Are Age-Progressive Occupational Pension Contributions Discriminatory? Evidence from a Difference-in-Differences Analysis of a Swiss Occupational Pension Policy Change

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Abstract

It is argued occupational pension contributions that increase with age disadvantage older workers by extending unemployment durations and disadvantage younger workers by reducing relative compensation. Studies applying a causal inference design have so far examined taxes on older workers, rather than pension contributions. In this paper we use a national reform, which reduced occupational pension contributions for women in Switzerland, looking at how a change in pension contributions impacted unemployment duration and reemployment wages. With three three-year age groups experiencing reductions, we estimate heterogeneous effects by age. Results suggest a change in occupational pension contributions in line with recent policy proposals has no impact on unemployment durations or reemployment income.

Introduction

Age-specific occupational pension contributions are an important topic in both Europe and the US. In Europe occupational pension contributions that increase with age were widespread, though they have been increasingly removed. Staggered contributions are viewed as discriminatory against older workers as high contributions supposedly discourage employers from hiring them, extending unemployment spells. In Switzerland, an initiative in 2024 proposed flattening age-staggered occupational pension contributions arguing it would reduce employer discrimination against older workers. At the same time, staggered contributions are also viewed as discriminatory against younger workers insofar as their total compensation is relatively lower if employers do not reduce comparable older workers' wages to adjust for the higher employer pension contributions.[1] Age-specific contributions were unsuccessfully challenged as discriminatory against younger workers in the Court of Justice of European Communities (European Union, 2013). In response to these critiques, some countries, such as Finland, have removed age staggered contributions.

In some countries the issue of age-staggered contributions is falsely perceived as irrelevant as occupational pensions have shifted from defined benefit (DB) to defined contribution (DC) (Rappaport, 2000). This is because DB plans were traditionally "backloaded," meaning there were higher employer contributions with more years of service (older workers) while employer contributions under DC plans are seemingly age neutral. That said, in countries with (partially) voluntary occupational pensions, de facto employer contributions *still* increase with age because (employer-matched) individual contributions generally increase with age (Poterba et al., 2007). As such, concerns about pension costs increasing with age are as also relevant in voluntary systems.

In this paper we use a quasi-experimental strategy to test whether occupational pension contributions impact hiring and wages. In Switzerland minimum employer and employee contributions are uniform across employers, set at the national level. In 2005 three different age groups of women (32-34, 42-44, and 52-54) experienced declines in contributions. Using administrative data from social security and unemployment records, we take a difference in differences (DiD) strategy to test whether contribution rates had an impact on how long unemployed women had to look for a job, and their reemployment income.

We find no evidence that contributions impact the wages and unemployment durations of workers. Results suggest that fears about occupational pensions contribution schemes discriminating against older workers might be mislaid.

Following the introduction, in the second section of this paper, we offer contextual background. We start by summarizing the findings and gaps in two areas of the labour economics literature: a. occupational pension contributions or other taxes on older workers and their impact on hiring and b. mandated (occupational pension) contributions and their impact on wages. We then describe the specific setting of our quasi-experiment—the Swiss occupational pension scheme and the female labour market in Switzerland. In the third section of the paper, we describe our data and methodological approach followed by two empirical sections- one showing how the legal changes impacted pension contributions, and a second showing how these changes in contributions impacted unemployment duration and reemployment income. In the appendix we re-analyse with another quasi-experimental design, comparing women who faced declining pension contributions to men of the same age with constant pension contributions.

Background

We begin by looking at the literature relevant to how age-specific occupational pension contributions might incentivize discrimination. There are two relevant literatures. The first focuses on how occupational pension contributions and other age-specific taxes impact the hiring of older workers, while the second looks at whether contributions (including pensions) are offset by lower wages.

2.1 Pension Contributions and Unemployment Duration

Older workers suffer longer unemployment spells following job loss (Chan & Huff Stevens, 2001). There are many proposed reasons including higher wage costs and discrimination (Di Nallo & Oesch, 2020; Skirbekk, 2004). It is argued that high pension contributions contribute to this problem by inflating the cost of older workers (Scott et al., 1995). The empirical literature testing this theory can be separated in two subgroups focusing either on variation of 1. pension generosity between firms and occupation or 2. taxes.

The first literature, primarily from the US and UK, taps into variability in occupational pensions at either the firm level (Daniel & Heywood, 2007; Scott et al., 1995) or by occupation or industry (Hirsch et al., 2000; Hutchens, 1986). With diverse pension designs across firms, researchers cannot study the impact of pension contribution costs on overall older worker (re)employment, and instead focus on the incidence of older new hires within firms or occupations.[2] Pensions are measured in various ways including the presence of a pension, contribution rates, or the value of benefits. Outcome variables include the percentage of new hires over a certain age threshold or the ratio of older to newer hires. The literature finds that employers with higher pension contributions or costs for older workers have fewer older hires (Daniel & Heywood, 2007; Garen et al., 1996; Heywood et al., 2010; Hirsch et al., 2000; Hutchens, 1986).

There are, however, three key limitations to this work. First, because estimates rely on variability in pensions across firms or occupations and because pension costs for older workers and the likelihood of hiring older workers are both correlated with firm characteristics like firm size, unionization, hierarchal organizational structures, and physically demanding work, these studies likely suffer omitted variable bias (Hernæs et al., 2011; Ilmakunnas & Ilmakunnas, 2014; Rappaport, 2000). Second, several studies had very high non-response rates correlated with factors like firm size and professional HR departments which are related to both pension design and hiring older workers (Garen et al., 1996; Scott et al., 1995). Third, the outcome variable, new-hires by age within firm, might not clearly reflect overall unemployment duration for older workers. In sum, these studies offer at best suggestive evidence that pension costs might be one reason that older workers have longer unemployment spells.

The second literature, from Europe, offers strong empirical evidence that high costs for older workers discourages hiring. In France, firms are taxed for laying off workers over age 50, with the intent of discouraging layoffs of older workers. A policy-change in 1992 exempted firms from the tax if the laid-off worker was hired after age 50. Taking a regression discontinuity approach, Behaghel et al. (2008) showed that the original tax suppressed the hiring of older workers. Similarly, Finland's "early retirement tunnel" offers older workers entering the unemployment system special funding to transition directly to early retirement. Employers who have laid off more older workers using this system pay more in taxes. In 2000 the costs associated with high experience ratings were increased for larger employers and remained stable for smaller. This change allowed researchers to use difference in differences to show that when firms are forced to shoulder more of the costs of laying off older workers, they are less likely to both fire and hire them (Hakola & Uusitalo, 2005; Ilmakunnas & Ilmakunnas, 2015). While this literature is methodologically well-grounded compared to the first, it is possible that higher taxes for firing older employees have different effects than higher pension costs due to heterogeneous valuation of benefits and differing deadweight losses (Summers, 1989).

In conclusion, existing empirical evidence suggests that pension costs that increase with age might encourage age discrimination and longer unemployment spells for older workers. However, direct empirical evidence is flawed. Studies looking directly at pension costs suffer methodological problems due to relying on variation in pension contributions across firms and occupations. Solid causal inference studies look at taxes associated with employing older workers, but not specifically at pension costs while the theoretical literature suggests the impact of a mandated benefit on wages is not necessarily the same as an additional tax. Our study adds to the empirical literature, taking a causal inference approach to directly study the effect of pension contribution levels on unemployment duration.

