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Closing the Gender Pension Gap - Labor Supply and Welfare Effects of Progressive Pensions*

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Abstract

Progressive pension supplements mechanically reduce the gender pension gap because women receive lower pensions on average. However, labor supply responses may offset these gains and create fiscal externalities. Using German administrative data, we analyze the distributional and welfare effects of a progressive pension supplement. We find no labor supply responses during working life, but small effects on women's employment and earnings in retirement. The supplement reduces the gender income gap by about two percent, with sizable heterogeneity across pension levels. The marginal value of public funds is 0.83. Means-testing based on household income improves welfare even with low redistributive preferences.

Keywords: gender, pensions, retirement, labor supply, redistribution

JEL: H55, H23, J16, J26, J22

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1 Introduction

Across the OECD, women's pensions are about a quarter lower than men's (OECD, 2025b). This gap is mainly explained by differences during working life. Shorter contribution histories, lower earnings, and longer career interruptions of women, largely related to care work, translate directly into lower pension entitlements in systems that link pension benefits to lifetime contributions. However, pensions are not the only source of income in old age. A considerable share of retirees continues to work, and there is again a clear gender gap. At ages 65-69, the average employment rate is 23.7% for women and 36% for men over all OECD countries (OECD, 2025a). The gender gap in income during retirement is therefore the combination of the gap in pension income and the difference in labor earnings during retirement.

In response to the gender pension gap, most countries have introduced pension reforms aiming to increase women's pension income and to narrow the income gap during retirement. In general, explicit gender-based pension rules are legally difficult to implement. The pension regulations are therefore formally gender neutral, but since they are tied to earnings, contribution and care histories, they are in practice strongly targeted at women. Examples are care-related pension credits, which compensate for career interruptions for child care or long-term care. In principle, men are also eligible for care-related pension credits, but since care is provided overwhelmingly by women, the implications of these instruments strongly vary by gender (Haan et al., 2025; OECD, 2025b).

Progressive elements in pension systems are also not gender neutral and have the potential to reduce gender gaps during retirement. Since women's pension entitlements are on average lower, women benefit more than men from a progressive system. Countries incorporate progressive elements into pension systems in different ways: through tax-funded or residence-based basic pensions (such as Australia's means-tested Age Pension and the Netherlands' residence-based AOW); income-tested top-ups or supplements (such as the UK's Pension Credit, Germany's supplement for people with long careers and low earnings, and Sweden's guarantee pension); or progressive formulas within earnings-related pensions (as in the United States and Switzerland).[1]

While progressive elements raise female pensions directly, they can also induce offsetting behavioral employment and earnings effects both during the working life and during the retirement phase, with negative implications for income during retirement. Reduced employment and earnings matter not only for individual income, but they can also induce fiscal externalities which are relevant for the welfare effects of a reform. The larger the negative earnings effect, the higher the fiscal externality and the lower the marginal value of public funds (MVPF) (Hendren, 2016; Hendren & Sprung-Keyser, 2020).

Therefore, assessing the effects of progressive pension policies on gender income gaps during

[1] For more details and other examples, see OECD (2025b).

retirement and the welfare implications of these policies requires quantifying labor market responses along different behavioral margins. First, employment before retirement, which includes transitions into non-employment and retirement claiming, might be reduced. Second, an increase in pension income due to more progressivity might crowd out employment and earnings during retirement.

In this paper, we focus on the implications of a progressive pension supplement targeted at pensioners with low pension entitlements and low household income. We analyze the German *Grundrente*, a pension supplement introduced in 2021. Individual eligibility depends on two conditions. The first condition is a contribution record of at least 33 years, with full entitlement after 35 years. Qualifying periods also include months credited for child-rearing and long-term care, which allows many women with interrupted careers to reach the required length. The second condition is low average lifetime earnings. This is the main reason why over three quarters of the eligible individuals are women. Among individuals who reach the required contribution length, women are much more likely than men to have persistently low earnings. This is in line with the intention of the lawmakers, who explicitly had women in mind when designing the reform.[2] In addition to the individual eligibility criteria, the receipt of the pension supplement is conditional on a test of household income which targets the supplement to pensioners in low-income households.

For the empirical analysis, we use administrative microdata from the German Pension Insurance covering the universe of retirees in Germany. We show that the pension supplement is quantitatively important. At the end of 2024, about 1.4 million pensioners received a supplement. This corresponds to about 5.4% of all statutory pensions. The average amount of the supplement was €102 per month for women and €82 for men.

To analyze behavioral responses induced by the supplement, we use two different methods. First, we test for take-up effects, quantifying whether individuals select into eligibility by manipulating contribution years before entering retirement via changes in non-employment or retirement timing. Because eligibility is governed by sharp cutoffs at 33 and 35 contribution years, manipulation would show up as excess bunching in the density of contribution years just above these thresholds. We implement the local polynomial density test of Cattaneo et al. (2020) separately for pre-reform and post-reform periods. Second, we use a dynamic difference-in-differences design (DiD) to quantify employment and earnings responses during retirement. We focus on individuals who were already retired prior to the reform and who became eligible for the pension supplement unexpectedly in 2021 (the treatment group), and compare them to individuals from the same birth cohorts who were also retired before the reform but never became eligible (the control group). We exploit the rich administrative data and

[2]The official justification of the bill states that women perform most of the care work and therefore often receive lower pensions. The government expected about 70 percent of the beneficiaries to be women (Deutscher Bundestag, 2020). The justification refers to the Second Gender Equality Report of the Federal Government, which uses the gender pension gap as a central indicator (Deutscher Bundestag, 2017). The law also requires an evaluation of whether the reform takes the employment biographies of women into account.

employ coarsened exact matching based on pre-reform observable characteristics, including full earnings and employment history, to construct the treatment and control group. Identification then relies on within-individual changes in pension income over time.

Despite the sizable financial incentives, we find no evidence of take-up effects, neither for women nor for men. The density of the contribution periods is smooth in the pre-reform and post-reform periods and shows no significant bunching at the relevant cutoff years. These findings are robust to different functional-form specifications. Thus, we can rule out significant behavioral responses before retirement entry. In contrast, based on the DiD estimator, we find significant albeit small effects on employment and earnings during retirement for women. Female employment falls by 1.46 percentage points, a response concentrated almost entirely in marginal employment. The earnings effects are small. Annual labor earnings decline by 58 euros, which, given the low pre-reform mean, corresponds to an earnings elasticity of -1.33, alongside a marginal propensity to earn out of pension income of -0.13. Thus, women give up about 13 cents of labor earnings for every additional euro of pension income. For men, all effects are insignificant. Men do not reduce employment or earnings due to the reform.

We find important regional differences between East and West Germany which are related to historically differences and the higher labor market attachment and earnings over the working life of women in the East. As a result, the gender pension gap is 40.7 percent in West Germany but only 9.2 percent in East Germany. In contrast, the gender gap in labor earnings during retirement alone is almost identical across regions, at 39.3 percent in the West and 39.2 percent in the East. The behavioral responses to the reform are stronger for West German women. Employment falls by 1.52 percentage points and annual earnings by 66 euros, both statistically significant, giving a marginal propensity to earn of -0.14. In East Germany, the corresponding estimates are 1.08 percentage points and seven euros, and neither is statistically significant.

In the final part of the analysis, we combine the mechanical and the behavioral effects to assess the overall effect of the reform on the gender gaps during retirement and to discuss the implications for welfare. Overall, the pension supplement reduces the gender gap in combined income, pensions plus labor earnings, in 2024 from 34.3 to 33.6 percent, closing 1.91% of the gap. We decompose the mechanical and the opposing behavioral effect and show that the changes in female earnings only have a small effect on the gender gap. Net of behavioral responses, the gender income gap would have been reduced by 2.11%. Thus, the negative earnings response reduces only 9.6% of the redistribution towards women. Results strongly vary over the distribution of pension income. Since women at the bottom of the distribution are often not eligible for the pension supplement, we find the largest reductions of 5.7% in the middle of the pension distribution. In contrast to the large differences in the gender pension gap between Eastern and Western Germany, we find that the reduction of the gender income gap is comparable between the regions, at 0.64 and 0.74 percentage points. Thus, the supplement closes 5.7% of the East German gender income gap but only 1.6% of the gap in the West.

According to our estimates, the fiscal externalities of the pension supplement induced by negative earnings responses during retirement are small, amounting to three cents per euro transferred, while the administrative costs of implementing the income test are sizable at roughly 18 percent of benefits paid. These results imply a MVPF of the supplement of 0.83. Once we allow the social marginal utility of income to be higher for poorer households, the social welfare of a euro spent on the supplement exceeds one even at low degrees of redistributive preference, because recipients of the supplement live in households with substantially lower equivalized income than pensioners who are not eligible. Finally, we discuss the importance of the income test of the pension supplement for the welfare effect. A generalized supplement without an income test is cheaper to administer and therefore has the higher MVPF, 0.96 against 0.83, but the generalized design extends the supplement to pensioners whose household incomes are not lower than those of pensioners who are not eligible at all. The income test thus raises the administrative cost of delivering an euro to recipients, yet concentrates those euros where the social marginal utility of income is higher. Thus, we show that the income test reduces not only the fiscal costs, as the number of beneficiaries is lower, but also leads to higher welfare if society has redistributive preferences.

Our study contributes to a growing literature on gender pension gaps. Most papers are descriptive and focus on the sources of the gender pension gap rather than policies trying to reduce it through changes in the pension system. Decomposition studies show that much of the gender pension gap can be traced to gender differences in employment histories, contribution years, and earnings over the life course (Bonnet et al., 2022; Even & Macpherson, 2004; Joubert & Todd, 2024; Kreyenfeld et al., 2018; Kuivalainen et al., 2020), though evidence for Britain suggests that differences in returns to these characteristics matter as much as the characteristics themselves (Bardasi & Jenkins, 2010). More recent work emphasizes the role of motherhood, showing that women's pensions fall systematically with the number of children they have raised, and that existing care credits close only a limited part of this motherhood pension gap (Haan et al., 2025). Related studies document how life course events such as divorce, part-time work, and reduced full-time attachment shape the gender gap (Kreyenfeld et al., 2023; Niessen-Ruenzi & Schneider, 2022; Rowold, 2026).

Furthermore, our study relates to a large body of literature examining employment and retirement decisions of older workers. Numerous studies exploit changes and discontinuities in pension systems to analyze the effects of pension reforms on employment and retirement transitions, yet not with a focus on gender gaps during retirement (see, e.g., Atalay & Barrett, 2015; Behaghel & Blau, 2012; Coile & Gruber, 2007; Cribb et al., 2016; Engels et al., 2017; Geyer & Welteke, 2021; Giesecke & Jäger, 2021; Hernæs et al., 2016; Manoli & Weber, 2016a; Manoli & Weber, 2016b; Morris, 2022; Rabaté & Tréguier, 2024; Seibold, 2021; Staubli & Zweimüller, 2013). Several studies focus explicitly on the behavioral effects of care-related pension benefits. Examples are Becker et al. (2023) and Artmann et al. (in press). These studies for Germany document a forward-looking reduction in labor supply during the working

life in response to an anticipated increase in pension wealth. The employment responses are likely related to specific features of the reform which were highly salient, and the financial implications were very transparent. Eligible mothers could easily infer the magnitude of the pension gain, since it depended in a mechanical and well-understood way on the number of children they have. In contrast, forward-looking behavioral responses are likely to be much more limited in the context of the pension supplement. As discussed in Section 2, the reform involves a comparatively complex eligibility and calculation procedure, and the resulting pension supplement is not directly communicated in a simple or easily predictable manner. As a consequence, individuals may have only an imperfect understanding of the policy-induced pension increase before entering retirement, reducing the scope for anticipatory labor supply adjustments prior to benefit receipt. A small literature also analyzes behavioral effects of previous pension supplements but not with a focus on gender gaps during retirement. Ye (2022) and Malavasi and Ye (2024) study an earlier pension subsidy for low-income earners that also predominantly benefited women and find that it advanced retirement timing and reduced mortality. In ongoing work, Kindermann et al. (2026) also exploit the introduction of the Grundrente in Germany and, similar to our study, they find a marginal propensity to earn out of pension income of around -0.10, driven by extensive margin exit. Their analysis focuses on the aggregate post-retirement labor supply margin. We complement their work along three dimensions. First, we extend the analysis beyond the post-retirement margin and also analyze pension take-up related to pre-retirement behavior. Second, we shift the focus to the gender dimension of the reform, since the vast majority of eligible individuals are women. Third, we derive the associated welfare consequences through the MVPF framework and highlight the importance of the income test.

The remainder of the paper is organized as follows. Section 2 provides institutional background on the German pension system and details the design of the reform. Section 3 describes the administrative data from the German Pension Insurance and outlines the construction of the main outcome variables. Section 4 presents descriptive statistics on the pension supplement and first-order effects on the gender pension gap. Section 5 presents our empirical strategy and results separately for each of the behavioral margins, take-up during the working life and employment and earnings after retirement, pairing each identification strategy with its corresponding findings. Section 6 evaluates the welfare implications of the reform using the MVPF framework. Section 7 concludes.

2 Institutional Background

The statutory pension system. The statutory public pension system is the central pillar of old-age income provision in Germany. It covers the large majority of dependent employees, excluding civil servants and most self-employed individuals. The system is financed on a pay-

as-you-go basis. In addition to old-age pensions, it provides disability and survivors' benefits.[3]

In principle, pension benefits are proportional to contributions. Benefits are calculated with a point system which reflects earnings over the entire working life. In each calendar year an insured individual accrues earnings points equal to the ratio of her annual earnings, capped at a contribution ceiling, to average earnings in that year (see, e.g., Börsch-Supan & Wilke, 2004; OECD, 2021). At claiming, the accumulated earnings points are multiplied by the current pension value, which gives the monthly pension. Periods without employment can also generate entitlements. The most important are child-rearing periods, for which one parent, typically the mother, receives pension points for raising children, with the total depending on the number of children. For cohorts born before 1947, the statutory retirement age was 65. It is being raised to 67 for cohorts born in 1964 or later. Appendix A.1 describes contribution rules and the pathways for early retirement in more detail.

Pensioners can work in regular employment or marginal employment while drawing a pension.[4] In contrast to regular employment, earnings from marginal employment (*mini-job*) are taxed at a flat rate paid by the employer and are exempt from employee social insurance contributions.

The pension supplement. The pension supplement was introduced in January 2021.[5] It increases individual entitlements through an income-tested supplement that is a function of contribution length and lifetime earnings. Eligibility is conditional on two criteria. First, the supplement requires at least 33 years, or 396 months, of qualifying periods (*Grundrentenzeiten*). These comprise compulsory contributions from employment and self-employment, together with credited periods for child-rearing, care, sickness and rehabilitation, irrespective of the income earned. Months of marginal employment for which no pension contributions are paid do not count. Full entitlement is reached at 35 years, or 420 months. Between the two thresholds the supplement increases, for more details, see Appendix A.2.[6]

Second, eligibility also depends on an individual's average lifetime earnings position, measured in pension points per month. Only months with earnings above a minimum threshold of 0.025 pension points, equivalent to 30 percent of average earnings or roughly €1,177 per month in 2024, count as assessment periods (*Grundrentenbewertungszeiten*). Then, the supplement is restricted to individuals whose average monthly pension points in these assessment periods remain below a ceiling that depends on the number of qualifying years. With exactly 33 years of qualifying periods, the maximum allowable average is about 0.033 pension points per month.

[3]Our analysis is restricted to old-age pensions. Disability and survivors' pensions do not enter any of our samples. The supplement is paid in all three pension types, but old-age pensions account for roughly 87 percent of all supplements.

[4]Above the statutory retirement age (SRA), no earnings limit applies. The limits that applied below the SRA are described in Appendix A.3 and never bind in our samples.

[5]Newly retiring individuals began receiving the supplement from July 2021 onward, while individuals who were already retired before that date had their entitlements reviewed retroactively and received their first supplement payments during 2022. The supplement was fully phased in for all eligible pensioners only by 2023 but paid retroactively.

[6]The legal provisions are set out in §76g SGB VI. A comprehensive discussion of the legislative framework is provided by Dünn et al. (2020).

This ceiling increases linearly with each additional qualifying month and reaches approximately 0.067 pension points per month once 35 years are completed. Expressed on an annual basis, this corresponds to an increase from roughly 0.40 to 0.80 pension points per year.[7]

The exact amount of the supplement depends on the retiree's average pension points during the basic pension assessment periods, the total number of months with basic pension periods, and the number of months assessed.[8] Individuals with long contribution histories and low average earnings receive the largest supplements, while those whose earnings exceed the ceiling receive no top-up. The maximum supplement is 12.25 pension points, which, for 2024, corresponds to a supplement to the gross pension of 481.67 euros.

The income test. In addition to the individual eligibility criteria, the pension supplement is subject to an income test at the household level. The assessment basis comprises taxable income, tax-exempt pension and benefit income, and capital income above the saver's allowance.[9] Separate thresholds apply to single persons and to married couples, at €1,375 and €2,144 per month respectively in 2024. Income above the threshold is credited against the supplement at a rate of 60%, and income above €1,759 for singles or €2,529 for couples at 100%. Taxable income is assessed by the tax authorities with a delay of up to three years. Therefore, the income test does not refer to income from the current period, but from previous years. Importantly, the pension supplement does not enter the assessment basis. Therefore, in the empirical analysis we can interpret the pension supplement as an income effect during the retirement period that does not affect working incentives via changes in the marginal tax rate.

The pension supplement in 2024. By the end of 2024 approximately 1.4 million, or 5.4% of all statutory pensioners, received a pension supplement (Buslei et al., 2025). Receipt is heavily concentrated among women, who account for 76.4% of recipients and who receive a larger supplement conditional on receipt, €102 per month on average against €82 for men. The high female share follows directly from the eligibility rule, since qualifying periods include child-rearing and caregiving, and since eligibility also requires low lifetime earnings.

The income test binds for a large share of pensioners. While roughly 2.75 million pensions satisfy the individual eligibility criteria, only about half receive a payment after the income test at the household level. More women are affected by the income test since they are more likely to have a high-income partner. Specifically, 48.8% of women who fulfill the individual criteria receive the supplement. For men, the share is considerably higher, at 59.3%.

[7] The average earnings ceiling of 0.033 pension points per month (0.40 EP per year) translates to roughly €1,570 per month in 2024. For 35 or more completed qualifying years, the maximum allowable average of 0.067 pension points per month (0.80 EP per year) corresponds to gross labor market earnings of approximately €3,139 per month.

[8] For a detailed analysis of the calculation, see Appendix A.2.

[9] § 97a(2) SGB VI in conjunction with § 2 EStG; see Appendix A.2.4.

3 Data

We use administrative microdata from the German Pension Insurance, accessed through its Research Data Center (Forschungsdatenzentrum der Rentenversicherung [FDZ-RV], 2026a, 2026c, 2026d, 2026e). The data are population registers, so we observe the universe of retirees and the full employment and earnings histories of all employees subject to compulsory social insurance. We include information for the years 2018 to 2024, which gives us three pre-reform years and four years after the reform. In Appendix B we provide a detailed overview of the data.

In the empirical analysis, we focus on two margins. First, we test for take-up effects, analyzing whether individuals select into eligibility by changing employment before entering retirement. Second, we quantify employment and earnings responses during retirement. The two margins differ in the timing of treatment relative to retirement and require different samples. Several restrictions are common to both samples. We keep only old-age pensioners and exclude individuals with disability pensions, survivors' pensions, and individuals who never worked. We keep only individuals resident in Germany, since the employment of pensioners living abroad is not recorded in the data.[10]

The first sample includes all individuals who entered old-age retirement between 2018 and 2024. Based on the full employment and earnings histories, we construct the number of qualifying months that determine eligibility for the pension. This is the central variable for the take-up analysis. We observe three cohorts that enter retirement before the reform and four after the reform. This allows us to compare the density of qualifying months across several years in the pre-reform and post-reform regimes. The sample with the described restrictions contains 2,657,821 women and 2,364,843 men over the full observation period. Each individual appears once, in their individual year of retirement claiming.

The second sample includes annual information for the period 2018-2024 with individuals born in 1950 or 1951 who entered old-age retirement between 2013 and 2017. These individuals had already retired when the reform was announced. Two considerations determine the sample restriction. Women who retired before 2013 lie outside the window in which we can reconstruct contribution histories, and cohorts from 1952 onwards no longer had access to the pension for women, so including them would change the mix of claiming pathways (Geyer & Welteke, 2021). The empirical approach and the construction of treatment and control groups are described in detail in Section 5.2.1. We require that an individual is observed in every year. Individuals who die before the end of the observation period are therefore dropped. The analysis sample follows 79,075 women from 2018 to 2024 and 553,525 person-year observations. For men, the corresponding sample contains 29,165 individuals and 204,155 person-year observations. The

[10]Furthermore, we exclude individuals whose qualifying periods cannot be determined from German contribution records: those without any compulsory contribution months, those with periods credited under the Foreign Pensions Act (*Fremdrentengesetz*), and recipients of pensions under international social security agreements (*Vertragsrenten*).

sample of men is considerably smaller, since fewer men receive the pension supplement.

The administrative data are organized at the individual level and contain no household or family identifiers. This has a direct consequence for our research design. The income test described in Section 2 operates at the household level and is based on household income, which is not included in the data. We therefore define treatment in Section 5.2 by eligibility status rather than by receipt. The resulting estimates are intention-to-treat (ITT) effects at the individual eligibility margin.

To analyze the implications of the income test at the household level for the welfare effects of the pension supplement, we use a subsample of the administrative pension data (SOEP-RV) which can be matched to the household data of the Socio-Economic Panel Study (FDZ-RV, 2026b; Goebel et al., 2019). The SOEP-RV includes the eligibility status for the pension supplement and whether individuals pass the income test. Different from the full population registers, the SOEP adds information about household income and therefore allows a comparison of total household income between eligible and non-eligible households. More information about SOEP-RV and a comparison to the population in the core administrative data is provided in Appendix B.2.

4 Descriptive Statistics

Before turning to the causal analysis, we document the gender pension gap and the gender gap in earnings during retirement. We also discuss the mechanical effect of the pension supplement on gender specific pensions and on the gender pension gap. For the descriptive analysis, we focus on all retired individuals aged 65 to 75 in 2024. We restrict the sample to individuals who retired before the reform took effect in 2021, so that their contribution records were complete and unalterable when the supplement was announced.[11]

Overall, men's pensions are higher than those of women (Figure 1a). Female pensions are more heavily concentrated at low levels, and less often exceed €2,000 per month. On average, men receive a gross pension of €1,881 per month, compared with €1,241 for women. This results in a sizable gender pension gap of €641, or 34.0% (Figure 1b).[12] The relative pension gap is largest at the bottom of the distribution and declines monotonically further up, from 53.3% in the bottom decile to 20.0% in the top decile. In euros, the profile is hump-shaped rather than declining.[13] The gap amounts to €447 per month in the bottom decile, peaks at €693 in decile 5, and falls back to €591 in the top decile, so the decline in relative terms reflects the

[11] Furthermore, we require at least 20 years of periods relevant for the pension supplement to remove very short contribution records of individuals changing from the public pension system to either self-employment or public servant pensions. The claiming window starts in 2013 because earlier entries fall outside the data window. The resulting sample comprises 3,996,067 individuals, 1,808,093 men and 2,187,974 women.

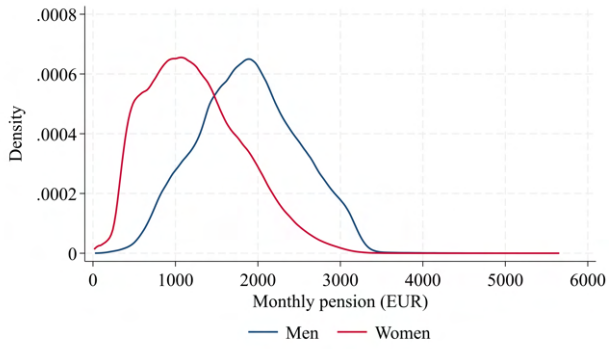
[12] Throughout, we define the gender pension gap as the difference between men's and women's average pensions, expressed as a percentage of men's average pension.

