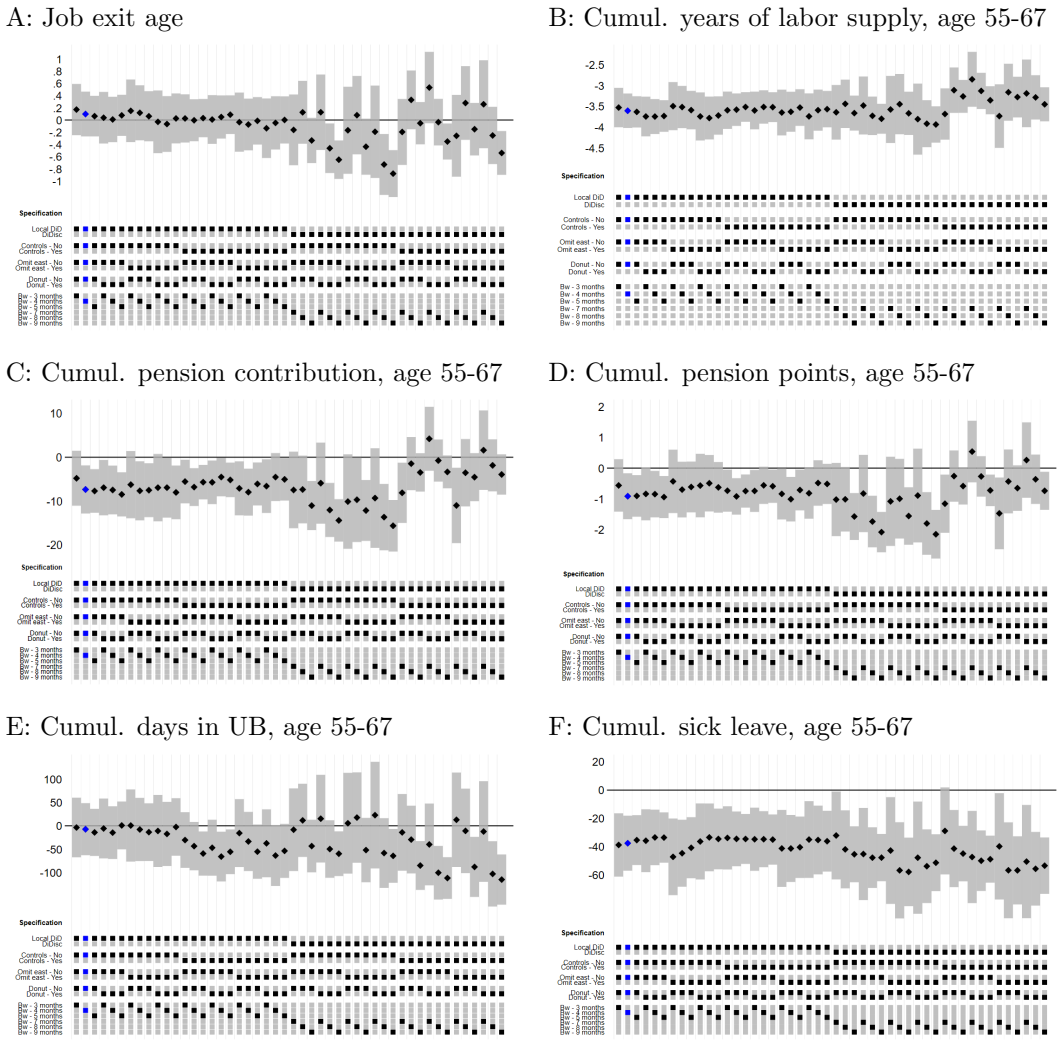


C: Job exit age



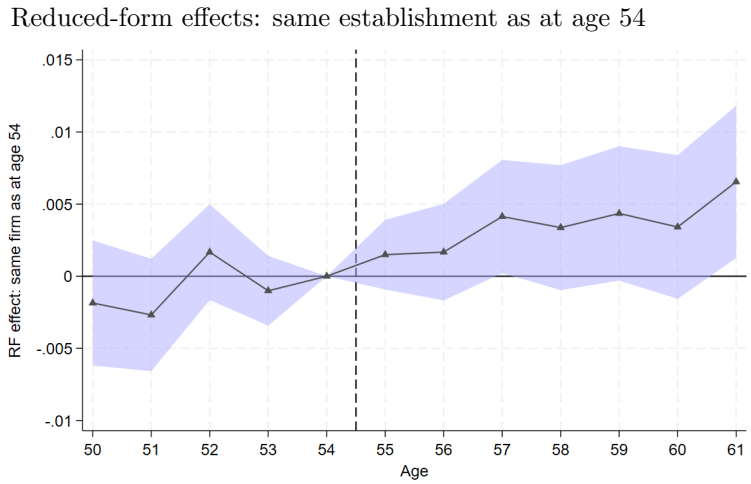
Notes: The panels plot outcome means by recentered month of birth for individuals who eventually use PR. The outcome in each panel is given in the panel heading. Whiskers denote 95% confidence intervals. Estimates in the upper-left corner are local DiD and DiDisc estimates from eqs. (1) and (2), respectively. Standard errors are shown in parentheses and clustered at the individual level.

Figure B11. IV Effects Across Specifications



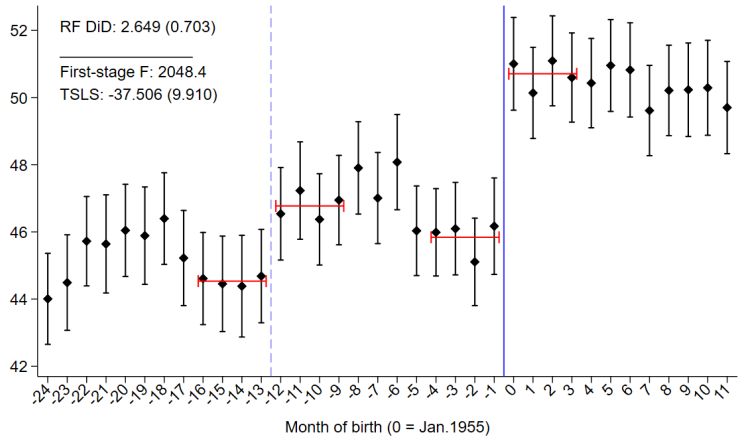
Notes: The figure reports two-stage least-squares estimates for the outcomes shown in the panel headings across alternative specifications, using the IV sample. Bars denote 95% confidence intervals based on standard errors clustered at the individual level. The blue marker identifies the preferred specification. Specifications with controls include the age-54 covariates listed in Table 2, excluding AKM-i. In donut specifications, observations born in January and December are omitted.