2.2 Pension Contributions and Compensating Differentials

The theory of compensating differentials (or equalizing differences) suggests that any fringe benefits will be offset by a corresponding wage differential (Black, 1987; Woodbury, 1983). Even if employers compete for employees by offering varying combinations of benefits and wages (to attract workers that match the institution or job), profit-maximizing firms must offer a total compensation package such that the marginal cost of employment equals the marginal revenue from employment. This means that firms should be indifferent between wage and pension costs, with an increase in one leading to a decrease in the other. (i.e. the isocost line between wages and pensions should have a slope of -1 (E. B. Montgomery et al., 1990)). One reason the slope might not be -1 is varying pension administrative costs across employers. Others argue that the law of supply and demand would dictate that the wage adjustment due to benefit is equal to the difference between the employer's cost and the employee's valuation, not the raw mandated cost (Summers, 1989).

The empirical evidence on whether pension costs are passed on is decidedly mixed,[3] finding positive (Haynes & Sessions, 2013), negative (Inkmann, 2006; E. Montgomery & Shaw, 1997), and null trade-offs (Pozzebon & Mitchell, 1989).[4] There are various explanations for the inconsistent results such as that compensating differential theory should include endogenous labour supply (i.e. pensions encourage productivity) (Creedy, 1994) and that low-wage workers exaggerate workplace disamenities (Elliott & Sandy, 1998). Studies also use diverse measures of pension costs including pension accrual, estimated costs to the employer, dummies for coverage, and contributions costs (Haynes & Sessions, 2013; Inkmann, 2006). The most common critique, however, is omitted variable bias (Haynes & Sessions, 2013; Hwang et al., 1992; Maestas et al., 2018). The reason for this critique is that the literature focuses on the US with just a few studies from the UK and Canada[5] with these studies predicting log wages based on various measures of pension generosity, drawing on pension variability at the firm level. Unanticipated positive correlations between benefits and wages then arise if measures of firms and individual capacity are imperfect— productive individuals are rewarded with more generous benefits *and* wages, or more productive firms offer higher pensions *and* wages.

Thinking about differences by income and age, we would intuitively expect that younger or low-skill workers have less bargaining power and thus employers would be more able to transfer the costs of pensions onto their wages, i.e., they are more likely to have compensating differentials approaching – 1. The empirical evidence indeed supports this conjecture by age, but not by skill. (Schiller & Weiss, 1980) find no compensating differentials for older workers, concluding, "improved pension provisions may represent a "free ride" for older workers that is paid for by disproportionately reduced wages for the youngest workers." Looking at the high skill labour market, analyses of executive compensation find compensating differentials between bonuses and pension benefits (Goh & Li, 2015). However, the literature on compensating differentials by age or skill is a small one and it suffers the same limitations as both the broader compensating differentials literature in that it relies on Anglo-Saxon cases where variation in pension benefits is at the firm level with results likely biased by selection and endogeneity.

If we look beyond pensions to consider other government-mandated benefits, we can draw on a much larger literature, though still primarily from the US, examining mandated health insurance coverage. Firms pass some significant portion of mandatory health insurance benefits down to workers—though the estimated range is very wide ranging from some 20 to 80% (Anand, 2017; Baicker & Chandra, 2006; Bailey, 2014; Kolstad & Kowalski, 2016; Lennon, 2019). One reason for the wide range of estimates is that researchers use different sources of variation to generate estimates—with changes specific to gender or age (Bailey, 2014; Lennon, 2019), or by geography (Baicker & Chandra, 2006). A second reason is that wages cannot be immediately adjusted when health care premiums increase, so studies looking at overall wages and not new hires or in a shorter time frame, show a lower percentage of costs passed through to workers (Lubotsky & Olson, 2015). There also seems to be real heterogeneity in the extent to which costs are passed on to workers, with lower wage workers paying a higher percent of the costs (Colla et al., 2017; Ma & Cheng, 2019), probably because of more fluid market conditions. At the same time, pass-through for exactly these workers can be limited due to union contracts or minimum wages (Clemens & Cutler, 2014).

What the literature on mandated benefits suggests for pensions, is that some portion, though not all, costs are probably passed through to employees. That percent of costs passed through depends on how efficient the labour market is for the relevant individuals and on institutional constraints like unions and minimum wages. It also suggests that if one does not look exclusively at new hires, results will be underestimates, because wages are sticky.

With respect to hiring and firing, the literature suggests mandated benefits have a greater impact when they generate costs specific to some specific sub-group. This could imply that the impact of age-specific costs is buffered by the window that employers consider—as over time employees age and enter a new cost-group. If employers consider a longer horizon, the "group" affected is broader and thus there is less incentive to discriminate.

The question of differences in how employer-mandated benefits impact workers' wages and hiring *by age* cannot be answered by turning to the literature on mandated health insurance. In observational studies age is too closely related to both health insurance costs and productivity. There are experiments specific to given age groups such as the mandated prostate screening experiment in Bailey (2014), but there are not parallel experiments conducted systematically across age groups.

In this paper, we look at a case where the increase in pension costs with age does not depend on firms, thus avoiding the primary limitation in the literature where unobservable firm-level factors impact age-specific contributions, and simultaneously the hiring and wages of older workers. Further, we can look directly at pension costs rather than taxes on older workers, which theory would suggest are not necessarily the same. As such, we offer the first quasi-experimental estimate of how mandated employer pension benefits impact wages and hiring. The paper also improves on the general literature on mandated benefits. With three separate and parallel experiments by age, we can rigorously examine heterogeneous age effects. The results have important policy implications with respect to how to structure mandatory employer benefits, including pension contributions.

We accomplish these goals by looking at a highly regulated occupational pension system where all workers at one point in time have the same age-specific contribution rates. The 2004/5 reform to the Swiss occupational pension system, changed contribution rates for three small age-ranges of women. This allows us to compare those in age ranges with changing contribution rates to those in age ranges with static contribution rates in the middle-term (5 years). We use a difference in differences estimate to look at unemployment duration and new hires' wages for three age groups using administrative data from the unemployment insurance system and social security records. It is important to use administrative data because the changes in contribution rates were relatively small, meaning that even effects that are proportionally high would be small in absolute terms, and thus only identifiable using many observations.

In the next subsection we describe the Swiss case. First, we describe the Swiss occupational pension system and the 2004/5 policy change, which impacted only women. We focus on a comparison to the US system, as the Swiss system is not as well-known. Second, we devote a short paragraph to describing the female labour market in Switzerland. This clarifies why women in the unaffected age groups are the proper comparison group and aids in assessing the study's generalizability.

2.3 The Swiss Case: Occupational Pensions, the 2004/5 Reform, and the Female Labor Market

The Swiss pension system has a "three pillar" system. The first pillar is the public pension system, Old-age and survivors' insurance (OASI). OASI (with supplementary benefits) is redistributive and should replace about 30% of the average worker's wages. The second pillar is occupational pensions, BVG, and the third pillar is personal savings (partially tax incentivized).

From the early 20th century through the 1970s Switzerland's occupational pensions were voluntary defined benefit (DB) and defined contribution (DC) schemes incentivized through the tax system. In 1972 legal control over occupational pensions was transferred to federal authority and in 1985 a national law made fully funded private occupational pensions *mandatory* with extensive government regulation. Occupational pensions are still private (run by employers or outsourced by employers to pension funds) but the Swiss government sets a minimum interest rate of return and dictates annuity conversion rates—both of which are also subject to public referendum. Employers are required to pay a minimum of 50% of contributions and to cover incomes over that fully insured by OASI (currently 24,645 CHF per year) and up to an income three times the maximum income insured by OASI (currently a maximum of 255,960 CHF—though most pension funds cover higher incomes).[6] The government also requires pension funds to diversify their portfolios in terms of assets and horizon and requires oversight committees of workers and employer representatives. Cantons audit management, accounting, and financial position of pension funds. The federal government runs a Security Fund that equalizes the risk profile of different retirement funds and offers limited reinsurance for individuals whose pensions are insolvent. Finally, the federal government requires that age credits, upon which benefits are based, are equal to at least 7% of wages when the worker is 25–34 years old increasing to 18% by age 55 (see Fig. 1). Employer contributions can exceed these rates, though there is evidence that generally the contribution rates increase at the government-suggested thresholds (Butler & Ruesch, 2007; Sheldon & Cueni, 2011). As illustrated in Section 2.4, pension fund contributions dropped following the legal change examined in this study.