[13] See Appendix C.1

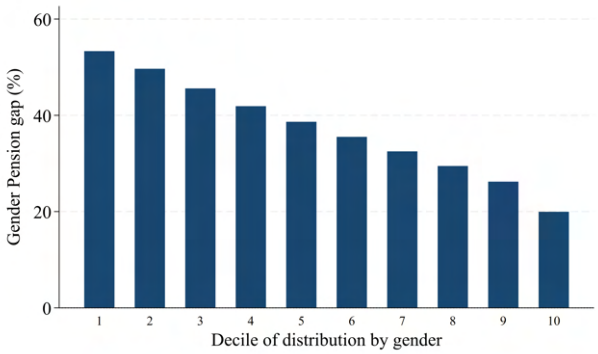
rising male benchmark rather than a narrowing absolute difference.

Splitting the sample by region shows that the gender pension gap is predominantly a West German phenomenon. On average, East German men receive a pension of €1,630 per month and East German women €1,479, which is a gap of 9.2%. In the West, the corresponding figures are €1,969 and €1,167, a gap of 40.7%.[14] While women in the East receive a higher monthly pension than women in the West, the opposite is true for men (Figure 1c). The regional contrast holds across the distribution. In the East, the gender pension gap ranges from 4.7% in decile 8 to 14.8% in decile 3 and follows no monotonic pattern, while in the West the gap is far larger throughout, declining monotonically from 57.8% in decile 1 to 22.9% in decile 10 (Figure 1d).

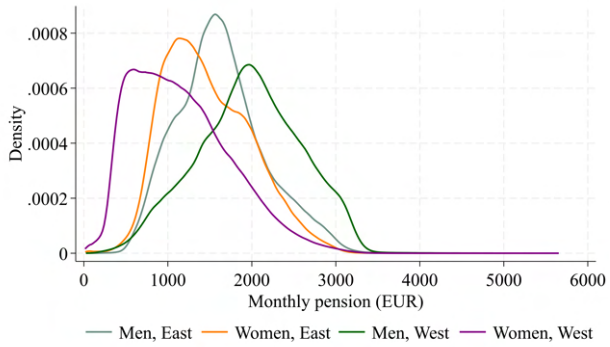
Figure 1: The Gender Pension Gap across the Pension Distribution



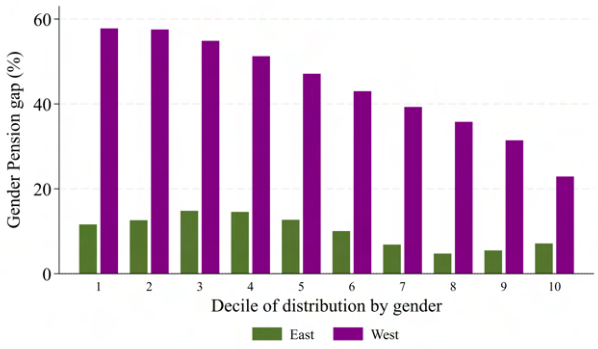
(a) Distribution of monthly pension income



(b) Gender pension gap by decile



(c) Distribution of monthly pension income, by region



(d) Gender pension gap by decile, by region

Notes. Panels (a) and (c) show distributions of monthly gross pension income (€) by gender, pooled and by region. Panels (b) and (d) show the gender pension gap by decile of the gender specific pension distribution, expressed as a percent of men's pension, before the supplement. Deciles in Panel (d) are computed within region and gender; they are not directly comparable to Panel (b). Analysis sample for 2024, ages 65 to 75, at least 20 years of contribution periods, N = 3,996,067 (West N = 3,012,903, East N = 983,164). *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

These regional differences are related to different gender norms and different employment

[14] We assign individuals to East or West Germany by their federal state of residence recorded in the data. The data distinguishes between East and West Berlin, so we assign each part to the corresponding region.

patterns. In East Germany, female employment rates are historically very similar to those of men, a legacy of the institutions of the divided period in Germany with persistent implications. To this day, East Germans hold more egalitarian sex-role attitudes (Bauernschuster & Rainer, 2012), East German women can out-earn their husbands without the behavioral consequences that follow in the West (Lippmann et al., 2020), and East German norms persist while West German ones adjust on exposure (Boelmann et al., 2025). These norms translate into different employment biographies (Boelmann et al., 2025; Jessen, 2022; Rosenfeld et al., 2004).

Pension income is only one part of the financial resources during retirement. Importantly, a growing fraction of retirees continues working. In 2024, 13.8% of pensioners in our sample have labor earnings, 16.0% of men and 12.0% of women. Averaged across the full sample, retired men receive €103.44 per month in labor earnings relative to €63.41 for women, a gap of 38.7%. Figure 2a shows how this gap varies by decile of the pension distribution.[15] It follows a pronounced U-shape. The gap is largest in decile 1 at €77.52 per month, narrows to a minimum of €15.11 in decile 8, and widens again to €43.63 in decile 10. The U-shape reflects two patterns at opposite ends of the distribution. At the bottom, men's earnings exceed women's by a wide margin; further up, earnings become progressively more gender balanced, until the top decile, where men's earnings rise sharply.

Panel (b) decomposes this pattern by region. On average, West German men earn €114.36 per month and West German women only €69.47, a gap of €44.88, while in the East the corresponding figures are €72.01 and €43.81, a gap of €28.20. In relative terms, the two regions are almost identical, 39.3% in the West and 39.2% in the East, in sharp contrast to the pension gap, which is more than four times larger in the West. To summarize, the gender gap in labor earnings during retirement increases the gender gap in old-age income mainly among individuals with low pension income in West Germany and, to a smaller degree, among high-pension retirees in both regions.

In the following, we turn to the mechanical effect of the pension supplement (Figure 3). In total, about 10.5% in our sample receive a positive supplement.[16] Nearly 80% of the recipients in our sample are female. Female recipients are spread across nearly the entire pension distribution of women in our sample rather than concentrated at the very bottom (Figure 3a). This is because the eligibility criteria, a long contribution record combined with low but not very low earnings, are met well beyond the poorest percentiles. Women with the very lowest pensions are in fact the least likely to receive anything, because most of these women have too few contribution years and do not qualify for the supplement.[17] Male recipients, by contrast, are concentrated

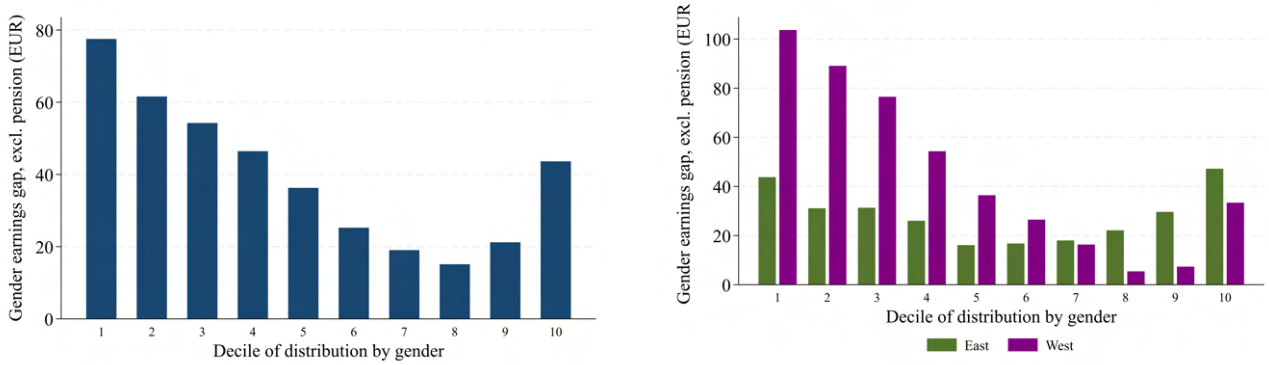
[15] Earnings are averaged over all individuals in a pension income decile, with non-working individuals included with zero earnings. The gender gap in earnings therefore reflects differences both in the share of retirees who work and in their earnings conditional on working; we do not separate these two margins here.

[16] This is considerably larger than the 5.4% in the full population of pensioners reported in Buslei et al. (2025). The difference is a direct consequence of our sample restrictions, most importantly the focus on old-age pensions and individuals with at least 20 years of qualifying periods.

[17] Among women in our sample, recipients have lower pensions on average than non-recipients (€1,074 vs. €1,270 per month, excluding the supplement), but receipt is far from confined to the bottom of the distribution

much more narrowly at the bottom of the male pension distribution in our sample: since men's pensions are generally higher, the combination of a long contribution record and persistently low earnings places them near the bottom of the male distribution.

Figure 2: The Gender Earnings Gap across the Pension Distribution



(a) Gender earnings gap by decile

(b) Gender earnings gap by decile, by region

Notes. Gender gap in average monthly labor earnings (€) during retirement, excluding pension income, by decile of the gender specific pension distribution. Panel (a) pools regions, Panel (b) splits by region with deciles computed within region and gender, so the two panels are not directly comparable. Averages include retirees who do not work, who enter with zero earnings. Analysis sample for 2024, ages 65 to 75, $N = 3{,}996{,}067$. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

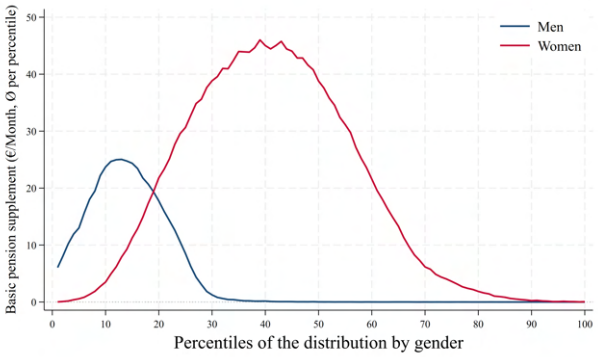
The average supplement conditional on receipt is €109.51 per month. Spread across all individuals unconditional on eligibility, this raises women's average pension by €17.51 per month and men's by €4.50. Including the supplement therefore mechanically lowers the average pension gap from 34.0% to 33.3%, a reduction of 0.77 percentage points or 2% of the pre-reform gender pension gap.[18] This modest aggregate effect conceals substantial heterogeneity across the distribution (Figure 3b). At the bottom, the supplement increases the pre-reform gap by 3.2% in decile 1 and 1.4% in decile 2, since many of the lowest-pension women fall short of the minimum contribution period and are ineligible despite their low entitlements. From decile 3 upward, the supplement reduces the gap, most strongly in deciles 4 and 5, by 6.1% and 6.0% of the pre-reform gap, before tapering to near zero at the top, where eligibility is rare for either gender.

The regional split shows that the aggregate effect masks two very different pictures. In the East, the supplement lowers the gender pension gap from 9.2% to 8.6%, a reduction of 0.66 percentage points that closes 6.8% of the pre-reform pension gap. In the West, the reform lowers the gap from 40.7% to 39.9%, a similar 0.79 percentage points in absolute terms but only 1.7% of a gap that is more than four times as large in the West as in the East. East German reductions peak in the middle of the lower half of the distribution, at roughly 15% in decile 4, and exceed the West German reductions in every decile up to decile 5. Reductions in both regions fall to near zero from decile 7 onward.

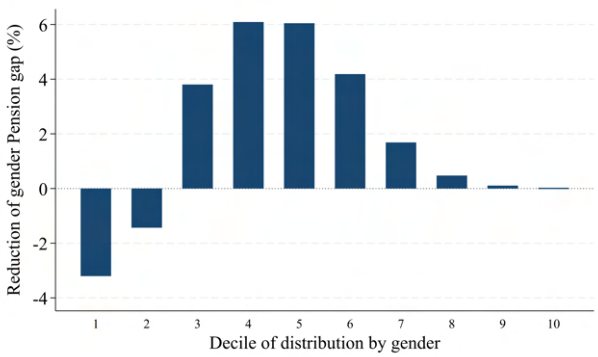
(Figure 3a).

[18] Measured in euros, the gap falls from €641 to €628.

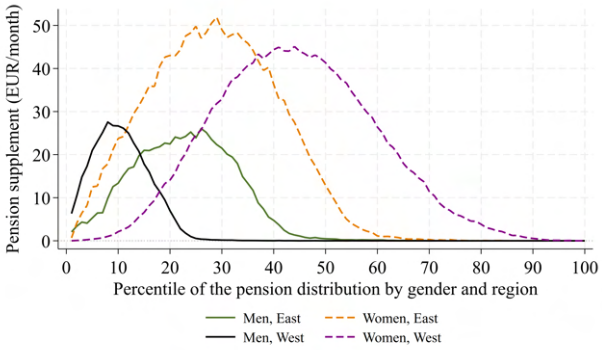
Figure 3: Effect of the pension supplement on the Gender Pension Gap



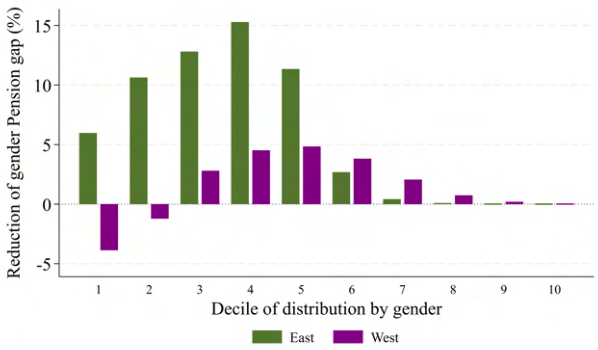
(a) Basic pension supplement by percentile



(b) Change in the gender pension gap by decile



(c) Basic pension supplement by percentile, by region



(d) Change in the gender pension gap by decile, by region

Notes. Panel (a) shows the average monthly supplement by percentile of the gender specific pension distribution, separately for men and women, with non-recipients entering at zero. Panel (b) shows the change in the pre-supplement euro gap by decile, as a percent of that gap, where negative values indicate that the supplement widens the gap. Panels (c) and (d) repeat Panels (a) and (b) by region, with percentiles and deciles computed within region and gender. Analysis sample for 2024, ages 65 to 75, $N = 3{,}996{,}067$. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

5 Behavioral Responses to the Pension Supplement

In this section, we analyze the behavioral effects of the pension supplement on employment and earnings during the working life and the retirement period.

5.1 Selection into Eligibility

First, we test whether the reform affects the timing of pension claiming and selection into eligibility. The supplement creates financial incentives to adjust the number of qualifying years, either by delaying retirement or by avoiding periods of non-employment that do not count towards eligibility. Individuals who would otherwise retire with fewer than 33 qualifying years have an incentive to continue accumulating qualifying periods until they become eligible,

and to continue up to 35 years, where the full supplement is reached.[19] Conversely, an individual who has just crossed the threshold may reduce contribution periods. Both responses distort the timing of the transition from work to retirement or to non-employment before retirement, with consequences for individual welfare, aggregate labor supply, and the fiscal cost of the reform.[20]

In the empirical analysis of take-up effects, we test for a discontinuity in the density of pension entries at the relevant eligibility threshold using the local polynomial density estimator of Cattaneo et al. (2020). The variable of interest, i.e., the running variable, is the number of qualifying contribution periods at the time of pension entry, reconstructed from complete monthly contribution histories (FDZ-RV, 2026e).

We focus on two eligibility thresholds, 396 months of contribution periods (33 years) for partial eligibility and 420 months of contribution periods (35 years) for full eligibility. The sample includes all individuals who enter old-age retirement between the years 2018 and 2024 with the sample restriction described in Section 3.

Intuitively, if individuals manipulate their contribution histories to cross a cutoff, the number of observations creates bunching above the threshold and thins out just below it, producing a discontinuity that a smooth, predetermined running variable would not display.[21] We run the test separately by gender for each year and the two thresholds. A reform-induced response should emerge or intensify after the year 2021 relative to pre-reform cohorts.

5.1.1 Main Results

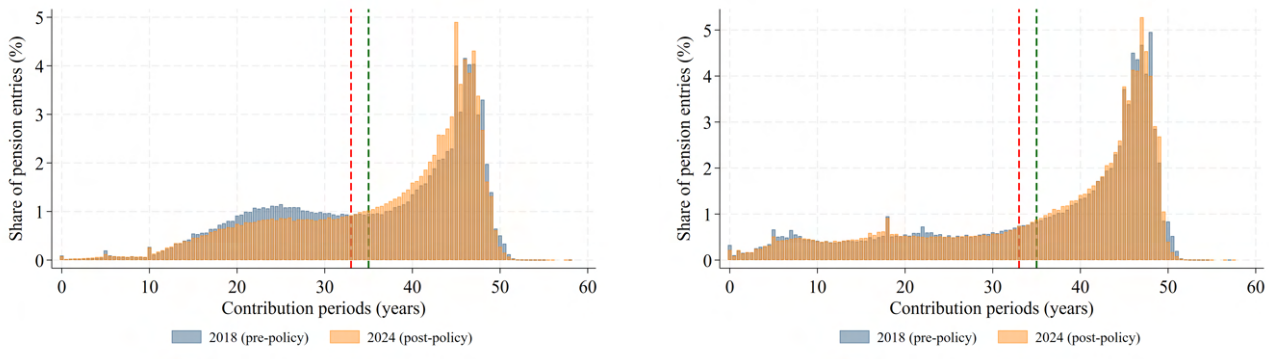
We start with descriptive evidence. Figure 4 shows the distribution of pension entries by qualifying contribution years by gender, comparing the pre-reform year 2018 with the post-reform year 2024. The two years are nearly superimposed throughout, both for women and for men. Neither shows a spike or a discontinuous jump at 33 years, and the same holds at the full eligibility threshold of 35 years. The figures reveal pronounced bunching elsewhere, at points unrelated to the pension supplement. The clearest is at 45 qualifying years, the eligibility requirement for the pension for especially long-term insured individuals described in Appendix A, which permits early retirement without actuarial deductions. Nonetheless, the spike at 45 qualifying years is informative for the interpretation of the analysis. It shows that the distribution of contribution periods is capable of displaying bunching when a threshold is salient.

[19] Seibold (2021) documents substantial bunching of retirement at statutory thresholds in the German pension system, including thresholds defined by required insurance periods.

[20] Periods of unemployment benefit receipt do not count as qualifying contribution periods under § 76g SGB VI. A month spent in unemployment therefore leaves the stock of qualifying years unchanged, so any substitution between employment and unemployment before retirement translates one for one into a slower accumulation of qualifying periods and becomes visible at the eligibility margin.

[21] For technical details see Cattaneo et al. (2020).

Figure 4: Distribution of Pension Entries Around the Eligibility Threshold



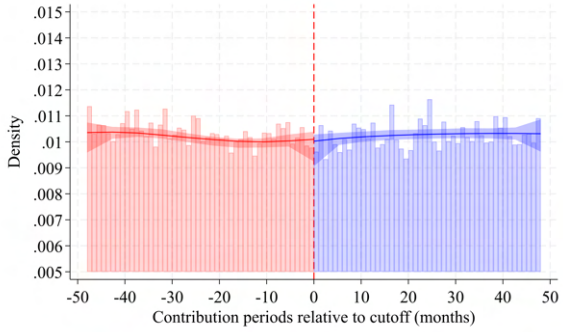
(a) Women (b) Men

Notes. Each bar reports the share of pension entries within a given year falling in a six-month bin of qualifying contribution periods, expressed in years. The red dashed line marks the 33-year threshold for partial eligibility and the green dashed line the 35-year threshold for full eligibility. Navy bars show the pre-reform year 2018 and orange bars the post-reform year 2024. Panel (a) covers female old-age pension entries ($N = 2{,}657{,}821$) and panel (b) covers male old-age pension entries ($N = 2{,}364{,}843$), in both cases across all entry years from 2018 to 2024. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

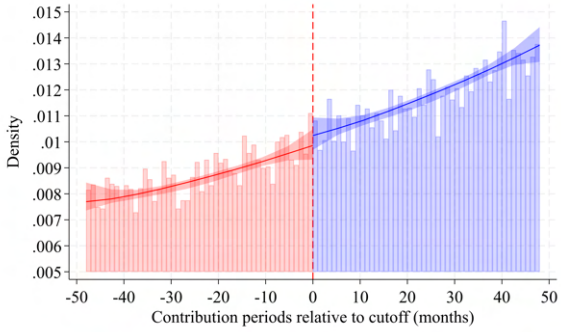
The formal test confirms the descriptive evidence. Figure 5 presents the estimated density curves and confidence bands for the pre-reform year 2018 and the latest available post-reform year 2024, at both thresholds. For both years and for both the 33-year and 35-year cutoffs, the estimated density is smooth through the cutoff, and the confidence bands for the left- and right-hand-side estimates overlap substantially at zero.

Figure 5: Local Polynomial Density Test at Both Eligibility Thresholds, by Gender

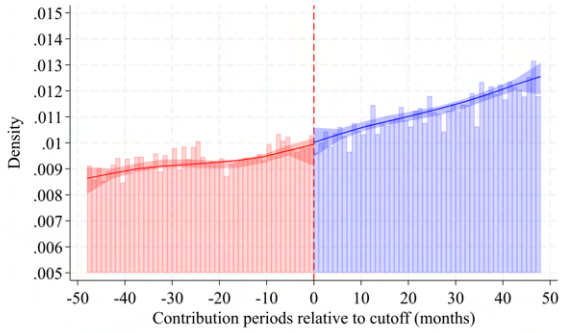
33 year threshold (396 months)



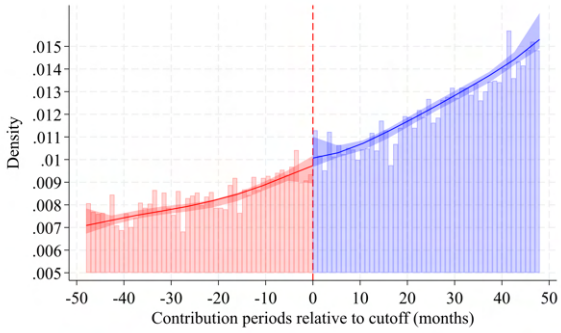
(a) Women, 2018



(b) Men, 2018

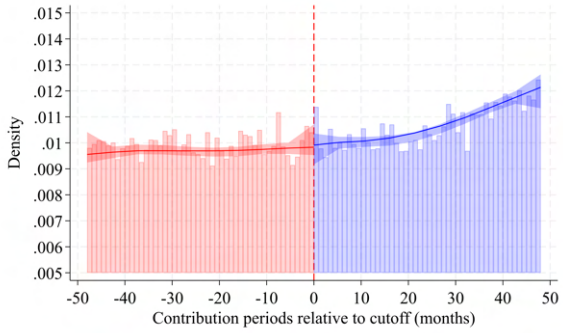


(c) Women, 2024

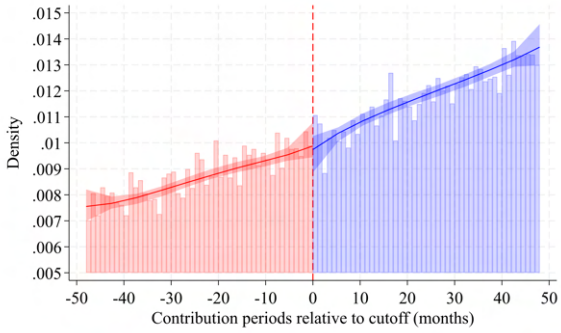


(d) Men, 2024

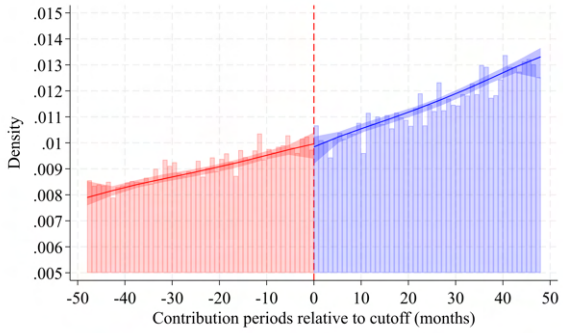
35 year threshold (420 months)



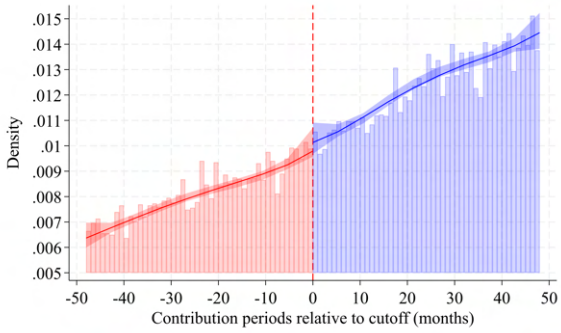
(e) Women, 2018



(f) Men, 2018



(g) Women, 2024



(h) Men, 2024

Notes. Local polynomial density estimates (Cattaneo et al., 2020) of the distribution of pension entries around the eligibility cutoffs, shown as zero on the x axis. The running variable is qualifying contribution periods relative to the relevant cutoff. Curves show the estimated density on each side of the cutoff with shaded 95% bias-corrected confidence bands. Polynomial order $p = 2$, bias correction order $q = 3$, triangular kernel, bandwidth selected within the estimation window by the default data driven procedure, separately on each side. Bars show one-month bins. The sample is restricted to entries within 48 months of the relevant cutoff. Robust t statistics are -1.09 ($p = 0.27$), 0.73 ($p = 0.47$), 0.25 ($p = 0.80$), 1.64 ($p = 0.10$), -0.20 ($p = 0.84$), -1.02 ($p = 0.31$), -0.64 ($p = 0.52$) and -0.13 ($p = 0.90$) in panels (a) to (h). *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

Table 1 extends the test to every entry year from 2018 to 2024 under varying kernels, separately by gender. For women, 25 test statistics out of 28 are insignificant. The exceptions are the 33-year threshold in 2022, significant at the 5 percent level under the triangular kernel and at 10 percent under the uniform, and the 35-year threshold in 2023, significant at 10 percent under the uniform kernel but not under the triangular, where the statistic falls to 0.60. Neither survives the full specification grid in Appendix C.2, and neither persists into an adjacent year, which is difficult to reconcile with manipulation. A behavioral response to a permanent eligibility rule should appear every year, since the incentives also hold in the following year. Panel B repeats the exercise for men. The picture is very similar. Again, 25 out of 28 tests are insignificant. The density is smooth at both thresholds in every pre-reform year, and there is no sign of a response that builds as the reform approaches or consolidates afterward. The exception is the 33-year threshold in 2021, which rejects at the 1 percent level under both kernels and across polynomial orders, with excess mass above the cutoff. This is also confined to a single year, so it does not describe a sustained adjustment to a permanent eligibility rule.