Figure B12. Employment at the Age-54 Establishment



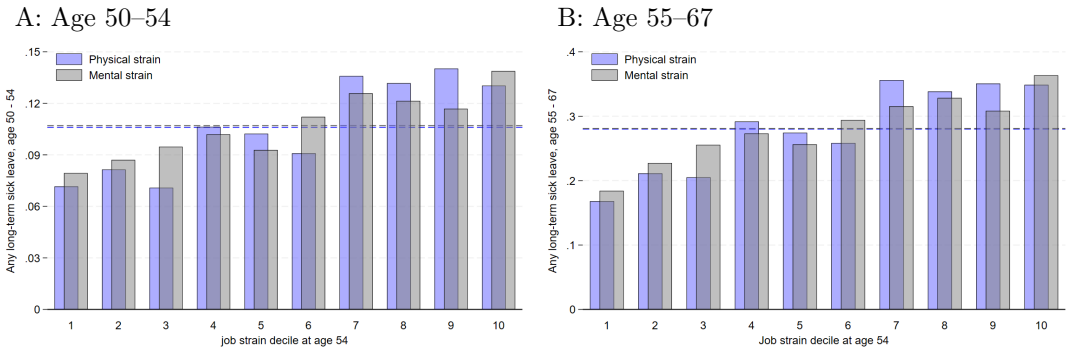
Notes: The figure reports age-specific local DiD estimates from eq. (1), using a four-month bandwidth. The outcome is an indicator equal to one if the individual works at the same establishment as at age 54. For each age, the establishment associated with the individual's main employment spell is used. Shaded areas denote 95% confidence intervals. Standard errors are clustered at the individual level.

Figure B13. Days in Inferred Long-Term Sickness Absence, Age 55–67, by Month of Birth



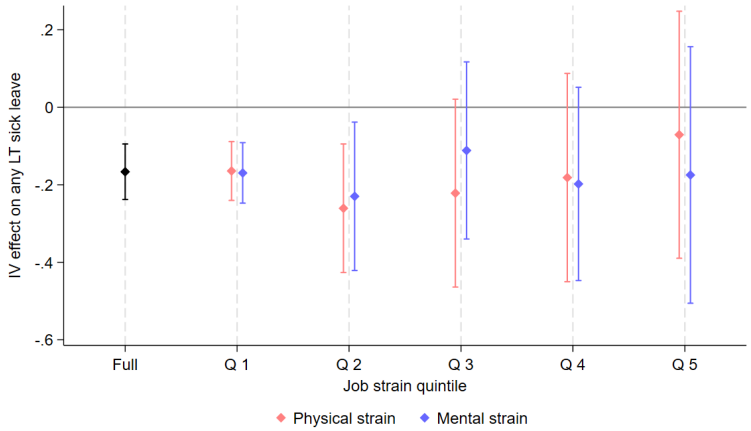
Notes: The figure plots the average number of days in inferred long-term sickness absence between ages 55 and 67 by recentered month of birth for the IV sample. Whiskers denote 95% confidence intervals. The upper-left corner reports the local DiD reduced-form and DiD-IV estimates from eqs. (1) and (3), respectively, using a four-month bandwidth. The endogenous variable is entry into PR by the end of age 56. Standard errors are shown in parentheses and clustered at the individual level.

Figure B14. Prevalence of Inferred Long-Term Sickness Absence by Age Group



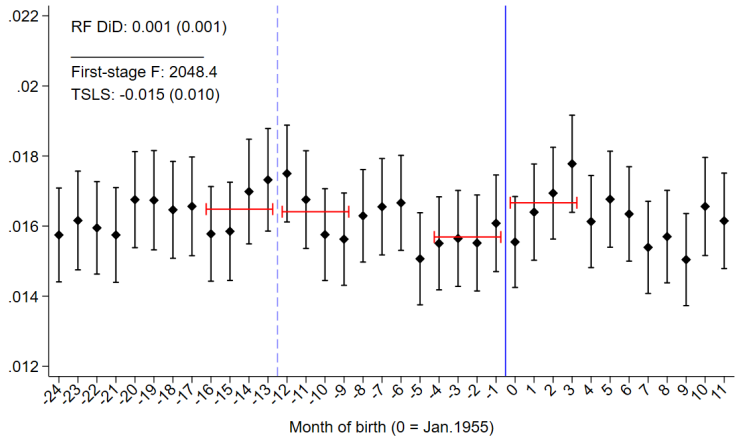
Notes: The panels plot the share of individuals with an inferred long-term sickness absence during the stated age window by job-strain decile at age 54. The dashed line shows the population average.

Figure B15. IV Effects on Inferred Long-Term Sickness Absence by Job Strain Without Controls



Notes: The figure replicates Figure 6 without control variables. It reports DiD-IV estimates from eq. (3) by quintiles of physical and mental job strain in the age-54 main job. The outcome is an indicator for any inferred long-term sickness absence between ages 55 and 67. Whiskers denote 95% confidence intervals. Standard errors are clustered at the individual level.

Figure B16. Mortality While Observed in the Labor Market



Notes: The figure plots premature mortality rates by recentered month of birth for the IV sample. Premature death is defined as death while the individual is observed in employment or unemployment-benefit receipt and before an inferred long-term sickness absence. Whiskers denote 95% confidence intervals. The upper-left corner reports the local DiD reduced-form and DiD-IV estimates from eqs. (1) and (3), respectively, using a four-month bandwidth. The endogenous variable is entry into PR by the end of age 56. Standard errors are shown in parentheses and clustered at the individual level.

C Firm-Level Analysis: Additional Results

This appendix collects the balance test, the horse race against composition trends, and the robustness checks for the establishment-level event studies in Section 4.1 and Section 4.2, followed by the construction and descriptive properties of the crisis-exposure measure used in Section 4.3 and additional results on the selection of workers into PR discussed in Section 4.4 and Section 4.5.

C.1 Establishment-Level Event Studies

Table C1. Baseline Establishment Characteristics and the Cohort-Difference Treatment

	Mean (1)	SD (2)	Coefficient (3)	t (4)	Std. effect (5)
Share in 1954/55 cohorts	0.089	0.064	0.005	4.90	0.013
			(0.001)		
Share aged 50–59	0.266	0.137	0.000	0.25	0.001
			(0.002)		
Share aged under 50	0.666	0.165	0.001	0.58	0.001
			(0.003)		
Share medium-qualified	0.749	0.195	0.001	0.35	0.001
			(0.002)		
Share female	0.492	0.310	0.009	2.43	0.005
			(0.004)		
Mean full-time daily wage	85.549	40.231	−0.865	−1.78	−0.003
			(0.486)		
SD of treatment z_j			0.161		
Establishments			334,363		