The Swiss occupational pension is perceived domestically as a privatized system of primarily DC funds. However, the politically determined minimum interest rate during the accumulation phase and annuity conversion rate at the payout phase, mean that participants are not paid out strictly based on underlying investment accumulation, i.e., regulation introduces DB elements into a DC structure. As in DB plans, the age-specific contribution rates increase with age, though in steps and consistently across the population. The government also manages risk equalization and re-insurance for the pension system. These diverse controls have led the OECD to often refer to Swiss occupational pensions as a quasi-public DB scheme even though in Switzerland it is regarded as a private (mostly) DC scheme. While other European countries also have mandatory occupational pensions, only Iceland and the Netherlands have similarly high levels of private pension spending and private sector coverage (Hinz et al., 2012). This makes the Swiss system somewhat unique, having a privatized system with regulations that push it to meet policy goals usually achieved with other structures more commonly seen in Europe, such as a larger and less progressively funded first pillars.

Strong regulation has helped the pension system meet policy goals including broad coverage, high replacement rates, and low risk for participants. The maximum insured income is three times the yearly maximum of social security (OASI), replacing about 50–60% of income on top of the approximately 30% covered by OASI, in sum replacing about 80% of the average household's earnings in retirement (BSV 2017). Some worried that the highly constrained public-private system would suffer insolvency during the Great Recession (Butler & Staubli, 2010). Although there was a short-term dip in total assets during the recession, the annual growth rate was 4.8% from 2004 to 2014 (BFS, 2016). Similarly, there was a strong recovery following the 2022 stock market downturn. The system is critiqued for inadvertently encouraging early retirement (Kuhn et al., 2021; Madero-Cabib & Kaeser, 2016; Sonnet et al., 2014), disadvantaging those in atypical careers (i.e., women) (Bonoli, 2003), and in popular media is often suggested to encourage age discrimination in hiring with contribution rates that increase rapidly with age (Frattaroli, 2024).

In October 2003 changes in the Swiss occupational system were enacted, coming into effect in January 2005. This reform increased women's retirement age from 62 to 64. To compensate for the additional years of contributions (at an overall contribution rate of 18%), rates for women ages 32–34, 42–44, and 52–54 declined, as illustrated in Fig. 1.[7] This change is the basis for our experiment, described in detail in the next section. The other changes, although occurring concurrently, should have no connection to our outcome variables of reemployment duration and reemployment income. Further, while employers must pay a minimum of 50% of contributions, they can choose to contribute more meaning that following the reform some employers might have maintained their earlier higher contributions for women in these groups. However, as shown in Section 2.4, contributions as a percent of salaries declined directly following the reform and pension funds with more women also had relatively lower contributions, suggesting firms did not maintain earlier contribution levels.

It is relevant to briefly describe the Swiss female labour market as the relevant policy change was specific to women and the study's finding might be limited in their generalizability to men. The dominant working model in Switzerland for families with children is one full-time male and one part-time female worker (Csonka & Mosimann, 2017). This means women are overwhelmingly employed part-time, usually following their first childbirth (60% of all women compared to 18% of men, or 82 vs. 8% for those with children under 12) (BFS, 2024). Consequently, the wage gap grows steadily over the life cycle increasing from 5–25% over the career-span (3–9% looking at just the "unexplained" wage gap) (Strub & Bannwart, 2017) with women's income distributions not having the strong right-hand skew normally seen in other countries or in the male income distribution. Women are also slightly more likely to be over-qualified for their jobs; 16% of women compared to 13% of men with a college education are in jobs requiring no college degree. Women's labour market participation rates, however, dropping from 91 to 76% from the age of 25 to 64, are similar to that of men, which drop from 95 to 82% for the same ages (BFS 2020). Finally, there is strong occupational segregation by gender. Solid estimates of wages only find comparable men for 55% of women in the private sector (Strittmatter & Wunsch, 2021).

The unique female labour market has several implications for our study. First, within the Swiss female labour market, it might be important to consider comparisons within a small age range, given the dramatic and enduring drop in wages that takes place during reproductive years. Second, women seem to have less labour market fluidity during the child-rearing years while the literature suggests that the less fluid the market is, the lower the percentage of mandated benefit costs are passed on. This would mean reemployment wage estimates would be a conservative floor compared to men. Third, because women's earnings peak very early, one might not anticipate rising age discrimination as pension contribution rates rise (because women's incomes are so low later in life—an increasing percentage going to pensions might not be perceived as a growing burden). Finding no increasing effects with age for women, might not mean that men, who have steeper age-income profiles, do not suffer increasing negative impacts of pension costs. Finally, dramatic gender labour market segregation throws some doubt on our secondary experimental strategy comparing men and women (Appendix 6.2).

2.4 Pension Reform and Declining Contributions

We can consider the size of the "treatment" in two ways. First, we can look at it in terms of the changing rules, i.e., deductively. The policy change, reduced contributions for the women in our treated groups − 3, -5, and − 3 percentage points (32–34, 42–44, and 52–54)—that is for employers − 1.5, -2.5, and − 1.5 ppts. For the employer side, it is impossible to know the window of future costs that employers consider in hiring someone and there is likely some sort of discounting where imminent costs are weighted higher than future costs. A very conservative estimate of employers considering a full 5-year window with no time-discounting implies drops in contributions of − .6, -1, and − .6. In contrast the control groups (25–26/35–36; 35–36/45–46; 45–46/55–56) had no change in a 5-year window. While these might sound like small adjustments, they are similar in scale to recent policy proposals. A 2024 referendum proposed would have increased employer contributions for the youngest workers by -1 ppts. and decreased contributions for the oldest workers by -2 ppts shifting the gap in employer pension contributions between the oldest and youngest from 5.5 ppts to 2.5. Income losses for the employee side mirror exactly the employer side (as contributions are split 50:50)—and as a small additional tax on labour, employee contributions might also be expected to reduce labour supply.

The second way to think about the size of the treatment is to examine it inductively or empirically, looking at whether there was an actual drop in contributions. The law's guidance changed with respect to age-specific contributions, but pension funds have some flexibility as to how to interpret them, and employers could have maintained the prior (higher) level of contributions. However, looking in publicly available aggregate data there is evidence that the legal change generated a change in practice.

In Fig. 2 in the left-hand panel, we see occupational pension contributions (employer and employee contributions) as a percent of salary. Generally, pensions are on an upwards trajectory because of the aging workforce, but both employer and employee contributions declined in 2005. There are no clear competing explanations for this change beyond the reduced contribution requirements for women in the three age groups affected by the pension reform. We do not know the differences in occupational contributions by gender and age so we cannot directly test whether these pension contribution reductions were among women in these three age groups. However, we do know contributions by pension fund and the percentage of female enrolees by fund type from 2004. The middle panel of Fig. 2 shows that public pensions have a higher percentage of female enrolees—14 to 15 percentage points more with the gap growing. While generally we expect contributions to increase because of the aging workforce, in 2005 public sector pension contributions per enrolee fell just slightly while private sector pension contributions continued to climb. These numbers include all employees in the sector—men, women, workers of all ages—so we do not anticipate a public sector decline as a result of the reform. A simple divergence in trends between public and private sector pensions suggests that the reform led to a relatively greater decline in contributions in funds with more women.