Table 1: Manipulation Test Results at Both Eligibility Thresholds, by Gender

	33 year threshold (396 months)		35 year threshold (420 months)	
Entry year	Triangular	Uniform	Triangular	Uniform
Panel A. Women				
2018	−1.094	−1.127	−0.201	0.926
2019	0.537	0.314	0.608	0.225
2020	1.550	1.618	−0.598	−1.131
2021	−0.072	−0.176	1.145	0.173
2022	2.052**	1.720*	−0.688	−0.840
2023	−1.395	0.031	0.602	1.782*
2024	0.247	−0.438	−0.644	−0.820
Panel B. Men				
2018	0.729	0.248	−1.024	−1.312
2019	−0.165	−0.092	−1.041	−2.250**
2020	0.712	0.750	1.307	0.835
2021	3.347***	3.010***	0.100	0.864
2022	1.154	1.607	−0.221	−0.055
2023	0.144	−0.431	−0.938	−1.042
2024	1.641	1.276	−0.127	0.024

Notes. Robust t statistics from the manipulation test of Cattaneo et al. (2020), run separately for each entry year, threshold, kernel and gender. The running variable is qualifying contribution periods relative to the cutoff, 396 months for partial eligibility and 420 months for full eligibility. Polynomial order $p = 2$ and bias correction order $q = 3$ throughout. The sample is restricted to entries within 48 months of the relevant cutoff, and the bandwidth is selected within that window by the default data driven procedure, separately on each side. A positive statistic indicates excess mass above the cutoff. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

In addition to the analysis of behavioral adjustment at the extensive margin, we also provide empirical evidence for the intensive margin and show that individuals did not adjust earnings before entering retirement (Appendix D).

5.1.2 Heterogeneity

We repeat the test separately for East and West Germany, using the same specification as in the pooled sample, at both thresholds and for each entry year from 2018 through 2024.

Table 2 reports the results for women. The corresponding table for men, which shows a very similar pattern, is relegated to Appendix C.2. The density of pension entries is smooth through both thresholds in almost every year in East and West Germany. Of the 56 specifications, 51 are insignificant at the 10 percent level, and only one, East Germany at the 33-year threshold in 2022, rejects under both kernels at once. The other three rejections appear under the uniform kernel alone and have no counterpart under the triangular kernel or in an adjacent year.

Table 2: Manipulation Test Results by Region, Women

	33 year threshold (396 months)		35 year threshold (420 months)	
Entry year	Triangular	Uniform	Triangular	Uniform
Panel A. East Germany				
2018	0.061	−0.368	0.713	0.793
2019	1.202	0.766	0.547	0.104
2020	0.515	−1.043	0.223	−0.459
2021	0.379	0.477	0.420	0.349
2022	1.692*	2.155**	1.133	1.242
2023	0.040	0.060	0.333	0.118
2024	−0.591	−1.004	1.461	1.744*
Panel B. West Germany				
2018	−0.838	−1.095	−0.627	0.015
2019	0.039	−0.548	0.440	0.211
2020	1.477	1.781*	−0.764	−1.072
2021	−0.291	−0.386	1.106	−0.034
2022	1.509	0.932	−1.247	−1.472
2023	−1.550	0.372	0.533	1.963**
2024	−0.090	−0.036	−1.330	−1.638

Notes. Robust t statistics from the manipulation test of Cattaneo et al. (2020) at the two eligibility thresholds, run separately for each entry year, region, threshold and kernel. Polynomial order $p = 2$ and bias correction order $q = 3$ throughout. The sample is restricted to female old age pension entries within 48 months of the relevant cutoff, and the bandwidth is selected within that window by the default data driven procedure, separately on each side. A positive statistic indicates excess mass above the cutoff. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

The absence of behavioral adjustment before entering retirement yields two important findings. First, although the reform introduces a sizable financial incentive at sharp thresholds defined by contribution months, individuals do not adjust their contribution records to manipulate eligibility. This is consistent with the low salience of the reform prior to retirement and the opaque link between individual behavior and supplement receipt, and it mirrors evidence from other complex social insurance programs in which eligible individuals often fail to claim benefits they are entitled to because of administrative complexity and transaction costs rather than a considered choice not to participate (Buslei et al., 2019; Chan & Stevens, 2008; Currie, 2006). Either the incentive itself was not perceived by affected individuals prior to retirement, or the margin of adjustment was too costly to make manipulation worthwhile.

Second, the absence of take-up effects supports the validity of our difference-in-differences design applied in the following analyses. A necessary condition for the comparison between eligible and non-eligible individuals to identify the causal effect of the reform is that treatment status is not itself the product of a behavioral response to the reform. A smooth distribution of pension entries at both thresholds in every year, including post-reform years, indicates that individuals do not select into eligibility through strategic adjustment of their contribution histories once the reform is announced or implemented.

5.2 Post-Retirement Labor Supply

The second behavioral margin of interest is whether individuals adjust their labor supply and earnings during retirement due to the introduction of the pension supplement.

5.2.1 Empirical Strategy

As mentioned in Section 3, for the analysis of post-retirement labor supply, we focus on individuals born in 1950 or 1951 who had already claimed an old-age pension before the reform and are observed annually from 2018 through 2024.

Since the data do not include information about the household, we define the treatment and control group based on the individual eligibility criteria regardless of the income test on the household level. As a consequence, the coefficients we report identify an intention-to-treat (ITT) effect at the eligibility margin. The treatment group consists of individuals who are eligible for the supplement after its introduction and whose average earnings points during the assessment period are at least 0.5 points per year.[22] The control group is the union of two groups of ineligible individuals who fail the eligibility criteria along a single dimension. The first consists of individuals with at least 35 years of qualifying contributions, the full eligibility length, whose average earnings points exceed the eligibility ceiling of 0.8 points per year but have a maximum of one earnings point on average. The second consists of individuals with average earnings points at or below the eligibility ceiling, who therefore fail to qualify solely because their contribution history falls short, as they have between 25 and 33 qualifying years.[23]

Eligibility is determined mechanically by contribution histories and lifetime earnings rather than randomly assigned, thus individuals in the treatment and the control groups differ along observable characteristics. To account for these differences, we use coarsened exact matching (Blackwell et al., 2009; Iacus et al., 2012) and construct a comparable control group, separately by gender. We match treated individuals to control individuals who are identical in German

[22] Thus, we exclude the small number of eligible individuals with the very lowest earnings positions near the bottom of the eligibility range.

[23] We assess robustness to this second control group definition in Appendix C.3.4 by varying its contribution length window to 25 to 35 years and to 30 to 33 years and show that results are robust in sign, significance and magnitude.

citizenship, birth year, federal state of residence, pension type, year of pension entry, number of children, and who fall into the same decile of qualifying contribution months and the same decile of total lifetime earnings points, all measured in the year 2020, the last pre-reform year. With the exception of federal state of residence, which may change through migration, the characteristics are predetermined and fixed at retirement. We do not match on pre-treatment labor market outcomes, so we can test whether pre-reform trends of employment and earnings remain significantly different between the treatment and control group. An overview of the matched samples for women and men and descriptive labor market trends can be found in Appendix C.3.1.

To identify the post-retirement labor market response, we estimate an event study model,

$$Y_{it} = \alpha_i + \lambda_t + \sum_{\substack{\tau=-K \\ \tau \neq -1}}^{L} \beta_\tau \, D_i \times \mathbf{1}\{t - T_0 = \tau\} + X_{it}'\gamma + \varepsilon_{it}, \tag{1}$$

where Y_{it} denotes annual pension income, employment status, or annual labor earnings of individual i in year t. The specification includes individual fixed effects α_i, which absorb time-invariant heterogeneity, and year fixed effects λ_t, which capture aggregate shocks common to a given year. The indicator D_i equals one for individuals in the treatment group and zero for the matched control group. The term $\mathbf{1}\{t - T_0 = \tau\}$ is an indicator for event time τ relative to the reform year $T_0 = 2021$, and the coefficients β_τ measure the dynamic effects relative to the omitted baseline period $\tau = -1$, the year immediately preceding the reform. The vector X_{it} contains time-varying characteristics, which include pension income without the supplement and federal state of residence. Standard errors are clustered at the individual level throughout.

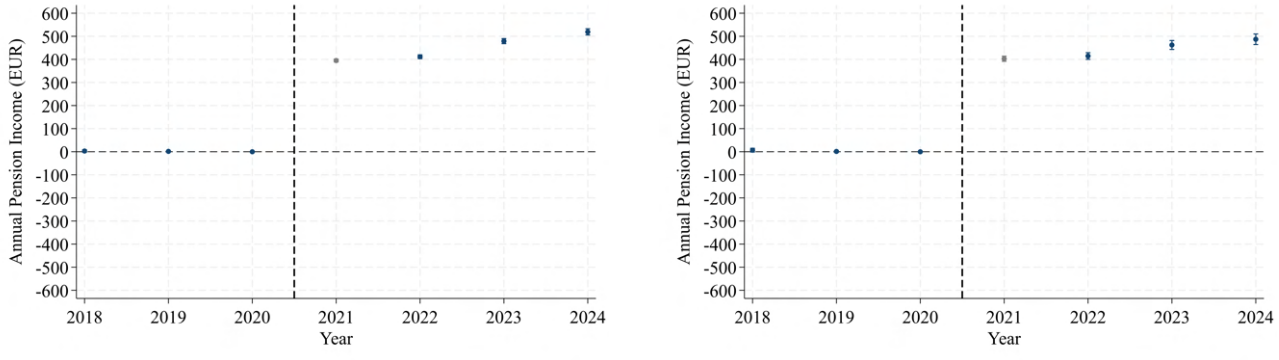
To translate the reform effects into elasticities, we exploit the reform-induced variation in pension income in a two-stage least squares framework, instrumenting log pension income with eligibility interacted with the post-reform period. Appendix C.3.2 presents the first- and second-stage specifications and reports the corresponding results.

5.2.2 Main Results

First, we present the reform's direct effect on pension income, which is also the first-stage estimation used in the calculation of elasticities. Eligibility raises annual pension income for women by €449.60 in the baseline specification and by €456.28 once we add time-varying controls, both significant at the 1 percent level. Relative to the 2020 mean pension income of the eligible group, €11,730.36, this corresponds to an increase of roughly 3.8%. For men, the absolute effect is similar. The estimated income effects are considerably lower than the effects reported in Section 2 since here we focus on the population of eligible individuals irrespective of the income test. Figure 6 presents the corresponding event study estimates for women and men. Pension income evolves similarly for treated and control individuals before 2021, and the

increase that follows the reform is sustained rather than transitory throughout the remainder of the sample period.

Figure 6: Dynamic Effects on Pension Income



(a) Women (b) Men

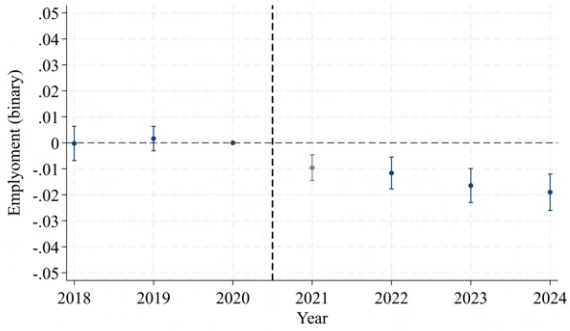
Notes. The figure plots event study estimates of Equation (1) for the effect of eligibility for the pension supplement on annual pension income, separately for women in panel (a) and men in panel (b). Individual and year fixed effects are included. The vertical line marks the year the supplement took legal effect (2021). 95% confidence intervals shown. Robust standard errors are clustered at the individual level, with 79,075 clusters for women and 29,165 for men. CEM weights are applied in the estimation. The 2021 estimate reflects the entitlement accrued in that year rather than payments actually received in it. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

In Figure 7 we turn to the labor market effects and show the event study estimates for employment and earnings. In the years before the reform, employment, formal employment, and marginal employment all evolve similarly for treated and control individuals, with no evidence of significant pre-trends, supporting the parallel trends assumption underlying our design. While post-reform effects are insignificant throughout for men, we find significant behavioral effects of the reform for women. Employment among eligible women declines relative to the control group, growing from -0.95 percentage points in 2021 to -1.90 percentage points by 2024, against a pre-reform mean of 23.4%, indicating a persistent rather than transitory adjustment in labor supply. The decomposition across margins shows that this decline at the extensive margin is entirely concentrated in marginal employment. Formal employment above the marginal threshold shows no significant movement at any point in the post-reform period. The event study estimates for holding only a marginal job rise in magnitude from -0.76 percentage points in 2021 to -1.80 percentage points in 2024, against a pre-reform mean of about 22%.[24] By contrast, the coefficients for employment above the marginal job threshold are small, close to zero, and statistically insignificant in every post-reform year. Annual labor earnings decline modestly beginning in 2021, falling from €-35 in 2021 to €-58 by 2024.

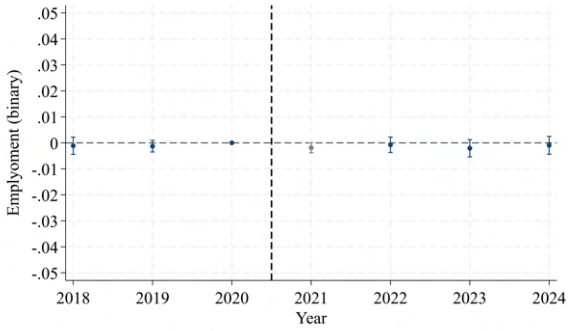
[24]One concern specific to a means-tested benefit is that eligible individuals might cut earnings in order to pass the income test. Yet, marginal employment does not enter the base, and the assessment draws on income from three years earlier rather than the current year, so no contemporaneous earnings decision can affect the supplement a pensioner receives.

Figure 7: Main Dynamic Employment Results

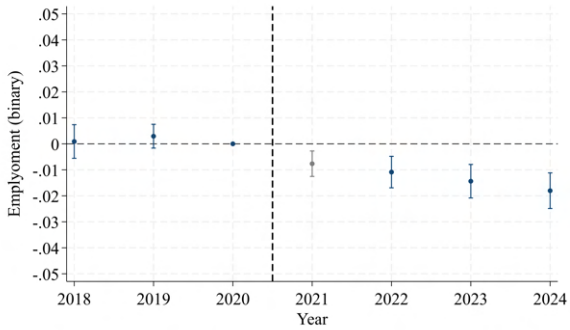
Panel A. Women



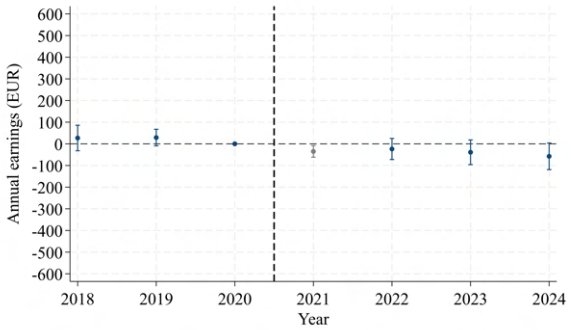
(a) Any employment



(b) Formal employment

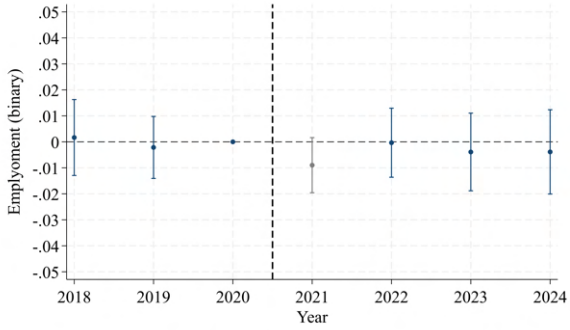


(c) Marginal employment

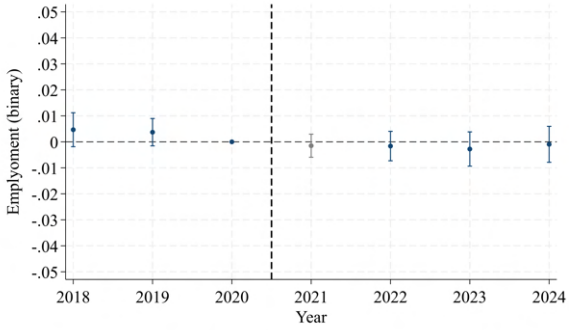


(d) Annual labor income

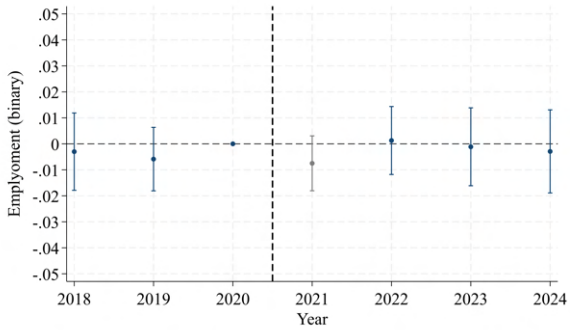
Panel B. Men



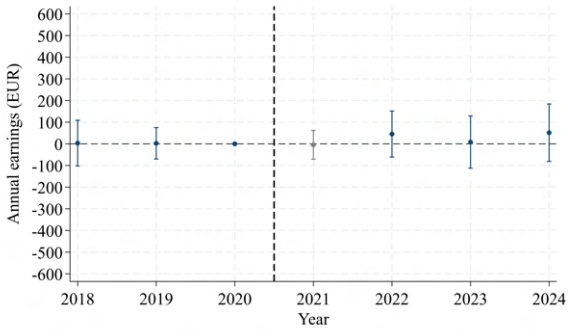
(e) Any employment



(f) Formal employment



(g) Marginal employment



(h) Annual labor income

Notes. The figure plots event study estimates of Equation (1) for the effect of eligibility for the pension supplement on labor market outcomes, using the matched samples for women in Panel A and men in Panel B. Any employment covers all employment, formal employment covers employment above the mini-job threshold, and marginal employment covers mini-job employment only, so the latter two sum to the first. Annual labor earnings are measured in euros and set to zero for those who do not work. All specifications include individual and year fixed effects. The omitted baseline is 2020, the year immediately preceding the reform, and the vertical line marks the year the supplement took legal effect (2021). 95% confidence intervals shown. Robust standard errors are clustered at the individual level, with 79,075 clusters for women and 29,165 for men. CEM weights are applied in the estimation. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

Table 3 reports the corresponding average effects over the post-period. Overall female employment falls by 1.46 percentage points, significant at the 1 percent level and essentially unchanged once controls are added. Relative to the 2020 mean employment rate of the eligible group, 23.38%, this is a decline of about 6.2%. Formal employment above the marginal job threshold is essentially unaffected, with a coefficient of -0.0006 that is small and statistically indistinguishable from zero. Marginal employment falls by 1.40 percentage points, almost identical in magnitude to the decline in overall employment, confirming that the entire extensive margin response operates through mini-job work rather than formal employment. Annual labor earnings decline by nearly €60, depending on specification, significant at the 5 percent level. Relative to the 2020 mean earnings of the eligible group, €1,103.78, this is a decline of about 5.2%. The pattern of a larger proportional response in employment than in earnings is consistent with the labor supply adjustment being concentrated among low-earning, marginal jobs rather than higher-earning formal employment, mirroring the decomposition across employment margins from above. Columns (3) and (4) document the results for men and confirm that despite a significant effect on pension income, point estimates for employment and earnings are very small and not significant.

Taken together, these results show that the reform induced a sizable increase in pension income for eligible individuals. Relating the employment and earnings effects to the reform-induced variation in pension income in the instrumental variables approach detailed in Appendix C.3.2, we estimate for women an employment elasticity with respect to pension income of -1.60, and an elasticity of -1.82 for marginal employment. The earnings elasticity amounts to -1.33, which corresponds to a marginal propensity to earn (MPE) out of pension income of -0.13. For every additional euro of pension income generated by the reform, annual labor earnings fall by roughly 13 cents. This is close to the MPE of approximately -0.1 that Kindermann et al. (2026) obtain for the same reform.

Table 3: Main Results on Pension Income and Employment, by Gender

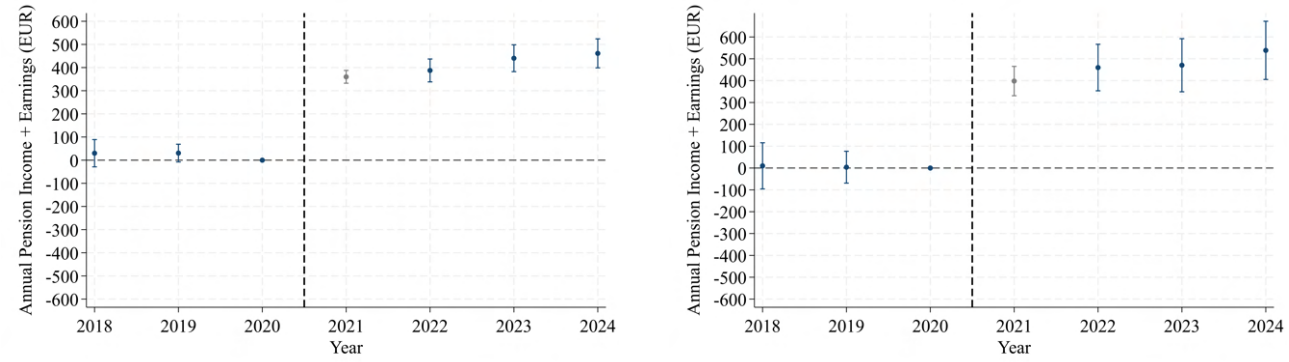
	Women		Men	
	(1)	(2)	(3)	(4)
	Baseline DiD	+ Controls	Baseline DiD	+ Controls
Panel A: Pension income (EUR)				
Post × Eligible	449.599***	456.280***	439.085***	458.578***
	(5.357)	(3.438)	(8.856)	(5.311)
Panel B: Any employment				
Post × Eligible	−0.0146***	−0.0146***	−0.0041	−0.0050
	(0.0031)	(0.0031)	(0.0070)	(0.0069)
Panel C: Formal employment (above mini-job threshold)				
Post × Eligible	−0.0006	−0.0006	−0.0045	−0.0045
	(0.0015)	(0.0015)	(0.0030)	(0.0029)
Panel D: Marginal employment (mini-job only)				
Post × Eligible	−0.0140***	−0.0140***	0.0004	−0.0004
	(0.0030)	(0.0030)	(0.0070)	(0.0070)
Panel E: Annual labor earnings (EUR)				
Post × Eligible	−57.459**	−58.079**	23.184	19.161
	(24.081)	(24.092)	(54.591)	(54.106)
Panel F: Combined income (EUR)				
Post × Eligible	392.139***	398.201***	462.269***	477.739***
	(24.588)	(24.311)	(54.878)	(54.378)
Observations	553,525		204,155	
Clusters (individuals)	79,075		29,165	
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Time varying controls	No	Yes	No	Yes

Notes. The table reports difference-in-differences estimates of the effect of eligibility for the pension supplement on pension income and labor market outcomes, estimated on the matched samples for women and men. Each coefficient comes from a separate regression of the outcome on an indicator for being eligible in the post reform period. Panel A considers annual pension income including the supplement, Panel B any employment, Panel C formal employment above the mini-job threshold, Panel D marginal employment in mini-jobs only, Panel E annual labor earnings in euros, set to zero for those who do not work, and Panel F combined income, the sum of annual pension income and annual labor earnings. Panels C and D are mutually exclusive and sum to Panel B, and Panels A and E sum to Panel F. Columns (1) and (2) use the matched sample of women and columns (3) and (4) the matched sample of men, both covering 2018 to 2024 as a balanced panel. All columns include individual and year fixed effects. Columns (2) and (4) additionally control for the monthly pension benefit without pension supplement and federal state of residence. Robust standard errors, clustered at the individual level, are shown in parentheses. CEM weights are applied in the estimation. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

Finally, we use the same specification (Equation 1) and examine how the pension reform affects income during retirement, combining *annual pension income* and *annual labor market earnings* during retirement. Figure 8 shows no differential pre-trends in combined income between eligible and non-eligible individuals prior to the reform. Following the reform, the combined income for women strongly increases despite the decline in female labor earnings. Thus, the increase in pension income more than offsets the negative employment and earnings effects. Since employment and earnings effects of men are not significant, the increase in combined income mechanically follows the effects of pension income. Over the full post-reform

period, we find that combined income increases by €392.14 for women and €462.27 for men (Panel F in Table 3), both significant at the 1 percent level.