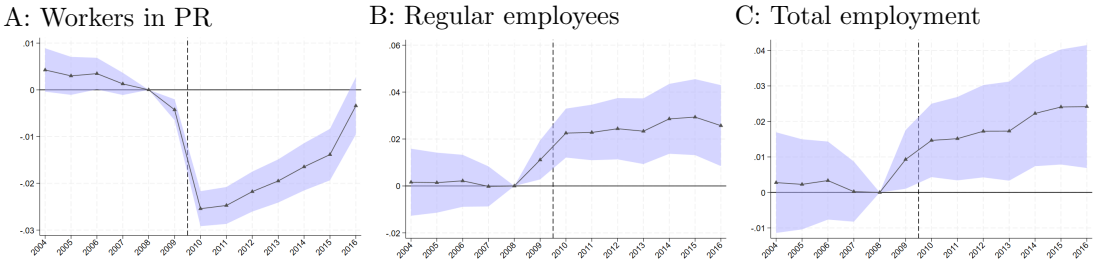
Notes: Each pair of rows reports a separate cross-sectional regression of a 2008 establishment characteristic on the relative cohort difference z_j defined in eq. (4). Columns (1) and (2) report the mean and standard deviation of the characteristic across establishments. Column (3) reports the coefficient on z_j with heteroskedasticity-robust standard errors in parentheses, and Column (4) its t-statistic. Column (5) reports the standardized effect, the coefficient multiplied by the standard deviation of z_j and divided by the standard deviation of the characteristic, so that it measures the shift in the characteristic, in its own standard deviations, from a one-standard-deviation move in the treatment. Shares are counts of the respective group divided by total establishment employment. The sample comprises establishments with 5 to 2,000 employees in the Establishment History Panel in 2008. Asterisks are omitted.

Table C2. Total Employment: Event-Study Coefficients Under Alternative Composition Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. variable: log total employment								
2010	0.012**	0.012**	0.012**	0.012**	0.012**	0.012**	0.011**	0.011**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
2011	0.018***	0.018***	0.018***	0.018***	0.018***	0.018***	0.017***	0.016***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
2012	0.020***	0.021***	0.020***	0.020***	0.020***	0.020***	0.020***	0.018***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
2013	0.021***	0.021***	0.021***	0.020***	0.021***	0.021***	0.020***	0.018**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
2014	0.025***	0.025***	0.025***	0.024***	0.025***	0.024***	0.023***	0.021***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
2015	0.028***	0.029***	0.028***	0.027***	0.028***	0.028***	0.026***	0.024***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
2016	0.027***	0.028***	0.027***	0.026***	0.027***	0.026***	0.025***	0.023**
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Baseline characteristic interacted with year								
Share in 1954/55 cohorts		✓						✓
Share aged 50–59			✓					✓
Share aged under 50				✓				✓
Share medium-qualified					✓			✓
Mean full-time daily wage						✓		✓
Share female							✓	✓
Pre-trend p-value	0.936	0.733	0.935	0.962	0.936	0.937	0.909	0.884
Within R^2	0.000	0.002	0.012	0.014	0.001	0.003	0.001	0.020
Observations	3,814,890							
Establishments	326,419							

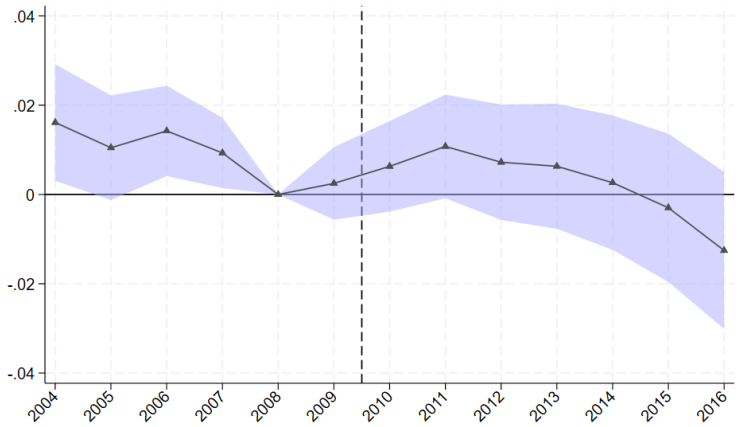
Notes: The table reports the post-reform coefficients β_τ for $\tau = 2010, \ldots, 2016$ from eq. (5), with log total employment as the outcome and 2008 as the reference year. Column (1) is the specification in Panel C of Figure 8. Columns (2)–(7) each add the interaction of year indicators with one baseline establishment characteristic measured in 2008 and standardized to unit variance, so that the event-study coefficients are identified from establishments with the same trajectory in that characteristic; Column (8) includes all six. All columns are estimated on the common sample of establishments with nonmissing values for every characteristic. The pre-trend p-value is from a joint test that the 2004–2007 coefficients equal zero. All specifications include establishment and year fixed effects. Standard errors are clustered at the establishment level and shown in parentheses. Asterisks denote statistical significance at the 10 percent (*), 5 percent (**), and 1 percent (***) levels.

Figure C1. Establishment-Level Event Studies: Balanced Panel



Notes: The figure replicates Figure 8 on the balanced panel of establishments observed in every year from 2008 to 2016, so that the exit of establishments after the reference year cannot contribute to the estimates. The panels report the coefficients β_τ from eq. (5) with 2008 as the reference year; outcomes are the log of one plus the number of workers in PR (Panel A), the log number of regular employees excluding PR (Panel B), and the log of total employment (Panel C). All specifications include establishment and year fixed effects. The dashed vertical line marks the expiration of reimbursement. Shaded areas denote 95% confidence intervals based on standard errors clustered at the establishment level.

Figure C2. Placebo: Total Employment Using the 1953 and 1954 Cohorts



Notes: The figure replicates Panel C of Figure 8 with a placebo treatment constructed from the 1954 and 1953 cohorts, $(N_j^{1954} - N_j^{1953})/N_j^{reg}$, which shares the one-year age gap of the actual treatment but faced no change in reimbursement eligibility: both cohorts could begin reimbursable PR before the deadline. The outcome is log total employment, 2008 is the reference year, and the specification includes establishment and year fixed effects. The dashed vertical line marks the expiration of reimbursement. Shaded areas denote 95% confidence intervals based on standard errors clustered at the establishment level.

Figure C3. Employment by Age Group



Notes: The panels report the coefficients β_τ from eq. (5) with 2008 as the reference year, using the log number of employees aged 60 and older (Panel A) and the log number of employees under 50 (Panel B) at the establishment as outcomes. Age groups are built from the Establishment History Panel's age-band employment counts. The under-50 series is available from 2006. All specifications include establishment and year fixed effects, and the sample comprises establishments with 5 to 2,000 employees. The dashed vertical line marks the expiration of reimbursement. Shaded areas denote 95% confidence intervals based on standard errors clustered at the establishment level.

C.2 Crisis Exposure

The exposure measure used in Section 4.3 is constructed from the universe of establishments in the Establishment History Panel with more than five employees on 30 June 2008. For each establishment, the employment change to 30 June 2009 is the symmetric growth rate $g_j = (E_{j,2009} - E_{j,2008})/\frac{1}{2}(E_{j,2008} + E_{j,2009})$, which is bounded between -2 and 2. Establishments are assigned to cells defined by commuting zone (*Arbeitsmarktregion*) and two-digit industry (WZ 2008), both measured in 2008. Exposure is the mean of g_j, weighted by 2008 employment, over all other establishments in the cell that are still observed in 2009, with the establishment's own growth rate excluded, multiplied by -1 so that higher values indicate a larger decline. Establishments no longer observed in 2009 do not enter the cell means, because a disappearing establishment identifier frequently reflects an identifier change, a takeover, or a restructuring rather than a closure (Hethey-Maier and Schmieder, 2013). They are nevertheless assigned the exposure of their cell. Exposure is set to missing if fewer than ten other establishments in the cell are observed in both years. Establishments in employment activities (WZ 78) are excluded throughout. The measure is fixed at its 2008–09 value throughout the analysis and, where it enters continuously, standardized by its cross-sectional standard deviation among establishments in 2008.