Drop in overall contributions as a percent of salary following the reform (L), Female enrolees in private vs. public pensions (C), Total pension contribution private vs. public (R)

Data and Methods

3.1 Data

The base file for the analysis is administrative data from Swiss unemployment records(Swiss Federal Council, 2016). These records include information on gender, age, region, education level, occupation, last job function, citizenship and country of origin. We merge in data from Swiss Social Security individual accounts data including income amount and type (earnings, self-employment, unemployment insurance, and retirement). We included those ages 25–56 at start of unemployment, to avoid years that might be impacted by schooling or early retirement.[8]

We use data for the registered unemployed. At the time of this study there was almost universal take-up of UI because of the system's generosity (70–80% reimbursement rates for up to 126,000 CHF/yr for 1.5 to 2 years), with this high take-up reflected in the low gap between administrative and ILO unemployment rates at the time (4 vs. 5%).

Comparing labour market outcomes for those experiencing a UI spell pre- (2000–2003) versus post- (2004–2006) reform, for women we see an overall decline in the average duration of unemployment by 18 days and a decrease in reemployment wages one year post unemployment of 5,539 CHF/year (a change in wages that holds even 3 years post-employment). These changes cannot be attributed to the change in pension policy but might be due to changes in labour market conditions, other policy changes, shifts in population composition—or any combination of factors. We address these issues by using a quasi-experimental DiD design.

3.2 Quasi-experimental Design

Our approach is like (van Ours & Vodopivec, 2006), using quasi-experimental methods with administrative data to examine how a change in unemployment insurance rules impacted unemployment durations. In our case, we examine whether the 2005 change in pension contribution rules for women in Switzerland impacted unemployment insurance (UI) spell duration and post-UI reemployment income.

In our analysis we look at the reemployment of all women. In contrast to previous research, we can directly measure unemployment duration because pension contributions are the same for the whole economy, varying only by age and period. The analysis of whether employers pass the cost of pension contributions on to workers focuses on reemployment wages as current employees' wages cannot immediately adjust following a change in pension contribution rules. Wages are sticky—with workers having a very low chance of a wage change in any given quarter, particularly with long tenures (Barattieri et al., 2014) as in Europe (Diamond, 2011). Finally, in the main analysis we use women as controls, because of significant occupational segregation in Switzerland.[9]

The changing pension rules impacted three age groups: 32–34, 42–44, and 52–54. However, of course every female worker is ultimately "treated" because all will go through the same (new) age-specific contribution rates as they age, so the window of costs that employers consider is relevant. Median job tenure is about ten years in Europe with about 15% of Swiss workers leaving their job in the first year (BFS, 2024; Goulart & Oesch, 2024), with tenure varying by age and occupation.[10] Employers might consider pension costs of varying windows, applying time-discounting to future costs. For the analysis we chose control groups at least five years out from the effective change in pension contributions, assuming employers do not consider costs more than 5 years out. This means the 32–34 treatment group is compared with those ages 25–26 and 35–36; 42–44 is compared to those 35–36 and 45–46; 52–54 is compared to those 45–46 and 55–56. While the control groups do not experience a change in their planned contribution rates, they are slightly asymmetric around the treatment group, meaning the control group is slightly younger (see the descriptive statistics in Table 1 and the weighted statistics in the appendix). Using these age definitions and selecting one unemployment spell per woman, we have 52,489 women in the control group and 53,293 in the treatment, observing a total of 1,158,488 person-years.

3.3 Estimation

We take a macro level difference in differences (DiD) approach, calculating the difference in changes for the treatment and control groups post vs. pre-reform. These include the DiD estimates of duration *d* and income *i*:

$$D_d = \left(\bar{d}_{c,\ post} - \bar{d}_{c,\ pre}\right) - (\bar{d}_{t,\ post} - \bar{d}_{t,\ pre})$$

1

$$D_i = \left(\bar{i}_{c,\ post} - \bar{i}_{c,\ pre}\right) - (\bar{i}_{t,\ post} - \bar{i}_{t,\ pre})$$

2

Taking a DiD approach solves both the problem of being unable to disentangle changes in economic conditions from policy changes, the problem of sparse individual level controls and allows us make inferences on the aggregate level even with the same individuals not experiencing both the pre and the post regimes. While all women experienced improved labour market outcomes, we would expect no difference in the *change* in average labour market outcomes for women ages 32–34, 42–44, and 52–54 versus the change for a group of women slightly younger and older with the same *average* age. If there is a significant difference, it must be due to lower pension contributions—the only difference between the groups.

Estimating the DiD using sample means is only effective if there are no confounders—a situation that is seldom the case. Wage growth throughout the lifecycle is relevant when comparing the treatment and controls. For this reason, in addition to an overall estimate, we compare within narrower age windows. As illustrated in Table 1, the control group is slightly younger than the treatment group in the pre period (2 months) and while both groups are on average older in the post-period (due to demographic shifts), with the treatment group aging more (3-month difference). Other control variables with DiD differences include slight growth in lower SES jobs among the treated (.01%) and a relative increase in the proportion Swiss in the controls (.023%). In the right-hand column, we show that all differences disappear with the use of entropy weights (last column of Table 1). (See Appendix 6.1 for the full weighted descriptives.)

To test the robustness of the estimates given these DiD differences we present standard DiD regressions controlling for these variables and present results using entropy weighting to adjust for differences. We cannot weight for age within the subgroup analyses—as age is too closely related to the definition of the experimental design—but differences are small because of the simultaneous older/younger comparison. (See Appendix 6.1 for descriptive statistics by age group). To examine potential age biases more closely, in Appendix 6.2, we run the analysis using an alternative experimental design, comparing women in the “treated” age groups to men in the *same* age groups.

Table 1
Control variables for pooled age groups by treatment/control, and pre/post reform. Descriptive statistics, DiD estimates, and DiD estimates post-weighting. Statistical significance of DiD estimates: * = p < .05; ** = p < .01; ***=p < .001.

	Pre		**Post**				
	Control	Treatment	Control	Treatment	DiD, OLS		DiD, Weights
Age	35.17	39.217	35.339	39.665	0.278	**	0.001
Education: less than vocational degree	0.287	0.314	0.243	0.263	-0.007		0.001
Education: vocational degree	0.575	0.547	0.59	0.57	0.008	.	-0.002
Education: Tertiary	0.138	0.139	0.167	0.167	-0.001		0.002
Occupation: Lower (ISCO 7–9)	0.205	0.211	0.167	0.181	0.009	*	-0.001
Occupation: Middle (ISCO 3–6)	0.667	0.652	0.671	0.65	-0.006		0.001
Occupation: High (ISCO 1–2)	0.128	0.137	0.162	0.169	-0.002		0.001
Swiss citizenship	0.624	0.64	0.656	0.648	-0.023	***	-0.003

Most DiD estimators use a simple regression model controlling for the confounder (Zeldow & Hatfield, 2021). There are however arguments that there should be an adjustment before running the DiD. In contrast, Baser (Baser, 2006) argues regression adjustment doesn't work if the two groups are too distinct in their means or variances and if the ratio of residuals is far from 1. In this case, it is preferable to use matching or balancing methods, like entropy score balancing in which case scalar weights are assigned to everyone such that the reweighted groups distributions match each other in terms for the first and second moments (means and variances) of the covariates, age and prior income (Hainmueller, 2012). Here we present results using the standard DiD estimate as well as an estimate using entropy weights, with the weighting considering pre-unemployment earnings, education, national origin, and age—though age is included only for the weights for the overall estimates—it is too closely related to treatment group to be included in the age-specific group weightings.