Figure 8: Dynamic Effects on Combined Income



(a) Women (b) Men

Notes. The figure plots event study estimates of Equation (1) for the effect of eligibility for the pension supplement on annual combined income, separately for women in panel (a) and men in panel (b). Combined income is the sum of annual pension income including the supplement and annual labor earnings. All specifications include individual and year fixed effects. The omitted baseline is 2020, and the vertical line marks the year the supplement took legal effect (2021). 95% confidence intervals shown. Robust standard errors are clustered at the individual level, with 79,075 clusters for women and 29,165 for men. CEM weights are applied in the estimation. The 2021 estimate reflects the entitlement accrued in that year rather than payments actually received in it. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

5.2.3 Heterogeneity

In the following, we focus on heterogeneous effects for women.[25] Figure 9 splits the main event study by region of residence (East versus West Germany) in the last pre-reform year 2020. Figure 9a confirms that both regions experienced a sharp, well-identified increase in pension income. Annual pension income rises by 460 euros for eligible women in the West. The increase in East Germany is lower, at 383 euros. The difference is explained by the regional difference in the amount of the pension supplement conditional on receipt and the share of eligible women receiving the supplement after the income test.[26] The larger average supplement in the West reflects fewer but larger individual payments.

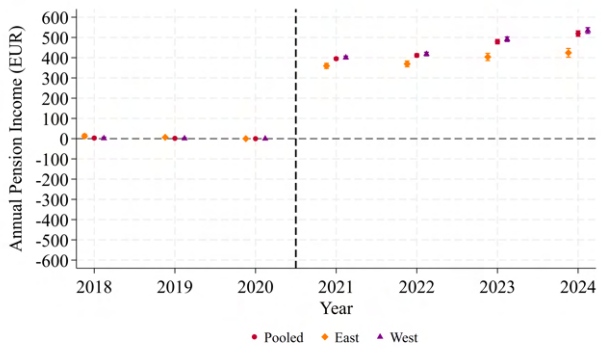
Labor market reactions for employment and earnings outcomes are shown in panels 9b through 9e for the different regions and the pooled German sample. In both East and West Germany, we find negative effects for employment and marginal employment after the reform date. In West Germany, the post-reform decline is a smooth, precisely estimated change from the pre-trend, mirroring the pooled results in Section 5.2.2. In East Germany, the point estimates are smaller but trace a similar downward path, with visibly wider confidence bands, consistent with the

[25] Results for men are insignificant for all subgroups (see Appendix Figure C9).

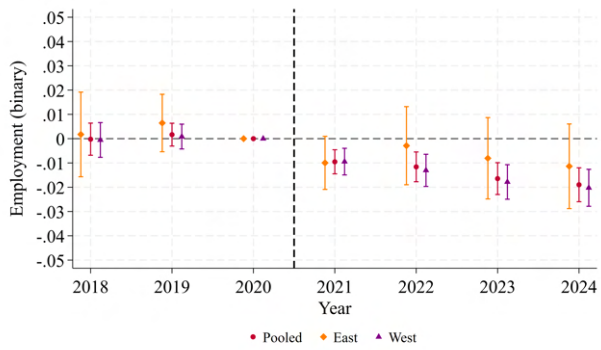
[26] In East Germany, 57 percent of eligible women receive a pension supplement payment in 2022, relative to only 41 percent in the West because of the higher household income in the West. However, conditional on receiving, the supplement itself is markedly smaller in the East, averaging 670 euros against 1,033 euros in the West (not reported in Figure 9).

smaller sample of East Germans. The regression estimates confirm these findings (Table C8 in the Appendix). In West Germany, eligibility reduces employment by 1.52 percentage points, significant at the 1 percent level, which corresponds to an elasticity of -1.55 with respect to pension income. In East Germany, the reduction in employment is 1.08 percentage points and not statistically significant. The same pattern holds for marginal employment. In West Germany, eligibility reduces marginal employment by 1.42 percentage points, significant at the 1 percent level, an elasticity of -1.71. In East Germany, the reduction is 1.31 percentage points, significant only at the 10 percent level. Annual labor earnings fall by 65.7 euros in West Germany, significant at the 5 percent level, with an elasticity of -1.39. In East Germany, the decline is much smaller (7.5 euros) and statistically insignificant. The marginal propensity to earn (MPE) out of pension income in Column (5) of Table C11 and Table C10 is -0.143 in West Germany, significant at the 5 percent level, against a statistically insignificant -0.020 in East Germany, consistent with the smaller earnings response documented above.

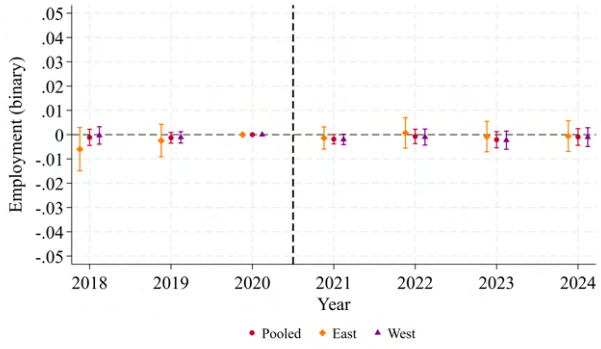
Figure 9: Employment and Income Results for women, by region



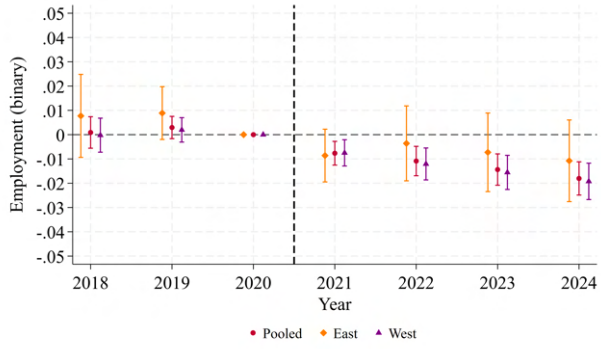
(a) Pension income



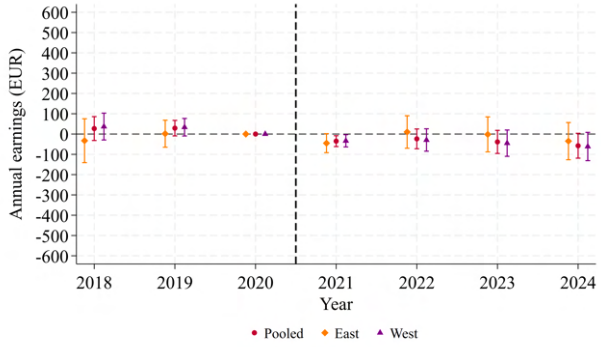
(b) Any employment



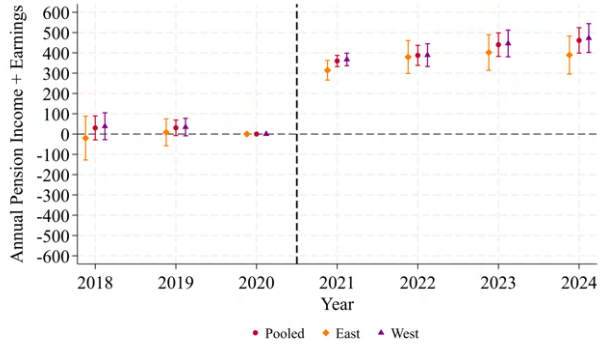
(c) Formal employment



(d) Marginal employment



(e) Annual labor income



(f) Combined income

Notes. The figure plots event study estimates of Equation (1) for women, shown separately for the pooled sample, East Germany and West Germany, with region fixed at residence in 2020. Pension income is the annual gross pension including the supplement. Any employment covers all employment, formal employment covers employment above the mini-job threshold, and marginal employment covers mini-job employment only, so panels (c) and (d) sum to panel (b). Annual labor income is measured in euros and set to zero for women who do not work. Combined income in panel (f) is the sum of pension income and annual labor income. All specifications include individual and year fixed effects and use the matched sample, with 11,284 women in the East and 67,791 in the West. CEM weights are applied in the estimation. 95% confidence intervals shown. Robust standard errors are clustered at the individual level. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

6 Effect on Gender Gap and Welfare Analysis

We have documented that the pension supplement mechanically reduces the average gender pension gap (Figure 3) as the majority of individuals receiving the pension supplement are women. In the following, we focus on the effect on the gender income gap during retirement, including labor earnings of pensioners, and discuss the importance of the estimated behavioral effects. On average, the income gap during retirement in the year 2024 amounts to 34.3% without the supplement and 33.6% with the pension supplement and the observed behavioral response. To understand the role of the behavioral effects, we compare the reduction in the gender gap with and without behavioral changes.[27]

Panel (a) of Figure 10 shows that earnings responses of women only have a moderate effect. Instead of the decrease in the gender income gap from 34.3% to 33.6% that we observe, the gap would have fallen to 33.5% without any earnings response. Thus, the gender income gap would have been reduced by 2.11% without a behavioral response compared to 1.91% with adjustment. The negative earnings response reduces only 9.6% of the redistribution towards women.

Panel (b) shows the comparison with and without estimated earnings reduction by deciles of the gender specific pension distribution.[28] The reduction is strongly hump-shaped. It is negative (the gender income gap increases) at the bottom two deciles, at −2.7% and −1.3%, because many of the lowest-pension women fall short of the 33-year minimum and are therefore ineligible. In contrast, more men benefit at the very low deciles. The largest reductions are concentrated in the middle of the pension distribution, at 5.7% in deciles 4 and 5, and fall to essentially zero from decile 8 upward, where eligibility is rare for either gender. The pattern is very similar with and without estimated earnings responses. At all deciles, the negative earnings responses only moderately affect the change in the gender gap.

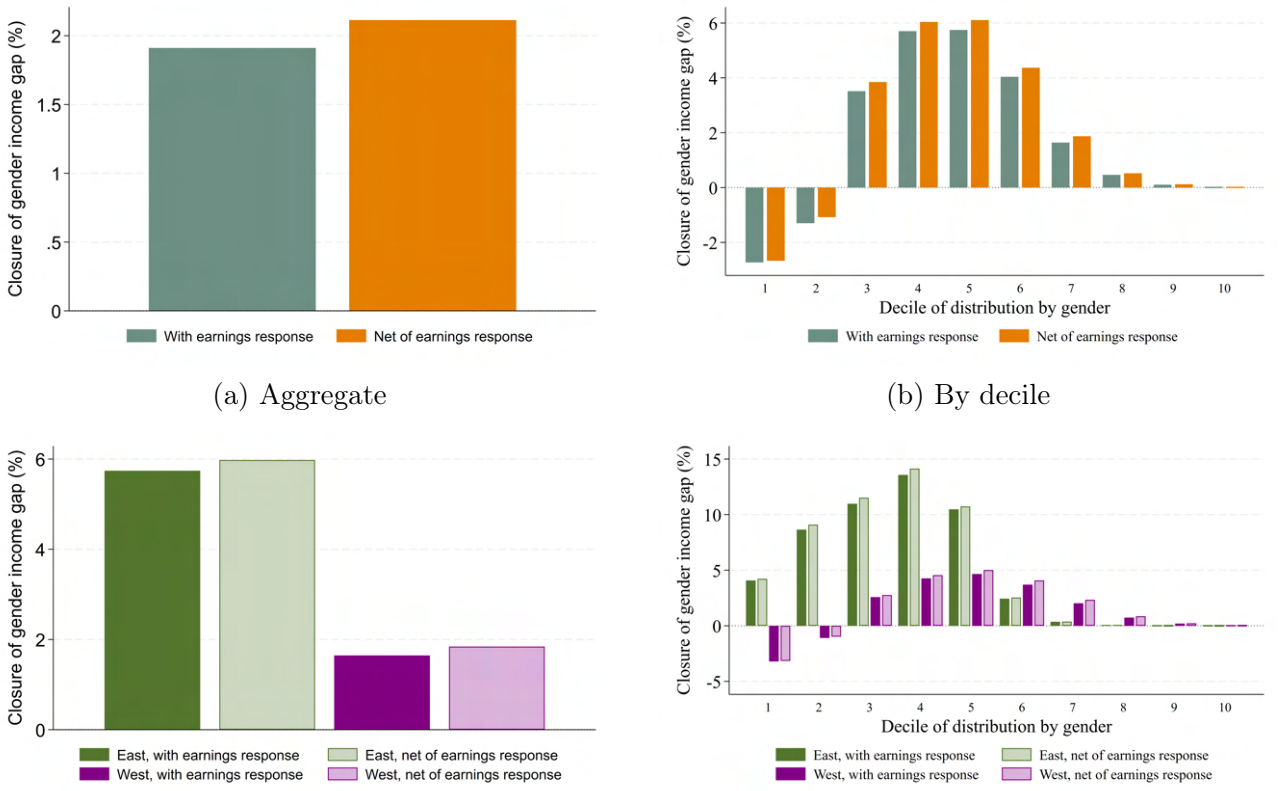
Panels (c) and (d) split the comparison by region. The supplement closes 5.7% of the East German gender income gap once the behavioral response is taken into account, against 6.0% without response. The relative effect in the West is clearly smaller. The supplement closes 1.6% of the gap and 1.8% absent behavior. Behavior therefore reduces only 4.0% of the mechanical closure in the East against 10.7% in the West, consistent with the smaller East German earnings response documented in Section 5.2.2. Across the distribution, the regional difference is even

[27] To calculate the reduction in the gender income gap without behavioral responses, we add the estimated earnings response of pensioners back to the labor earnings of each eligible man and woman, using the event study estimates from Section 5.2.2. As an example, West German female pensioners earn €833.68 per year on average in 2024, averaged over workers and non-workers. For those eligible for the supplement, we add back the estimated effect of €65.75 from Section 5.2.2, taking this to be the earnings they would have had absent a behavioral response. Since 29.3% of West German women are eligible for the supplement, this raises the average annual earnings to €852.96. We apply the corresponding estimate separately by gender and region.

[28] Estimating the earnings response separately within each decile is not feasible, since the number of eligible individuals per decile leaves too little power to identify a response that is small in the aggregate. We therefore apply the average estimate from Section 5.2 uniformly across the distribution, separately by gender and region, which assumes that the earnings response does not vary with the level of pension income.

stronger. The reduction is positive in every East German decile, peaking at 14.1% in decile 4, while in the West it is negative in the bottom two deciles, at −3.2% and −1.0%, comparable to the results of the pooled sample.

Figure 10: Closure of the Gender Income Gap, with and without the Earnings Response



(a) Aggregate

(b) By decile

(c) Aggregate, by region

(d) By decile, by region

Notes. The figure shows the share of the gender gap in combined income closed by the pension supplement, expressed as a percent of the gap that would prevail without it. Combined income is the sum of gross pension income and annual labor earnings. Each panel contrasts two series. The first counts the supplement alone and is therefore purely mechanical. The second additionally removes the estimated labor supply response, by adding back to each eligible individual the earnings change estimated in section 5.2.2, separately by gender and region. Panels (a) and (c) show the closure for the sample as a whole, Panels (b) and (d) by decile of the gender specific pension distribution. Deciles in Panel (d) are computed within region, so the two regions are not directly comparable in levels. Negative values indicate that the supplement widens rather than narrows the gap. Sample as in Section 4, the 2024 cross section of pensioners aged 65 to 75 with at least 240 months of relevant periods. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

Finally, we discuss the welfare effects of the pension supplement using the marginal value of public funds (MVPF) framework of Hendren (2016) and Hendren and Sprung-Keyser (2020). To understand the importance of the income test for the welfare implications of the pension supplement, we calculate the MVPF for two different scenarios. First, for a pension supplement with an income test (Policy A), and second, for a generalized supplement for which eligibility depends only on the individual contribution history and lifetime earnings and not on the income test (Policy B).

The MVPF is the ratio of beneficiaries' willingness to pay for a policy to the net cost of the policy to the government: $MVPF = WTP/(1 + FE + AC)$, where WTP is the willingness to pay, FE is the direct fiscal externality generated by behavioral responses, and AC is the administrative costs of the policy. Intuitively, WTP measures how much individuals would be willing to pay out of their own income for the additional publicly provided resources, while FE captures how labor supply adjustments feed back into the government budget through changes in taxation and social security payments, and AC measures potential administrative costs of the policy.

An important implication of our empirical results is that the true fiscal cost of the pension supplement exceeds the mechanical transfer cost, because higher pension income crowds out labor earnings during retirement for women with an earnings elasticity of -1.33 and creates fiscal externalities (Appendix C.3.2). For men, we do not find significant crowding-out effects.[29] For the calculation of the MVPF, we combine female and male responses with the relevant shares of recipients and derive an average elasticity of -1.06.[30]

To quantify the fiscal externality, we approximate the effective marginal tax and social security contribution rate on labor income, t. The relevant wedge is relatively low in this setting since the entire female employment response operates through marginal employment, which falls by 1.40 percentage points. Marginal earnings are exempt from employee income tax and social insurance contributions, and the only revenue forgone is the employer flat rate levy of approximately 30 percent. Taking $t = 0.30$, the earnings reduction implies a behavioral revenue loss of €13.79 per year. Thus, the fiscal externality amounts to three cents per euro transferred ($FE = 0.031$).[31]

To better understand how the different elements affect the MVPF of a pension supplement, we start with the scenario with an income test but ignore potential administrative costs. Following the standard benchmark in the MVPF literature, we first assume equal social marginal utility over the income distribution and we normalize $WTP = 1$. Thus, all beneficiaries value one euro of additional pension income at one euro. Then, the MVPF is

$$MVPF \;=\; \frac{WTP}{1+FE} \;=\; \frac{1}{1+0.031} \;\approx\; 0.97.$$

The German Pension Insurance reports fairly high administrative costs of the supplement of roughly 18 percent of benefits paid, far above the share for ordinary pension administration, reflecting the complexity of the income test requiring an annual data exchange with the tax

[29] As we have documented that take-up is not affected by the policy (Section 5.1), the reform generates no additional fiscal externalities related to behavioral responses before retirement.

[30] Women make up about 80% of eligible individuals. Since we find no significant earnings response for men, we set their response to zero and weight the two groups by their shares, giving a population elasticity of $0.8 \times (-1.33) = -1.06$ and a population earnings response of $0.8 \times 57.46 = 45.97$ euros per year.

[31] The population-weighted earnings reduction of the supplement is €45.97 per year. Applying the tax and contribution wedge gives $0.30\times$ €45.97 = €13.79 of forgone revenue, which, relative to the population-weighted mechanical transfer of €447.50 per eligible retiree per year, yields $FE = 13.79/447.50 = 0.031$.

authorities for every potentially eligible pension (Bundesregierung, 2023). Applied to the mechanical transfer, this gives $0.18\times$ €447.50 = €80.55 per eligible retiree per year, which is almost six times the behavioral cost. If we account for the administrative costs, the MVPF is strongly reduced and amounts to

$$MVPF = \frac{WTP}{1+FE+AC} = \frac{1}{1+0.031+0.18} \approx 0.83.$$

Welfare conclusions change if we allow for redistributive preferences, that is, once society values a euro reaching a retired household with low resources more than a euro reaching the average household. The MVPF itself is a positive object and does not depend on redistributive preferences. They enter through $\bar{\eta}^j$, the average social marginal utility of income among the beneficiaries of policy j. The effect on social welfare per euro of government spending is then the product $\bar{\eta}^j \cdot MVPF^j$ (Hendren, 2016; Hendren & Sprung-Keyser, 2020). Shifting a euro of spending from one policy to another raises social welfare if and only if the product is larger for the receiving policy. We normalize the social marginal utility of income by its average in the pensioner population.

We measure resources by equivalized annual household income rather than own pension income, since the income test operates at the household level. Because the core administrative data of the pension insurance contain individual information only, we use for this step a subsample of the administrative data matched to the household data of the Socio-Economic Panel (SOEP-RV) and measure equivalized household income $\bar{c}^j$ for the beneficiaries of each policy.[32]

We use a standard CRRA utility function $u(c) = c^{1-\sigma}/(1-\sigma)$, so that the social marginal utility of income of a beneficiary with resources c is $u'(c) = c^{-\sigma}$. Evaluating it at the beneficiary average and normalizing by the population average gives

$$\bar{\eta}^A = \left(\frac{\bar{c}^A}{\bar{c}}\right)^{-\sigma} = \begin{cases} 1, & \sigma = 0, \\ 1.451, & \sigma = 1, \\ 2.106, & \sigma = 2, \end{cases}$$

The left panel of Table 4 presents the result for different values of σ. The social value of a euro spent with $\sigma = 0$ replicates the results of the MVPF if the social marginal utility of income does not vary with income. When the social marginal utility of income is higher for pensioners with lower household income, the social value of a euro spent increases and exceeds 1 for values larger than 0.5. This shows that a pension supplement which is targeted to low-income households

[32] FDZ-RV (2026b), persons aged 66 and over, weighted, matched to administrative pension records. Pensioners actually receiving a payment under the income test have an average equivalized household income of $\bar{c}^A$ = 18,932 euros, and all pensioners eligible on their contribution history average $\bar{c}^B$ = 24,709 euros. The average over all pensioners in the matched sample is $\bar{c}$ = 27,473 euros. Eligible pensioners whose supplement the income test reduces to zero average 29,612 euros, more than pensioners who are not eligible at all. For more detail, see Appendix B.2.

has positive welfare effects even with low redistributive preferences.

To understand the importance of the income test, we calculate the welfare effects of a pension supplement without an income test (Policy B). The administrative costs for the supplement, for which eligibility only depends on the individual contribution history and lifetime earnings, are considerably lower. We approximate them by the ordinary administrative and procedural costs of the statutory pension insurance, which amount to about 1.45 percent of benefits paid (Bundesregierung, 2025), thus in this scenario $AC = 0.0145$.[33] In contrast, the marginal direct fiscal externality does not change ($FE = 0.031$).