Table C3 summarizes the measure for the construction population and for the estimation sample; the two distributions are close, with a mean decline of 2.9 to

3.1 percent and a standard deviation of 0.04 to 0.05. Figure C4 and Figure C5 show average exposure in the estimation sample by two-digit industry and by labor market region. Figure C7 plots its density and the distribution of cell sizes. Figure C8 shows that exposure predicts the establishment's own 2008–09 employment decline, with a slope of 0.65 in a binned scatter, so that the leave-one-out cell mean is informative about the shock the establishment itself faced, and that it is essentially unrelated to the cohort-difference treatment z_j: a one-standard-deviation increase in z_j moves exposure by 0.002 standard deviations. Table C4 reports the corresponding regressions and adds the 2008 establishment characteristics used in Table C1. Exposure is not balanced on these, nor does the design require it to be: establishments with fewer women were considerably more exposed, which reflects the concentration of the crisis in manufacturing, whereas establishment size, wages, and the age structure are only weakly related to exposure. What the design requires is that exposure be unconditionally unrelated to z_j, which the first column confirms, and that the differential response to z_j not be driven by characteristics correlated with exposure, which cannot be tested directly and is discussed in the main text.

Table C3. Crisis Exposure: Summary Statistics

	N	Mean	SD	P10	P50	P90
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All establishments with more than 5 employees						
Crisis exposure (employment-weighted)	869,724	0.029	0.042	−0.014	0.028	0.071
Crisis exposure (unweighted)	869,724	0.032	0.037	−0.008	0.030	0.073
Own employment decline 2008–09	869,724	0.106	0.458	−0.200	0.000	0.391
Other establishments in cell	869,724	1,923	2,504	118	920	5,169
Panel B: Estimation sample						
Crisis exposure (employment-weighted)	309,216	0.031	0.046	−0.018	0.030	0.078
Crisis exposure (unweighted)	309,216	0.034	0.041	−0.012	0.033	0.083
Own employment decline 2008–09	309,216	0.083	0.398	−0.154	0.000	0.270
Other establishments in cell	309,216	1,590	2,265	97	735	4,125

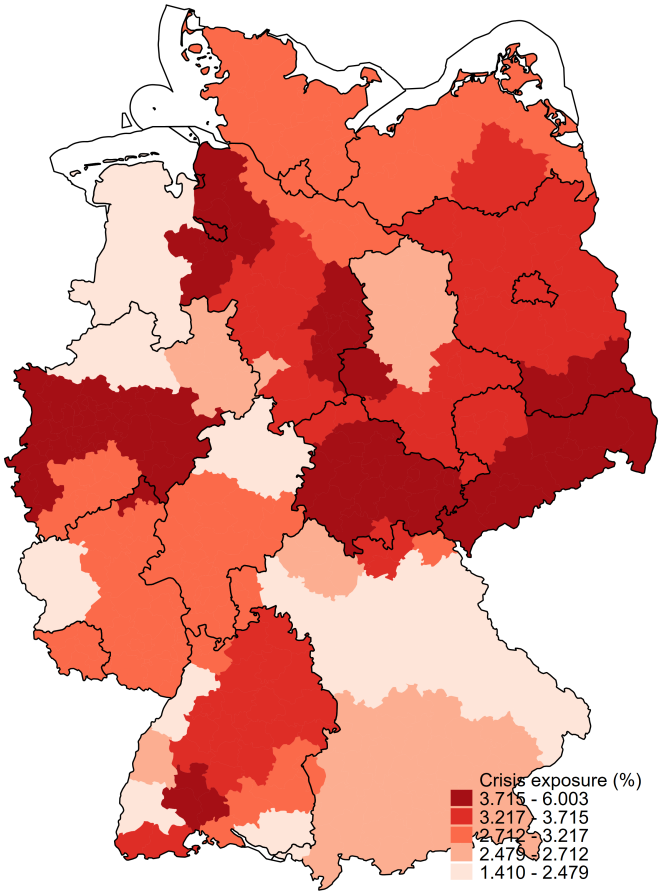
Notes: Panel A covers all establishments outside employment activities (WZ 78) with more than five employees on 30 June 2008 in the Establishment History Panel with nonmissing exposure; Panel B the subset in the estimation sample of Section 4, establishments with 5 to 2,000 employees and at least one employee born in 1954 or 1955. Crisis exposure is the leave-one-out mean of the sign-reversed symmetric employment growth rate between June 2008 and June 2009 among other establishments in the same commuting zone and two-digit industry that are still observed in 2009, weighted by 2008 employment (first row) or unweighted (second row); positive values indicate a decline. Own employment decline is the establishment's own sign-reversed symmetric growth rate over the same period. Other establishments in cell is the number of establishments over which the leave-one-out mean is taken. Columns (4)–(6) report the 10th, 50th, and 90th percentiles.

Figure C4. Crisis Exposure by Industry



Notes: The figure reports the average crisis exposure of establishments in the estimation sample by two-digit industry (WZ 2008), in percent. Crisis exposure is the leave-one-out, employment-weighted mean employment decline between June 2008 and June 2009 among other establishments in the same commuting zone and two-digit industry that are still observed in 2009. Employment activities (WZ 78) are excluded. Blue bars mark manufacturing (WZ 10 to 33). The dashed line marks the average across all establishments. Industries with fewer than 20 establishments are omitted.

Figure C5. Crisis Exposure by Labor Market Region



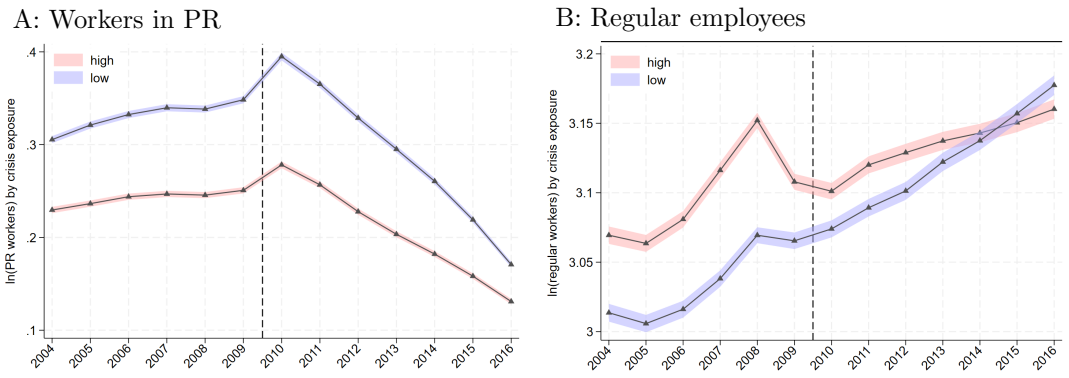
Notes: The map reports the average crisis exposure (in quintiles) of establishments in the estimation sample by labor market region (*Arbeitsmarktregion*), in percent. Crisis exposure is defined as in Figure C4. Districts are shaded with the value of their labor market region, and black lines mark state borders.