The baseline equation is:

$$dur_j = \beta_0 + \beta_1 a_j + \ \beta_3 i_j + \ \beta_4 t_j + \beta_5 p_j + \beta_{DiD}(t_j * p_j) + \alpha_j$$

and

$$inc_j = \gamma_0 + \gamma_1 a_j + \ \gamma_3 i_j + \gamma_4 t_j + \gamma_5 p_j + \gamma_{DiD}(t_j * p_j) + \epsilon_j$$

Where we have individual *j* with age *a*, pre-unemployment income *i*, treatment/control group *t* and pre/post *p* with the DiD estimators being β_{DiD} and γ_{DiD}. We did not use a log-transform for wages because it was not skewed, and we did not include age-squared because the narrow age ranges of our experimental and control groups meant there was too much collinearity (and thus did not improve estimates).

A critical assumption in a DiD analysis is the parallel trends assumption, which can approximately be tested by looking at trends in the control and treatment groups before treatment (i.e., "pre-trends") (Wing et al., 2018). For this to hold we would expect the difference between the control and the treatment group to remain the same in future periods following the base period.

Figure 3 shows the difference in unemployment durations for the treated compared to the controls, in the left-hand panel unweighted and the right-hand panel weighted. For the subgroups, age could not be weighted out because it is too closely aligned with treatment.

The top panel, illustrating the overall experiment, where age could be weighted, shows that trends prior to treatment are parallel until the policy change is announced. When the policy was announced durations increased slightly and temporarily, but it seems unlikely to be announcement effects, as the jump is in the opposite direction as expected.

The subgroups (which could not be age- weighted) show primarily increasing durations for the treatment age of the prime-age group. The divergence pre-dates the policy reform, is in the opposite direction as expected, and might be due to the fact that the unweighted age-bias was highest for this group (the treatment group grew relatively younger by 6 months). The trend for prime age workers thus suggests not a policy effect, but rather potentially structural changes relatively disadvantaging younger workers. In contrast, the youngest group of treated does show a small increase in unemployment duration during the period of policy change, mirrored in the age-adjusted overall results. This is, again, however in the opposite direction as expected-younger workers with falling pension contributions had longer durations. Most importantly, for the older group there is no drop in unemployment duration during the period of policy change.

In Fig. 4 we can see the change in the difference in reemployment income between the treatment and controls versus in each given year compared to 2000 with and without weighting. As with duration we can confirm that there are parallel trends until 2003, the year the policy was announced. The overall weighted results show null effect. The subgroups, which could not be weighted for age, show potentially a drop in income for prime-age and older workers—again the direction of change is the reverse of what one might anticipate if there were some kind of pass through of retirement costs (since contributions decline one would expect an increase).

Results

4.1 Unemployment Duration

Figure 5 shows the DiD estimates for unemployment duration using OLS to adjust for covariate imbalance, in addition including weights in the right-hand panel. (Point estimates are in Table 2.) The DiD estimates pool the pre- and post-reform years (2000–2003 vs. 2004–2006) As anticipated by the descriptive graphs, the overall experiment finds null effects. Subgroup effects (again age could not be weighted—but here age is included in the regression) suggest if anything reverse effects, with longer unemployment durations for prime-age and older women when contributions declined

4.2 Reemployment Income

Figure 6 illustrates DiD estimates of reemployment income in years 1–2 (point estimates are in Table 2). For the overall experiment we find null effects in the unweighted estimates and reverse effects in the weighted estimate: a drop in pension contributions did not lead to an increase in reemployment income—if anything it was associated with a drop in income. These reverse effects are concentrated among older women.

Table 2 includes DiD estimates for income in years 3-5 following unemployment in addition to the point estimates illustrated in Figures 5 & 6. For the unweighted estimates there are null for years 3-5 with marginally significant reverse effects in year 5 for young women and marginally significant effects for prime age workers in the year 5. For the weighted results there are significant reverse income effects consistently through year 4, largely driven by the oldest group. These consistent reverse estimates for the older group: longer unemployment durations (about 11 days) and lower reemployment income (about 1,000 CHF less per year) up to four years out from unemployment are hard to explain. One possibility might be that the broad public discussion around the reform drew attention to the fact that women ages 52-54 were about to face high contribution levels of 18% (9% employer funded). Regardless, there is clearly no evidence that lower contribution rates helped older workers.

Table 2
Unweighted and weighted DiD estimates for unemployment duration and reemployment income 1 to 5 years following reemployment

		duration		income y1		income y2		income y3		income y4		income y5	
unweighted	overall	6.929	*	-60.806		-187.802		-335.790		-253.118		-153.910	
	(se)	*(2.727)*		*(199.767)*		*(214.930)*		*(228.316)*		*(238.070)*		*(245.586)*	
	young	-0.136		100.713		-138.877		-450.882		-467.551		-607.083	.
	(se)	*(3.518)*		*(280.849)*		*(303.328)*		*(322.471)*		*(336.786)*		*(348.214)*	
	prime	8.361	.	168.953		152.592		88.840		396.100		691.040	.
	(se)	*(4.418)*		*(316.231)*		*(338.672)*		*(359.047)*		*(373.786)*		*(385.920)*	
	older	14.196	*	-496.804		-552.262		-648.971		-529.898		119.443	
	(se)	*(6.244)*		*(389.915)*		*(416.872)*		*(441.693)*		*(457.004)*		*(468.751)*	
weighted	overall	0.785		-226.341		-541.057	**	-837.842	***	-955.152	***	-958.456	***
	(se)	*(2.533)*		*(190.254)*		*(203.922)*		*(216.713)*		*(226.008)*		*(233.471)*	
	young	1.027		-332.011		-436.806	.	-524.066	.	-550.600	.	-566.628	.
	(se)	*(3.076)*		*(245.767)*		*(264.392)*		*(281.270)*		*(293.498)*		*(303.701)*	
	prime	9.380	*	-35.237		-4.286		-142.593		128.996		563.632	.
	(se)	*(3.860)*		*(280.312)*		*(298.444)*		*(316.710)*		*(329.810)*		*(340.523)*	
	older	10.896	*	-685.445	*	-1034.070	**	-1231.311	**	-1078.882	**	-385.732	
	(se)	*(5.508)*		*(346.416)*		*(368.849)*		*(390.416)*		*(404.221)*		*(414.982)*	

Discussion and Conclusion

While Switzerland's mandatory age-specific occupational pension contributions are relatively rare today, many countries continue to link employer pension costs to worker age—either through firm's pensions' age-weighted contribution formulas or higher voluntary employer contributions as employees age. One reason countries have moved away from staggered systems is concern about age discrimination: rising contributions with age means younger workers receive less compensation and that there is a disincentive to hire older workers. A flat-rate contribution system is perceived as fairer to both younger and older workers.

Rising age-specific pension contributions increase the cost of hiring older workers, theoretically disincentivizing the hiring of older workers and extending older workers' unemployment durations (Scott et al., 1995). However, using difference-in-differences analysis of policy-induced reductions in occupational pension contributions for specific female age groups, we find no significant effect on unemployment duration. If anything, the effects go in the opposite direction: slightly longer unemployment spells.[11] These inverse results may reflect anticipatory behaviour tied to upcoming contribution changes and media coverage, rather than labour demand responses or could be due to selection—as the treatment group in the older cohort was slightly older than the younger, with age differences adjusted only with OLS-not weighted out. Our results diverge from the existing literature, which often examine pension variability at the firm, occupational, or industry level (Daniel & Heywood, 2007; Hirsch et al., 2000), or rely on the impact of direct hiring taxes for older workers (Behaghel et al., 2008; Hakola & Uusitalo, 2005; Ilmakunnas & Ilmakunnas, 2015). Our results likely diverge from the first literature relied in inter-firm, -industry, or -occupational variability, which all correlate with compensation. Our results likely diverge from the second literature because we look specifically at pension contributions, not taxes related to firing older workers—which could well have different effects.