In the right panel of Table 4, we present the resulting MVPF. For $\sigma = 0$, the social value of a euro spent equals the MVPF and is 0.96, clearly higher than the corresponding MVPF of the supplement with income test (0.83). The difference in the MVPF is only related to the difference in the administrative costs. The results change when the redistributive preferences increase, because pensioners who are eligible for the supplement based on their individual working and earnings history but would fail the income test have considerably higher household incomes.[34] At $\sigma = 0.5$ the two policies deliver essentially the same social value per euro of public spending, 0.99 with the income test against 1.01 without it, and the two series cross at $\sigma \approx 0.55$. Beyond that point the supplement with an income test delivers the higher social value, and the difference increases the higher the value of σ. For example, the social value of a euro spent of the pension supplement with an income test is 13 percent higher with $\sigma = 1$, 47 percent higher with $\sigma = 2$, and 92 percent higher with $\sigma = 3$. The household income test therefore reduces not only the fiscal costs, as the number of beneficiaries is lower, but also leads to higher welfare if society has redistributive preferences.

[33] This is a lower bound. Even without an income test, entitlement and the size of the supplement still have to be determined for every potentially eligible pension, which requires establishing the qualifying periods and the average earnings position over the assessment periods from the complete insurance record.

[34] On average, annual household income of this group (€29,612) is even larger than the average household income of pensioners who are not eligible irrespective of the income test.

Table 4: Social welfare per euro of government spending

	Policy A: with income test			Policy B: without income test		
σ	$\bar{\eta}^A$	$MVPF^A$	$\bar{\eta}^A \cdot MVPF^A$	$\bar{\eta}^B$	$MVPF^B$	$\bar{\eta}^B \cdot MVPF^B$
0.0	1.000	0.826	0.826	1.000	0.956	0.956
0.5	1.205	0.826	0.995	1.054	0.956	1.009
1.0	1.451	0.826	1.198	1.112	0.956	1.063
1.5	1.748	0.826	1.444	1.172	0.956	1.121
2.0	2.106	0.826	1.739	1.236	0.956	1.182
2.5	2.537	0.826	2.095	1.304	0.956	1.247
3.0	3.056	0.826	2.523	1.374	0.956	1.315

Notes. $\bar{\eta}^j = (\bar{c}/\bar{c}^j)^\sigma$, with $\bar{c}$ = 27,473 euros the average equivalized household income of all pensioners aged 66 and over in the matched sample. With the income test $\bar{c}^A$ = 18,931.93 euros, the average among those actually receiving a payment; without it $\bar{c}^B$ = 24,709.23 euros, the average among all eligible pensioners. Both columns assume $FE = 0.031$, the fiscal externality from the population weighted earnings response with women at 80% of eligible individuals and no response among men; administrative costs are $AC = 0.18$ with the test and $AC = 0.0145$ without it, the latter being net administrative and procedural costs of the statutory pension insurance relative to benefits paid. The two policies cross at $\sigma \approx 0.55$. The final column of each block is the social value of one euro of public spending, measured relative to a euro reaching the average pensioner. *Source.* Bundesregierung (2023, 2025) and FDZ-RV (2026b), own calculations.

7 Conclusion

This paper studies how progressive elements in pension systems affect the gender gap in retirement income, which includes pension income and labor earnings after claiming. Such elements mechanically narrow the gap, since women accumulate lower entitlements, but they can also induce labor supply responses that work in the opposite direction and generate fiscal externalities.

In the empirical analysis, we use administrative microdata from the German Pension Insurance covering the universe of retirees and focus on a progressive pension supplement, which targets pensioners with low pension entitlements and low household income. Despite sizable financial incentives, we find no evidence of behavioral responses during the working period. Individuals do not select into eligibility by manipulating their contribution periods through changes in non-employment or retirement timing. In contrast, we find significant, albeit small, effects on employment and earnings during retirement for women. Female employment falls by 1.46 percentage points, a response concentrated almost entirely in marginal employment. The earnings response corresponds to an earnings elasticity of -1.33 and a marginal propensity to earn out of pension income of -0.13. For men, all effects are insignificant.

Combining mechanical and behavioral effects, we find that the pension supplement reduces the gender gap in total retirement income from 34.3% to 33.6% in 2024, a reduction of 1.9% of the initial gap. Women's behavioral earnings responses have little effect on this reduction. The effects vary considerably across the income distribution and between regions. Because many women at the bottom of the distribution are not eligible for the supplement, the largest reduction, 5.7%, occurs around the middle of the pension distribution. Although the gender gap

differs substantially between East and West Germany, the supplement reduces it by a similar number of percentage points in each region: 0.64 points in the East and 0.74 points in the West. These reductions amount to 5.7% of the East German gap but just 1.6% of the West German gap.

The fiscal externality from reduced earnings is small, three cents per euro transferred but administrative costs are substantial. These costs largely explain the why the estimated MVPF amounts to 0.83 and is below one. A supplement without an income test would be cheaper to administer and has the higher MVPF. Yet, it delivers less social welfare per euro of public spending once society places even a low weight on redistribution, because the income test directs the transfer to households with resources well below those of other eligible pensioners.

Declaration of generative AI use

During the preparation of this work, the authors used generative AI tools to check grammar and spelling, and to draft and edit manuscript text. The tools were also used to provide comments on and suggest revisions to the Stata analysis code. The authors reviewed and edited the manuscript text and reviewed, implemented, and executed all code themselves. All analysis was conducted within the secure, offline environment of the Research Data Center of the German Pension Insurance. No research data were transmitted to or processed by any AI tool. The authors take full responsibility for the content of the published article.

Data availability

The analysis uses confidential administrative records held by the German Pension Insurance and accessed through its Research Data Center (FDZ-RV). The authors are not permitted to share the data. Researchers may access them by application, subject to the requirements of German social data protection law. All analyses are conducted within the FDZ-RV's secure environment, which is not connected to the internet. The datasets used are cited in the reference list and identified by their persistent identifiers.

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A Institutional Details

A.1 Contribution rules and pathways into retirement

Participation in the statutory scheme is mandatory for dependent employees. The contribution rate is 18.6% of gross earnings up to a legally defined contribution ceiling, shared equally between employees and employers. Entitlement to a regular old-age pension requires a minimum contribution period of five years.

Apart from disability pensions, granted to individuals unable to work more than three hours per day, two early-retirement pathways matter for our analysis. Individuals with at least 35 years of qualifying insurance periods may claim the old-age pension for the long-term insured from age 63, with benefits permanently reduced by 0.3% for each month of claiming before the SRA, implying a maximum reduction of 14.4% once the SRA reaches 67. The same reduction applies to the pension supplement. Since 2014, individuals with at least 45 years of qualifying periods may claim the pension for the especially long-term insured from age 63 without deduction. For cohorts born from 1953 onward, this age is raised in steps of two months per cohort and reaches 65 for those born in 1964 or later (for more details, see Dolls & Krolage, 2023). The definition of qualifying periods differs slightly between the two pathways (Deutsche Rentenversicherung Bund, 2025). This 45-year eligibility threshold produces a pronounced spike in the distribution of qualifying periods, visible in Figure 4, that is unrelated to the reform we study. Finally, women born before 1952 could claim an old-age pension from age 60 under the *Altersrente für Frauen*, a pathway phased out for later cohorts (Geyer & Welteke, 2021). This restriction is why our post-retirement analysis covers only the 1950 and 1951 birth cohorts.

A.2 Pension supplement

A.2.1 Eligibility and the supplement formula

The supplement raises individual entitlements as a function of contribution length and lifetime earnings position. Eligibility requires at least 33 years, or 396 months, of qualifying periods, denoted G_i. Qualifying periods comprise compulsory contributions from employment and self-employment together with credited periods for child-rearing, care, sickness and rehabilitation, irrespective of the income earned. Months of marginal employment for which no pension contributions are paid do not count. Full entitlement is reached at 35 years, or 420 months.

Eligibility further depends on the individual's average lifetime earnings position. Let B_i denote the set of calendar months t that carry at least 0.025 earnings points, corresponding to about 30% of average earnings; these are the assessment periods (*Grundrentenbewertungszeiten*). We denote their number by $n_i = |B_i|$. Since a month may count towards the contribution requirement without clearing the earnings floor, $n_i \leq G_i$. The average monthly earnings

position over the assessment periods,

$$\alpha_i \;=\; \frac{1}{n_i}\sum_{t\in B_i} EP_{it}, \tag{2}$$

determines both eligibility and the size of the supplement. Following Geyer et al. (2020), we state α_i and the ceiling on a monthly basis; multiplying by twelve gives the annual equivalents. Eligibility requires that α_i not exceed a ceiling ω that depends on the length of the qualifying period,

$$\omega(G_i) \;=\; \begin{cases} 0.0334 + (G_i - 396)\times 0.001389, & 396 \le G_i < 420, \\ 0.0667, & G_i \ge 420. \end{cases} \tag{3}$$

Annualized, the ceiling rises linearly from 0.4008 earnings points per year at 33 years to 0.8004 at 35 years. The supplement, expressed in earnings points, is then

$$\beta_i \;=\; 0.875 \cdot \min\left\{\alpha_i,\; \omega(G_i) - \alpha_i\right\}, \qquad \tau_i \;=\; \min\left\{n_i,\; 420\right\}\cdot \beta_i, \tag{4}$$

and the resulting gross annual amount is $12\,\tau_i\,PV_t(1-d_i)$, with PV_t the monthly pension value and d_i the actuarial deduction for early claiming (Geyer et al., 2020). The maximum entitlement is 12.25 earnings points, worth €520.87 per month at the 2026 pension value.[35]

The phase-in has two consequences that matter for the analysis. First, the month at which a given individual first qualifies depends on her own earnings position rather than on a threshold common to everyone,

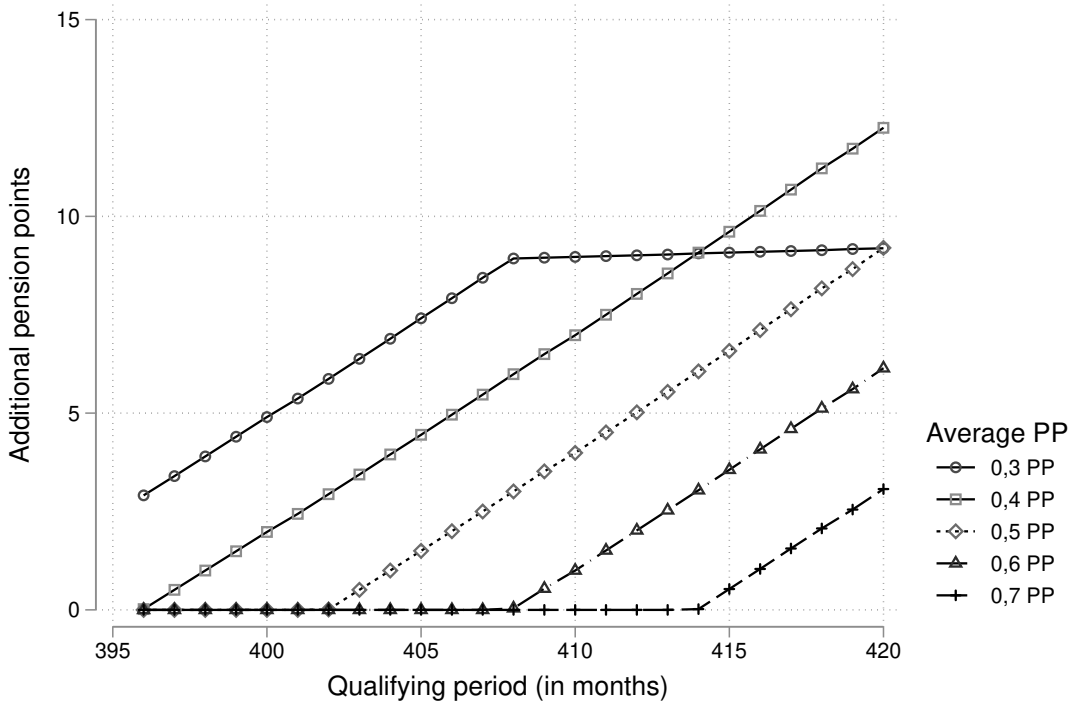
$$m^*(\alpha_i) \;=\; 396 + 24\cdot\frac{12\alpha_i - 0.4008}{0.3996}, \tag{5}$$

so that an individual averaging 0.5 annual earnings points qualifies at 402 months, one at 0.6 at 408 months, and one at 0.7 at 414 months. Only individuals at the very top of the eligible earnings range are constrained by the 420-month threshold. Second, because $\beta_i = 0$ when $\alpha_i = \omega(G_i)$, entry into eligibility at $m^*(\alpha_i)$ is continuous. The supplement begins at zero and rises with each additional qualifying month, so there is no discontinuity in the amount at the point at which most individuals become eligible.

The exception is the lower boundary. At exactly 396 qualifying months, the ceiling is 0.4008, so an individual whose average earnings position lies between the 0.30 floor and that ceiling moves discontinuously from no supplement to one worth $(0.4008 - 12\alpha_i)\times 0.875 \times 33$ earnings points. This jump is largest, at 2.91 earnings points or roughly €123 per month at the 2026 pension value, for an individual at the floor itself, and it shrinks to zero as the earnings position approaches the ceiling. A second discontinuity operates in the earnings dimension. Months below 0.025 earnings points do not count as assessment periods at all, so an individual averaging 0.29 annual earnings points receives nothing while one averaging 0.30 receives the jump just described.

[35] PV_t is indexed to wage growth and adjusted annually on 1 July. Euro amounts in this appendix are stated at the pension value of the year noted.

Figure A1: The pension supplement schedule



Notes. The figure plots the supplement in earnings points against the length of the qualifying period, separately by average earnings position, assuming that qualifying and assessment periods coincide. Each line begins at the month $m^*(\alpha_i)$ at which that earnings position first clears the ceiling, at a value of zero, with the exception of the lowest line, which begins at 396 months at 2.91 earnings points. Beyond 420 months, the supplement is constant. *Source.* Geyer et al. (2020), Figure 2.

A.2.2 Computing the supplement: an example

Consider an individual with 45 years of qualifying periods, of which the first 20 years were spent in employment paying less than 0.025 earnings points per month and the last 25 years above that threshold. Only the final 25 years count as assessment periods. Suppose the average monthly earnings position over those periods was $\alpha_i = 0.05$, so that $12\alpha_i = 0.6$ on an annual basis. Since this lies between 0.4 and 0.8, the relevant annual basis is $0.8 - 0.6 = 0.2$ earnings points, reduced by 12.5% and applied over $\min\{25, 35\} = 25$ years of assessment periods. The individual therefore receives $0.2 \times 0.875 \times 25 = 4.375$ additional earnings points. At a monthly pension value of €37.60 (the 2024 value), this corresponds to a gross annual supplement of about €1,974 before the income test, or roughly €165 per month.

A.2.3 Employment before retirement and the supplement

These rules imply several channels through which employment in the years before retirement affects the supplement, none of them large. Continued contributory employment adds to both the qualifying period G_i and the number of assessment months n_i, and for anyone short of 420 months on either count this loosens the ceiling and raises the entitlement. Its effect on the earnings position is ambiguous, since an additional month enters both the numerator and the denominator of α_i and raises it only where that month's earnings exceed the individual's own lifetime average. Marginal employment leaves α_i unchanged, since a mini-job at the statutory ceiling corresponds to roughly 0.011 earnings points per month and so falls below the 0.025 floor, while still counting towards G_i where pension contributions are paid.

A.2.4 The income test

The assessment basis for the income test is defined in § 97a(2) SGB VI. It comprises taxable income within the meaning of § 2(2), (3) and (5a) EStG, tax-exempt pension and benefit income, and capital income to the extent that it exceeds the saver's lump-sum allowance. Income from marginal employment is taxed at a flat rate by the employer and is not part of taxable income within the meaning of § 2 EStG; it therefore does not enter the assessment basis. The supplement itself is likewise excluded.

The thresholds are defined as multiples of the current pension value and are consequently indexed annually. Income between the lower and upper threshold is credited at 60%, and income above the upper threshold at 100%. Because taxable income is finally assessed by the tax authorities with a delay, and because entitlements for the following calendar year are determined in September, the assessment in practice draws on income from three years earlier rather than from the current year.

A.3 Earnings limits for pensioners below the statutory retirement age

Pensioners who have reached the SRA faced no earnings limit at any point during our observation period. Pensioners claiming before the SRA were historically subject to an annual limit of €6,300, with income above it credited against the pension at 40%. This limit was raised temporarily to €44,590 for 2020 and €46,060 for 2021 and 2022, and was abolished entirely from 2023. These changes do not affect our estimates. The post-retirement sample consists of the 1950 and 1951 birth cohorts, who are past the SRA throughout 2018–2024 and therefore face no earnings limit in any year of the panel.

A.4 Related provisions and the timing of the reform

Two further elements of the reform package are relevant but do not harm our identification strategy. First, the same legislation introduced an allowance for pension income in means-tested old-age assistance (§ 82a SGB XII), which exempts part of the statutory pension from being credited against *Grundsicherung im Alter*. The allowance requires the same 33 years of *Grundrentenzeiten* as the supplement itself and therefore applies to our treatment group as well as to the part of the control group that meets the contribution requirement. Receipt of *Grundsicherung* is not observed in our data. However, the share of pensioners receiving *Grundsicherung* is very low, at below 3%. Second, the ceiling on marginal employment rose during the treatment window. Because employment status is recorded directly in the administrative data rather than derived from reported earnings, these changes do not reclassify individuals between the marginal and formal categories. They do, however, raise the earnings attainable within marginal employment from October 2022 onwards, which bears on the level of annual labor earnings rather than on the employment status itself.

Table A1 collects the dated provisions that bear on the analysis. The supplement was granted automatically, with the pension insurance checking eligibility, computing the entitlement, and applying the income test without an application by the individual. Newly retiring individuals received payments from July 2021; individuals already retired had their entitlements reviewed retroactively and received first payments at varying points during 2022, with full phase-in reached in 2023 and arrears paid out. This staggered roll-out is the reason we treat 2021 as a partially treated year in the event studies that follow.

Table A1: Dated provisions relevant to the analysis

Date	Provision	Relevance
Jan 2021	Supplement enters into force	Reform year; treatment defined from 2021
Jan 2021	§ 82a SGB XII pension allowance	Co-treatment; requires 33 years of *Grundrentenzeiten*
Jul 2021	First payments to new retirees	
During 2022	Retroactive review and first payments to existing retirees	2021–22 partially treated; arrears paid
2023	Supplement fully phased in	2023–24 fully treated
Oct 2022	Mini-job ceiling €450 → €520	Raises earnings attainable within marginal employment; status is observed directly and is not reclassified
Jan 2024	Mini-job ceiling €520 → €538	As above
2020–2022	Earnings limit below the SRA temporarily raised	Does not bind in our samples
2023	Earnings limit below the SRA abolished	Does not bind in our samples

B Datasets

B.1 Administrative data

We draw on four datasets which can be linked using an individual identifier.

The Pensions in Payment Statistics (Versichertenrentenbestand, OSV.RTBN) cover the universe of individuals receiving statutory pension benefits in each reporting year. The data provide pension type, monthly benefit levels, pension start dates, and key demographic characteristics, including credited child-rearing periods and federal state. They further identify recipients of the basic pension supplement and report total pension income at the individual level. The data are organized at the individual level and contain no household or family linkages, which precludes any analysis of intra-household interactions or joint retirement decisions.

The New Insured Pension Awards Statistics (Versichertenrentenzugang, OSV.RTZN) contain similar information but capture individuals only once, at the point their pension claim newly enters into payment. We observe everyone who enters retirement in a given year, along with the pension type claimed, the pathway into retirement, and the pension start date.

The Active Insurance Statistics (Aktiv Versicherte, OSV.AKVS) cover all individuals who are actively insured in a given calendar year and provide detailed information on employment status, employment type, and earnings. Reliable indicators of marginal employment for individuals above the statutory retirement age are available only from 2018 onward, which is why our observation window runs from 2018 to 2024.

Finally, we draw on completed contribution histories from the insurance account clarifications (Vollendete Versichertenleben, OSV.VVL). The number of qualifying contribution months relevant for the pension supplement is directly observed in the RTBN for individuals who receive the supplement, but not for non-recipients. Since qualifying periods and average earnings are exactly what we need to construct a comparison group with similar contribution profiles, we reconstruct complete qualifying histories for all individuals from the monthly records in the VVL.[36] The VVL is available only for individuals who retired from 2013 onward, which restricts the cohorts included in our analysis. Earlier cohorts show very low labor market attachment during the observation period, so this restriction is unlikely to affect the substantive interpretation of our results.

For the analysis of labor supply before retirement, which we report in Appendix D, we use individuals born in 1958 or 1959 who retired in 2024, observed annually from 2018 to 2023. Contribution histories are complete only once the insurance record closes at retirement, which is the reason for this narrow window. Since these women had not yet retired when the reform was introduced, we project eligibility from the contribution record as of 2020. After matching, the sample contains 14,838 women and 89,028 person-year observations. The men's pre-retirement matched sample is 2,900 individuals and 17,400 person-year observations, over six years from 2018 to 2023.

B.2 The linked SOEP-RV data

The core administrative data are recorded at the individual level and contain no household identifiers, so they cannot be used to measure the household income on which the income test operates. For the distributional welfare analysis in Section 6, we therefore use SOEP-RV, which links the survey data of the German Socio-Economic Panel to the administrative records of the German Pension Insurance for those respondents who consented in writing to the linkage and provided their social security number.[37] The linkage gives us, for the same individual, the administrative record of eligibility and of the supplement actually paid, and the survey record of household composition and household income.

We restrict the sample to persons aged 66 and over in 2023 with a successful linkage and a non-missing basic pension record, which leaves 1,504 individuals. Equivalized household income is post-government household income divided by the modified OECD equivalence scale, and all statistics are weighted with the SOEP cross-sectional weights. We distinguish three groups using the administrative supplement variable. Individuals with a positive supplement

[36] The VVL captures the main components of contribution histories, but the exact eligibility thresholds cannot be reconstructed for the control group, because the underlying calculation depends on administrative information we do not fully observe. Our constructed qualifying periods and average earnings are therefore approximations.

[37] We use SOEP-Core v41 together with SUF.SOEP-RV.RTBN 2023, version 2.0 (FDZ-RV, 2026b; Goebel et al., 2019). The pension portfolio component records all pensions in payment on 31 December of the reporting year. Version 2.0 is the first to include the basic pension variables, namely the supplement itself, its qualifying periods and assessment periods, which is what makes this analysis possible.

are recipients, individuals with an entitlement before the income test are eligible individuals, and individuals coded as having no entitlement are not eligible.

Table B1 shows that the SOEP-RV sample is fairly comparable with the full administrative population FDZ-RV (2026a). Importantly, the average monthly payments in the two samples, both for the eligible and the non-eligible hardly differ. Moreover, the share of pensioners who are eligible and who actually receive a payment is very similar. The share of women is lower in SOEP-RV than in the administrative population. This reflects the small number of cases rather than a systematic difference. Given the small sample size, we use the SOEP-RV data only to measure averages of household income not separated by gender, and we do not perform any estimation based on this sample.

Table B1: SOEP-RV compared with the administrative population, 2023

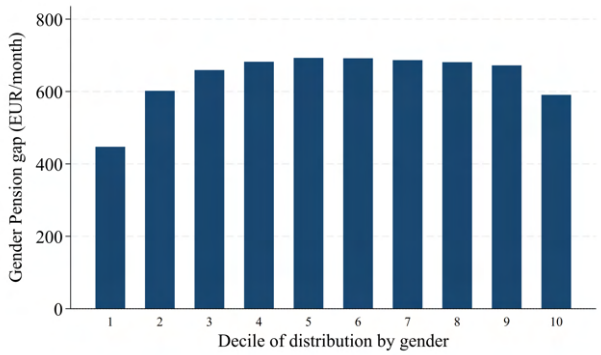
	SOEP-RV	Administrative population
Mean monthly pension, eligible (€)	982	1,006
Mean monthly pension, not eligible (€)	1,234	1,203
Share eligible (%)	10.2	10.6
Share receiving a supplement (%)	4.8	5.4
Take-up among the eligible (%)	46.8	50.9
Women as a share of recipients (%)	66.7	76.5
Observations	1,504	26,010,129

Notes. SOEP-RV column: persons aged 66 and over in 2023 with a successful linkage and a non-missing basic pension record, weighted with SOEP cross-sectional weights. Administrative column: full population pension data. Eligibility and receipt are measured from the administrative supplement variable in both sources. Mean monthly pension is the pension payment excluding the supplement. *Source.* FDZ-RV (2026b, 2026c), own calculations.