Table C4. Crisis Exposure and Baseline Establishment Characteristics

	Dependent variable: crisis exposure (standardized)		
	(1)	(2)	(3)
Cohort difference z_j	−0.002		−0.001
	(0.002)		(0.002)
Log employment		0.007***	0.007***
		(0.002)	(0.002)
Share in partial retirement		−0.042***	−0.042***
		(0.002)	(0.002)
Share aged 55 and older		0.024***	0.024***
		(0.002)	(0.002)
Share aged 50–59		−0.031***	−0.031***
		(0.002)	(0.002)
Share female		−0.339***	−0.339***
		(0.002)	(0.002)
Share medium-qualified		0.031***	0.031***
		(0.002)	(0.002)
Mean full-time daily wage		−0.006***	−0.006***
		(0.002)	(0.002)
Observations	309,216	303,589	303,589

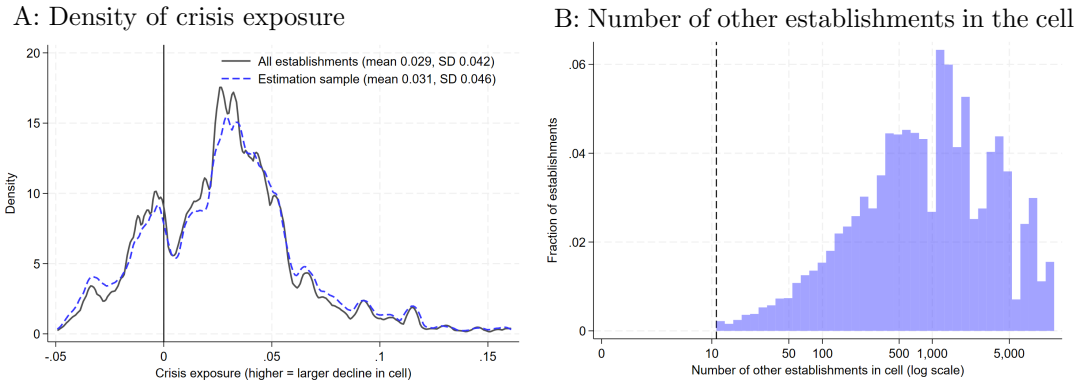
Notes: The table reports cross-sectional regressions of standardized crisis exposure on the standardized relative cohort difference z_j (Column 1), on standardized 2008 establishment characteristics (Column 2), and on both (Column 3), in the estimation sample of Section 4. All variables are standardized to unit variance, so that each coefficient is the change in exposure, in standard deviations, per standard deviation of the regressor. Shares are counts of the respective group divided by total establishment employment in 2008. Columns (2) and (3) lose establishments with a missing mean full-time wage. Heteroskedasticity-robust standard errors in parentheses. Asterisks denote statistical significance at the 10 percent (*), 5 percent (**), and 1 percent (***) levels.

Figure C6. PR Workers and Regular Employees by Crisis Exposure



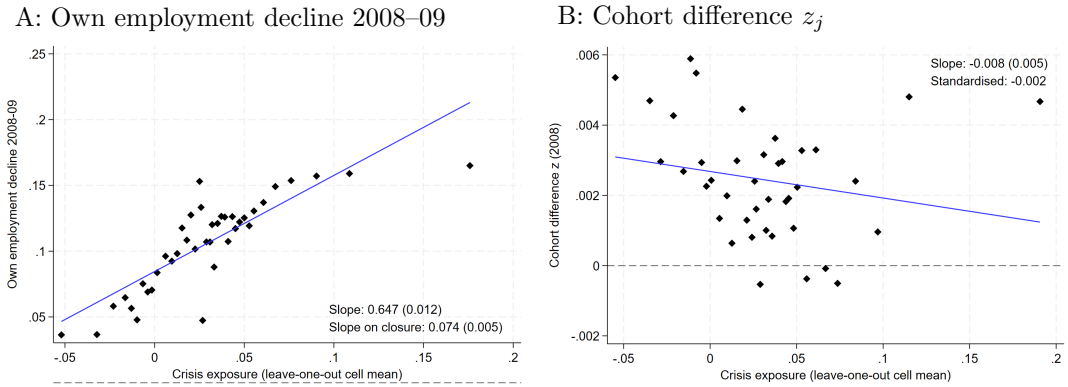
Notes: The panels plot the yearly mean of the log of one plus the number of workers in PR (Panel A) and of regular employees (Panel B) per establishment, separately for establishments with crisis exposure above (high) and below (low) the median. The sample comprises establishments with 5 to 2,000 employees in the Establishment History Panel that are observed in every year from 2008 to 2016 and have nonmissing exposure. The dashed vertical line marks the expiration of reimbursement. Shaded areas denote 95% confidence intervals based on standard errors clustered at the establishment level.

Figure C7. Crisis Exposure: Distribution



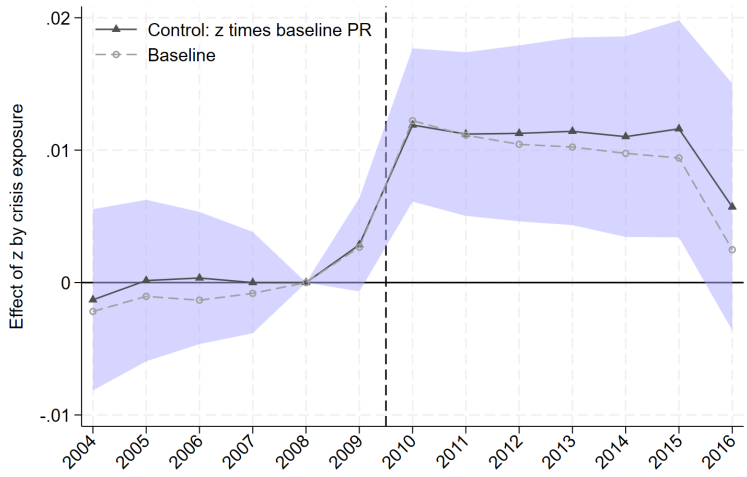
Notes: Panel A plots the kernel density of the employment-weighted leave-one-out crisis exposure measure for all establishments with more than five employees in 2008 (solid) and for the estimation sample (dashed), trimmed at the 1st and 99th percentiles; means and standard deviations are reported in the legend. The measure takes one value per commuting-zone-by-industry cell, up to the leave-one-out adjustment, so the density is a smoothed distribution of cell values weighted by the number of establishments. Panel B plots the distribution of the number of other establishments in the cell that are still observed in 2009, on a log scale, for establishments with nonmissing exposure; the dashed line marks the minimum of ten other establishments below which exposure is set to missing.