We also no not find support for the theory of compensating differentials, which predicts that lower pension costs should be offset by higher wages (Black, 1987; Woodbury, 1983). Our findings align instead with more recent studies reporting no pass-through of benefit cost changes to wages (Clemens & Cutler, 2014; Lubotsky & Olson, 2015; Schiller & Weiss, 1980).

The most plausible explanation for our null results is that the policy change had a small absolute impact on costs. Even in 2023, half of Swiss women earned less than CHF 4,470 per month (Lampart et al., 2023), meaning the maximum annual reduction in employer pension costs for a median earner today would be just CHF 112 (while in 2004 women's incomes were even lower in real terms). Quite simply, a few percentage points in pension costs is unlikely to affect hiring decisions or discourage labour supply. With respect to pass-through this would align with the absence of evidence for compensating differentials for those with lower incomes (Colla et al., 2017) and leave room for the possibility that one might find evidence among higher wage (male) workers (Goh & Li, 2015).

We acknowledge several limitations. First, the reform was relatively modest: a temporary reduction in pension contributions that phases out for the individual after 2 years. Second, we examine only *decreases* in pension contributions; findings from behavioural economics suggest that employers may respond more strongly to *increases* (i.e., losses have more impact than gains). Third, our study focuses on Swiss women—a population with relatively low wages and high part-time employment. The male labour market, with higher wages and more fluid dynamics, may be more responsive to pension cost changes. Fourth, while we document that employer pension contributions declined overall, we cannot determine whether firms have flatter contribution structures within their pensions. These limitations suggest that it is certainly possible that pension costs matter more in other contexts.

Nevertheless, we would anticipate similarly null results in broader settings. A full flattening of the occupational pension system in Switzerland would imply a universal employer contribution rate of roughly 13%, representing a 5-percentage point reduction for older workers—comparable to the changes studied here. Even for a median male manager earning CHF 136,000 annually, this implies a cost difference of just CHF 3,400. More importantly, changes in wages over the life course dwarf pension cost differences: average monthly wages for men increase from CHF 5,500 in their 20s to CHF 8,090 by their 60s. Age discrimination in hiring is likely real but driven more by wage expectations than modest pension cost differentials.

While economically modest, the changes we study are not politically insignificant. They mirror the scale of the 2024 referendum proposal, which sought to reduce the age gap in employer contributions from 5.5 to 2.5 percentage points. These adjustments are also consistent with the expectations under an age-neutral contribution system—a reform many advocate for on equity grounds. Ultimately, the argument that pension costs drive age discrimination may be a red herring. It offers the reassuring prospect of a policy fix, when the deeper challenge lies in the unrealistic expectation that wages can remain stable or continue rising throughout a worker's career. On the other hand, flattening pension contributions *would* be fairer to younger workers, if there are truly no pass throughs of employer pension contributions.

By leveraging quasi-experimental variation in pension costs, our study provides the first causal estimates of how employer contribution rates affect unemployment durations and reemployment wages—offering a more robust basis than previous studies that relied on within-firm or sectoral variation. We also contribute some of the first empirical evidence on age-specific effects using a national policy change. Taken together, our findings suggest that much of the existing literature may overstate the role of pension costs in shaping labour market outcomes for older workers—and may offer a fragile foundation for age-related pension reforms.

Declarations

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Author Contribution

DH wrote the manuscript and conducted all analyses. DK offered sample code for the processing of administrative data, consulted on the methodology and graphic, and edited several versions of the paper. PN financed the project and offered advice on the final paper. All authors read and approved the final manuscript.

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Data Availability

The administrative data used for this project was received from the Swiss Statistical Agency (BFS) and originates with the Swiss Social Security and Unemployment Systems. While we cannot directly share this data, we can share our data application to the BFS, which can be used to re-request the data for a replication study. Aggregate data on pension contributions is publicly available on the BSV website. We are happy to share our code for both analyses.

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Footnotes

1. An additional critique is that increasing contributions might not optimize labor market supply (Fenge et al., 2002).
2. Research looking at public (not occupational) pensions can use quasi-experimental methods to test the effect of changes in contributions on labor market supply and can consider differing effects by age. This work suggests that young workers are *less sensitive* to contributions or taxes (French & Jones, 2012).
3. Here we draw on an excellent literature review by Haynes & Sessions (2013)—finding no studies on the topic since.
4. There is also evidence with opposite effects for employee pension contributions compared to employer contributions (Inkmann, 2006).
5. There is a much broader literature on compensating differentials covering other benefits such as health insurance (Currie & Madrian, 1999; Fichtenbaum & Olson, 2002; Jensen & Morrisey, 2001), maternity benefits (Gruber, 1995), work-related injury and sickness insurance (Gruber & Krueger, 1991), paid vacation leave (Altonji & Usui, 2007), and family friendly policies (Heywood et al., 2007).
6. Throughout this paper we report in the original currency, CHF. At the time of this writing (2019) 1 CHF = 1.2 dollars.
7. Other key changes included mandatory annual statements to be sent from the pensions to participants, employee representation on investment boards, mandatory pension portability between jobs, lower minimum income thresholds, the option of a minimum partial lump sum payment of 25% (rather than a strict annuity or lump sum), a lower annuity conversion rate (from 7.2–6.8%), and a higher minimum interest rate (2.25–2.50%). The newspaper articles of the time seem to focus almost exclusively on changes in the conversion rate and minimum interest rate, suggesting changes to women's contributions were viewed as less salient.
8. The beginning and end of unemployment spells in administrative data can reflect not an actual change, but rather an administrative adjustment. We assumed two spells of UI receipt directly following one another were a single spell, that the longer of two spells with the same start date was the actual spell, and that the last/first recorded earned income before/after UI receipt is the individual's pre- or post- UI earned income. Finally, we assumed that for two identical employment spells with different incomes, the higher income was valid.
9. Alternative estimates using men as a control group are presented in Appendix 6.2. Results for duration are the reverse of what was anticipated, with the treatment group of women with declining contributions having increasingly long unemployment durations in relative terms. Results for income are largely null and reverse for prime-age workers. These results are likely due to an increasingly strong labor market for male occupations relative to female occupations in from 2003 to 2008.
10. While in the presented results we define the control group as being 5 years away from any contribution change, we also tested a control group just 1–2 years younger/older than the treatment group.
11. We tested another control groups (up to 2 years younger and older than the treatment groups), re-defined the policy treatment period with a short hole around the policy change (in case there was an adjustment period), and tested propensity score compared to entropy balancing.