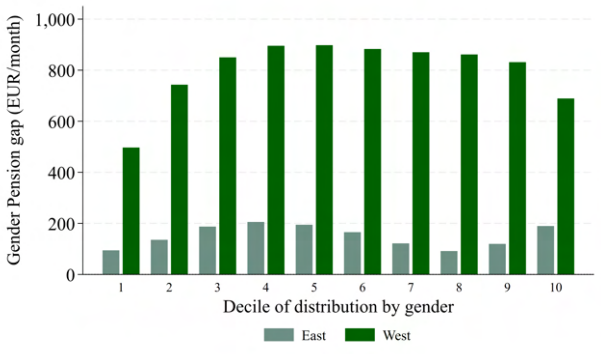
C Additional Results

C.1 Gender Pension Gaps

Figure C1: The Gender Pension Gap in Euros across the Pension Distribution



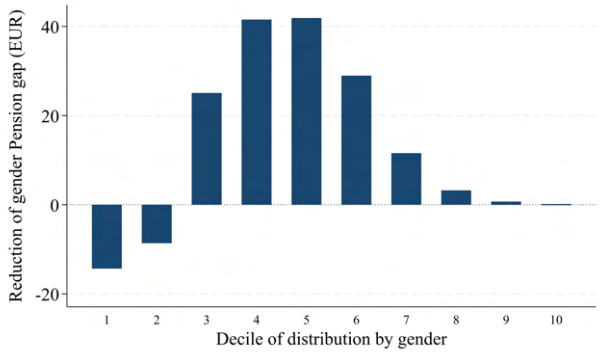
(a) Gender pension gap by decile



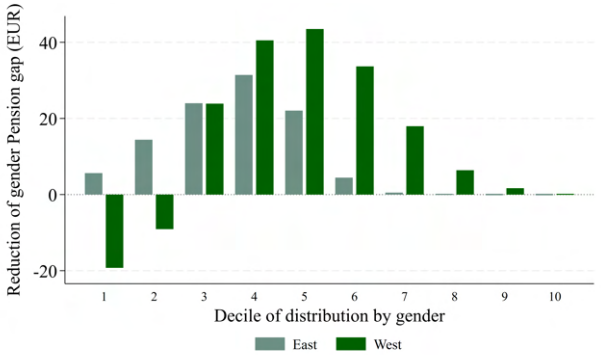
(b) Gender pension gap by decile, by region

Notes. Gender gap in average monthly gross pension income (€) by decile of the gender specific pension distribution, before the supplement. This is the euro counterpart to Figure 1b, which expresses the same gap as a percent of men's pension. Panel (a) pools regions. Panel (b) splits by region, with deciles computed within region and gender, so the two panels are not directly comparable. Analysis sample for 2024, ages 65 to 75, at least 20 years of contribution periods, $N = 3{,}996{,}067$ (West $N = 3{,}012{,}903$, East $N = 983{,}164$). *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

Figure C2: Effect of the Pension Supplement on the Gender Pension Gap in Euros



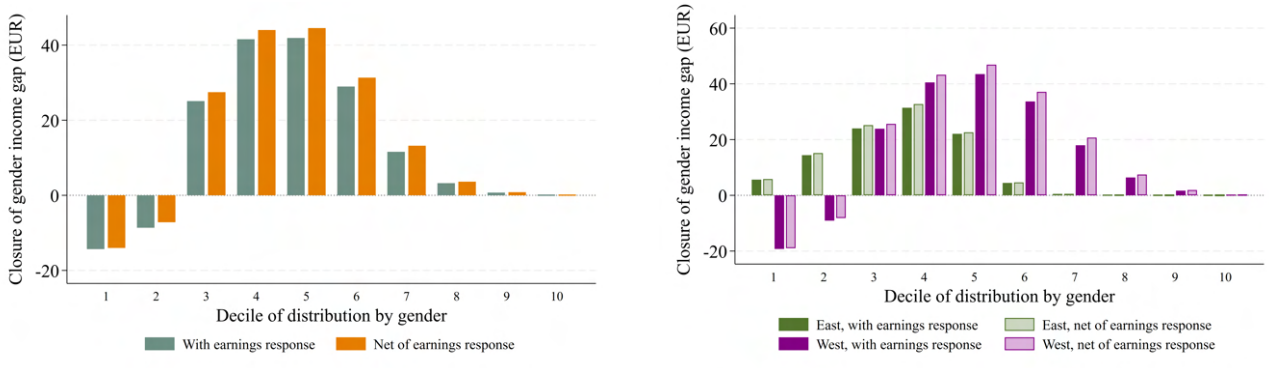
(a) Change in the gender pension gap by decile



(b) Change in the gender pension gap by decile, by region

Notes. Change in the gender pension gap in euros per month by decile of the gender specific pension distribution, measured as the pre supplement gap minus the post supplement gap, so that positive values indicate that the supplement narrows the gap and negative values indicate that it widens the gap. This is the euro counterpart to Figure 3b, which expresses the same change as a percent of the pre supplement gap. Panel (a) pools regions. Panel (b) splits by region, with deciles computed within region and gender, so the two panels are not directly comparable. Analysis sample for 2024, ages 65 to 75, $N = 3{,}996{,}067$. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

Figure C3: Closure of the Gender Income Gap in Euros, with and without the Earnings Response



(a) By decile

(b) By decile, by region

Notes. Euro counterpart to Figure 10. The figure shows the amount of the gender gap in combined income closed by the pension supplement, in euros per month. Combined income is the sum of gross pension income and annual labor earnings. Each panel contrasts the mechanical effect of the supplement alone with the effect net of the estimated labor supply response, which adds back to each eligible individual the earnings change estimated in Section 5.2.2, separately by gender and region. Deciles are of the gender specific pension distribution and, in Panel (b), are computed within region and gender. Negative values indicate that the supplement widens rather than narrows the gap. Sample as in Section 4, the 2024 cross section of pensioners aged 65 to 75 with at least 240 months of relevant periods. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

C.2 Take-up and Selection into Eligibility

Table C1: Manipulation Test at the 33 Year Threshold, Women, All Specifications

	Triangular kernel			Uniform kernel		
Entry year	$p=1,\ q=2$	$p=2,\ q=3$	$p=3,\ q=4$	$p=1,\ q=2$	$p=2,\ q=3$	$p=3,\ q=4$
2018	−1.221	−1.094	0.690	−0.927	−1.127	−0.314
	(0.222)	(0.274)	(0.490)	(0.354)	(0.260)	(0.753)
2019	0.252	0.537	0.063	0.138	0.314	0.614
	(0.801)	(0.592)	(0.950)	(0.890)	(0.754)	(0.539)
2020	0.597	1.550	0.826	−0.619	1.618	1.246
	(0.551)	(0.121)	(0.409)	(0.536)	(0.106)	(0.213)
2021	−0.559	−0.072	−0.002	−0.730	−0.176	0.233
	(0.576)	(0.943)	(0.998)	(0.466)	(0.860)	(0.816)
2022	1.253	2.052**	1.063	0.484	1.720*	1.984**
	(0.210)	(0.040)	(0.287)	(0.628)	(0.085)	(0.047)
2023	0.962	−1.395	−2.125**	1.855*	0.031	−2.454**
	(0.336)	(0.163)	(0.034)	(0.064)	(0.975)	(0.014)
2024	−0.677	0.247	1.024	−0.856	−0.438	0.836
	(0.499)	(0.805)	(0.306)	(0.392)	(0.661)	(0.403)

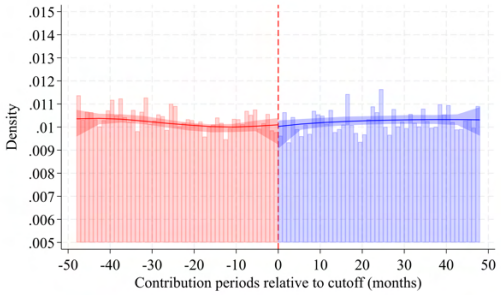
Notes. Robust t statistics from the manipulation test of Cattaneo et al. (2020), run separately for each entry year, polynomial order and kernel, with p values in parentheses. The running variable is qualifying contribution periods relative to the cutoff, here 396 months for partial eligibility. Each column reports one combination of local polynomial order p and bias correction order q. The specification $p=2$, $q=3$ with the triangular kernel is the main specification reported in Table 1. The sample is restricted to female old age pension entries within 48 months of the cutoff, and the bandwidth is selected within that window by the default data driven procedure, separately on each side. A positive statistic indicates excess mass above the cutoff. Sample sizes range from 52,070 to 63,827 entries per year. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

Table C2: Manipulation Test at the 35 Year Threshold, Women, All Specifications

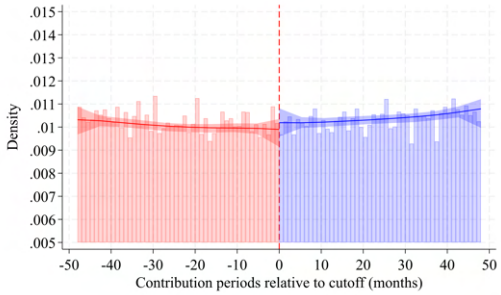
	Triangular kernel			Uniform kernel		
Entry year	$p=1,\ q=2$	$p=2,\ q=3$	$p=3,\ q=4$	$p=1,\ q=2$	$p=2,\ q=3$	$p=3,\ q=4$
2018	0.132	−0.201	−1.238	0.162	0.926	−0.578
	(0.895)	(0.841)	(0.216)	(0.871)	(0.354)	(0.564)
2019	−0.470	0.608	0.532	−0.231	0.225	0.306
	(0.638)	(0.543)	(0.595)	(0.817)	(0.822)	(0.759)
2020	−0.917	−0.598	0.304	−0.530	−1.131	0.229
	(0.359)	(0.550)	(0.761)	(0.596)	(0.258)	(0.819)
2021	−0.441	1.145	1.649*	−1.107	0.173	1.891*
	(0.659)	(0.252)	(0.099)	(0.268)	(0.863)	(0.059)
2022	0.069	−0.688	0.728	0.957	−0.840	−0.640
	(0.945)	(0.491)	(0.466)	(0.339)	(0.401)	(0.522)
2023	1.318	0.602	−1.475	0.761	1.782*	−0.662
	(0.188)	(0.547)	(0.140)	(0.447)	(0.075)	(0.508)
2024	−1.206	−0.644	−0.247	−1.275	−0.820	−0.269
	(0.228)	(0.519)	(0.805)	(0.202)	(0.412)	(0.788)

Notes. Robust t statistics from the manipulation test of Cattaneo et al. (2020), run separately for each entry year, polynomial order and kernel, with p values in parentheses. The running variable is qualifying contribution periods relative to the cutoff, here 420 months for full eligibility. Each column reports one combination of local polynomial order p and bias correction order q. The specification $p = 2$, $q = 3$ with the triangular kernel is the main specification reported in Table 1. The sample is restricted to female old age pension entries within 48 months of the cutoff, and the bandwidth is selected within that window by the default data driven procedure, separately on each side. A positive statistic indicates excess mass above the cutoff. Sample sizes range from 53,248 to 70,593 entries per year. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

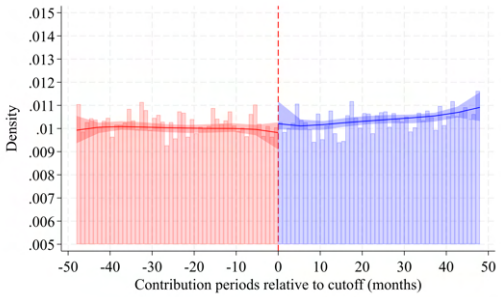
Figure C4: Local Polynomial Density Test at the 33 Year Threshold, by Entry Year



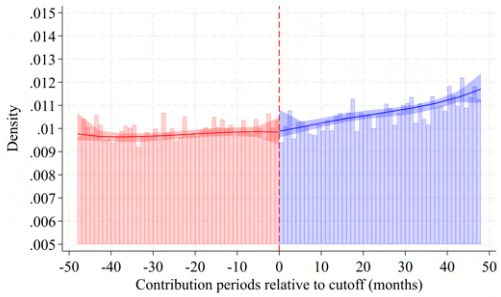
(a) 2018 (pre reform)



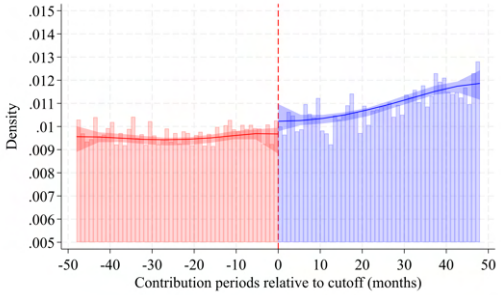
(b) 2019 (pre reform)



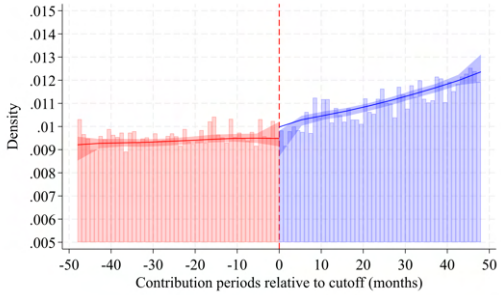
(c) 2020 (pre reform)



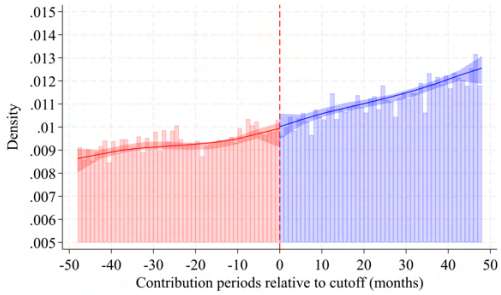
(d) 2021 (reform year)



(e) 2022 (post reform)



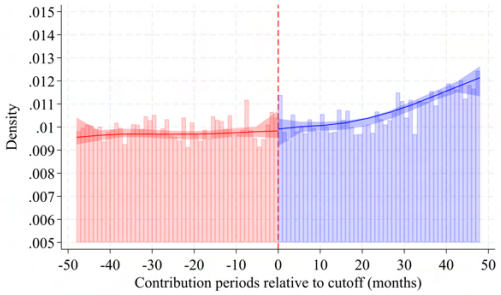
(f) 2023 (post reform)



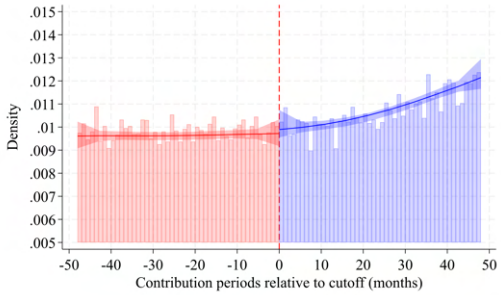
(g) 2024 (post reform)

Notes. Local polynomial density estimates (Cattaneo et al., 2020) of the distribution of pension entries around the 33-year eligibility threshold (396 months), shown as zero on the x-axis, separately by entry year. The running variable is qualifying contribution periods relative to the cutoff. Curves show the estimated density to the left and to the right of the cutoff with shaded 95% bias-corrected confidence bands. Local polynomial order $p = 2$, bias correction order $q = 3$, triangular kernel. The sample is restricted to female old-age pension entries within 48 months of the cutoff, and the bandwidth is selected within that window by the default data-driven procedure, separately on each side. Bars show the binned histogram using one-month bins. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

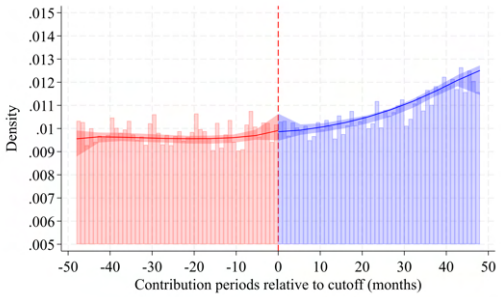
Figure C5: Local Polynomial Density Test at the 35 Year Threshold, by Entry Year



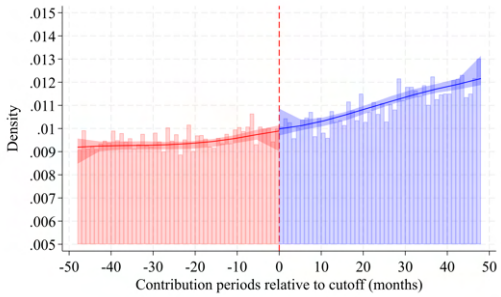
(a) 2018 (pre reform)



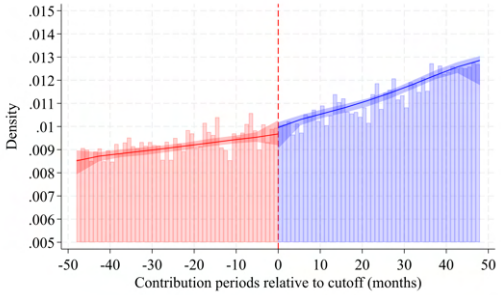
(b) 2019 (pre reform)



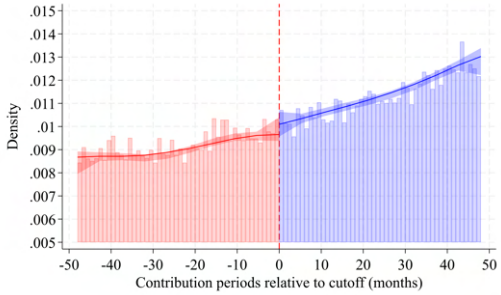
(c) 2020 (pre reform)



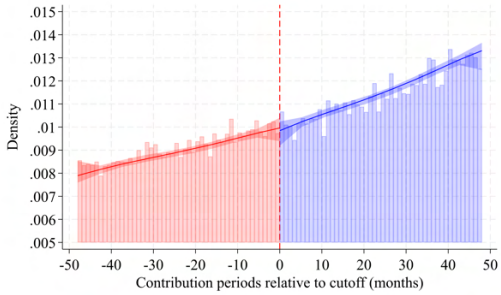
(d) 2021 (reform year)



(e) 2022 (post reform)



(f) 2023 (post reform)



(g) 2024 (post reform)

Notes. Local polynomial density estimates (Cattaneo et al., 2020) of the distribution of pension entries around the 35 year eligibility threshold (420 months), shown as zero on the x-axis, separately by entry year. The running variable is qualifying contribution periods relative to the cutoff. Curves show the estimated density to the left and to the right of the cutoff with shaded 95% bias-corrected confidence bands. Local polynomial order $p = 2$, bias correction order $q = 3$, triangular kernel. The sample is restricted to female old-age pension entries within 48 months of the cutoff, and the bandwidth is selected within that window by the default data-driven procedure, separately on each side. Bars show the binned histogram using one-month bins. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

Table C3: Manipulation Test at the 33 Year Threshold, Men, All Specifications

	Triangular kernel			Uniform kernel		
Entry year	$p=1,\ q=2$	$p=2,\ q=3$	$p=3,\ q=4$	$p=1,\ q=2$	$p=2,\ q=3$	$p=3,\ q=4$
2018	0.684	0.729	−0.544	0.925	0.248	−0.600
	(0.494)	(0.466)	(0.586)	(0.355)	(0.804)	(0.548)
2019	−0.177	−0.165	−1.205	−0.358	−0.092	−0.298
	(0.859)	(0.869)	(0.228)	(0.720)	(0.927)	(0.765)
2020	1.535	0.712	0.682	1.712*	0.750	0.401
	(0.125)	(0.476)	(0.495)	(0.087)	(0.453)	(0.689)
2021	3.128***	3.347***	2.505**	2.465**	3.010***	2.798***
	(0.002)	(0.001)	(0.012)	(0.014)	(0.003)	(0.005)
2022	2.005**	1.154	0.387	1.944*	1.607	0.413
	(0.045)	(0.248)	(0.699)	(0.052)	(0.108)	(0.679)
2023	−0.748	0.144	0.819	−0.880	−0.431	0.456
	(0.455)	(0.885)	(0.413)	(0.379)	(0.667)	(0.648)
2024	0.518	1.641	1.888*	−0.307	1.276	1.685*
	(0.605)	(0.101)	(0.059)	(0.759)	(0.202)	(0.092)

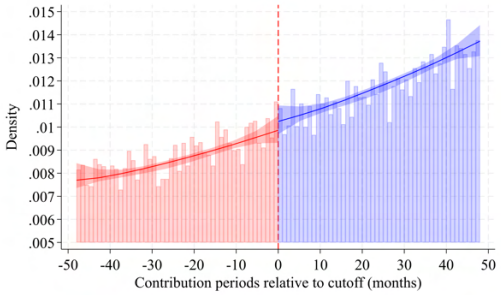
Notes. Robust t statistics from the manipulation test of Cattaneo et al. (2020), run separately for each entry year, polynomial order and kernel, with p values in parentheses. The running variable is qualifying contribution periods relative to the cutoff, here 396 months for partial eligibility. Each column reports one combination of local polynomial order p and bias correction order q. The specification $p=2$, $q=3$ with the triangular kernel is the main specification reported in Table 1. The sample is restricted to male old age pension entries within 48 months of the cutoff, and the bandwidth is selected within that window by the default data driven procedure, separately on each side. A positive statistic indicates excess mass above the cutoff. Sample sizes range from 34,583 to 45,030 entries per year. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

Table C4: Manipulation Test at the 35 Year Threshold, Men, All Specifications

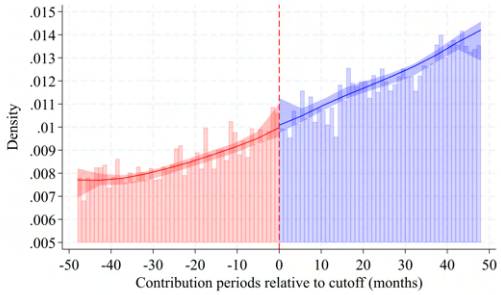
	Triangular kernel			Uniform kernel		
Entry year	$p = 1$, $q = 2$	$p = 2$, $q = 3$	$p = 3$, $q = 4$	$p = 1$, $q = 2$	$p = 2$, $q = 3$	$p = 3$, $q = 4$
2018	−0.896	−1.024	−0.468	−0.342	−1.312	−0.782
	(0.370)	(0.306)	(0.640)	(0.732)	(0.190)	(0.434)
2019	−2.265**	−1.041	0.045	−1.841*	−2.250**	−0.047
	(0.024)	(0.298)	(0.964)	(0.066)	(0.024)	(0.963)
2020	0.428	1.307	1.575	−0.101	0.835	1.332
	(0.668)	(0.191)	(0.115)	(0.920)	(0.404)	(0.183)
2021	2.851***	0.100	−0.343	4.043***	0.864	−0.876
	(0.004)	(0.920)	(0.732)	(< 0.001)	(0.388)	(0.381)
2022	0.196	−0.221	−0.935	0.244	−0.055	−0.697
	(0.845)	(0.825)	(0.350)	(0.807)	(0.956)	(0.486)
2023	−1.077	−0.938	−0.132	−0.851	−1.042	−0.560
	(0.282)	(0.348)	(0.895)	(0.395)	(0.297)	(0.575)
2024	0.478	−0.127	−0.728	0.661	0.024	−0.418
	(0.632)	(0.899)	(0.467)	(0.508)	(0.981)	(0.676)

Notes. Robust t statistics from the manipulation test of Cattaneo et al. (2020), run separately for each entry year, polynomial order and kernel, with p values in parentheses. The running variable is qualifying contribution periods relative to the cutoff, here 420 months for full eligibility. Each column reports one combination of local polynomial order p and bias correction order q. The specification $p = 2$, $q = 3$ with the triangular kernel is the main specification reported in Table 1. The sample is restricted to male old age pension entries within 48 months of the cutoff, and the bandwidth is selected within that window by the default data driven procedure, separately on each side. A positive statistic indicates excess mass above the cutoff. Sample sizes range from 39,946 to 54,395 entries per year. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

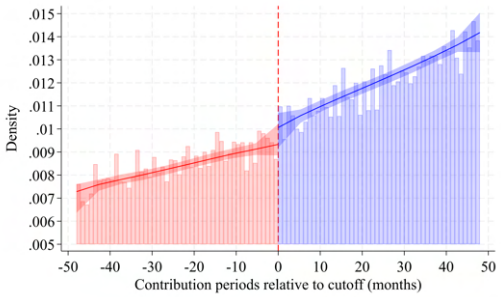
Figure C6: Local Polynomial Density Test at the 33 Year Threshold, Men, by Entry Year