Figure C8. Crisis Exposure: Relation to Own Employment Change and to the Cohort-Difference Treatment



Notes: Both panels are binned scatters with 40 equal-sized bins of crisis exposure and a linear fit weighted by the number of establishments per bin. Panel A plots the establishment's own sign-reversed symmetric employment growth rate between June 2008 and June 2009 against exposure for all establishments with more than five employees in 2008. Panel B plots the relative cohort difference z_j from eq. (4) against exposure in the estimation sample. The legends report the slope of the underlying establishment-level regression with heteroskedasticity-robust standard errors in parentheses and, in Panel B, the standardized slope in standard deviations of z_j per standard deviation of exposure.

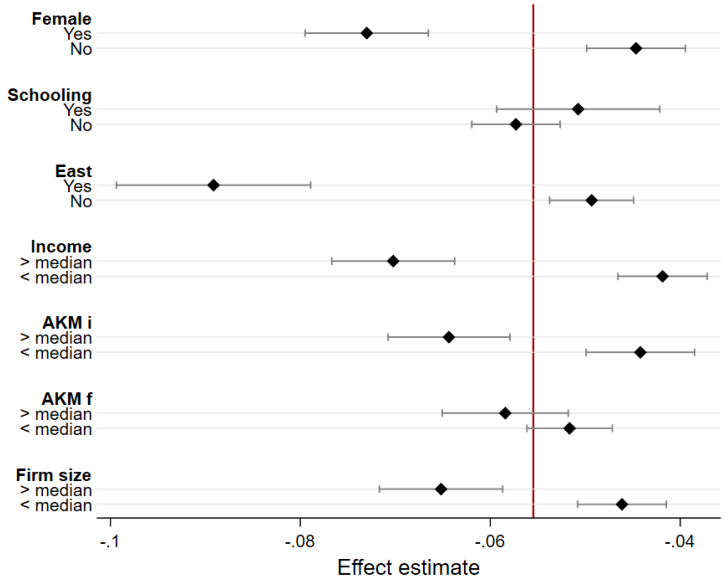
Figure C9. Triple Difference Controlling for Past PR Use



Notes: The solid line reports the coefficients γ_τ on the triple interaction of z_j, standardized crisis exposure, and year indicators from eq. (7), with additional interactions of the year indicators with past PR use and with the product of past PR use and z_j. Past PR use is the establishment's average share of workers in PR between 2004 and 2008, standardized across establishments. The dashed line reports the baseline estimates of Figure 9, Panel B, on the same sample. The outcome is the log of one plus the number of workers in PR, with 2008 as the reference year, and all specifications include establishment and year fixed effects. The dashed vertical line marks the expiration of reimbursement. Shaded areas denote 95% confidence intervals based on standard errors clustered at the establishment level.

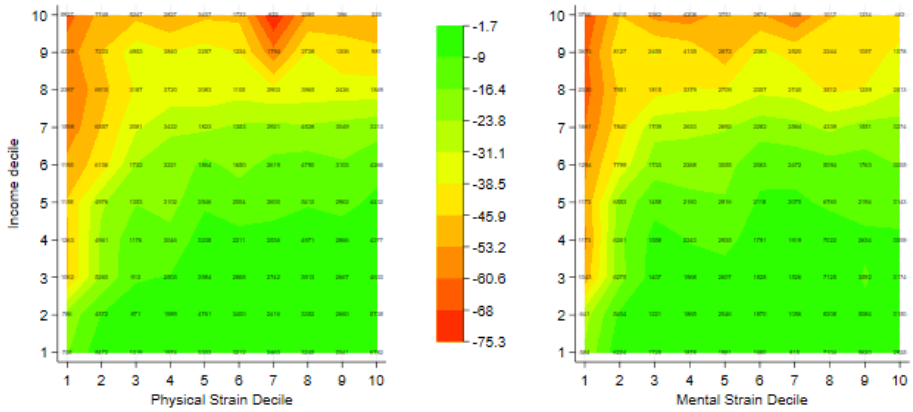
C.3 Selection into PR and Terms of the Agreements

Figure C10. Heterogeneous Local DiD Effects on Lifetime PR Uptake



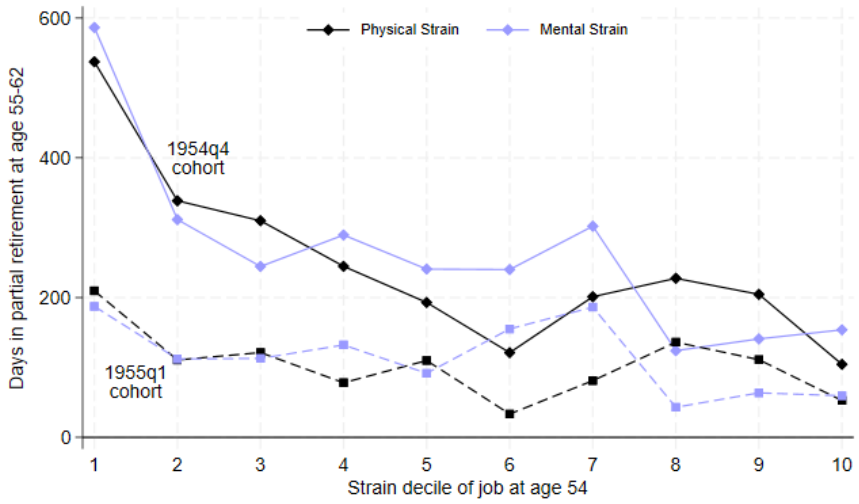
Notes: The figure reports local DiD estimates from eq. (1), using a four-month bandwidth, the IV sample, and an indicator for ever entering PR (lifetime uptake) as the outcome. Estimates are shown separately for the two groups defined by each age-54 characteristic listed on the vertical axis. The red vertical line is the full-sample estimate. Whiskers denote 95% confidence intervals based on standard errors clustered at the individual level.