Figures

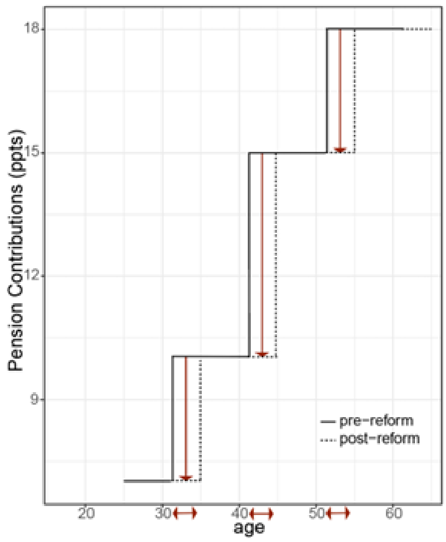

Figure 1

Policy reform in occupational-pension contributions for women

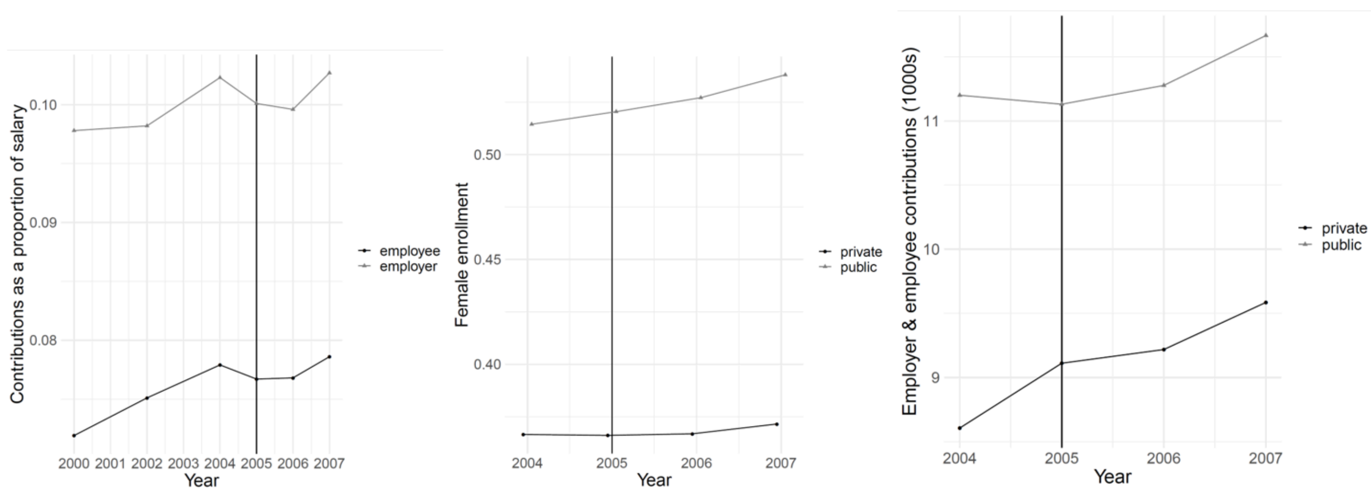

Figure 2

Reductions in pension contributions due to the reform.

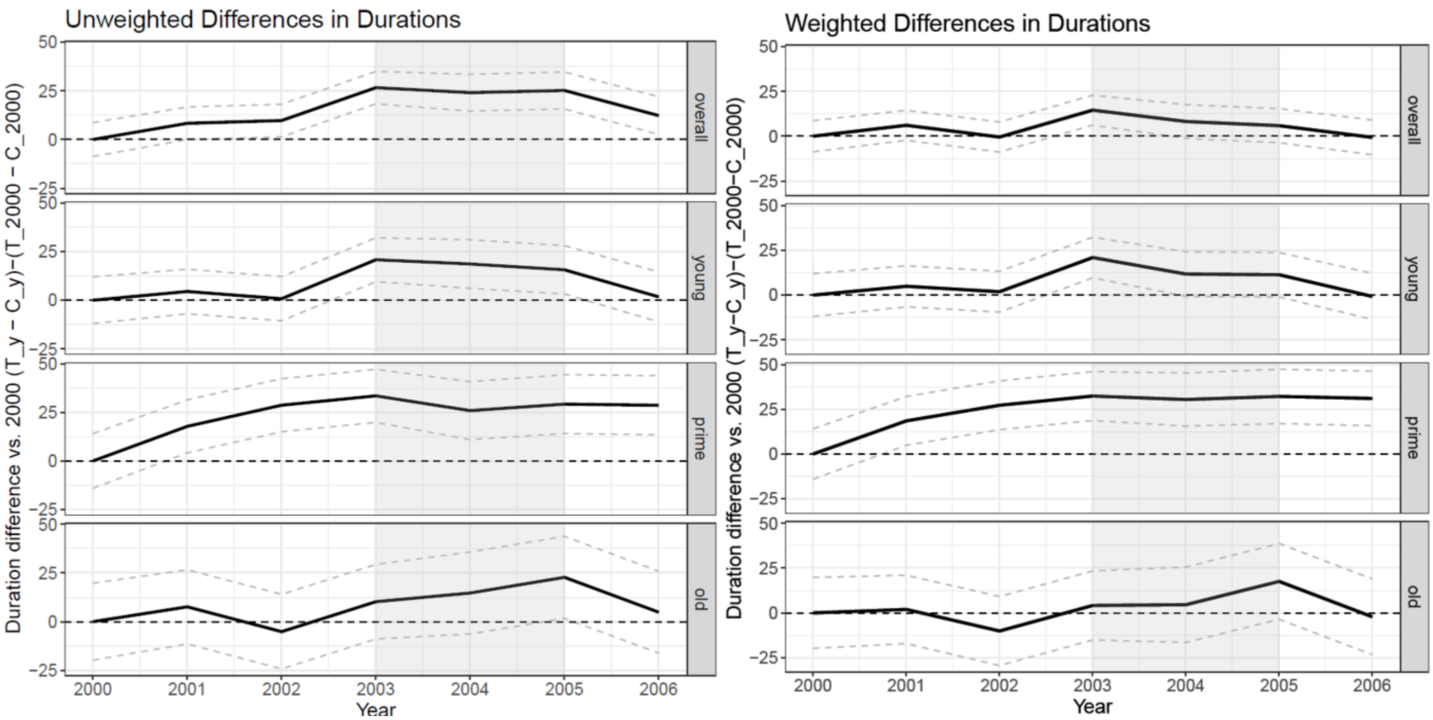

Figure 3

The difference between the unemployment duration (days) for the treated and control groups versus the difference in 2000, by year of unemployment start, raw (L) and weighted (R). Shaded area indicates time from policy announcement to implementation.

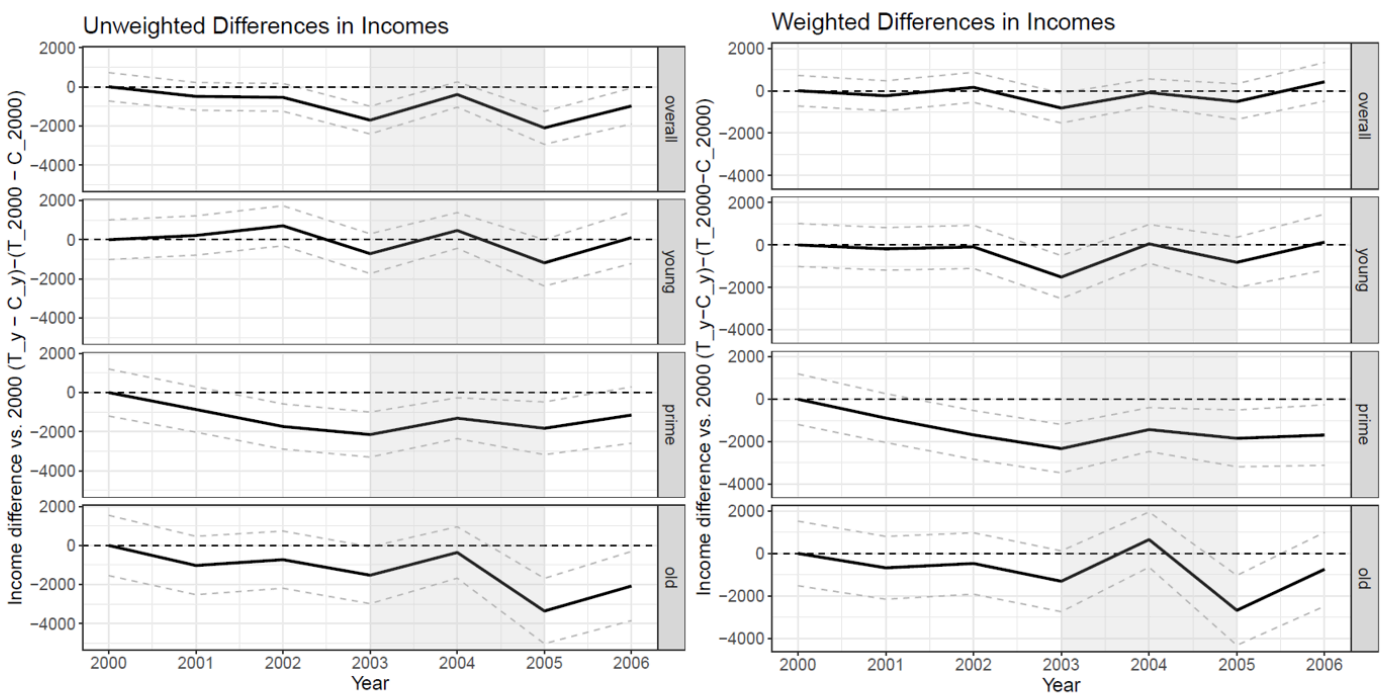

Figure 4

The difference between annual reemployment income for the treated and control groups versus the difference in 2000, by year of unemployment start, raw (L) and weighted (R). Shaded area indicates time from policy announcement to implementation.