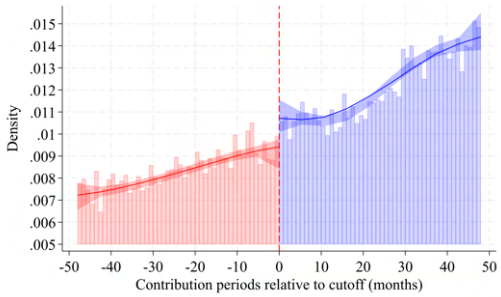
(a) 2018 (pre reform)



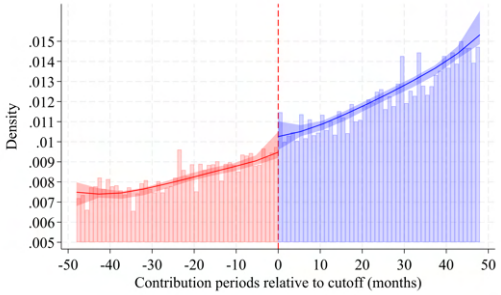
(b) 2019 (pre reform)



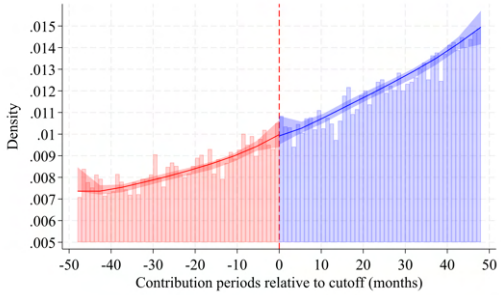
(c) 2020 (pre reform)



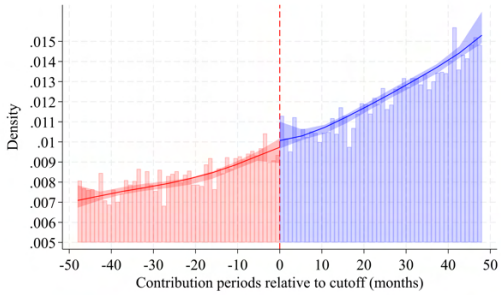
(d) 2021 (reform year)



(e) 2022 (post reform)



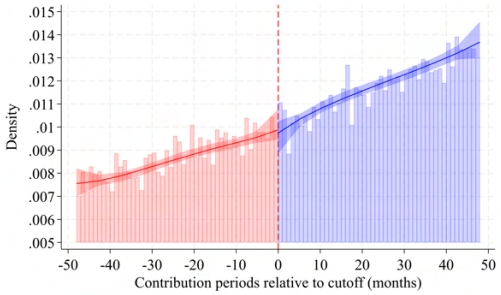
(f) 2023 (post reform)



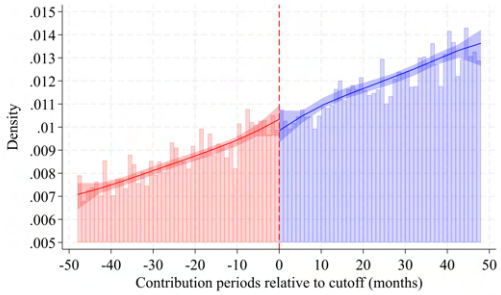
(g) 2024 (post reform)

Notes. Local polynomial density estimates (Cattaneo et al., 2020) of the distribution of pension entries around the 33-year eligibility threshold (396 months), shown as zero on the x-axis, separately by entry year. The running variable is qualifying contribution periods relative to the cutoff. Curves show the estimated density to the left and to the right of the cutoff with shaded 95% bias-corrected confidence bands. Local polynomial order $p = 2$, bias correction order $q = 3$, triangular kernel. The sample is restricted to male old-age pension entries within 48 months of the cutoff, and the bandwidth is selected within that window by the default data-driven procedure, separately on each side. Bars show the binned histogram using one-month bins. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

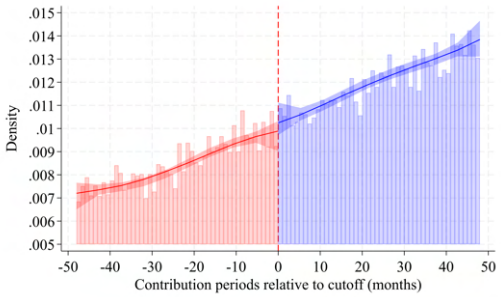
Figure C7: Local Polynomial Density Test at the 35 Year Threshold, Men, by Entry Year



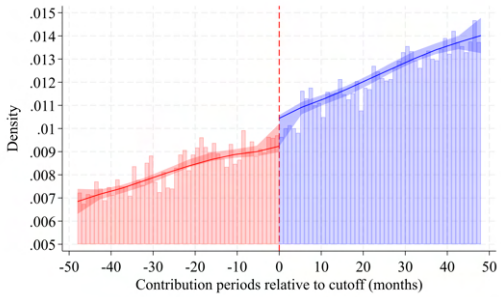
(a) 2018 (pre reform)



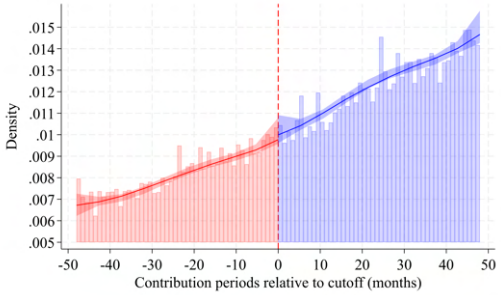
(b) 2019 (pre reform)



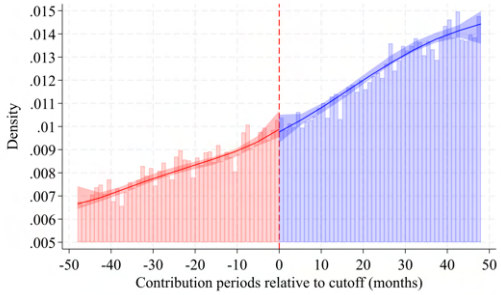
(c) 2020 (pre reform)



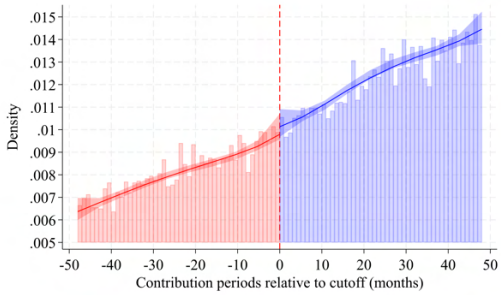
(d) 2021 (reform year)



(e) 2022 (post reform)



(f) 2023 (post reform)



(g) 2024 (post reform)

Notes. Local polynomial density estimates (Cattaneo et al., 2020) of the distribution of pension entries around the 35-year eligibility threshold (420 months), shown as zero on the x-axis, separately by entry year. The running variable is qualifying contribution periods relative to the cutoff. Curves show the estimated density to the left and to the right of the cutoff with shaded 95% bias-corrected confidence bands. Local polynomial order $p = 2$, bias correction order $q = 3$, triangular kernel. The sample is restricted to male old-age pension entries within 48 months of the cutoff, and the bandwidth is selected within that window by the default data-driven procedure, separately on each side. Bars show the binned histogram using one-month bins. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

Table C5: Manipulation Test Results by Region, Men

	33 year threshold (396 months)		35 year threshold (420 months)	
Entry year	Triangular	Uniform	Triangular	Uniform
Panel A. East Germany				
2018	2.448**	0.755	1.128	0.244
2019	0.145	−0.331	−1.455	−1.795*
2020	0.570	0.964	2.660***	2.073**
2021	1.603	1.831*	0.028	1.056
2022	1.481	1.220	0.306	0.532
2023	−0.389	−0.026	−0.727	−0.519
2024	1.489	1.029	−1.065	−1.202
Panel B. West Germany				
2018	−0.645	−0.096	−1.726*	−1.644
2019	−0.512	0.075	−0.439	−1.627
2020	0.519	0.376	0.259	−0.017
2021	2.938***	2.438**	0.098	0.491
2022	0.593	1.192	−0.332	−0.274
2023	0.316	−0.475	−0.693	−0.902
2024	1.115	0.924	0.317	0.543

Notes. Robust t statistics from the manipulation test of Cattaneo et al. (2020) at the two eligibility thresholds, run separately for each entry year, region, threshold and kernel. Polynomial order $p = 2$ and bias correction order $q = 3$ throughout. The sample is restricted to male old age pension entries within 48 months of the relevant cutoff, and the bandwidth is selected within that window by the default data driven procedure, separately on each side. A positive statistic indicates excess mass above the cutoff. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

C.3 Post-Retirement Labor Supply

C.3.1 Descriptive Statistics and Covariate Balance

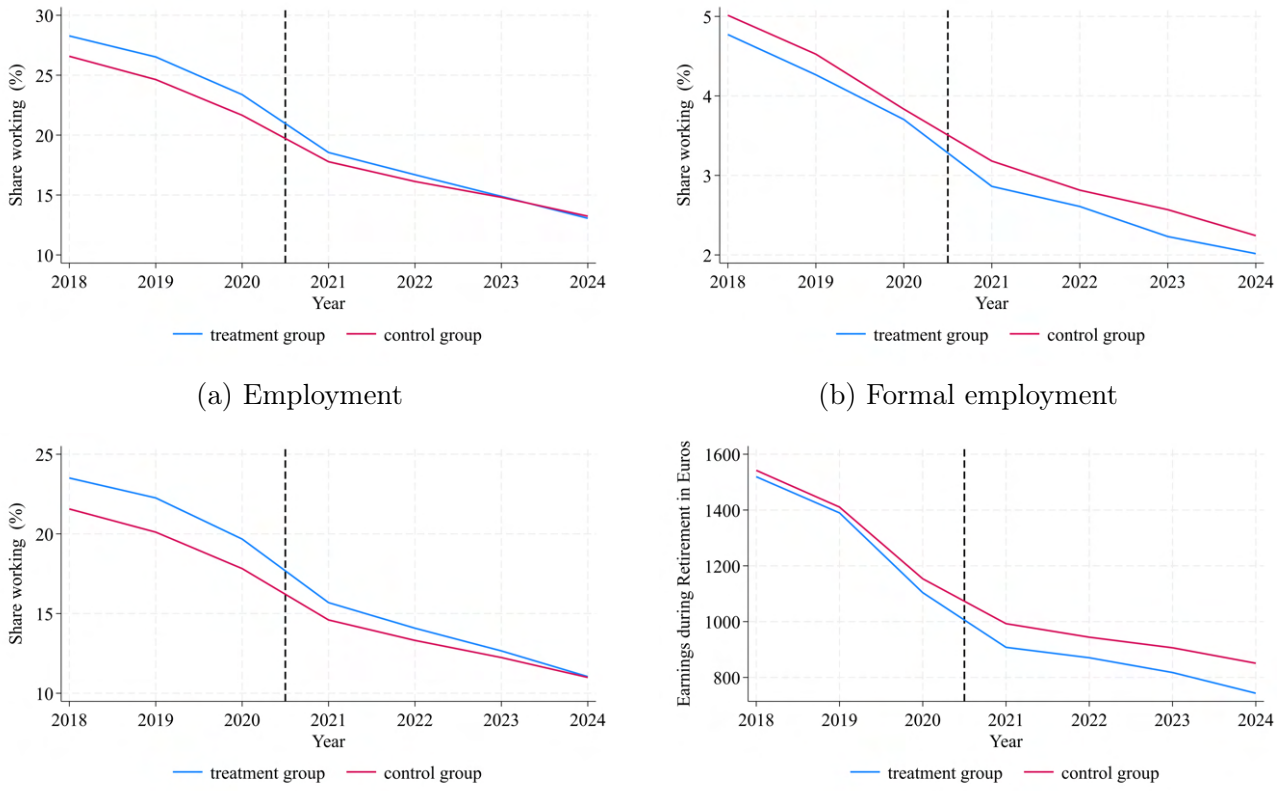
Table C6 reports covariate balance in the matched sample, which comprises 39,128 treated and 39,947 control women, weighted to the treatment strata. Variables matched on exactly are balanced by construction, as are qualifying contribution months and total earnings points, which are matched by decile. Residual imbalance concentrates in average earnings points during the assessment period, at 1.52 standard deviations, which is not a matching variable but is, together with contribution length, one of the two determinants of eligibility, so treated and control women must differ along this dimension. Treated women also hold 22 more months of mandatory contributions, a difference of 0.25 standard deviations. A smaller gap remains in labor market status in 2020, with treated women 1.7 percentage points more likely to be employed, almost entirely in marginal employment. Since identification rests on the difference-in-differences comparison rather than on covariate balance, this residual imbalance does not threaten identification provided the parallel trends assumption holds, which we assess directly in Section 5.2.2.

Table C6: Descriptive Statistics for Matched Sample, Year 2020

Variable	**Treated** (1)	**Control** (2)	**Diff.** (1) - (2)	**Std. Diff.** (1) - (2)
Matching variables				
Female	1.000	1.000	0.000	–
German citizenship	0.995	0.995	0.000	0.000
Age	69.44	69.44	0.000	0.000
Number of children	1.799	1.799	0.000	0.000
Total earnings points	28.003	28.036	-0.033	-0.004
Qualifying contribution months	520.39	517.25	3.139	0.054
Further covariates				
East Germany	0.141	0.141	0.000	0.000
Eligible for pension supplement	1.000	0.000	1.000	–
Ever received a pension supplement	0.559	0.000	0.559	1.592
Avg. monthly earnings points (assessment p.)	0.0579	0.0688	-0.0108	-1.519
Monthly pension benefit (EUR)	977.53	980.58	-3.048	-0.011
Mandatory contribution months	472.41	450.62	21.788	0.247
Employed in year prior to retirement	0.769	0.768	0.0004	0.001
Labor market characteristics, 2020				
Any employment	0.234	0.217	0.017	0.041
Formal employment (above mini-job threshold)	0.037	0.038	-0.001	-0.007
Marginal employment (mini-job only)	0.197	0.178	0.019	0.048
Annual labor earnings (EUR)	1,103.78	1,153.66	-49.88	-0.015
Observations (N)	39,128	39,947		
Weighted observations ($\sum w$)	39,128	39,128		

Notes. The table reports descriptive statistics for eligible (treated) and non eligible (control) women in the matched sample in 2020, the last year before the reform. The matched sample is constructed using coarsened exact matching. Treated women are matched to control women with identical values of gender, German citizenship, birth year, federal state of residence, pension type, year of pension entry and number of children, and with total earnings points and qualifying contribution months grouped into deciles, so the latter two are balanced by decile rather than exactly. Reported means are weighted by the matching weights, with $\sum w = 39{,}128$ in both groups. Diff. reports the raw difference in weighted means. Std. Diff. reports the standardized mean difference, the difference in means divided by the pooled standard deviation of the two groups. It is reported as a dash where both groups have zero variance and the standardized difference is therefore undefined, which applies to gender and to eligibility itself. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

Figure C8: Post Retirement: Descriptive Trends in Labor Market Outcomes by Treatment Status



(a) Employment

(b) Formal employment

(c) Marginal employment

(d) Annual labor income

Notes: The figure shows descriptive labor market outcomes for the treatment and control groups between 2018 and 2024. Employment, formal employment, and marginal employment are binary indicators. Marginal employment refers to mini-jobs. Annual labor income is measured in euros and set to zero for unemployed individuals. All series are computed for the weighted matched sample. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

C.3.2 Instrumental Variables Estimates and Elasticities

This appendix presents the two-stage least squares strategy summarized in Section 5.2.1 in full, together with the corresponding results. We focus on results for women, as only they show a behavioral change in response to the reform.

In the first stage, we instrument log pension income with eligibility for the pension supplement interacted with the post-reform period,

$$\log(\mathrm{PI}_{it}) = \theta_i + \tau_t + \delta\,\mathrm{Post}_t \times D_i + \kappa X_{it} + \varepsilon_{it}, \tag{6}$$

where θ_i and τ_t denote individual and year fixed effects, D_i identifies individuals eligible for the pension supplement, and X_{it} contains time varying individual characteristics. In the second

stage, we relate labor market outcomes to the predicted component of pension income,

$$y_{it} = \alpha_i + \lambda_t + \beta_2 \log(\widehat{\mathrm{PI}}_{it}) + \xi X_{it} + \nu_{it}, \tag{7}$$

where y_{it} denotes employment status or earnings. The coefficient β_2 identifies the semi-elasticity of labor supply with respect to pension income induced by the reform. We additionally estimate a version of this second stage with pension income entering in levels rather than logs, which yields a marginal propensity to earn directly rather than an elasticity.

For the exclusion restriction to hold, eligibility-induced variation in pension income must affect labor market outcomes only through pension income itself. This is plausible in the present context, since eligibility is determined solely by predetermined contribution histories and lifetime earnings and does not alter contemporaneous work incentives, contribution rates, or the marginal return to labor. The reform leaves the pension benefit formula for the underlying pension unchanged and does not affect earnings tests or eligibility conditions for retirement.

Table C7 reports the results. Eligibility induces a statistically significant increase in pension income, with a first-stage F statistic far exceeding conventional thresholds for weak instrument concern in every column. Employment falls significantly in response to higher pension income, an effect concentrated entirely in marginal employment, while formal employment shows no significant response. Earnings decline significantly whether pension income is measured in logs or in levels, and the close correspondence between the two specifications, an elasticity of -1.33 against an implied elasticity of -1.36 from the level specification, indicates that the earnings response is not sensitive to functional form.

Table C7: Main IV results, Women

	(1) Any employment	(2) Formal employment	(3) Mini-job only	(4) Earnings	(5) Earnings
Second stage					
log Pension income	−0.374***	−0.016	−0.359***	−1471.215**	
	(0.0786)	(0.0372)	(0.0762)	(616.582)	
Pension income (EUR)					−0.1278**
					(0.0536)
First stage					
Instrument (z)	0.0391***	0.0391***	0.0391***	0.0391***	449.599***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(5.357)
First-stage F-statistic	10,662.5	10,662.5	10,662.5	10,662.5	7,043.7
Reference values for interpretation (2020, eligible group)					
Employment rate	0.2338	0.0370	0.1968	–	–
Mean annual earnings (EUR)	–	–	–	1,103.78	1,103.78
Mean annual pension income (EUR)	11,730.36	11,730.36	11,730.36	11,730.36	11,730.36
Implied elasticity w.r.t. pension income					
Elasticity	−1.601	−0.422	−1.823	−1.333	−1.358
Observations			553,525		
Clusters (individuals)			79,075		
Year fixed effects			Yes		
Individual fixed effects			Yes		

Notes: The table reports two-stage least squares estimates of the effect of pension income on labor market outcomes. Columns (1)–(3) consider employment outcomes: any employment, formal employment (employment above the mini-job threshold), and marginal employment (mini-job only). Columns (4)–(5) report effects on annual labor earnings. Pension income is instrumented by eligibility for the pension supplement (z). In columns (1)–(4), the endogenous regressor is log annual pension income; in column (5), pension income is measured in annual euros. All specifications include year and individual fixed effects. Robust standard errors are clustered at the individual level and reported in parentheses. Reference values are 2020 means for the eligible group, the last year before treatment. Elasticities are computed by scaling the IV coefficient by the reference employment rate or mean earnings, and for column (5) by additionally scaling by mean pension income. CEM weights are applied in the estimation. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

C.3.3 Heterogeneity

Table C8: Results on Pension Income and Employment, Women, by Region

	East Germany		West Germany	
	(1)	(2)	(3)	(4)
	Baseline DiD	+ Controls	Baseline DiD	+ Controls
Panel A: Pension income (EUR)				
Post × Eligible	382.663***	403.254***	460.329***	464.488***
	(9.051)	(6.784)	(5.548)	(3.840)
Panel B: Any employment				
Post × Eligible	−0.0108	−0.0113	−0.0152***	−0.0153***
	(0.0078)	(0.0078)	(0.0033)	(0.0033)
Panel C: Formal employment (above mini-job threshold)				
Post × Eligible	0.0023	0.0020	−0.0011	−0.0011
	(0.0034)	(0.0034)	(0.0016)	(0.0016)
Panel D: Marginal employment (mini-job only)				
Post × Eligible	−0.0131*	−0.0134*	−0.0142***	−0.0141***
	(0.0073)	(0.0074)	(0.0032)	(0.0033)
Panel E: Annual labor earnings (EUR)				
Post × Eligible	−7.466	−16.021	−65.745**	−66.289**
	(45.144)	(44.980)	(27.037)	(27.037)
Observations	78,988		474,537	
Clusters (individuals)	11,284		67,791	
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Time varying controls	No	Yes	No	Yes

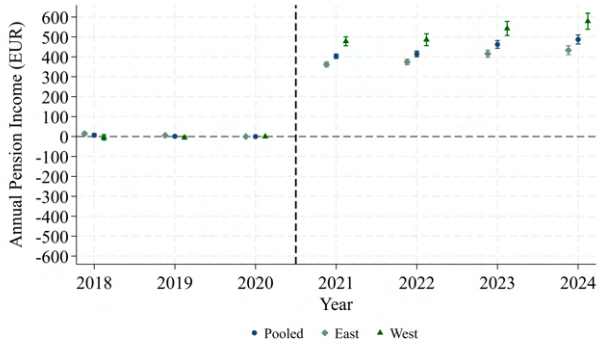
Notes. The table reports difference-in-differences estimates of the effect of eligibility for the pension supplement on pension income and labor market outcomes for women, split by region of residence in 2020. Each coefficient comes from a separate regression of the outcome on an indicator for being eligible in the post reform period. Panel A considers annual pension income including the supplement, Panel B any employment, Panel C formal employment above the mini-job threshold, Panel D marginal employment in mini-jobs only, and Panel E annual labor earnings in euros, set to zero for women who do not work. Panels C and D are mutually exclusive and sum to Panel B. Columns (1) and (2) use the East German subsample and columns (3) and (4) the West German subsample, both covering 2018 to 2024 as a balanced panel. All columns include individual and year fixed effects. Columns (2) and (4) additionally control for the monthly pension benefit without pension supplement and federal state of residence. Robust standard errors, clustered at the individual level, are shown in parentheses. CEM weights are applied in the estimation. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

Table C9: Results on Pension Income and Employment, Men, by Region

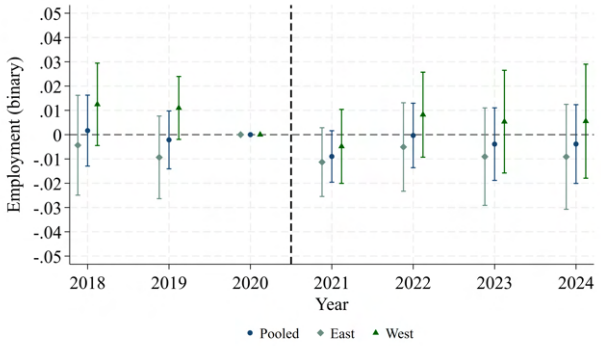
	East Germany		West Germany	
	(1)	(2)	(3)	(4)
	Baseline DiD	+ Controls	Baseline DiD	+ Controls
Panel A: Pension income (EUR)				
Post × Eligible	389.540***	408.928***	524.327***	548.302***
	(8.545)	(5.526)	(15.320)	(11.083)
Panel B: Any employment				
Post × Eligible	−0.0041	−0.0051	−0.0043	−0.0065
	(0.0097)	(0.0097)	(0.0088)	(0.0087)
Panel C: Formal employment (above mini-job threshold)				
Post × Eligible	−0.0042	−0.0043	−0.0054	−0.0062
	(0.0035)	(0.0035)	(0.0054)	(0.0053)
Panel D: Marginal employment (mini-job only)				
Post × Eligible	0.0001	−0.0009	0.0011	−0.0002
	(0.0098)	(0.0099)	(0.0082)	(0.0082)
Panel E: Annual labor earnings (EUR)				
Post × Eligible	25.444	19.242	14.325	−4.592
	(61.258)	(60.663)	(106.640)	(105.433)
Observations	120,253		83,902	
Clusters (individuals)	17,179		11,986	
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Time varying controls	No	Yes	No	Yes