Figure C11. Causal Forest Results on Cumulative Days in Partial Retirement



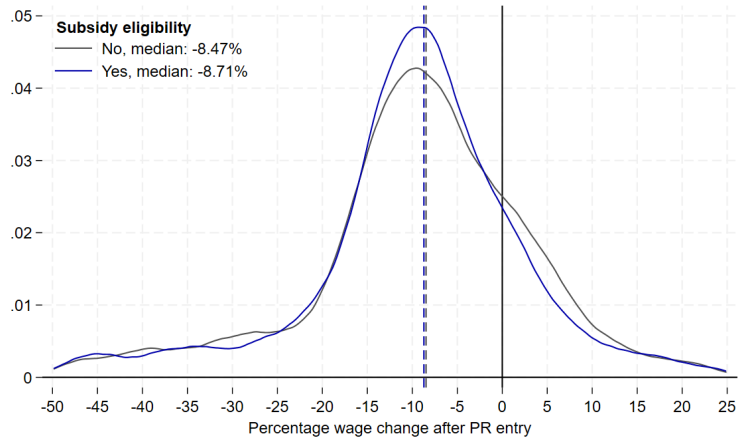
Notes: The figure reports conditional average reduced-form effects of reimbursement expiration estimated with a causal forest (Wager and Athey, 2018). The outcome is cumulative days in PR between ages 55 and 67, and effect heterogeneity is estimated jointly over age-54 wage and the physical- and mental-strain indices of the age-54 main job. The left and right panels summarize predictions by wage and physical-strain deciles and by wage and mental-strain deciles, respectively. The job-strain measures follow Kroll (2015), vary at the five-digit KldB-2010 occupation level, and increase with occupational strain. Colors indicate estimated effects in days; numbers within cells report cell sample sizes.

Figure C12. Uptake of Partial Retirement by Strain Decile of Individuals' Baseline (Age=54) Job



Notes: The figure reports mean lifetime days in PR by birth cohort and decile of job strain in the individual's main employment at age 54.

Figure C13. Percentage Change in IEB-Reported Earnings upon Entering PR



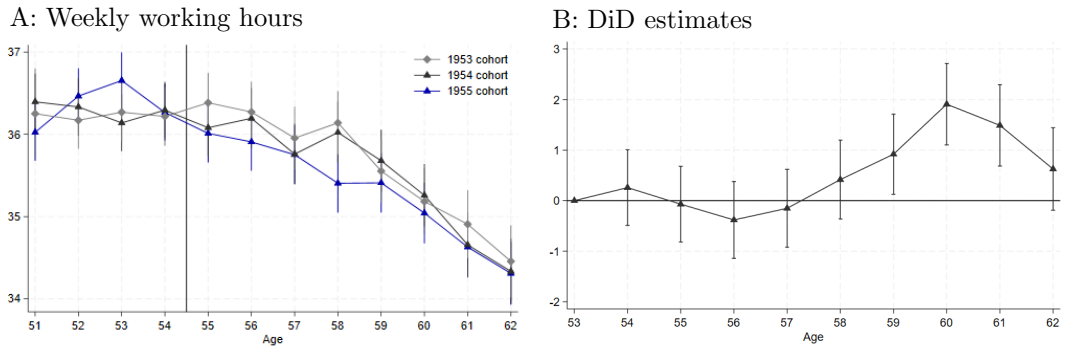
Notes: The gray distribution shows the percentage change in daily IEB-reported earnings from the year before to the year after PR entry for ineligible members of the 1955 cohort who enter PR at age 55 or 56. The blue distribution shows the corresponding change for a 1:1 matched group (without replacement) of reimbursement-eligible workers who enter PR at the same age. The median changes are −8.47 and −8.71 percent, respectively. The matching is exact on sex, residence in eastern or western Germany, full employment during ages 50–54, and one-digit occupation. Among potential matches, the nearest neighbor is selected using a propensity score based on the remaining covariates listed in Table 2. The top and bottom 1 percent of the distribution of changes are omitted. Before PR entry, reported earnings generally measure contribution-liable gross remuneration. During PR, they instead record the pension-contribution base $R + F$ and exclude the cash top-up A. The figure therefore does not show changes in cash earnings or hourly wages; in the uncapped standard case, the reporting rule mechanically implies a value of approximately $0.9W$ during PR.

D Additional Material from Microcensus Data

The Microcensus is the largest household survey in Germany. Each year, it interviews about 1 percent of the population, almost 800,000 persons living in private households or in collective accommodations such as nursing homes, and participation is mandatory for all members of a selected household. Its labor market questions follow the EU Labour Force Survey and cover employment status, usual and actual weekly working hours, occupation, and whether a respondent currently works reduced hours because of PR. Unlike the IEB, the Microcensus therefore measures working hours and identifies PR participants from their own reports. It also records monthly net income, which contains the tax-exempt top-up that employers pay during PR, as well as residence in a nursing home and the receipt of long-term-care benefits. Net income is reported in brackets, and Table D1 uses the bracket midpoints. I access the data at the Research Data Centre of the Statistical Offices of the Federation and the Länder, which provides respondents' month of birth.

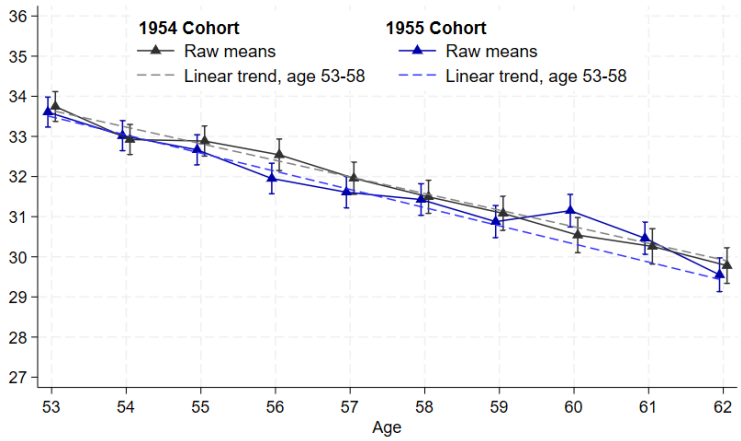
Two features limit the Microcensus to supplementary evidence. First, it is a sample rather than a register. Each cohort-by-age cell contains only a limited number of respondents (Figure D5), and the cells around a birth-month cutoff are smaller still, so estimates are far less precise than those based on the IEB. Second, all information is self-reported. Reassuringly, the self-reported PR status reproduces the pattern in the administrative data. The share of employed respondents who report a PR-related hours reduction rises from age 55 onward and is lower for the 1955 cohort than for the 1954 cohort at every age (Figure D6), mirroring the uptake gap in the IEB. Table D1 uses the waves from 2008 to 2019 and thus avoids the redesign of the survey in 2020.

Figure D1. Average Working Hours by Age and Cohort



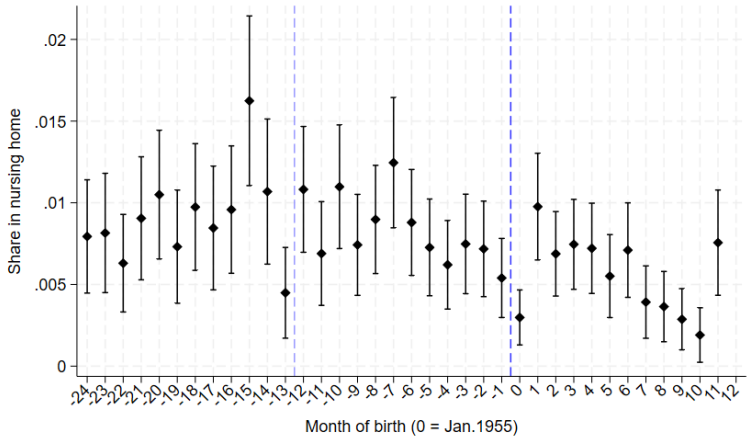
Notes: Panel A plots mean weekly working hours by age for employed Microcensus respondents of the 1954 and 1955 cohorts. Dashed lines denote cohort-specific linear trends fitted over ages 53 to 58. Panel B reports DiD estimates of the difference in weekly working hours between the 1955 and the 1954 cohort at each age, relative to age 53. Whiskers denote 95% confidence intervals based on standard errors clustered at the individual level.