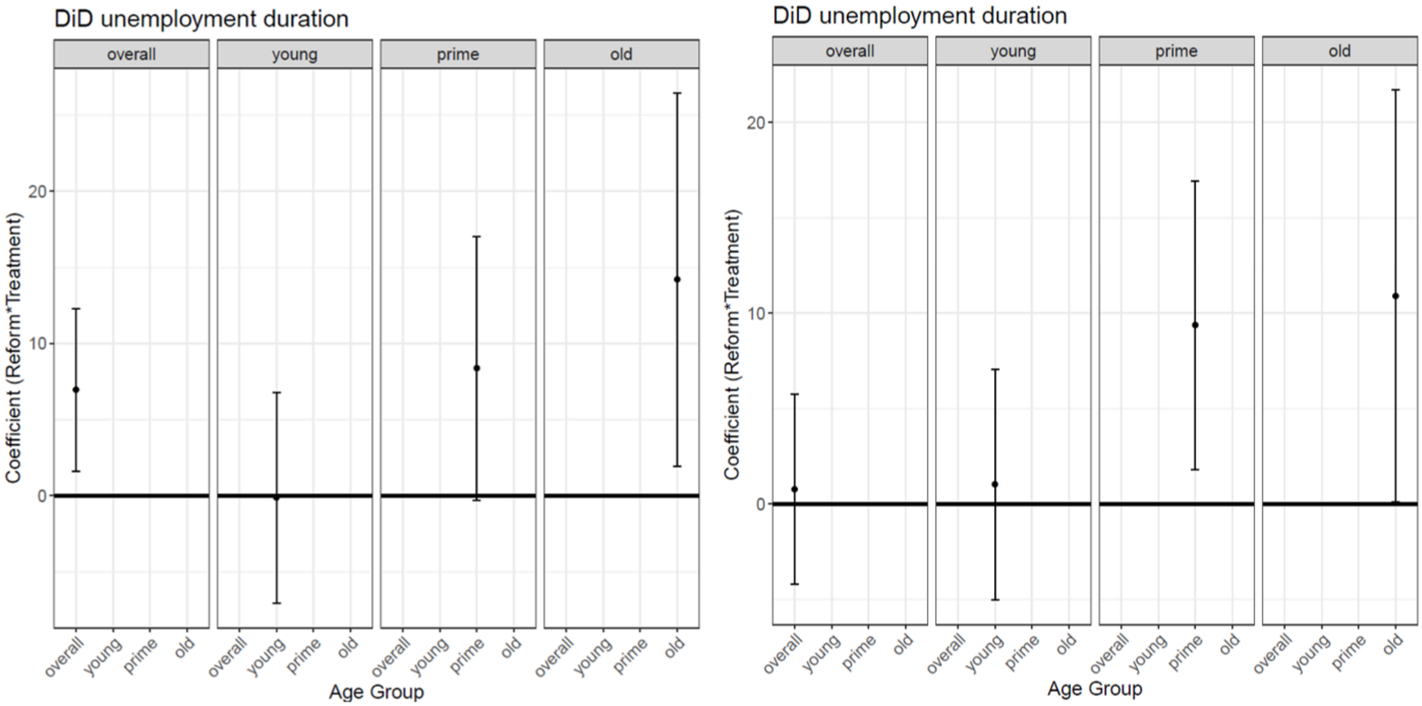

Figure 5

DiD estimates for days unemployment duration. OLS DiD estimate (L) OLS with entropy weights (R)

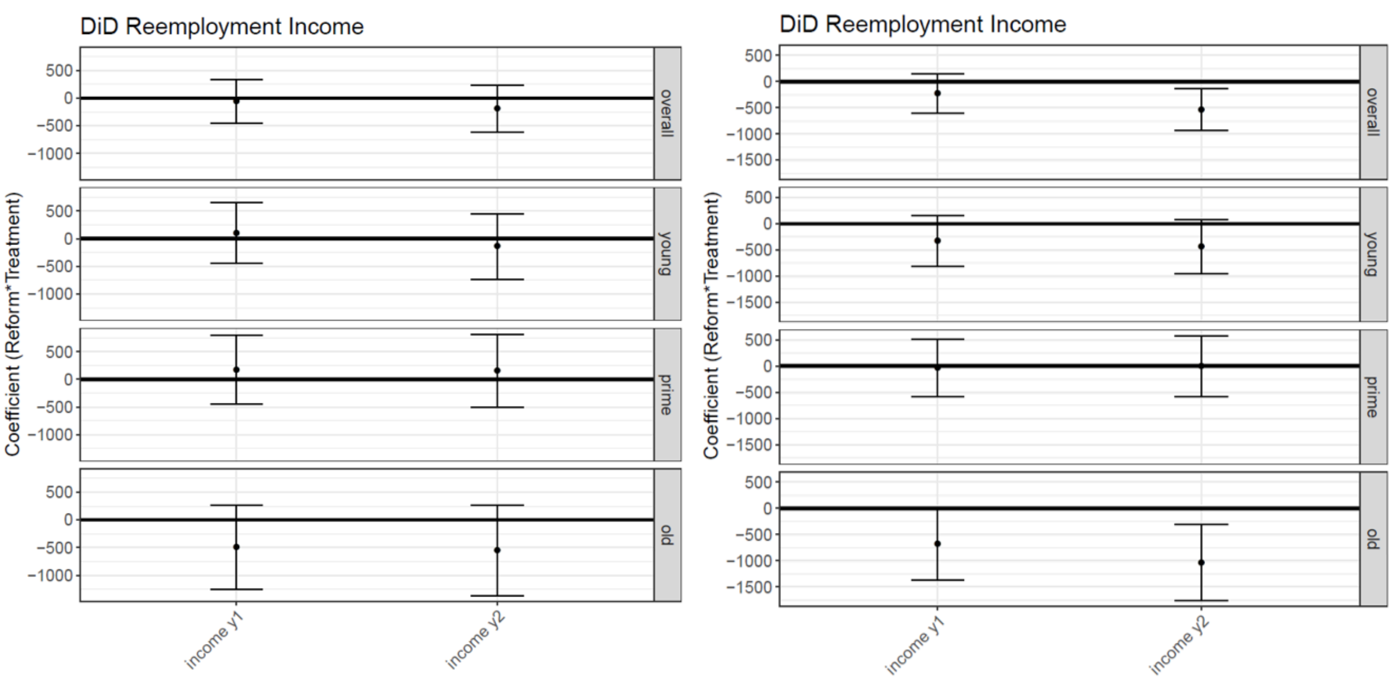

Figure 6

DiD estimates for annual reemployment income in years 1 & 2 following unemployment start. OLS DiD estimate (L) OLS with entropy weights (R)

Supplementary Files

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- Appendices.docx