Notes. The table reports difference-in-differences estimates of the effect of eligibility for the pension supplement on pension income and labor market outcomes for men, split by region of residence in 2020. Each coefficient comes from a separate regression of the outcome on an indicator for being eligible in the post reform period. Panel A considers annual pension income including the supplement, Panel B any employment, Panel C formal employment above the mini-job threshold, Panel D marginal employment in mini-jobs only, and Panel E annual labor earnings in euros, set to zero for men who do not work. Panels C and D are mutually exclusive and sum to Panel B. Columns (1) and (2) use the East German subsample and columns (3) and (4) the West German subsample, both covering 2018 to 2024 as a balanced panel. All columns include individual and year fixed effects. Columns (2) and (4) additionally control for the monthly pension benefit without pension supplement and federal state of residence. Robust standard errors, clustered at the individual level, are shown in parentheses. CEM weights are applied in the estimation. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

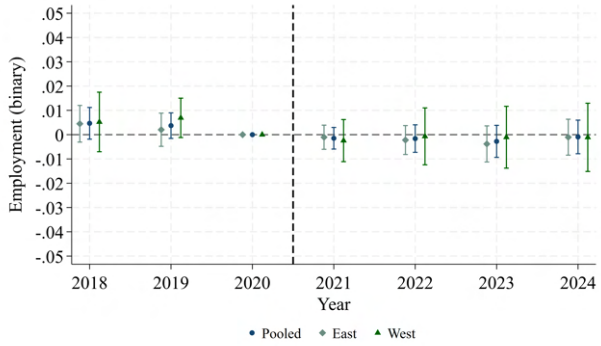
Figure C9: Employment and Income Results for men, by region



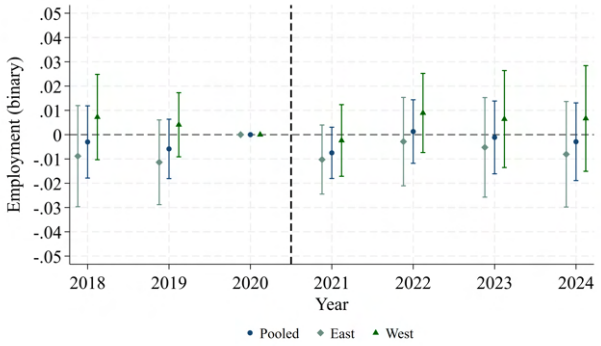
(a) Pension income



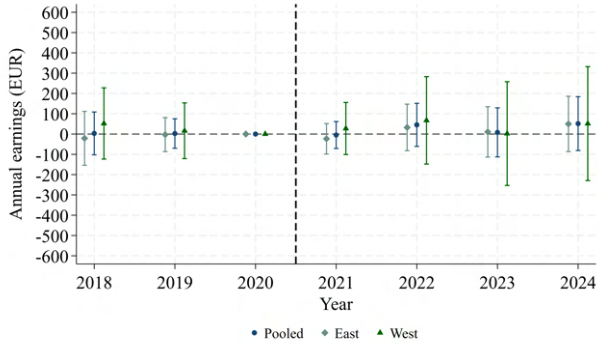
(b) Any employment



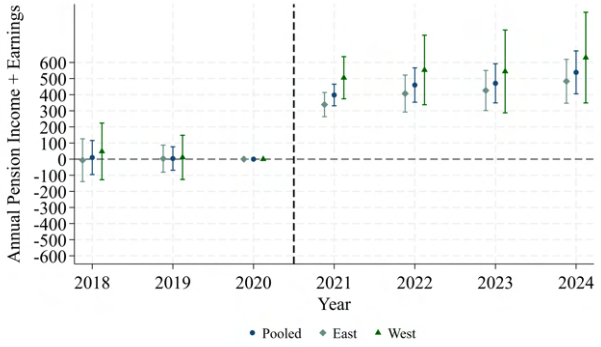
(c) Formal employment



(d) Marginal employment



(e) Annual labor income



(f) Combined income

Notes. The figure plots event study estimates of Equation (1) for men, shown separately for the pooled sample, East Germany and West Germany, with region fixed at residence in 2020. Pension income is the annual gross pension including the supplement. Any employment covers all employment, formal employment covers employment above the mini-job threshold, and marginal employment covers mini-job employment only, so panels (c) and (d) sum to panel (b). Annual labor income is measured in euros and set to zero for men who do not work. Combined income in panel (f) is the sum of pension income and annual labor income. All specifications include individual and year fixed effects and use the matched sample, with 17,179 men in the East and 11,986 in the West. 95% confidence intervals are shown. Robust standard errors are clustered at the individual level. CEM weights are applied in the estimation. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

Table C10: Main IV results, East Germany

	(1) Any employment	(2) Formal employment	(3) Mini-job only	(4) Earnings	(5) Earnings
Second stage					
log Pension income	−0.356	0.075	−0.432*	−246.205	
	(0.2558)	(0.1130)	(0.2419)	(1488.638)	
Pension income (EUR)					−0.0195
					(0.1180)
First stage					
Instrument (z)	0.0303***	0.0303***	0.0303***	0.0303***	382.663***
	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(9.051)
First-stage F-statistic	2,793.4	2,793.4	2,793.4	2,793.4	1,787.5
Reference values for interpretation (2020, eligible group)					
Employment rate	0.1751	0.0258	0.1493	–	–
Mean annual earnings (EUR)	–	–	–	687.68	687.68
Mean annual pension income (EUR)	12,978.04	12,978.04	12,978.04	12,978.04	12,978.04
Implied elasticity w.r.t. pension income					
Elasticity	−2.035	2.915	−2.892	−0.358	−0.368
Observations			78,988		
Clusters (individuals)			11,284		
Year fixed effects			Yes		
Individual fixed effects			Yes		

Notes: The table reports two-stage least squares estimates of the effect of pension income on labor market outcomes for East Germany for women. Columns (1)–(3) consider employment outcomes: any employment, formal employment (employment above the mini-job threshold), and marginal employment (mini-job only). Columns (4)–(5) report effects on annual labor earnings. Pension income is instrumented by eligibility for the pension supplement (z). In columns (1)–(4), the endogenous regressor is log annual pension income; in column (5), pension income is measured in annual euros. All specifications include year and individual fixed effects. Robust standard errors are clustered at the individual level and reported in parentheses. Reference values are 2020 means for the eligible group in East Germany, the last year before treatment. Elasticities are computed by scaling the IV coefficient by the reference employment rate or mean earnings, and for column (5) by additionally scaling by mean pension income. CEM weights are applied in the estimation. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

Table C11: Main IV results, West Germany

	(1) Any employment	(2) Formal employment	(3) Mini-job only	(4) Earnings	(5) Earnings
Second stage					
log Pension income	−0.377***	−0.027	−0.350***	−1624.053**	
	(0.0825)	(0.0395)	(0.0803)	(667.885)	
Pension income (EUR)					−0.1428**
					(0.0587)
First stage					
Instrument (z)	0.0405***	0.0405***	0.0405***	0.0405***	460.329***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(5.548)
First-stage F-statistic	9,239.0	9,239.0	9,239.0	9,239.0	6,884.4
Reference values for interpretation (2020, eligible group)					
Employment rate	0.2435	0.0389	0.2046	–	–
Mean annual earnings (EUR)	–	–	–	1,172.31	1,172.31
Mean annual pension income (EUR)	11,524.87	11,524.87	11,524.87	11,524.87	11,524.87
Implied elasticity w.r.t. pension income					
Elasticity	−1.547	−0.691	−1.710	−1.385	−1.404
Observations			474,537		
Clusters (individuals)			67,791		
Year fixed effects			Yes		
Individual fixed effects			Yes		

Notes: The table reports two-stage least squares estimates of the effect of pension income on labor market outcomes for West German women. Columns (1)–(3) consider employment outcomes: any employment, formal employment (employment above the mini-job threshold), and marginal employment (mini-job only). Columns (4)–(5) report effects on annual labor earnings. Pension income is instrumented by eligibility for the pension supplement (z). In columns (1)–(4), the endogenous regressor is log annual pension income; in column (5), pension income is measured in annual euros. All specifications include year and individual fixed effects. Robust standard errors are clustered at the individual level and reported in parentheses. Reference values are 2020 means for the eligible group in West Germany, the last year before treatment. Elasticities are computed by scaling the IV coefficient by the reference employment rate or mean earnings, and for column (5) by additionally scaling by mean pension income. CEM weights are applied in the estimation. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

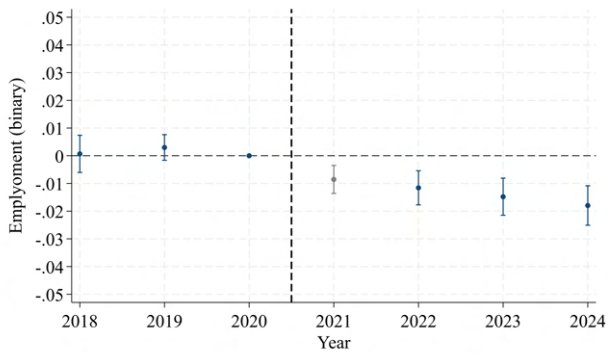
C.3.4 Robustness

Table C12: Robustness: alternative control group bandwidths

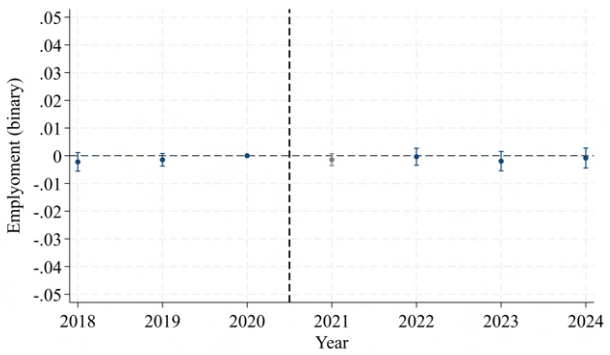
	(1)	(2)	(3)	(4)
	Control band 25–35 years		Control band 30–33 years	
	Baseline DiD	+ Controls	Baseline DiD	+ Controls
Panel A: Pension income (EUR)				
Post × Eligible	446.492***	453.511***	459.700***	459.499***
	(5.305)	(3.403)	(5.618)	(3.486)
Panel B: Any employment				
Post × Eligible	−0.0144***	−0.0145***	−0.0159***	−0.0159***
	(0.0031)	(0.0031)	(0.0029)	(0.0029)
Panel C: Formal employment (above mini-job threshold)				
Post × Eligible	0.0001	0.0001	−0.0020	−0.0020
	(0.0015)	(0.0015)	(0.0015)	(0.0015)
Panel D: Marginal employment (mini-job only)				
Post × Eligible	−0.0145***	−0.0145***	−0.0139***	−0.0139***
	(0.0030)	(0.0030)	(0.0028)	(0.0028)
Panel E: Annual labor earnings (EUR)				
Post × Eligible	−50.041**	−50.803**	−64.544***	−64.282***
	(24.587)	(24.607)	(24.135)	(24.124)
Panel F: Combined income (EUR)				
Post × Eligible	396.450***	402.708***	395.155***	395.218***
	(25.004)	(24.819)	(24.655)	(24.357)
Observations	577,157	577,157	554,183	554,183
Clusters (individuals)	82,451	82,451	79,169	79,169
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	No	Yes

Notes. The table reports difference-in-differences estimates of the effect of eligibility for the pension supplement on pension income and labor market outcomes for women, under two alternative definitions of the control group's qualifying contribution periods. Each coefficient comes from a separate regression of the outcome on an indicator for being eligible in the post-reform period. Panel A considers annual pension income including the supplement, Panel B any employment, Panel C formal employment above the mini-job threshold, Panel D marginal employment in mini-jobs only, Panel E annual labor earnings in euros, set to zero for those who do not work, and Panel F combined income, the sum of annual pension income and annual labor earnings, measured gross of taxes and social insurance contributions. Panels C and D are mutually exclusive and sum to Panel B, and Panels A and E sum to Panel F. Columns (1) and (2) define the control group over qualifying periods of 25 to 35 years, columns (3) and (4) restrict it to 30 to 33 years, immediately below the eligibility threshold. The main specification in Table 3 uses 25 to 33 years. All columns use the matched sample covering 2018 to 2024 as a balanced panel and include individual and year fixed effects. Columns (2) and (4) additionally control for the monthly pension benefit without pension supplement and federal state of residence. Robust standard errors, clustered at the individual level, are shown in parentheses. CEM weights are applied in the estimation. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

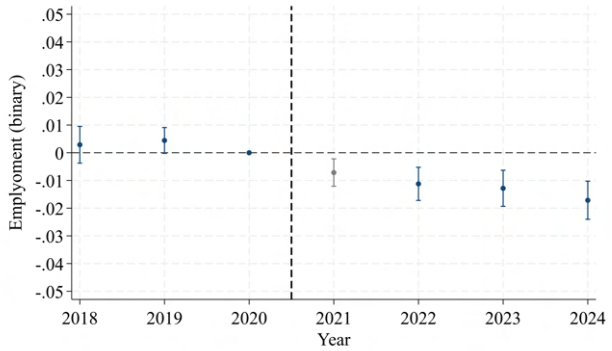
Figure C10: Robustness: Employment results for the larger control group



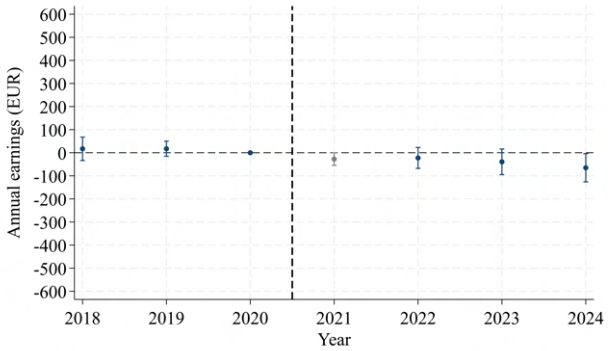
(a) Employment



(b) Formal employment



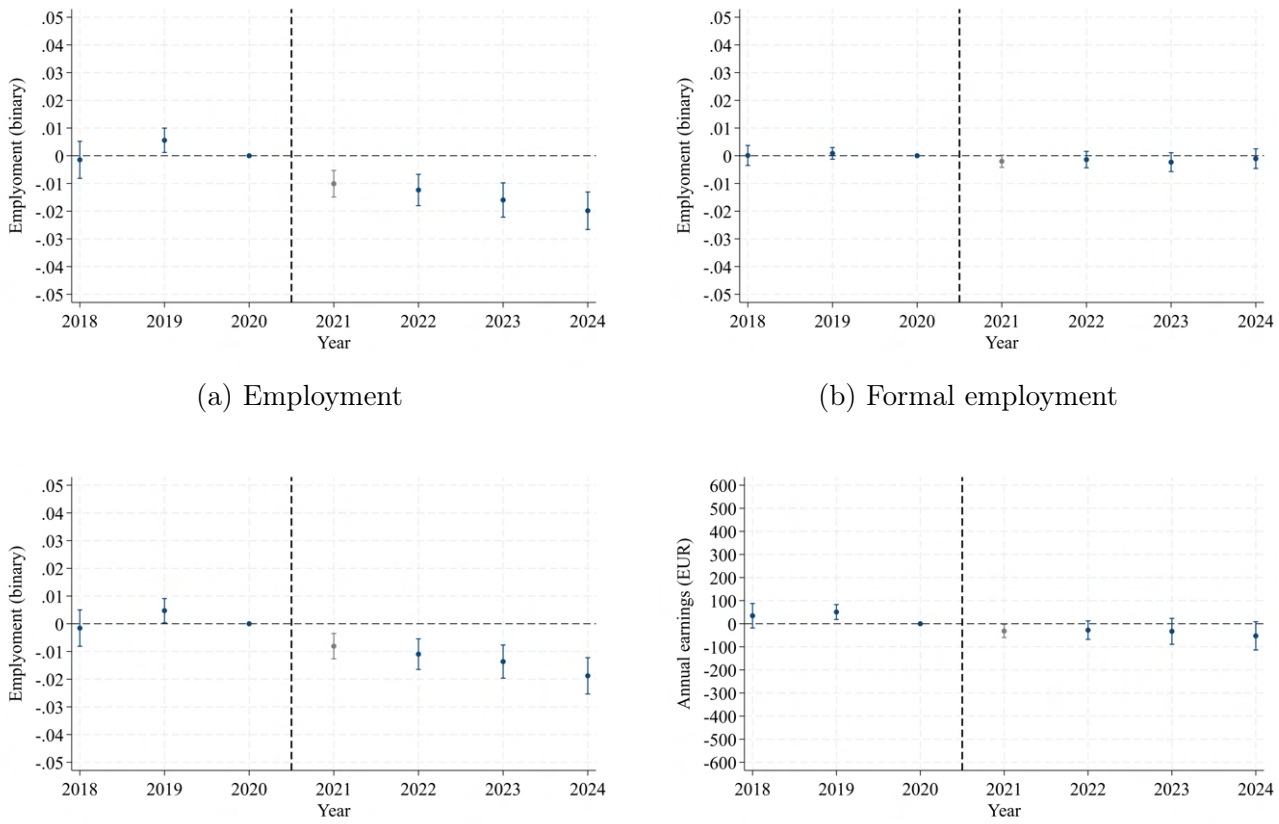
(c) Marginal employment



(d) Annual labor income

Notes: The figure plots event-study estimates of Equation 1. Control group over qualifying periods of 25 to 35 years. Estimates are obtained from a difference-in-differences specification with individual and year fixed effects, using the matched sample. The vertical line indicates the year of the reform introduction (2021). 95% confidence intervals included. Robust standard errors are clustered at the individual level. CEM weights are applied in the estimation. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

Figure C11: Robustness: Employment results for the smaller control group



(a) Employment

(b) Formal employment

(c) Marginal employment

(d) Annual labor income

Notes: The figure plots event-study estimates of Equation 1. Control group over qualifying periods to 30 to 33 years. Estimates are obtained from a difference-in-differences specification with individual and year fixed effects, using the matched sample. The vertical line indicates the year of the reform introduction (2021). 95% confidence intervals included. Robust standard errors are clustered at the individual level. CEM weights are applied in the estimation. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.

D Pre-Retirement Labor Earnings

In this appendix, we focus on the effects of the reform on annual labor earnings before entering retirement. The appendix therefore contributes to answering the question of anticipation and pre-retirement behavioral changes and complements the analysis in Section 5.1.

D.1 Empirical Strategy

The empirical approach follows the DiD analysis of post-retirement effects in Section 5.2, however we use a different sample. We focus on individuals born in 1958 or 1959 who retire in 2024, the most recent year for which complete pre-retirement contribution histories are observed. The resulting sample is considerably smaller than the post-retirement one, for two reasons. First, complete contribution histories become available only once the insurance record closes at retirement, which restricts us to a single entry cohort rather than the five entry years available after retirement. Second, individuals must appear in the Active Insurance Statistics in the years before retirement, so anyone who left insured employment earlier is not observed. The sample therefore lacks the statistical power to mirror the detailed decomposition and heterogeneity checks applied throughout the paper. However, based on this sample we can analyze the effect on pre-retirement labor earnings for one retirement cohort 2024.

For the 1958 and 1959 birth cohorts, we draw a balanced panel, with each individual observed annually from 2018 through 2023. Thus, we focus on labor supply responses for individuals aged 59-65. In this context, this is the most relevant group. First, individuals at the end of the working career tend to respond more strongly to changes in financial incentives related to pensions. Second, from an institutional perspective, information about changes in financial incentives becomes clear only shortly before entering retirement.

Because these individuals have not yet retired, eligibility for the pension supplement cannot be observed directly and must instead be projected from each individual's contribution record as of 2020, the last year before the reform. We classify individuals as prospectively eligible, and therefore assign them to the treatment group, if the qualifying contribution months and average earnings points as of 2020 already satisfy the statutory eligibility rule described in Section 2. Specifically, individuals in the treatment group have at least 35 years of qualifying contribution years and average annual earnings points at or below the corresponding ceiling of 0.8 pension points, or a shorter contribution history between 33 and 35 years paired with the correspondingly lower ceiling implied by the phase-in formula. These qualifying months and average earnings points are measured as of 2020, four years before the individuals in our sample actually retire, precisely so that treatment status is fixed well before the window in which they could still adjust their labor market behavior in response to the reform.

The control group is constructed to be comparable to the treatment group along the same two

dimensions that determine eligibility, contribution length and average lifetime earnings. We require control individuals to have accumulated at least 30 years of qualifying contributions as of 2020, three years short of the statutory minimum, so that control individuals also have long, stable labor market attachment rather than sporadic or marginal employment histories.[38] We further require their average earnings points as of 2020 to exceed the eligibility ceiling of 0.8 pension points on average, but to be no more than one pension point on average. This ensures that control individuals sit just above the earnings threshold rather than being drawn from an unrelated, higher-earning segment of the distribution. We deliberately exclude individuals with similarly low earnings but fewer contribution years from the control group, since such individuals would have a direct incentive to extend their working lives to reach the 33 or 35-year threshold.

We follow Section 5.2.1 and implement a coarsened exact matching procedure (Blackwell et al., 2009; Iacus et al., 2012), matching treated and control individuals who are identical in birth year, birth month, federal state of residence, and number of children, and who fall into the same decile of qualifying contribution months and the same decile of total earnings points, all measured as of 2020. We do not match on pre-treatment labor market outcomes. After matching, the sample includes 14,838 women and 2,900 men per year.

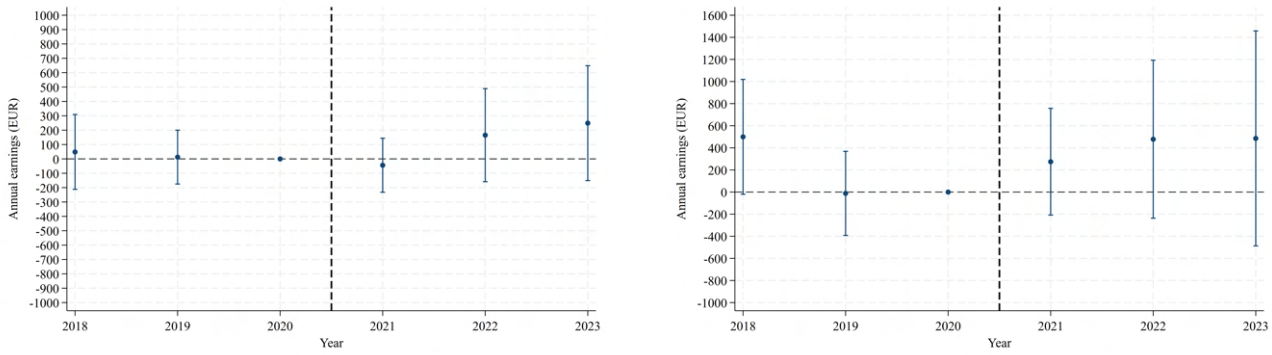
D.2 Results

Figure D1 plots the DiD estimates of equation (1), tracing the dynamic response of earnings around the reform's introduction in 2021. The post-reform period shows no significant pattern for either women or men.[39] The average effects confirm this, with an insignificant yearly increase of €103 for women and €250 for men. These are small relative to the earnings base at this stage of the life cycle. Treated women earn €20,684 per year on average in 2020, so the estimated effect amounts to 0.5% of pre-reform earnings.

[38] We test robustness to this choice by constructing the control group instead with a minimum of 25 and of 35 years of qualifying contributions.

[39] Given the smaller sample size, we cannot separately analyze the effects for marginal and regular employment.

Figure D1: Pre-Retirement Annual Labor Earnings



(a) Women (b) Men

Notes. The figure plots event study estimates of Equation (1) for the effect of prospective eligibility for the pension supplement on annual labor earnings in the years before retirement, measured in euros and set to zero for those who do not work. Eligibility is projected from the contribution record as of 2020. Estimates are obtained from a difference-in-differences specification with individual and year fixed effects, using the matched samples of women and men born in 1958 or 1959. The omitted baseline is 2020 and the vertical line marks the year the supplement took legal effect (2021). 95% confidence intervals are shown. Robust standard errors are clustered at the individual level, with 14,838 clusters for women and 2,900 for men. The corresponding two-way fixed effects estimates of the average post-reform effect are 103.08 (141.46) for women and 250.09 (316.51) for men, neither of which is statistically distinguishable from zero. CEM weights are applied in the estimation. *Source.* FDZ-RV (2026a, 2026c, 2026e), own calculations.