Figure D2. Average Working Hours of Employees Without PR



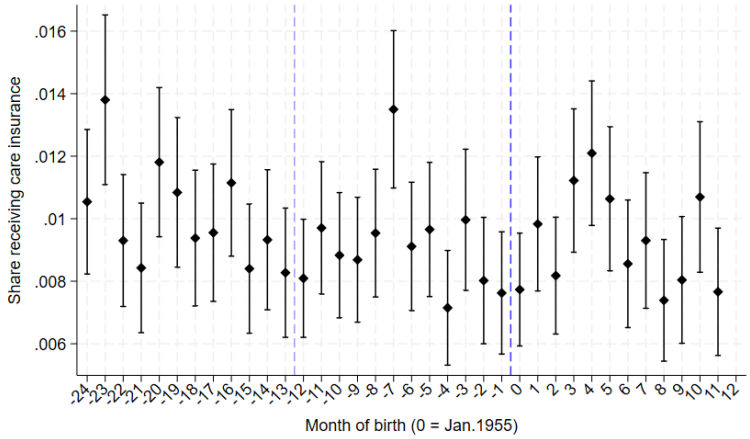
Notes: The figure replicates Panel A of Figure D1, excluding respondents who report working reduced hours because of PR. It plots mean weekly working hours by age for employed Microcensus respondents of the 1954 and 1955 cohorts. Dashed lines denote cohort-specific linear trends fitted over ages 53 to 58. Whiskers denote 95% confidence intervals based on standard errors clustered at the individual level.

Figure D3. Nursing-Home Residence by Month of Birth



Notes: The figure plots the share of Microcensus respondents living in a nursing home by recentered month of birth. The vertical lines mark the January 1955 and January 1954 cutoffs. Whiskers denote 95% confidence intervals. The figure reports raw means.

Figure D4. Long-Term-Care Benefit Receipt by Month of Birth



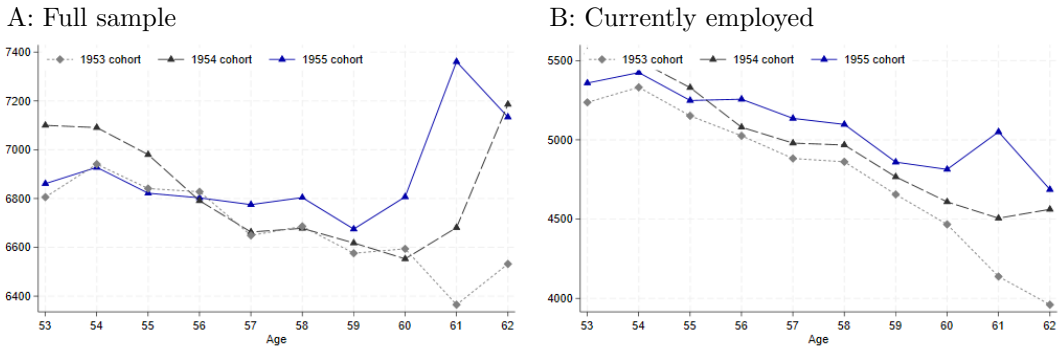
Notes: The figure plots the share of Microcensus respondents receiving long-term-care benefits by recentered month of birth. The vertical lines mark the January 1955 and January 1954 cutoffs. Whiskers denote 95% confidence intervals. The figure reports raw means.

Table D1. Net Earnings of Partial Retirees in Reimbursement-Eligible and Ineligible Cohorts

	Net monthly earnings		Net monthly earnings (deflated)	
	(1)	(2)	(3)	(4)
Born in 1955–56	205.05***	163.98***	162.42***	120.53***
	(37.16)	(35.58)	(38.84)	(37.22)
Age and sex FE	Yes	Yes	Yes	Yes
2-digit occupation FE	No	Yes	No	Yes
Working hours	No	Yes	No	Yes
Observations	3,646	3,646	3,646	3,646
R^2	0.16	0.29	0.16	0.29
Outcome mean	2,164.98	2,164.98	2,283.65	2,283.65

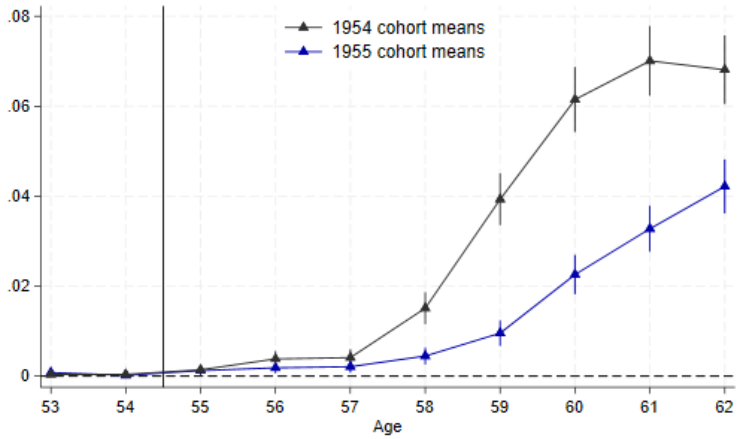
Notes: The table reports OLS estimates from the Microcensus, waves 2008–2019, for respondents of the 1953–1956 birth cohorts observed at ages 55–62 who report working reduced hours because of PR. The outcome is current net monthly earnings, in nominal euros in Columns (1) and (2) and deflated to 2020 euros in Columns (3) and (4). The regressor of interest is an indicator for membership in the 1955 or 1956 cohorts, which could not begin a reimbursable PR arrangement; the omitted category is the reimbursement-eligible 1953 and 1954 cohorts. All specifications include age and sex fixed effects; Columns (2) and (4) add two-digit occupation fixed effects and current weekly working hours. The cohorts are compared without the birth-date design used elsewhere in the paper. Standard errors are clustered at the individual level. Asterisks denote statistical significance at the 10 percent (*), 5 percent (**), and 1 percent (***) levels.

Figure D5. Sample Size per Age and Cohort



Notes: The figure reports the number of Microcensus observations by age for the 1954 and 1955 cohorts. Panel A covers all respondents, Panel B respondents in any form of employment.

Figure D6. Worker Share with Hour Reduction due to Partial Retirement



Notes: The figure plots, by age, the share of employed Microcensus respondents of the 1954 and 1955 cohorts who report currently working reduced hours because of partial retirement. Whiskers denote 95% confidence intervals based on standard errors clustered at the individual level.